

FoMRHI Quarterly

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

FELLOWSHIP of MAKERS and RESEARCHERS of HISTORICAL INSTRUMENTS

Bulletin 82

January, 1996

I'm a bit late off the mark this quarter I'm afraid. I've been doing a big consultancy job elsewhere, and had to get it finished. It'll keep the wolf from the door for a month or two. The one snag about being retired – one has to scratch around for the jam to put on the crusts which a pension provides!

An incidental advantage of being late is that we have a far higher number of renewals in than we've ever had before at this stage! Happy New Year to you all.

LOST MEMBER: Anyone come across **Malcolm Prior** recently? He's gone away from Bartholomew Place.

FURTHER TO: comm.1388 Exit Occam. I am afraid that Ephraim Segerman and I will never agree. His reply (C-1388) invites for so many comments and questions, that I will refrain from doing so. But I cannot resist to make just a few remarks. Art scholarship is not concerned with assessing the quality of art works; to quote a well-known art scholar: "There is really no such thing as Art." (Gombrich). The quality of a work of art is an opinion, not the result of scholarship. (Music) history IS concerned with the "why" question, (much) more than the chronological listing of facts and events. WHY did William the Conqueror go to England in 1066? WHY did the recorder not survive the 18th century, but the traverse flute did? Such questions (and answers) ARE important for the interpretation of (music) history. And the methodologies used are totally different from the rules in physics. But to paraphrase Alec Loretto (C-1368): "All arguments have been stated" and I agree to disagree. Amen. Charles Stroom

THINGS AVAILABLE: NEMA Yearbook. Most of you are likely to have received the new Yearbook by now. If you haven't, maybe it's because you've not joined NEMA or are not listed in their register. If so, I suggest that you write to NEMA, Register of Early Music, 20 Wolseley Gardens, London W4 3LP and get a registration form. Although NEMA stands for the National Early Music Association, the Register is getting more and more international and it's beginning to look as though anyone is welcome. Certainly anyone is welcome to become a member. Their administrator is Annette Heilbron, who's in our List of Members under NEMA (8 Covent Garden, Cambridge CB1 2HR).

Condit Manufacturing Co Inc would like to send you an illustrated catalog of specialized tools for making and repairing string instruments and bows. If you'd like one, send name, address, and fax (or phone) to them at 29 Philo Curtis Road, Sandy Hook, CT 06482, USA, saying you're a FoMRHI member.

A letter from David & Michaela Freeman (who are in this country at the moment, though I've not seen them yet) asks me to say that Jiří Čepelák has produced four lute plans for the Muzeum České Hudby, Odd.Hud.Nástrojů, Lázenská 2, 110 00 Praha 2, Czech Republic, a Laux Maler, a Marx Undervorben, a Magno Dieffopruchar, and a Martin Brunner Theorbo Lute. These are

available by post from the museum for an unknown price. **Nota Bene**, however, that the Instrument Collection is still closed!

Two members have excellent books appearing, or about to appear, in paper back. Peter Holman's *Four and Twenty Fiddlers* should be out in paperback from OUP this month, and Bob Barclay's *The Art of the Trumpet-Maker* will be back in print again, in paperback, in April. These I can tell you because I get the OUP publication news. Anyone else who has had such books appearing, let me know, please.

Zuckermann Harpsichords have sent me their newsletter which says, among other things, that there is a **harpsichord email list**. If you're interested, send an email message to listserv@uacsc2.albany.edu with nothing in the subject line and the only message: `subscribe hpschd-l [your name]`. That's an L after hpschd, not figure one, and the square bracket must be replaced by John Smith or whatever your name actually is. Apparently there are already 300 or more people on the list. For **museum style queries** there is also the CIMCIM list. The address for that is listserv@nrm.se and the message is similar: `subscribe cimcim-l [your name]`. Zuckermann also have a Web site: <http://www.geopages.com/Paris/1684>.

METRIC UNITS: I meant to put this in last Q but forgot, and it only applies in Europe, maybe only in the UK since the rest of Europe is pretty slack about European Union regulations. It is now forbidden to use Imperial units of measurement for commercial purposes – everything must be metric. It did get into the papers when it came into force on October 1st because it affected a lot of foods and so forth. Presumably it also affects organ and harpsichord builders? No more 4' and 8' stops – by European command it must now be 1219.2mm and 2438.4mm. However, it only seems to apply if you're selling things, so presumably we can still write about them in feet, and you can use both measurements until December 1999. Any requests for further information and advice should be sent to Gareth Harper, Consumer Affairs Division, Department of Trade & Industry, 1 Victoria Street, London SW1H 0ET. It does apply to 'any contract, agreement, licence, authority, undertaking,' etc and there are swingeing fines for breaches. I don't suppose that they even thought about us. One consolation: 'traders will also be free to serve (though in metric units) customers who ask for goods in imperial units.'

EARLY INSTRUMENT EXHIBITION: All of us who exhibited will by now have had a letter from Jonathan Askey telling us that they're going to try going annual. The next one will be 6, 7, 8 September **THIS** year, and similar dates in 1997. Reactions by then will determine whether it stays annual or goes back to every two years. So make a note of September 1996 now. We are stuck with early September because of term dates at the RCM, and there was general preference for staying there rather than going back to the Horticultural Hall. There is a problem for us: this September clashes with the European Seminar in Ethnomusicology, of which I am President, and I shall have to be in Toulouse. Would anyone like to volunteer to run the FoMRHI stand?

COURSES, CONFERENCES, Etc: Bate Weekends are continuing under the new management, I'm glad to say. 9-10 March will be a **Gamelan Weekend**; 23-24 March will be a **Bow rehairsing weekend**, as will 25-26 May. There will also be a **Bow-Making Summer School**, 4-10 August. More exciting than that, there will be, in term time, **New Saturday morning openings**.

These will apply: 20 January - 9 March inclusive, 10.00 am - 12.00 pm, and 27 April - 15 June inclusive, same times. And in addition, on 25 May, in National Museums' week, a Bate open day, morning opening 10.00 am - 12.00 pm, and afternoon 2.00 - 5.00 p.m.

Also for National Museums' Week, there will be a Pitt Rivers Museum Music Event on 18 May, in the Balfour Building (60 Banbury Road, Oxford): Music and archaeology, 2 - 4.00.

And on either 29 or 30 June, whichever is National Music Day, there will be Saturday and Sunday musical events at the Pitt Rivers Museum in the afternoons, the museum will be open 1.00 - 4.30 p.m. the event to happen 2.00-4.00 p.m. Maybe also something in the Bate on the Saturday morning.

The **Händel-Haus in Halle** is running a Ruckers Conference on 14-15 September. I'm sending Eph a two-page note on it, but mentioning it again here in case there isn't room for it as it looks very interesting. For further information, write to Christiane Rieche, Händel-Haus Halle, Grosse Nikolaistraße 5, D-06108 Halle (Saale), Germany, fax +49 345 50090411.

The **British Clavichord Society** is holding a two-day meeting in Edinburgh, 31 August-1 September, including master-class, recitals, and visit to Russell Collection. Further information will be in their next newsletter, or from John and Sheila Barnes at 3 East Castle Road, Edinburgh EH10 5AP; 0131-229 8018. *If you don't already belong to the Society, Sheila will be delighted to take of that, too. Then you'll get their next newsletter.*

West Dean College is running their annual Summer School in Early Music Performance, with tuition on lute, violin, viol, harp, and voice. Dates August 18-23. Cost £379 if you want to be clean; £359 for a single room without bath or shower. Address: West Dean, Chichester, W Sussex, PO18 0QZ.

Magnano is running theirs too, August 15-25, with Bernard Brauchli on clavichord and forte-piano, and Lorenzo Ghielmi on organ, Georges Kiss on harpsichord, Alberto Galazzo on organology, Jörg Gobeli & Thomas Wälti on organology (interesting, the former in Italian is organologia and this is organaria - we can't do it in English!), and Eva Kiss, voice. Information from Corsi di Musica Antica a Magnano, Via Roma 48, I-13050 Magnano (BI), Italy. There are also concerts around the same period.

NEW PUBLICATIONS: *La Lira di Orfeo* is a new quarterly devoted to bowed instruments and guitars. If you're interested, write to Nello Ascolese, CP 26022 Castelveverde, Cremona, Italy.

I've decided to start publishing from my own collection (which is always accessible on request - 2,500+ instruments, all types except keyboards, world-wide, all periods). First will be *Instruments of the Bible*, rather more detail than in the Bate Handbook I wrote with the same title, and some pictures. I'm waiting for an ISBN. It'll cost £3.00 (plus 50p postage) or \$5.00 (cash preferred, rather than cheque - I can spend a five dollar bill, whereas a five dollar cheque has to go back to America to be cashed!) Including surface postage. Other currencies, with luck there's a note worth around £3.50 for surface postage, or else remind me that I owe you! That's likely

to be followed by sectional catalogues – maybe bells first because that one's part done. Depends on what I've time for and inclination towards, and of course requests for.

USEFUL INFORMATION: David Freeman has also sent an address in Prague that they recommend as a Pension for visitors – they've found it good for their festivals: Jana Vimrová, Pension Jana, U přechodu 7, 160 00 Praha 6, Czech Republic; tel +42-2-350694. It's about 50 minutes pleasant stroll to the Old Town, or an easy tram ride, and rates are very reasonable.

CODA: That's about it unless something arrives while I'm doing the address list. Have a look at my note at the end of that list, and do respond to it.

DATE FOR NEXT Q: All Fool's day as usual, I think (April 1st for anyone not in that tradition) and we'll see if I can be prompter. Maybe I should have kept that bit about metrication for then, but I assure you it's true – 8 pages of single-sided printing sent by the DTI to all trade associations, business organisations and professional bodies in the United Kingdom – that's what our taxes go on, but doubtless they'll recoup the costs with the fines. They don't give the amount of the fine in this, but my memory from the newspaper reports was something ludicrous like £3,000 for a packet of biscuits sold in ounces.

Do enjoy yourselves!

Jeremy Montagu
Hon. Sec. FoMRHI
remember, 171 Iffley Road

Invitation to a Ruckers Symposium at the Händel-Haus in Halle

The Händel-Haus Ruckers harpsichord has been chosen as the centrepiece of a symposium to be held in September of 1996. This instrument was bought from the Neu-pert Collection in 1939, not only because it is a valuable Flemish Baroque harpsichord, but also as an instrument built by the Ruckers family of instrument makers. From the beginning, this harpsichord formed an essential part of the Händel-Haus collection. Georg Frideric Handel himself probably owned a harpsichord made by one of the Ruckers.

The Händel-Haus Ruckers, made by Ioannes Ruckers in 1599, is, according to Grant O'Brien, the oldest known Ruckers double-manual harpsichord. In its original state it was a transposing harpsichord with a smaller compass than normal. In the eighteenth century the compass was enlarged, a change many Ruckers harpsichords underwent. The last alteration to the instrument was made during the restoration in 1963, where the main emphasis was to bring the instrument into playable condition.

Now, thirty years later, our instrument has deteriorated to such a state, that we must again think about a restoration. However, we do not wish to make any assumptions about the aims of such a restoration without reflection. An increased knowledge about the Ruckers instruments and changed standards in the conservation of museum objects require a re-thinking of the very concept of restoration.

We would like to invite you to a discussion on this theme.

The dates are as follows: The arrival date is the 13th of September 1996, with the symposium held on the 14th and the 15th of September, with an excursion to Leipzig on the 16th of September as an additional offer.

If you are interested and if you want to give a paper, would you please to send us a short information as soon as possible. For more details please contact: Christiane Rieche, Händel-Haus Halle, Grosse Nikolaistrasse 5, 06108 Halle (Saale), Germany. Fax: +49 345 50090411.

We would like to discuss the following main topics at the symposium:

1. The Ruckers in Antwerp - construction, design and style of their instruments

First of all we would like to give an overview of the construction methods and musical design principles and the influences on later instrument makers of the Ruckers workshop. Despite the large amount of published material on this topic, we would be pleased to learn of new aspects about this subject.

We believe that the Händel-Haus can contribute with a good documentation of the present state and with some special analyses of our instrument. As a result of this first part of the symposium we would like to establish the original state of our Ruckers as a basis for building a copy. In the context of the original state, we would like to raise the subject of the transposing harpsichords during the symposium.

2. Adaptation to the needs of the musical practice of the 17th and 18th centuries

A discussion of the new directions taken in the sound ideals and the construction and design principles to the alteration of the Ruckers harpsichords, which reflected the musical practice throughout the 18th century, are all important to the evaluation of the altered state of the 1599 Ruckers harpsichord, which underwent a "Ravalement" at this time.

Of equal importance to us is the musical aspect. It lies with us to discover the reasons for the modifications to many Ruckers harpsichords by looking at the changes in the musical practices of the time.

Furthermore, this topic is of interest to us in connection with George Frideric Handel. We know from John Hawkins: "A general history of the science and practice of music", London 1853, p. 912, that Handel may have owned a Ruckers instrument. We should take into consideration that around 1720 Ruckers instruments in London were being modified and modernized.

A small exhibition of copies of Ruckers harpsichords will form part of the symposium and will give a practical musical background in concerts as well as in lectures.

3. The instrument in the museum: conservation / restoration / reproductions in connection with a museum's pedagogical approach

The possibility of a partial restoration of our Ruckers harpsichord will be the basis of a discussion about restoration at past and present. Certain colleagues will speak about their own personal experience of Ruckers restorations in the past.

To think about a restoration of the Ruckers harpsichord brings into question the role of our instrument in the museum up to now, and how to use it in the future. We would especially like to hear about current means of communicating to the visitor a wide range of information about the instruments on exhibition, while allowing active participation by him or her. This pedagogical approach requires a definite framework which could include a restoration. We therefore look forward to an exchange of ideas on the topic "Musical instruments in the museum".

The J J K Rhodes Bursary Fund

St Cecilia's Hall Niddry Street Cowgate Edinburgh EH1 1LJ

The fund will make an eighth and final award for a research project into either the technical or decorative aspects of historical keyboard instruments or into the musical matters that can be illuminated by such instruments. This will normally be expected to lead to a publication of some kind. Preference will be given to projects related in some way to the Russell Collection, University of Edinburgh. Applications for the award of up to £900 should be submitted by April 30th 1996.

Application forms are available from:- RHODES FUND COMMITTEE, ST CECILIA'S HALL, COWGATE, EDINBURGH EH1 1LJ

Mission Statement

The Care of Collections Forum exists to draw together the diverse knowledge and skills that are needed to address the issues surrounding and activities relating to preventive conservation for the care of collections.

Scope

Preventive conservation is central to our aims. However 'Care of Collections' more accurately reflects the holistic, multi-disciplined approach that is to be encouraged.

The *Care of Collections* encompasses the plans, procedures and actions that cover all aspects of their conservation and protection.

The generic term 'collections' describes broadly the facets of our material heritage which we wish to preserve.

For effective long term preservation the issues surrounding preventive conservation and the care of collections must be addressed. These include the use of and access to collections and the potential information they contain, for users now and in the future. We hope to encourage discussion and the exchange of information between all those involved in the care of collections and preventive conservation.

Aims

- To evaluate and disseminate information on preventive conservation in a concise and understandable form.
- To encourage discussion within and communication between all parties involved in the care of the collections.
- To highlight the role of education and training at all levels as critical to the care of collections.

Target membership

We wish to establish as broad a membership base as possible in the public and private sectors. This will enable the group to assemble and disseminate information to the wide range of specialists involved.

All are welcome including:

Archaeologists, Archivists, Architects, Building Services Engineers, Collectors, Conservators, Conservation Managers, Conservation Scientists, Curators, Collection Managers, Designers, Education Officers, Emergency Planners, Energy Consultants, Engineers, Environmental Consultants, Exhibition Planners, Heritage Managers, Lighting Specialists, Material Scientists, Museum Assistants, Museum Designers, Museum Directors, Registrars, Restorers, Security Specialists, Site Managers.

Join Now

The *Care of Collections Forum* is the specialist organisation in the United Kingdom dedicated to the care of collections and preventive conservation.

Although based primarily in the UK, International membership and cooperation is actively encouraged. Annual membership is only £10.00. To participate in the development of the *Care of Collections Forum* please use the application form on the reverse of this section.

The group is actively exploring links with other bodies working in this area. If you have any suggested contacts please forward details to the Coordinator.

Membership Includes

Newsletter

- Articles
- Book Reviews
- Critical debate
- Contacts
- Letters

Meetings, visits and events

Membership Form

Name _____

Occupation _____

Organisation _____

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Fax _____

Annual membership fee £10.00 sterling
from 1st January

Please make cheques payable to
Care of Collections Forum

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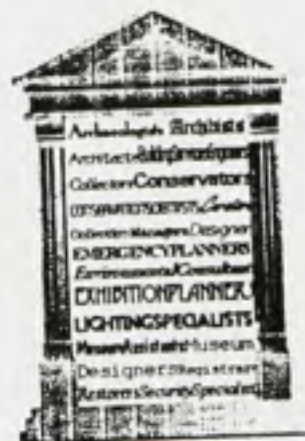
Jane Henderson
Council of Museums in Wales
The Courtyard, Letty Street
Cathays
Cardiff
CF2 4EL
United Kingdom

Contacts

For further information on any aspect of
the activities of the *Care of Collections*
Forum please contact the Coordinator:

Jane Henderson
Council of Museums in Wales
The Courtyard, Letty Street
Cathays, Cardiff
CF2 4EL
United Kingdom

Telephone 01222 225432
Facsimile 01222 668 516



Care of Collections
FORUM

Review of: **MAKERS OF THE HARPSICHORD AND CLAVICHORD 1440-1840.**
 Donald H. Boalch, Third Edition, edited by Charles Mould.
 Oxford, Clarendon Press, 1995. 788 pages; £80 (UK).

The much-heralded Boalch 3 is out! If, like me, you have been eagerly waiting for this for some years, your patience now is rewarded.

No-one should under-estimate the amount of work that goes into a reference work of this kind, though it is also too easy to do so. With disarming candour Charles Mould admits that he did so himself. For, after signing up with OUP in 1984, he found that his initial promise, to supply a completed script by May 1987, was wildly optimistic.

Now, eight years farther down the road, we can at last see the fruits of his labours: 788 pages compared with 225 in the second edition. No pictures this time. A much augmented list of equivalent technical terms (not strictly a glossary as the editor remarks), now including Spanish, Dutch and Swedish - thanks to Dr. A. Roth. Otherwise all of the useful features of Boalch 2 are retained; the geographical-chronological conspectus included.

Makers of the Harpsichord and Clavichord always comprised two basic elements: the biographies of historical makers and the list of surviving instruments. This time they are separated. Part I comprises 210 pages devoted to the lives of the makers from Acciari to Zumpe, while Part II details, in a much more methodical way than before, the basic information about surviving instruments. Some 2000 are recorded, while in Part I, 440 new entries have been inserted, with a further 400 makers subjected to significantly revised entries.

For some readers there will be a disappointment as the early plans for an alternative publication in electronic data format have been shelved. In a way this is a serious impediment because it is not just the professed researcher who might be legitimately frustrated by this.

Anyone who reads Boalch might think of questions that only the database could answer. Were it available, we could very speedily discover: "What is the date of the earliest known English spinet with an FF keyboard?", or "What proportion of post 1740 clavichords are fretted?" Direct access would be a tremendous boon. As things stand Charles Mould offers, in his preface, to conduct such searches on request and suggests that, a year from now, the database will be available through Internet, courtesy of the Russell Collection in Edinburgh. We must wait and see.

In the meantime we must trawl through laboriously to discover the riches or shortcomings of Boalch 3.

My initial examination should be understood in that light; I'm sure that over the coming years I shall find valuable insights imbedded in the new Boalch just as in the old. What a lot of pointers for further enquiry and illuminating little gems Donald Boalch managed to include in his original work! Idiosyncratic sometimes, but often entertaining. How does the new editor's work compare?

First impressions include both pleasure and pain. How good it is to see that many biographical entries are now thoroughly revised and extended to take into account recent researches. Since the 1972 edition, Michael Mietke has come much more into focus, with the Bach tercentenary and the discovery of instruments previously overlooked. The papers by Sheridan Germann and Dieter Krickeberg presented fresh information and the new biographical entry takes due note. In the same way O'Brien's work on Ruckers, and Anne French's discoveries relating to Merlin have each resulted in more complete and accurate biographies. On the other hand one is surprised and dismayed

to find that the laborious research which was reported in the Harpsichord & Fortepiano Magazine as long ago as 1987 has not led to any revision of the entry for Backers. The error concerning the Louis Bas "harpsichord" of 1781 - not in fact a harpsichord converted to a piano but an original and important fortepiano - has been reported at least twice since it was acquired by the Shrine to Music Museum in 1989, but the error remains.

The count of entries marked as new or revised can be somewhat misleading. Many are in fact one line repetitions. For example the little known Utrecht maker Alexander Besocki accounts for 5 of the starred (i.e. wholly new) entries, as he is listed under four aliases. Brosi is almost as greedy with three: Brosi, Brozy, Prosi. And so it goes on. The entry for Antunes is drastically revised but not marked as being so.

Still in Part I, one might object to the appearance of several men who were patently not harpsichord or clavichord makers in the true sense - i.e. they did not make instruments for a livelihood. Perhaps we forgive Donald Boalch his Arnault de Zwolle, and Charles Mould's deferential retention of it, but one can not so easily countenance further aberrations, such as Doublet.

Turning to Part II - surely the part that will be most frequently used though it will inevitably date the sooner - it is a pleasure to see how many newly discovered instruments have come to light. Predictably, the list of Kirckman harpsichords and Hitchcock spinets is vastly lengthened. This is all to the good. Now that we have an adequate sample, it becomes all the more worthwhile to undertake a statistical analysis. One of my initial enquiries, with the new Boalch ready to hand, was to compare the chronological distribution of Kirckman's surviving instruments with those of Burkat Shudi. Some puzzling anomalies occur. So if there is

anyone reading this review who needs ideas for a research project can I suggest one? Further research on Kirckman's output is much needed. A painstaking analysis of surviving instruments might answer a number of nagging questions. Firstly, how many of the surviving "Kirckman" harpsichords are fakes? Quite a number I would wager. Who, besides Robert Falkner, were the fraudsters? Some potential for interesting results exists.

Instruments have also been attributed to Kirckman in recent times (and even had his name added to them!) when the grounds for such an attribution are very insecure. Every such case only serves to obscure or reduce the credit of quite able makers who deserve a better fate. Still more uses could be found for such an analysis, as the careful documentation of specifications and dimensions might also help in affixing a more accurate date to the genuine examples whose nameboards have been removed, for reasons that are not too hard to guess.

A careful analysis of the Hitchcocks' output might clear up a frustratingly murky corner. If the various models and the serial numbers could be properly correlated with their dates, some light might at last be thrown on a highly visible part of our musical heritage. It remains a cause for concern that perfectly serious catalogues should still solemnly pronounce on the work of Thomas the Elder or Younger when the former is only inferred from the unreliable and totally implausible dating of two spinets which can no longer be found.

This perhaps is just the value of Boalch; that it is not only an indispensable reference work, but also suggests areas for future research, either by provoking questions, or by furnishing the initial information that can point the researcher in the direction of known instruments.

Hats off, gentlemen, to Donald Boalch and to his successor.

Review of: Stewart Pollens, *The Early Pianoforte*, Cambridge University Press, 1995, hard back, 298 pages, illustrated, £55.00.

Everyone with an interest in the history of the piano will be seriously thinking of buying this book. It isn't easy reading, and there is a tiresome amount of repetition, but gathered here under one cover is a greater wealth of technical information than has ever appeared in any previous study.

Stewart Pollens is already well known for several significant contributions to the study of the early piano - notably his initial paper on the work of Cristofori, published in the *AMIS Journal* in 1984. This was particularly welcome at the time as it was the first thorough report of the three surviving pianos. Pollens followed this with similar papers on *The Early Portuguese Piano* (EM 1985) *Gottfried Silbermann* (Organ Yearbook 1986), *Giovanni Ferrini* (GSJ 1991), and *Domenico Del Mela* (GSJ 1992). Readers who are familiar with these papers will discover that the greater part of the contents of *The Early Pianoforte* is a repetition of this material. But for those who may have missed any of them, they are all conveniently gathered here under one set of boards, amply illustrated, and well produced: making a much better package for the bookshelf than a sundry collection of scattered papers. The one serious quibble as regards production would be the quality of the photographic plates which are sometimes frustratingly dark and ill-defined.

Looking over his accumulated research some two years ago, Stewart Pollens reckoned that he had almost completed a survey of all the known pianos made up to 1763. Friederici was notably absent; three Pyramide uprights allegedly survived. Then there was the earliest known square piano, dated 1742. So Pollens set off to examine these, extending his travels still further to include a visit to Spain to see for himself the

two grand pianos said to be by Pérez Mirabal of Seville. Not much is added to the book for all this effort. The Pyramides had been grossly restored, and therefore seriously compromised as historical sources. Not one could be affirmatively attributed to Friederici. Then, after examining the "Socher" square piano Pollens has to report his grave doubts about its authenticity. However, the Spanish trip yielded a little new information on the two eighteenth century grand pianos. Nevertheless, whether positive or negative, these findings are all in some sense useful.

To complete *The Early Pianoforte*, Pollens has added to this solid central corpus of information, a lengthy preamble and postlude of much less substance. Two introductory chapters deal with the pre-Cristofori "pianoforte". This leads into areas of speculation about fifteenth and sixteenth century instruments which are not entirely convincing. There is also a final chapter on the piano in France, chiefly taken up with a re-examination of Jean Marius's proposals for hammer harpsichords (which were never built); a wreck that has been pretty well examined in earlier surveys - see Rosamond Harding. None of this speculative or theoretical material adds much to our understanding of the pianoforte as such.

Reading through this sort of unproductive matter one begins to question the very premise on which it is built. What is a "pianoforte"? On page 5 Pollens makes it clear that he takes the inclusive view. For him, any stringed keyboard instrument in which the sound is produced by hitting the strings with a hammer or free tangent may be called a pianoforte. So he is able to discuss Arnault de Zwolle's 1440-ish drawings of a crude and wretchedly inadequate mechanism, which he insists is "not necessarily anachronistic" under the

name "pianoforte". Can we swallow this? The essence of Cristofori's instrument was its extraordinary capability for dynamic inflection, or expressive gradation in *crescendi* and *diminuendi*: such a requirement is omitted in Pollens' definition. The means by which Cristofori achieved it was a hammer action with a clever escapement mechanism. In our efforts to be objective are we perhaps confusing the form with the function? Now, some would argue that the potential for dynamic flexibility is latent in any struck-string mechanism, and like Pollens, they might go further and say that the construction of an instrument with the means, implies that the function that we perceive retrospectively was realised historically. There is a problem here; and it concerns not just pre-Cristofori hammer action instruments (if we can show that they existed), but also some eighteenth century instruments.

There are difficulties of another kind when we come to Pollen's second chapter. Here he quotes, in extenso, four letters written by Hippolito Cricca in 1598, which refer to the *piano et forte* owned by a nobleman of Ferrara. There is no actual description but it appears to have been some unspecified type of keyboard instrument with an organ underneath. The texts give no hint whatever of the actual mechanism, but on the basis of the name alone one might conclude that, either alone or in combination with the organ, it was capable of playing loudly or softly. But Pollens proceeds from there to a statement that this instrument was capable of dynamic gradation (p.27) or dynamic flexibility (p.31). This is a quite a large step, even by Neil Armstrong's standards, but Pollens does not seem to observe himself taking it. He also proposes that the instrument most probably had a hammer mechanism.

Completing his prehistory section there is a description of the little Bonafinis pentagonal *spinettino* in the Metropolitan Museum (about which he has previously written in JAMIS). This began as a sixteenth century

miniature virginal, originally fitted with a conventional plucking action, but later converted to a free tangent action (of the crudest possible kind) perhaps during the seventeenth century. What was the purpose of this? To obtain dynamic nuances? Or to do away with quills? Pollens does not address this question.

Nevertheless, taking Arnault, Cricca and Bonafinis together Pollens' resultant thesis is quite breath-taking: 'Bartolomeo Cristofori is credited with being the "inventor" of the pianoforte', but 'this is clearly not the case'. (p.5)

In the introduction the author states that his cut-off date is 1763. He apologises that this "leaves us with very little material on England". Rather curiously we then find that this self-imposed boundary is freely ignored; not that we mind. On pages 137ff, for example, there is a useful description of the Antunes piano dated unequivocally '1767'. And of course, we do wonder about the dates of the anonymous instruments and the assumption that the author is tacitly making. However, after his apology concerning the English piano, it is surprising to find that in the five page "Conclusion" at the end of the book, after strolling along very predictably summarising for about a page, he suddenly launches into a totally unexpected and very ill-considered outline of the origins of the early piano in England. Here, some of the statements are just plain wrong. For example: "As Burney indicates, the earliest pianos made in England were those of Zumpe and Pohlman". But Burney expressly says otherwise in the very passage that Pollens quotes. Then, on page 228 we read: "English-made pianos began being exported to the continent by around 1760". Yet four pages earlier we were told: "Before the 1760s there is no piano making in England". Obviously this has not been thought through carefully, so it makes a sadly inappropriate end, after all the careful documentation that went on before.

Review of: Stewart Pollens, *The Early Pianoforte*, Cambridge UP, 1995. 297 pp, illus with photos, drawings, tables. £55.00 (\$84.95). ISBN 0 521 41729 5.

This is a complete survey of the earliest pianos, from the beginning, a point to which I shall return shortly, to the 1760s or so. It is admirably detailed with much technical information provided with equally admirable clarity.

It begins with the pre-Cristofori hammer-action instruments. The first, of course, is Arnaut de Zwolle's hammer action which could be fitted to harpsichord, clavichord, or dulce melos to make any of them sound like the last named. Pollens raises the possibility that such actions may have survived continuously thereafter, if only rarely and in a few places. He continues with a discussion of the possibilities of the tangent piano. This is a curious term, with which we are stuck because it has been used for so long. The true tangent, that of the clavichord, is fixed to the distal end of the key and touches the string, as implied by the true meaning of the word tangent, and remains in contact with it so long as the note endures. The tangent of the tangentialflügel, or tangent piano, on the other hand, is a free tangent, a loose piece of wood, more nearly resembling a harpsichord jack without a quill, which, like a jack, rests on the key but is not attached to it. If the rise of the key is abruptly stopped by a rail or other device, the "tangent" flies on and strikes the string, rebounding so that it acts as a free hammer, but without any of the subtlety achieved by a true piano action. Like any hammer, it can hit gently or strongly (*piano e forte*), but it isn't capable of any very subtle gradations of volume, nor is it in any sense a true piano mechanism, nor really a tangent action. It does, in fact, return to Arnaut's dulce melos or hammer dulcimer in conception and effect.

Pollens is undoubtedly correct that there were forms of hammer action long before Cristofori, but they were not pianos. It is, as so often, a matter of semantics and definitions. He cites the main instrument classification systems, that 'the pianoforte is defined as a keyed chordophone whose strings are struck'. Fair enough, but surely there is more to a piano-

forte than that. I think that most would agree that the piano, as distinct from a keyed dulcimer, needs the refinements that Cristofori gave it, and that one does need to distinguish between the true piano and its undoubted precursors. Pollens comes out firmly in his Introduction: '... Bartolomeo Cristofori is credited with being the "inventor" of the piano. This is clearly not the case ...' Well, yes and no. It depends entirely on what you mean by "the piano" doesn't it? Of the hammered keyboard string instrument, certainly he was not; of the true piano, certainly he was. It is good and refreshing to have these reassessments, and especially good to have so clear an explication and translation of Arnaut's manuscript.

One of the early instruments he mentions is the Bonafinis tangent-hammer action spinettino (small virginals) in the Metropolitan Museum in New York. As it happens, I have very recently been examining the only other instrument which has been suggested to be by the same maker, the virginals labelled Doanusa Benismis (see GS/ XVII). This is now in the Cambridge Faculty of Music. It is plainly pretty fake. Some of it is probably sixteenth century, but it was several different instruments! And much of it looks late nineteenth century. Whether it and/or both it and the Bonafinis passed through Franciolini's hands, I don't know (there's no trace of either name in his catalogues), but if not I suspect that there were other dealers of similar propensity. Certainly I would hate to use anything that looks like the Cambridge instrument as the first evidence of anything. But then I've not seen the New York one (and the photos of it in this book, like a number of others there, are so dark that nothing can be seen at all of any of the inscriptions to which the captions direct our attention), and it may be quite unfair to tar it by the same brush.

Why, in this day and age of high technology, are illustrations almost always so much worse than they were twenty years ago? I've lost all count of the books where I've complained of this. It began with the first flush of reprints,

and then, in the early days of photo-reproduction, we had to get used to plates of murk on murk. To an extent it was fair enough – the technology was beginning and it was wonderful to have books from people like Da Capo that had been unavailable for so long. But now it's coming with first-run books, and from what we thought were first class publishers, too. I've complained of it from Batsford – Batsford a famous art-book publisher, and from OUP, and now from the other of our two great university presses. What's wrong? Lack of skill? Lack of care? (it's only music, anything will do?). Certainly it is the publisher at fault, and nothing to do with the author, who, I'm willing to bet, is even more marked by it than I am. Maybe he should have published with Yale, who publish my sister's books on Italian sculpture – admirable plates, with excellent clarity. If Yale can do it, why can't Oxford and Cambridge?

What is wonderful to have in this book is enlarged and revised versions of Pollens's articles in the *Journal of the American Musical Instrument Society* (on Cristofori's pianos and the Bonafinis virginals), *Early Music* (on Portuguese pianos), *The Organ Yearbook* (on Silbermann pianos), *The Galpin Society Journal* (on the pianos by Ferrini and Del Mela, Cristofori's successors). These are the most important articles of our time on the early piano, and now not only to have them all available together in one book would be exciting enough, but to have them expanded and revised is even better.

The chapters on the Italian and Iberian instruments follow those on the early tangent instruments and they are very detailed, with some illustrations which I do not remember seeing in the original publications. Especially interesting for comparison are the detailed drawings of the actions and tables of measurements which come at the end of each section. Particularly valuable, too, are the photos of action parts, such as hammers and escapements, all taken from the same aspect, which makes comparison between the work of different makers so interesting.

The next chapter, which is on the piano in Germany, starts by taking us back to the earlier problems which I discussed above. The first hammer action instruments were tangent pianos, imitating Hebenstreit's Pantaleon, a giant dulcimer, and once again they should not be considered to be true pianos. Like the instruments similar to the Bonafinis, they are not capable of any of the subtleties of Cristofori's action. One tends, of course, to forget (this goes beyond the period of this book) how quick many makers were to forget some of those subtleties, and how long it took, a century or so, for piano makers to reinvent some of the details of Cristofori's instruments.

The descriptions of the Silbermann pianos are of course important and valuable, but the other German true pianos are valuable only in the negative sense. The "earliest existing square piano" (by Socher in 1742, according to the label) Pollens has now assigned to the 1790s, and the Friederici pyramid uprights look to be by three different makers, one perhaps by Friederici, and at least one of the others probably being much the same date as the 'Socher' square. Negative evidence can be just as important as positive, and Pollens gives us all the figures and other information we need.

A short chapter on the French instruments is useful in giving all the details of the Marius non-piano (the hammers were solid with the keys, so it could never have worked – perhaps it was as well that Marius never got round to producing it!

In sum, this is an essential book (despite some flaws, for which see the accompanying review by Michael Cole, far more expert than I on this subject), as Michael also says. It is, too, rather more reasonably priced than many Cambridge publications. The same cost of £55, or something close to it, has been charged for other books much slighter and with less illustration than this. While the older University, to which I belong, usually manages to keep prices rather lower, on this occasion, the importance of the book is paralleled by a not unreasonable cost.

Review of: *Larigot, Bulletin de l'Association des Collectionneurs d'Instruments à Vent, IV & V Spécial & no.16. Available from 93 rue de la Chapelle, Apt 166F, F-75018 Paris. Subscription 150 FF.*

I have been neglecting *Larigot*, as well as the Edinburgh catalogues, and my apologies for this. I have three issues here (I hope that I've not lost anything between them in the move homeward from the Bate, but I suspect that I may have done).

The two November issues are both makers' catalogues, that for 1994 the Lecomte catalogue from 1907, issued by Ch & J Ullman who had taken them over. Fascinating what was still regarded as normal in Paris at that date: 6-key clarinets, 5-key flutes to D only, système 3 oboes, Stölzel valve cornets. Of course they made Boehm flutes, too, both conical and cylindrical, and Boehm clarinets and Boehm and Conservatoire oboes (but no thumb plate). Saxophones all look as though they have the low B♭ key, though it is specified on some and not on others. Horns were valve horns only (French model with Périnet valves and crooks of course), but one could order a set of ten crooks & some people must still have played on the hand. Tenors were the only size of trombone on offer – no alto and no bass, but there are four models of valve tenor trombone, all available in either C or B♭. You could still buy ophicleides, either B♭ or C, as well as tubas. These last were all called saxhorns and ranged in size from petit bugle in E♭ (no suraigu in piccolo B♭) through E♭ alto, B♭ baryton and B♭ bass (both narrow bore, but clearly wider than the baryton, and wide bore, what we'd call euphonium), contrabasses in E♭ of various shapes, to contrabasses in B♭. What's missing is the five-valve French C tuba.

November 1995 is a Hawkes catalogue of slightly later date – it was a pernicious habit of the English makers not to date their catalogues, but this one includes 'The 20th Century Clarinet' invented by Cavalier Papeschi with the patent number (11841), and the date for that patent is May 21 1907. No Stölzel-valve cornets, but there are handhorns as well

as valve. They offered Roman buccinas in a variety of keys and Aida trumpets. Trombones came in all three sizes, either valve or slide (I've got a valve bass but I've never seen a valve alto). Flutes seem to have no less than 8 keys, and clarinets no less than 13 keys. Oboes all seem to have the butterfly key.

Because *Larigot* is the organ of the Association des Collectionneurs d'Instruments à Vent one only gets the wind sections (and percussion – we're presumably honorary wind players). A pity because the string sections are usually also interesting, but far better to have just the wind than nothing.

The intervening issue that I have is February 1995 and that includes the woodwind section of the Husson & Buthod catalogue of 1856, quite well up to date with conical Boehm flutes and Boehm clarinets, as well as simpler systems. Not the Albert clarinet – the simple system has a brille for the right hand but only the third-order lever for the right little finger and long levers for the left little finger. While the cor anglais has a brille, the oboe hasn't, nor a whisper key – it does have an octave key – eight keys in all.

This is followed by an article on one of the first Selmer bass saxophones, and that by an important one by Jean Jeltsch on Baumann clarinets.

While the articles in *Larigot* are always interesting, it is for the catalogue reprints that I, for one, am most grateful. These are entertaining, when looking at the prices, useful for identifying anonymous instruments, and extremely important for assessing who was playing what where and when. If Hawkes list their handhorns as suitable for military bands or orchestras, this tells us something about some military band parts of the early twentieth century. Equally, if only tenor trombones were available in France in 1907 that tells us something too.

All thanks to *Larigot*.

Review of: *Historic Musical Instruments in the Edinburgh University Collection*, ed Arnold Myers. 'Cornets & Tubas' (£5), 'Oboes' (£4), 'Clarinets' (£5), 'Stringed Instruments of Regional Cultures Worldwide' (£3), 'Viols & Violins' (£4). Each fascicle plus £1 postage in UK, plus £2 postage surface abroad. Each is also available (same prices) as a DOS file on 3½" disk, either ASCII or WordPerfect 5.1. Available from The Curator, Edinburgh University Collection of Historic Musical Instruments, Reid Concert Hall, Bristo Square, Edinburgh EH8 9AG, Scotland

There is a whole batch of fascicles of the Edinburgh Collection catalogue here because Arnold made me promise not to touch them until I had written a much overdue review for him for the *Galpin Journal*. That is now done so my conscience is clear, there at least.

Since I think we can all agree on the value of these catalogues and can also all acclaim the speed with which Arnold Myers is publishing them, a speed and a general quality which puts all the rest of us to shame (Arnold is the part-time honorary curator, yet he is producing far more than those of us who are full-time, full-paid), I intend to go straight (if *straight is the word after a sentence like this*) to detail comment, in other words nit-picking, beginning, because that's the one that was on top of the pile, with Cornets and Tubas.

'CORNETS & TUBAS'

This fascicle, which was compiled by Arnold Myers with measurements etc by Raymond Parks, will complete the Brass instruments, with a final fascicle on the mouthpieces to come. We've had Horns & Bugles and Trumpets & Trombones, or as they are called in the illustration volume, Conical Brasswind and Cylindrical Brasswind. This third fascicle is the Intermediate Brasswind. What's going to happen when they get a modern trumpet? Be honest, perhaps, and put it with the cornets? They didn't with the horns – there's nothing very conical about a Lidl compensating double horn – all the extra tubing to go from the B♭ to the F has to be cylindrical.

There are, as always, odd points of detail. If an instrument has a tuning slide, what is its pitch? I would assume with the slide right in,

variable to as far out as is safe. But a posthorn here with a tuning slide plays at A=440 "tunable plus or minus 50 cents", which suggests that the stated pitch is with the slide half out.

It's a pity that we don't have tube lengths, at least for some of the instruments. One posthorn is overall size 348mm, nominal pitch B♭, two coils, 5 straight parallel yards. So what does it look like and which B♭ is it in? I think it looks like a small twice round trumpet. Which B♭ is more difficult to work out. 348 times 5 (no allowance for 4 semicircular bows, but never mind for the moment) is 1740 mm long. Divide by 25.4 gives us 68½ inches, and divide that by 12 gives us 5¾ ft; add say about an inch and a half for each of 4 bows and we are over 6 ft. B♭ is either 4' 6" or 9'. This seems to be E or E♭. For an instrument that's obviously a funny, tube length or a little more information would help a lot. And there's no point in being with-it and using only metric in a brass catalogue; anyway the introduction, very sensibly, refers to 13½ ft E♭ and 18 ft B♭, as we all do (how do they manage on the Continent in normal conversation I wonder? 2743.2 millimetre si bémol? Gets a bit hairy I'd think).

Another that I'd love to know what it looks like is the Knapsack tuba. The overall size is only 516 and it's a BB♭, ie 18 foot nominal tube length (5486.4 mm).

The Nominal pitch terminology seems to be confusing. Some instruments (we're still on cornets) are B♭, some are B♭ etc. The difference seems to be whether there is one shank (B♭) or no shank (also B♭) or more than one shank/s and/or crooks (B♭ etc). But

where there is provision for shanks, all could be B♭ etc because even if only a B♭ survives, there could be any others missing, certainly, as was very common well into this century, an A. Agreed it's important to know that the conventional B♭ shank does produce B♭ and not C (the shank was usually the same length on the two instruments – it is the main tubing that usually differs), but other than that one can't know.

I think this is enough; there other details that puzzle me, but I don't want to give a false impression. There is an enormous of extremely useful information, usually very clearly expressed. It is a very impressive collection, very wide-ranging and pretty comprehensive. I can see some gaps, as in any collection, but there are very few serious ones. I would guess that Edinburgh is now the premier collection of brass instruments in this country and I hope that this may long continue.

'OBOES'

Turning now to the Woodwind and starting with the oboe. This fascicle was written by Simon Milton and includes shawms and similar instruments. The second instrument listed is one I remember writing about in a Q or a Bull many years ago, Charles Foster's stille shawm, which he built from the description of the *Mary Rose* instrument and brought to one of the Early Instrument Exhibitions. Another interesting shawm is a Buffet Crampon 'North African', fork and all. It's not clear to whom that belongs; it's catalogued as Langwill Collection, but whether that was bequeathed to, loaned to, bought by, EUCHMI is nowhere stated here (OK, it is stated in Vol I, but will everyone have a copy of that to hand? Langwill Collection Gift or something to that effect would save a lot of wondering. Another small cataloguing detail is the introduction of a variant on Phil Young's terminology (4900 *Historical Woodwind Instruments*). Phil uses SATK for Spring Attached To (the) Key, or SATW (wood) or SATI (ivory). Here we have SATB (body) – saves

worrying about what the body's made of, but I think it's slightly less elegant.

Again an impressive collection, combining as it does the Rendall and Brackenbury collections as well as others. Cataloguing is very clear in all details, and many instruments are available as plans by Dick Earle. EUCHMI and Bate are the undoubted leaders in this for wind instruments.

'CLARINETS'

This fascicle is by T K Dibley, and I'm afraid that I do have one immediate dispute with Arnold as editor. Where Simon Milton carefully distinguished between duplicate keys (two keys which did the same job) and alternative touches (two levers which operate on the same keyhead), Tom Dibley distinguishes equally carefully between alternative keys (two keys which do the same job) and duplicate touches or levers (which operate on the same keyhead). Personally I vote for Simon Milton, but it doesn't make a lot of odds which way one chooses so long as one is consistent. It's when one fascicle goes one way and the next the other that one gets confusion.

Terminology is otherwise extremely precise, though I'd have thought that the brille is well enough known under that name to use it instead of spectacle-key, and I'm not sure about hinge-key – would it not be more precise, and more accurate for that matter, to refer to a third-order lever as Philip Bate does (*The Flute*, p.156, note 4) in his discussion of the Siccama flute? Equally, I've never seen any objection to using engineers' terminology, as for example 'female mouthpiece' rather than the 'blackwood socket type' used here (no. 1722) for the sort of clarinet that has the tenon on the barrel instead of on the mouthpiece. Where there is common technical terminology one might as well use it.

As one would expect, this is one of the most important parts of the collection, based as it initially was on Geoffrey Rendall's Collection. It is, too, more detailed in some respects than other sections. For example, we have in the two previous fascicles met many

instruments from the Brackenbury Collection, which came to the EUCHMI as a Government allocation. Here Tom Dibley usually gives us the price that Brackenbury paid (eg £0/6/0 – six shillings, thirty pence for younger readers in the Gallowgate in Glasgow for a 6-key C clarinet). Unfortunately we don't have this information for any of the Rendall instruments that I've spotted, not even for those bought at the Nettlefold sale, where of course there must be a record. It's not information that's of great importance, but it is interesting to see how such things have changed, and it makes the rest of us envious.

The large number of bassett horns is a particular feature of the collection, interestingly all in F here (I remember an article a while back of Nick Shackleton's in *GSJ* 40 pointing out that they do often come in other keys) and it is interesting how few, on the other hand, there are of basses; we have far more in the Bate Collection. One slight surprise in the terminology on the larger instruments is a Tenor in F compared with an Alto in E♭.

'STRINGED INSTRUMENTS'

Here the author is Peter Cooke, and correspondingly, just as a number of clarinets were lent by Tom Dibley, and of brass by Arnold Myers, so most of the Ugandan instruments, and some others were lent by Peter Cooke. This is always a generous act, but it is also a dangerous one. It's not so long ago that I had to raise £50,000 to keep four instruments in the Bate Collection. Probably I should not talk because I've got some of my collection on loan in the Bate and one day Hélène may have to raise money if she wants to keep them. Let's keep our fingers crossed and wish Edinburgh luck and all well with this, but at present they have got a very high proportion of loans.

This fascicle is arranged, as it should be, in Hornbostel-Sachs classification order, with the H-S numbers. Many of the instruments are of respectable age, and many of these were included in the 1885 International Inventions Exhibition. A number too are in the beautiful

Hipkins & Gibb *Musical Instruments, Historic, Rare & Unique*. Some are as early as any non-European instruments in collections in this country, and important therefore. We don't have the *Kunstkabinett* collections of exotica here that there are in some Continental capitals, unfortunately, and Captain Cook was about the only early explorer who bothered to collect such material. We are lucky in Oxford in having some of his material in the Pitt Rivers Museum, and also some Trades-cant material on loan there from the Ashmolean, but on the whole Copenhagen, Vienna, and elsewhere can do a lot better than we can here. So it's good to have the catalogues of this material in Edinburgh, which otherwise was only known through Hipkins's catalogue of the 1885 exhibition and his 1888 picture book.

'VIOLS & VIOLINS'

This fascicle is by Darryl Martin and it covers all the European bowed instruments, and also tools for making them, but not bows, which will appear in a separate fascicle.

There is only one violin in original state (described by Kenneth Skeaping in *GSJ* 14); I remember that we let Yehudi Menuhin play it when he opened the Galpin Exhibition there in 1968 – it was the first time he'd ever played an original instrument and he made some quite interesting comments, particularly that he didn't feel the lack of a chin-rest because, with the wedge neck, he felt that he was squeezing it into his neck and even when shifting position down he still felt secure. There is also a bass which looks original (it's difficult to make out from the description, but there's a photo in Vol.I). Everything else is in modern state. The more recent instruments include the complete set of The New Violin Family, produced by Carleen Hutchins, which were at the Royal College of Music for some years and then in Cardiff. It's good that they are still in this country and accessible for research – whether they are accessible for playing, which is their main purpose as products of the Catgut Acoustical Society, I don't know.

The viols and d'amores are, of course, all in original state, though at least one has been fairly heavily repaired. There are three basses, one of them a small division viol, and four d'amores, one of them possibly a seven-string treble since it has no sympathetic strings. There are two cither viols and four hurdy-gurdies, one of them possibly seventeenth century (eighteenth century in the illustration volume) of which a measured drawing is available.

There is quite a lot of odd bits, including a large boxful from the Brackenbury Collection, and some violin-making tools from J & R Glen. I remember them as bagpipe makers

(of course) and a general instrument shop (I bought a tenor trombone off them and a Guards model side drum way back in the 1950s) but I'd not realised that they were violin makers as well.

As with all the Edinburgh Catalogues, there is much information in all these fascicles, compact and concise, but usually clear and adequate for initial purposes. Further information is always available from Arnold and there's certainly enough here to tell you whether it's worth asking for more. We must all be grateful to him for all that he does – would that the rest of us had the same energy.

FoMRHI Comm. 1407

Cat(apult)lin(e)s

John Catch

"Saturday, August 6 [1715] – Played upon my viol. Read some of *Catiline Conspiracy*". (Diary of Dudley Ryder; see *Journal of the VdGS America*, 1984, 21, p.65).

No-one I trust will infer that cat(i)lin(e)s date from 62 B.C. More seriously: that unhistoric spelling catline with its misleading suggestion of cordage of some kind is unfortunate, for it prompts misleading conjectures about the meaning of the term. Let me offer some parallels: codlines, purlines, poplines, saplines, couplines, kidlines ("blithe and merry"), and the commodities trafficked in by the elder Osbaldistone ("Rob Roy", Chapter Second) called titlines. If only we had a hotlin to Kirtline Mr. Jenkins would be able to tell us just how catlin(g)s really were fabricated.

The Auloi of Porphyrius: some thoughts about differences in pitch between wider and narrower pipes of the same length.

Last year I got a question from mr. Waszink of the University of Leiden (Netherlands) about a text of Porphyrius (234- c. 305 AD). This Greek philosopher wrote a book with comments upon the "Harmonica" of Ptolemaeus:

εἰς τὴν ἁρμονικὴν Πτολεμαίου ὑπομνημα

On page 34, line 11 to 21 (Edition Düring, Göteborgs Högskolas Årsskrift, 37, 1932), the text is (translated in English):

"And again; if you take two Auloi, of the same length, but different in the width of the bore, such as the Phrygian are related to the Greek (Auloi), then you will see in the same way that those (instruments) with the wide bore produce a sharper sound than those (instruments) with a narrow bore. For we see that the Phrygian (instruments) are narrow in the bore and produce much flatter sounds than the Greek (instruments).

And also in this case the velocity of the movement is responsible. For in (Auloi) with narrow bores a slower movement of the air occurs, which flows slowly and is hampered by the small diameter of the bore; and on one which has a wider bore, for here is no hampering at all, the flow of the air is faster; it is even possible to observe this same (phenomenon) on one Aulos."

We have to know that the Aulos is a double instrument, consisting of two pipes, so we can better speak of "Auloi" (Greek, plural for Aulos) or "Tibiae" (Latin, plural for Tibia).

Well, one question was about the difference between Greek and Phrygian instruments, and I myself could not help Mr. Waszink, because I never made studies about instruments of the Greek and Roman civilisations. Jeremy Montagu however told me that the Phrygian Aulos was the one with one straight pipe and one curved (and longer) pipe.

The other question (also interesting for woodwind makers nowadays) was about the differences in pitch between instruments with a wide and a narrow bore. Because, we know now (and I checked it on some brass pipes with different diameters) that on recorders and traverso's the wider pipe has a lower pitch than the narrower pipe. I made this observation blowing two pipes of the same length over the top corner, as on a pan flute (stopped and open).

However, when I played the same pipes with a double reed (of an baroque oboe), the pipe with the wider bore was clearly sharper in pitch than the other pipe! A bagpipe maker told me that this also happens on his instruments: reaming a pipe will give a higher pitch.

Conclusion: the observation of Porphyrius makes it evident that the Auloi were played with a reed, and not as a flute (blown over the open end). This is not a new discovery by me, it is only a proof what other people already assumed.

But now, what is the reason that there is a difference in pitch between pipes with different bores? The remark of Porphyrius sounds reasonable: wider pipes have less resistance to the flowing and vibrating air. And we know: on a recorder or traverso, the pitch will be sharper if we clean or polish a rough bore. But there is another point, and that is the end-correction. The length of the

vibrating air column is somewhat longer than the length of the bore of the instrument. Otto Steinkopf ("Zur Akustik der Musikinstrumente", 1983, Edition Moeck No. 4029) has calculated that on a renaissance-traverso with a cylindrical bore with a diameter of 18.5 mm the end-correction at the lower end of the flute is about 6 mm, on a flute with a diameter of 21.5 mm this end-correction is about 7.5 mm. So: the wider pipe has a longer end-correction, that will give a longer vibrating air-column and therefore a lower pitch. For a traverso there is also an end-correction at the mouth-hole. Steinkopf gives on page 18 of his book a complicated formula, but the result is that the mouth-hole correction is about proportional to the diameter of the bore.

My conclusion: the effect on the pitch of the end-correction on (cylindrical) flutes and recorders is evidently more important than the other effect, described by Porphyrus.

However, it is more complicated to find a solution for the observation that just the opposite effect occurs if the pipes are played with a reed. It does not matter if that is a double reed (oboe, shawm) or a single reed (clarinet, chalumeau). I asked some woodwind makers and they had the same conclusion: making the bore wider on a cylindrical reed-blown instrument makes the pitch higher.

But why these differences? There is an interesting complication, and again it was Jeremy Montagu who drew my attention to the fact that cylindrical bores overblow alternate harmonics (ie fundamental, twelfth, etc just like a clarinet), whereas conical bores overblow all harmonics (fundamental, octave, twelfth, double octave, etc). And this effect has nothing to do with the reed, it applies whether the reed is single (clarinet) or double (oboe). Though the effects of the conicity of the bore on the way of overblowing are clear, I do not exactly understand why this so is!

On instruments which overblow alternate harmonics, the place of the nodes and anti-nodes is fundamentally different compared with the instruments which have "normal harmonics" (like flutes and recorders, and the conical baroque oboe). There must be an different effect of reaming the bores between (reed blown) cylindrical and conical instruments, because there is a difference in effects in reaming the bore on a place where is a node (where the air molecules move fast and the pressure is low) or an anti-node (where the pressure is high and the air molecules move slowly) of the sound wave.

Peter van der Poel, who makes copies of historical clarinets, told me that if he makes the bore wider over the whole length, the pitch will be sharper, but making the bore wider near the reed the pitch will go down (for here all notes on a clarinet have an anti-node).

It is interesting to observe that old clarinets not always have a perfect cylindrical bore. See for instance Herbert Heyde, in his book "Musikinstrumentenbau" (Leipzig, 1986), on page 66, where is a picture and description of a workshop drawing of two clarinets by Kruspe (Leipzig, about 1850). On some places in the bore the measurement tool had to be going through with some difficulty, on other places however the bore was a bit wider (by sanding or intensive polishing).

How far the clarinet maker could go with changing the bore before the instrument was overblowing in the other way?

But, back to my original question: what is the fundamental reason that of two (cylindrical) pipes with different diameters, the wider pipe has the higher pitch if it is played with an oboe reed - and this in contrast with the observation that the same pipe has a lower pitch if it is played like a pan flute (or flute, blown over the open end)?

Who can give me an answer?

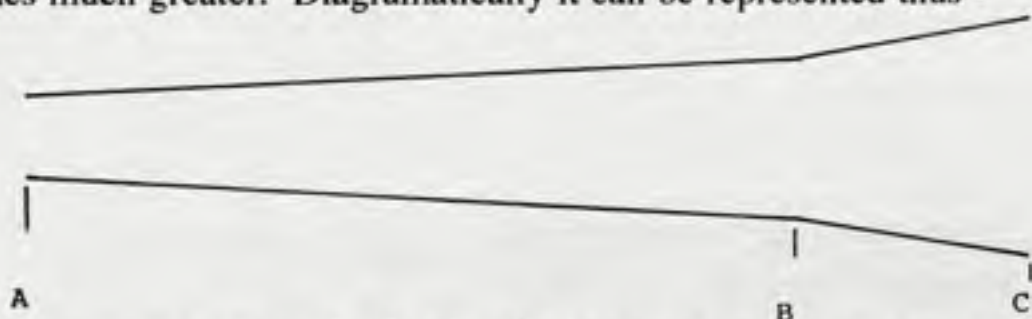
- 11- πάλιν δ' ἂν λάβῃς δύο αὐλαὺς τοῖς μὲν μήκεσιν ἴσους, ταῖς δ' εὐρύτησι τῶν κοιλιῶν διαφέροντας. καθάπερ ἔχουσιν οἱ Φρύγιοι πρὸς τοὺς Ἑλληνικοὺς, εὐρήσεις παραπλησίως τὸν εὐρυκοῖλον ὀξύτερον προῖένον φθόγγον τοῦ στενοκοιλίου. θεωρῶν γὰρ τὰ ταῦς Φρυγίας στενοὺς ταῖς κοιλίαις ὄντας καὶ πολλῷ βραδυτέρως ἤχους προβάλλοντας τῷ Ἑλληνικῷ. καὶ ἐπὶ τούτων οὖν τὸ ταχὺς τῆς κινήσεως αἷτιον. ἐπεὶ γὰρ τῶν στενοπόρων δυσοδοῦντος τοῦ πνεύματος καὶ τῇ μικρότητι τοῦ πόρου θλιβομένου βραδυτέρα κίνησις αὐτοῦ γίνεται, ἐπὶ δὲ τῷ εὐρυτέρῳ τῇ κοιλίᾳ κεχρημένον, ἅτε δὴ μηδεμιᾶς ἐγκοπῆς γινομένης ἢ διεξοδος τοῦ πνεύματος ταχύτερα συμβαίνει καὶ ἐφ' ἑνὸς κύλου ταῦτό κατανοῆσαι δυνατόν ἐστι.



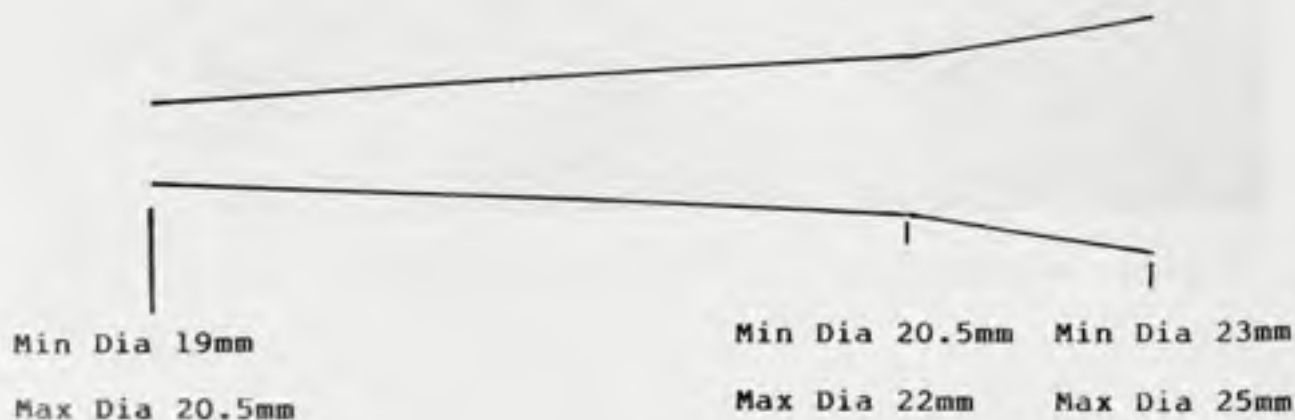
Aulos-player on a Greek amphora (450 b.C.)

The Ganassi Bell Diameter

Since the days when many Museums introduced their 'no measuring - no touching - only looking' policies, there has been a marked increase in my correspondence, seeking clarification on some of the puzzling and possibly inaccurate dimensions on recorder plans. These plans are available from institutions and individuals - some of the latter providing them gratis. No dimension has attracted more requests than the bell diameter of the now famous Ganassi Recorder in the Vienna Collection - Catalogue Number C8522 - a contributing factor being the conflicting evidence that has appeared in print. How the confusion arose over such a measurement is very easily explained. But first, here's an unusual question. How much bigger (expressed as a percentage) is the diameter of your leg compared with the diameter of your wrist? Think about it. Now for Mr Ganassi. The bore of the Ganassi recorder is old, deformed, and damaged by much measuring. It can however be regarded as a cone which gradually increases from the blockline until it reaches a point towards the bell, from where the increase becomes much greater. Diagrammatically it can be represented thus -



If the instrument was perfectly circular in cross section, percentage-increase calculations of the bore would be quite straightforward. But it is nothing like circular and is in fact a changing and irregular oval along its whole length. Therefore, each of the bore points shown has a maximum and a minimum diameter. Here are some much simplified figures making for easy calculations -



Now imagine that one wishes to express as a percentage-increase, the diameter at point C (the bell) compared with the diameter of the bore elsewhere. Which part of the bore would one select to compare with Point C? Point A? Or point B? Or some other point in between? And having decided which point to use, one must then decide which diameters to compare - maximum with minimum? Or minimum with maximum? Or perhaps minimum with minimum? Or even, maximum with maximum? Calculations using the above figures will give answers that vary by over 20% depending on which figures are selected. Hence my query re leg and wrist at the start of this article. The wrist is a fairly localised part of the anatomy but which part of the leg should be compared with the wrist - ankle, calf, knee, or upper thigh? Now toss into the Ganassi equation the most unpredictable variable of all - mistakes made by those measuring recorders. The reasons why recorder measurers make mistakes is not really part of this article. But they do make them. I have in front of me as I type this, four dimensioned drawings of the same Stanesby Treble recorder, measured by four different makers. Five in fact, as one plan is the work of two people. The speaking length of the recorder differs by a little over 9mm!!! The greatest difference in the bore measurements is 1.15mm!!! These are huge errors and reflect the varying skills of those concerned. In my own work I have certainly found measuring errors which have necessitated another visit to the original to remove any doubts I might have. All makers with whom I have discussed this problem, are very honest about their fallibility. Morgan, for example, writing in Volume 89, Number 3 of the Early Music News of NSW states, 'Occasionally I have discovered a mistake in my measuring work which proved misleading' and goes on to admit that such errors cause problems. And one of the problems caused is that different plans and descriptions of the same recorder, contain conflicting information. These discrepancies in recorder measurements plus the inclusion of errors, partly explain why copies of the same recorder from a variety of workshops, can vary enormously

The above comments clearly show that a combination of measurements and errors can make for a confusing picture. So what do I tell my enquirers? What statements are they to believe? Who is correct? And how can one determine which plans contain errors? Recorder makers who are unable today to get their hands on museum originals to check things for themselves, are understandably perplexed. Are they to believe Angelo Zaniol's claims (so I am informed by a correspondent) in Continuo of January 1984, that the increase is 25%? Or Morgan, who has been quoted as believing the figure is less than half of Zaniol's figure - between 11% and 12%? Or are readers to believe my claim of 32%? Or should readers believe my correspondents, whose claims vary from 9% to 35%? The paradox is that all the above

Ganassi claims can be proven by using combinations of the instrument's maximum and/or minimum dimensions not forgetting of course the variable factor already mentioned - measuring errors. Writing about percentage increases therefore, can at best be puzzling, and at worst, meaningless. And if that sounds confusing, the moral of the story is not. Unless one states very exactly how the results have been calculated, and unless one states no less exactly what measurements have been used in these same calculations, percentage-increase bore measurements, for recorders at least, don't mean very much. Rather like the advertisements that tell us that this brand of toothpaste produces teeth 25% whiter; or this brand of rope is 18% stronger, and if one buys a hank of it one receives a tin of furniture polish which goes 30% further! These statements give the impression of saying something definite but they do in fact, like percentage-increase bore measurements, say very little. In future it might be wiser for those writing about recorders not to give percentage-increases, but instead to provide dimensions as accurately as their skills allow, and leave percentage calculations to others. I'm sure that I will be acting on such wise advice!

FoMRHI Comm 1410

Roy Chiverton

MORE REAMERS

As more or less every schoolboy knows, any triangle standing on the diameter of a circle and having its apex at the circumference is a right-angled triangle.

The corollary of this is that the hypotenuse of any right-angled triangle stands on the diameter of a circle and the other two sides meet on the circle's circumference (see diagram).

If, therefore, you take a piece of right-angled steel, say twelve or fifteen inches of one of the metal stakes sold in garden centres, and cut away the sides appropriately, you have a reamer which will function exactly like a semicircular reamer, with much less trouble in its fabrication. The sides don't even have to be symmetrical, though this seems desirable.

You can also go on sharpening it with no loss of semi-circularity. To ream to the same extent, it will merely have to go a bit further into the workpiece, having its end cut off if necessary. The right-angled cross-section makes for easy sharpening.

This is an application of the Principle of Least Complexity.

FoMRHI Comm 1411

Roy Chiverton

JUST A LAST(?) BANG AT OCKHAM

In Comm 1349, I tried to demonstrate the principle of Ockham's Razor. Then I read Comms protesting against the invocation of it as a basis for scholarship. I thought I ought to brief myself a bit better, so I looked up my dictionary of philosophy. And what do I find? Not quite what Alec Loretto (Comm 1369) found.

The alleged formula - "Entia non sunt multiplicanda praeter necessitatem" - doesn't appear in Ockham (dictionary spelling) 's extant works! Moreover, in the dictionary, it is explained as the "Principle of Ontological Economy". Much clearer! But ontology is concerned with existence itself rather than existing objects and ontological arguments are mainly concerned with the existence of God.

So I then looked up "ens". An "ens" *tout court* appears to be an entity as opposed to its attributes. The "ens realissimum" is God. So all the "razor" seems to say is that you should not increase beyond what is needful the things you think of as existing, and this in the field of philosophy. not scholarship.

This is a far cry from Segerman's elaborate definitions in a number of Comms. It would seem, then, that these try to apply a recommendation by a 14th century philosopher for which there is no direct evidence, apparently meaning something quite different from Segerman's versions, to a field which this philosopher did not have in mind.

How to reconcile these differences? I had recourse to another authority, an Oxford logician and mathematician, who wrote,

"There's glory for you!"

"I don't know what you mean by 'glory'," Alice said.

Humpty Dumpty smiled contemptuously. "Of course you don't - till I tell you. I meant 'there's a nice knock-down argument for you!'"

"But 'glory' doesn't mean 'a nice knock-down argument'," Alice objected.

"When I use a word," Humpty Dumpty said, in rather a scornful tone, it means just what I choose it to mean - neither more nor less."

"Impenetrability, that's what I say," said Humpty Dumpty.

And so say I! Do you suppose we could call the modern version, which obviously has its uses, something meaningfully different, like "The Principle of Least Complexity", for instance? And just leave poor old Bill Ockham out of it?

No offence, Eph. Just my fun, - but we're a long way from musical instruments.

FoMRHI Comm. 1412

Donald S. Gill

Narrow Long Hole Augers

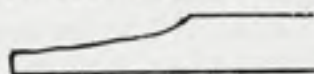
I make no claim for originalty or inventiveness in describing how to make these augers for narrow long hole boring. I was shown how to make them by Eric Moulder after I had ruined one of his augers, of which more later. There are two slightly different approaches to making the augers; one for those 4mm. or larger and the other for those less than 4mm.

4 mm. or larger

1. Cut an appropriate length off a bar of silver steel of the required diameter.

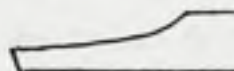
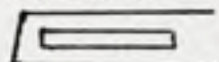
2. Drill a 2mm. hole down one end of the bar to a depth of about 15 mm.

3. Grind away the first 15 mm. of the bar till the thickness at the tip is equal to the radius of the bar. The rest of the ground area can be of greater thickness to give the auger greater strength, i.e. the ground end should look like this



4. Take a short length of 2 mm. silver steel rod and silver solder 2 - 3 mm. of rod into the groove at the tip of the bar then grind away the part of the rod which is proud of the ground area.

5. Grind relieving angles across and below the tip of the auger so that it looks like this



Less than 4 mm.

Omit steps 2 and 4. Instead file shallow slots across the ground area of the auger.

Use

Drill a short pilot hole before starting to use the auger. Don't do what I then did. I mounted the auger in a Jacobs chuck in the tailstock and screwed it into the wood. This turns the auger into a corkscrew. The correct thing to do is to fit a tap wrench on the end of the auger and feed it into the pilot hole gently. If you try to force the auger in too quickly it will merely turn round in the tap wrench with no damage to the auger. Withdraw at frequent intervals to clear chips.

FoMRHI Comm. 1413

Donald S. Gill

The Case Against Hot Hide Glue

After two Comms. in praise of hot hide glue I would like to put the case against it. For thousands of years it was the only strong glue available so there was no choice. Now we have a large number of alternatives available and I for one am very thankful.

In my opinion hot hide glue is smelly, messy, difficult to prepare and takes a long time to get ready. It should always be avoided if possible. Both John Rawson and Margaret Hood take paragraphs to describe the preparation; I can describe the preparation of Titebond in seven words, remove the cap and squeeze the bottle. John Rawson (Comm.1399) says of Margaret Hood 'she is right in saying that it is easy'. What Margaret Hood actually wrote in Comm. 1382 was 'While using hide glue is not easy, it is possible'.

As far as I know hide glue has only one advantage over other glues. Its glueing action is reversible with the application of heat and moisture. Who wants to unglue something they have glued together? Certainly not me. If I have glued a joint together I want it to stay glued. I can see some use in restoration where it is important to be able to undo the restored piece. There is also the case of string instrument makers who seem to want to be able to take the instrument to pieces after they have made it. I have never understood why but that is due to the ignorance of a woodwind maker. If I want to be able to take an instrument apart I use cork lined mortice and tenon joints or screws.

Then there is veneering. I don't do a lot of veneering (obvious?). When I do I use Glu-film, a thin uniform sheet of thermoplastic glue with a paper backing on both sides. Peel off the paper on one side, tack the film on the groundwork with a hot iron, remove the second backing sheet, place the veneer in position and iron on as with hide glue. No smell, no mess, no waiting 24 hr, no judging the right consistency and no problems spreading cooling glue evenly over a large area. Bubbles can be dealt with by reheating as with hide glue. At this point I would like to say a big thank you to John Rawson for his tip about using a wallpaper seam roller for smoothing. That tip alone is well worth my years subscription to FoMRHI. Why wasn't I clever enough to think of that?

My workshop has six adhesives in it: Titebond, Cascamite (the bodgers friend!), Araldite, Thixofax, Copydex and Glu-film. I shall keep the glue pot out as long as possible.

On the Expression of Emotion in Music

Shakespeare's *Twelfth Night* opens with Duke Orsino saying to the musicians:

If music be the food of love, play on;
Give me excess of it, that, surfeiting,
The appetite may sicken, and so die.
That strain again! it had a dying fall:
O, it came o'er my ear like the sweet sound,
That breathes upon a bank of violets,
Stealing and giving odour! ...

Orsino is in a state of heightened emotion, being frustrated that Olivia does not reciprocate the love he feels for and offers her. In the first three lines, he apparently hopes that the music will heighten his love feelings to the point of saturation and thus quell them, as orgasm does to sexual arousal and eating does to hunger. In the next two lines he refers directly to the character of the music, and in the final two lines he expresses how the music induces visual, secret movement and olfactory images he associates with the music. Presumably, these images are also associated with love, or at least of tender emotions. Thus the music was expected to effect emotions either directly or indirectly through images.

We respond to music emotionally because of resonance with emotions in our past experience. All music, and sometimes completely unfamiliar music, can move us by association with emotional non-musical images (clear or vague) from our past, while familiar music has the added associations of the emotional circumstances in which we have heard it before. If we newly listen to music of a completely different culture, the associations will be very different from theirs, and if we can appreciate it at all, it cannot be in the way that people who grew up in that culture can.

The culture in which our historical music was produced was in many ways quite different from ours today, and in other ways similar. If we want to try to understand how historical music was appreciated when it was produced, we have to study how the non-musical images that evoked emotional response were associated with music then, and what their experience with music was. Projecting our own images and emotional responses onto them seems so natural and easy for us that few of us ever think that there is any other possible approach to their music. So if we are truly interested in the history of music, we need to explore the differences and similarities between our imagery and musical experience and theirs.

Dance tempi

Dancing can be exhilarating in the enjoyment of the movement, and that is enhanced if it is with emotionally significant partners. Because of this experience, we can understand how that same dance music can be emotionally uplifting when heard in non-dancing circumstances. That is the same for the popular dance music then as now. We don't change the tempos or other characteristics of popular dance music now when it is intended for listening only. Changing the characteristics loses the meaningful associations. We have a right to expect the same then.

There is much rhythmically pronounced music in our 'serious' music culture which we respond to in terms of imaginary vague dancing images, moving parts of our bodies in time with it. We like to imagine dance as uninhibited freedom of movement, with a high level of emotional arousal. Since the heart beats faster in an aroused state, the association is with fast tempi. A consequence of this is that if we know that a piece of music is supposed to be a dance, unless we also know that it is supposed to be slow and dignified, we like to pick a tempo that is as fast as the music can take without appearing ludicrous. This is helped by the fact that we can make small movements with the parts of our bodies that are keeping time with the music rather faster than one can with proper dance steps.

Before late in the 17th century, we don't have direct evidence of what dance tempi were. We do have such evidence for general tempo standards, and we find the slow ones need doubling in speed for us to be comfortable with them. There is usually a wide range of tempi at which most sequences of dance steps can be taken. Modern reconstructions of sequences of dance steps given in historical manuals cannot help but be seriously affected by the modern taste for dance music to be fast. As a result, the early dance people have chosen rather faster tempi for many dances (particularly the slow ones) than the indirect evidence indicates. One consequence, for example, is that the divisions on dance steps occasionally mentioned (e.g. Arbeau's pavane) are rarely attempted, and when they are tried, it becomes a mad scramble without grace.

The sounds of the words

Nowadays, we can see very realistic expression of any kind of emotion in closeup in the movies and television. In the Renaissance and baroque, the showing of private emotions in public was not socially acceptable, and even was a sign of madness. For public acceptability, these expressions had to be modified by artifices, particularly that of rhetorical style. Thus the exaggerated presentation style of rhetoric became the major means of public communication of emotions. If people then were able to see one of our performances of Shakespeare, they would find it embarrassing. If we were able to see one of theirs, we would find it ludicrous. They wrote that performance of music also followed the style of rhetoric, presumably so that it should show appropriate emotional content. Thus it is most likely that if they could hear one of our performances of their music, they would consider it lifeless and rushed, while if we could hear one of their performances of it, we would consider it overly Romantic, over-interpreted and idiosyncratically deviant from their musical sources.

An important aspect of rhetorical delivery is that clarity in word enunciation is paramount. Thus the consonants are emphasised at the expense of the vowels. This pertained everywhere in the Renaissance, in the French baroque, and decreasingly in time during the Italian baroque. Thus when instruments imitated the voice, the initial noise transients of getting a note to resonate were not suppressed as they are today. They represented the initial consonants of words. The habit of suppression started with the Italian baroque, when expressing strong emotion imitating the noises of emotional expression was introduced, and the words became much less important. This has degenerated into pure display of vocal tone quality today.

Imitating the sounds of non-verbal expression of emotion

Most sounds of non-verbal expression of emotion involve continuous changes of pitch and of sound volume. It is rare for direct imitation of these sounds to be used for emotional communication in modern music making. It is not in our repertoire of associations. If we did not hear the words implying it, we probably would not recognise a singer's sigh. Early audiences much more expected the noises of emotion to be obviously included in musical expression. The most obvious component of the Italian baroque style was a kind of wailing (modified by the artifice of feats of vocal dexterity) to express strong emotion.

Somewhat more subtle was gracing. The trillo, which was a repeating of the same note (and which seems strange to us today), was an imitation of sobbing. Slower shakes seem to have had the same associations. Simple graces (such as appoggiaturas and slides) performed slowly were like types of sighs. These graces were described in the context of playing them on lutes, viols and keyboards where notes were at essentially fixed pitches, and so the descriptions involved slurring from one note to another without including any pitches in between. But we can readily imagine that on voices and instruments that had such in between notes available, a continuous pitch change was used at times to more closely approximate a sigh. This could be an important reason why wind instruments blowers then preferred more control over pitch by the lips than by the fingers, contrary to today's blowers.

After his Table of graces (reproduced on p. 17 Q 78), Simpson wrote:

'To these may be added the *Gruppo*, *Trillo*, or any other Movement of the Voyce imitated on the Viol, by playing the like-moving Notes with one motion of the Bow.

'Of these fore-mentioned Graces, some are more rough and Masculine, as your *Shaked Beats* and *Back-falls*, and therefore more peculiar to the *Bass*; Others, more smooth and Feminine, as your *Close-shake* and plain Graces, which are more natural to the *Treble*, or upper parts. Yet when we would express Life, Courage, or Cheerfulness upon the treble, we do frequently use both *Shaked Beats* and *Back-falls*, as on the contrary, smooth and swelling Notes when we would express Love, Sorrow, Compassion, or the like; and this, not only on the treble, but sometimes also upon the *Bass*. And all these are concerned in our *Division-Viol*, as imploying the whole Compass of the Scale, and acting by turns all the Parts therein contained.'

After the above discussion of graces, Simpson went on to the main purpose of the book, to guide the building of experience on which to base extemporaneous division. The objective apparently was so: 'a man may shew the Excellency both of his Hand and Invention, to the delight and admiration of those that hear him'. But later he wrote: 'we do sometimes for humour or variety...' and 'sometimes loud or soft, to express Humour and draw on Attention...'. The word 'humour' then meant 'mood', which certainly has emotional meaning. But division in itself was mainly decoration to provide tasteful busyness that demonstrated flair, without attempting to introduce any emotional depth. Such depth could nevertheless be added at will, the way Simpson indicated, i.e. by gracing. It is most likely that if a modern player chose to play Simpson's examples at the tempo he specified (crotchet = 75 MM in cut-time C), the itching idle fingers during slower divisions would strongly motivate the player towards adding gracing as another type of decoration. No doubt, graces (especially fast ones) were very often used for this purpose. But players and audiences, brought up on the modern aesthetic principle that simple and direct is honest and egalitarian while decoration is superficial Victorian and elitist, do not find the delight in decoration that pre-modern players and audiences did. And we miss out on the emotional associations that graces can have.

Orsino's 'dying fall'

What was Orsino referring to when he said that that strain had a dying fall? The word 'strain' then usually meant the music between double bars, corresponding with a line of poetry in a song. The word 'fall' usually meant a grace on a lute or viol where the left hand fingers 'fell' onto the fingerboard without any new articulation with the right hand. So this grace was an appoggiatura from a second below, or possibly a slide from a third below. It was an unfortunate name for the grace because pitch rose during its execution, and whether pitch rose or fell was important to them. After quite early in the 17th century, the name for this grace was modified or changed to lessen the linguistic conflict (e.g. the Margaret Board Book's 'a fale forward'). It is thus not clear whether slurred rising pitch is the only possible meaning for Orsino's 'fall'. A slow shaking after the appoggiatura would have continued the emotional affect longer. The 'dying' is a diminuendo which on a lute happens naturally during a grace unless one makes efforts to avoid it, and if it was on a viol, it can be effected by the bow. It is possible that 'fall' had the more general meaning of slurred notes. Then a larger portion of the strain could have been involved, and 'dying fall' could have meant that a series of notes were slurred with diminuendo.

Whatever 'dying fall' meant, it is clear that there was more communication of emotion in the music when performed originally than we experience in our performances of it now. We can never tune in completely to their emotional language in music, but we can get much better at it if we really wanted to try. It is a pity that scholars in the early-music movement have always been so devoted to selling how attractive early music is to wider and wider audiences that they would not think of deviating from modern taste in their interpretations of the music. Consequently, there has been no proper exploration of what original taste and response to music was. Indeed, many have convinced themselves that this is such a closed book that modern taste is the only possibility that can be considered. It is far from a closed book. There is plenty to read if one wants to, especially if one is willing to take it seriously on its own terms.

Anti-FoMRHI People in the Lute World

A friend has written to me saying 'Some (perhaps all) of the anti-FoMRHI people in the lute world that I've talked to have left FoMRHI because of space given to personalities instead of information'.

I have no doubt that there are anti-FoMRHI members in the lute world, and that they say such things. I used to be part of that world, in a small faction that felt that history has to do with objectively doing the best one can with the evidence available, that one trusted the evidence and new evidence that came along, that one always kept an open mind about interpretation, and that one avoided judgments, especially those based on taste or traditional opinions. In that faction, when the history led to possibilities that led to strange sounds or design features, beside exploring alternatives, we tried our best to tentatively accept these sounds or design features and to explore how they could have made sense on their own terms. Then, when a lot of lute players and makers turned professional, and customer satisfaction became of utmost importance, such exploration could not be practical, and acceptability to modern taste had to prevail. As almost the sole survivor of that faction that has kept the original attitude, I have become somewhat of a renegade in the lute world. I am sure that my expressing this attitude, claiming that it is the best way of doing historical scholarship, is the 'personalities' factor in the complaint.

There have been no 'information' Comms that have been displaced or inhibited (as far as I am aware) by 'personalities' Comms. So I suspect that what is disappointing to these former members is that more recent 'information' Comms have less appeal to them because they rarely offer new scope in doing what these people want to do. Their desire is to enhance their contribution to the early-music movement. There is much on the history of instruments that is still to be researched, but unfortunately, very little of that would be considered to be a constructive contribution by the early-music movement. The question of appropriate instruments for the movement is considered settled (unless clearly more attractive possibilities came along), so we have little to offer that would be acceptable.

Though they know that gut was original, the leaders (and the vast majority of others) in the lute world are happy with nylon strings. They always have been. Changing to gut strings now is almost inconceivable. Gut strings are more expensive, need more tuning and maintenance and become scruffy-looking more quickly. Treble strings break much more often and projection of the basses is considered inadequate.

A generation ago, lute players and makers were idealistic pioneers in insisting on historical accuracy in their instruments. Gut strings available then couldn't be made to work. Normal pitch was g' on the treble string and on the normal size of lute (with a 24 inch string stop), a gut string broke too quickly to be practical (no-one considered that the normal pitch standard could have been lower than modern). Thicker gut for bass strings was only available then as harp strings, and when plucked, they only gave a dull thud and could not remain in tune when fretted. It was generally agreed that the gut currently available was not as good as original gut, and nylon strings were adopted as a temporary practical expedient.

Our research then showed that the original pitch standard for normal-sized lutes was a tone lower than modern, and we discovered that thick gut strings with rope construction would remain in tune when fretted and sound much better than harp strings. Our solutions to the problems of gut stringing were welcomed by a minority of players. The gut treble sound was better than nylon, but the richness and projection of nylon metal-wound basses was missed. Some tried gut treble strings with nylon metal-wound basses, but treble-string life was uncomfortably short, especially since one often needed to play at more modern pitches. Retuning on stage during a concert became an embarrassment since modern audiences don't expect it like earlier audiences did.

Most players were well satisfied with nylon stringing, and would only switch to gut if the field demanded it. Their interests were in growth of their skills, repertoire and careers, not in changes in their equipment that have disadvantages. The public, and particularly the critics, accepted whatever equipment the leading players used, and probably were not aware of the difference between gut and nylon in treble sound. The minority fashions of playing on all-gut stringing and on mixed gut and nylon soon faded as early music fixed its practices to more effectively fight for acceptance in the professional 'classical' music world.

When asked about their nylon stringing, the players would still give the old answer that modern gut isn't as good as old gut. This is not just the cynical excuse that it sounds like. Most honestly believed (as most do today) that they are closely following in the footsteps of the early players, and they couldn't imagine having different tastes and judgments than the early ones did. So if they (and the majority of their colleagues) are unhappy with a proposed historical practice, or find it impractical, it couldn't possibly be authentic. Thus Segerman's research, or the claims he makes from it, must self-evidently be wrong. I am viewed as a false leader, a symbol of the gut stringing that has been rejected (with perhaps a little guilt).

The difference between people in the lute world who feel this way and are still members of FoMRHI, and those that have left, is that the former are still interested in history and technology, and the latter are only interested in early music.

Incidentally, as a result of recent Comms on gut string history, gut makers can now make strings (including smooth Catlines and not-so-smooth Lyons) that look as clear as Dowland and Mace said they were. On the lute, they look remarkably like nylon monofilament from top to bottom. In spite of the familiar look, few lute players would be interested. They must play at modern pitch standards and won't switch to smaller lutes. They have a stringing that works.

My main reason for writing Comms like this one is that as an historian, I expect that histories of the early-music movement will be compiled. History is usually first written from the point of view of the winners, and I want 'how it was', as seen by this loser, to be available to subsequent historians.

FoMRHI Comm. 1416

Ephraim Segerman

Some aspects of string technology bearing on sound quality

In Comm 563 (Q37,1984), I pointed out the correlation between perception of the quality of string sound and inharmonicity, and that the factor involving the properties of the string materials in the inharmonicity equation is the elastic modulus divided by the square of the density. Thus quality can be improved by lowering the elastic modulus or increasing the density.

The stretch or elongation of a string when put under tension has two components: the inelastic (or permanent) stretch and the elastic stretch. If the tension is removed, the elastic stretch recovers immediately, and the inelastic stretch remains in the string. The elastic modulus can be considered to be the resistance of the material to elastic stretch. If the string is a uniform rod, the amount of resistance to elastic stretch leads to a similar amount of resistance to elastic bending (or flexing). But if the string has rope construction, it's resistance to flexing is independent of its resistance to stretching. Thus Jaeger strings (a modern brand of bowed strings with steel ropes as cores) are more inharmonic than most competitors, and yet are more flexible in handling. Some players of early bowed instruments who appreciate the sound of roped gut strings have mistakenly associated how floppy a string is in handling with how well it will sound. They have generalised from very few examples from different makers in which

this happened to be the case. Dowland associated stiffness in handling with quality.

The best reason I can think of for why nylon treble strings have not been adopted by bowed instruments is that nylon provides very little absorption of the compressional (or longitudinal) standing waves along the string that are generated when one's bowing is not accurately perpendicular to the string. The twisted-fibre construction of gut is more absorbing of these squealing standing waves. So gut is more user-friendly because it demands less accuracy in bowing.

According to the theory associating quality with harmonicity, the reason why gut treble strings sound better than nylon is that gut is 25% more dense. A plastic monofilament could well be developed with a considerably higher density than nylon, and so sound as good as or better than gut. It would have to have a ratio of tensile strength divided by density at least as high as gut to reach the pitches gut can. Some years ago we found out about a polyester monofilament that had a density about the same as gut and was strong enough. We were able to get samples from a manufacturer that was testing out new equipment, and indeed it sounded better than nylon and as good as gut. We would have liked to make the lute players happy by offering them the polyester strings, but to make it worthwhile for the manufacturer to make it for us in the variety of diameters required would require many thousands of £'s which we didn't have. If we borrowed the money and invested in it, we would have had enough stock to supply all the lute players for a century. It was just not an economically viable proposition.

In recent years a new monofilament material was invented in Japan. It is polyvinylidene fluoride. Possibly because carbon fibre has been in the news (because of its remarkable properties), the lute players have been calling it 'fluorocarbon', and a few have completely mistakenly calling it 'carbon fibre'. It is considered a superior material for fishing line in Japan (I haven't heard of it being exported). The density appears to be over 30% higher than gut, so we would expect it to sound decidedly better than gut. This appears to be true for the thicker monofilament strings, but to my surprise, the sound is very disappointing on the top string. Either the theory is wrong or there is another factor in the situation that we have not yet taken into consideration.

I've just realised what that other factor might be. It has to do with a correlation between reports of inferior sound and strings undergoing unusual amounts of inelastic stretching. This is clearest in harpsichords, where the sounds of the brass strings are decidedly inferior during the first week or more when the strings are stretching faster than they do afterwards. This happens on a much shorter time scale with treble gut strings, which benefit tonally from a bit of 'playing in'. Faster stretching could also be the reason why the sound of a gut string deteriorates just before it breaks. Nylon fishing line is not considered to be an alternative to nylon music strings (even in an emergency), and we know that fishing-line nylon is made to stretch faster and further than musical-string nylon. The polyvinylidene fluoride (an appropriate short name would be PVF) that I have experience with stretches faster than either musical-string nylon or gut, and so it fits the pattern.

I don't remember seeing any technical discussion about why this correlation could be. How can the quality of a sound deteriorate because the string is stretching? The deterioration is the loss of higher harmonics. My suggestion is that there is direct energy transfer from the higher harmonics of the string's vibration to the little slips of one bit of the string material relative to the bit of material next to it that sums up to be the stretching. Each little slip probably makes a tiny noise, and there must be an overlap between the frequency spectra of the noise and of the string vibration. Thus the higher frequencies of the string vibrations often kick off some little slips before they would have happened without the vibration. This absorbs some of the vibration energy and makes the string stretch slightly faster. The last consequence here can be checked by measurement.

Any other ideas?

Maria Rita Brondi (*Il liuto e la chitarra*, Torino 1926), reports an interesting procedure for self-making musical instrument strings, starting from the sinews of horse-backs. It is a recipe from Timotheo Rossello's book "Summa de secreti universali" printed in Venice, 1574 c (Biblioteca universitaria di Padova segn. 506 191/1-2) Which is a wide collection of recipes ranging from medicine to clothes dyeing to colinary art. Ivo Magherini has kindly translated the text "A far corde da instrumento": "Take the tendons (sinews) of a horse's back which you will have pounded in a cloth with a wooden mallet until they become soft, then have them twisted well together with animal glue. Then dried them and they will be very strong and good strings and they will been almost of silk..." (Libro sexto, cap. 123. P. 151). Although the twisting technique is not described in detail, the use of glue, recalling the method used to make silk-strings (in which case the binding mean is gum-arabic) leads us to exclude the usual rope and cable making procedures which, as well knows, do not require any gluing. This recipe remains altogheter confined within the do-it your self area. Although it is the first mention known to me of musical strings made of animal sinews.

John Downing (Comm. 1394) rightly points out that in the 16th and 17th centuries iconography, the strings look rather curly and the unused part out of the pegbox bundled up like if were a soft cord (or shoe-laces).

Modern plain gut strings, on the other hand, because of their stiffness, are as straight as pins and must be rolled up in circular shape, lest they get damaged. This present fashion of rolling up strings is already mentioned in Diderot's "Encyclopedie", c. 1760; in De Lalande (*Voyage en Italie*, Paris 1761 and more precisely in the description of string making in the work-shop of the neapolitan string-maker Angelucci) and in Grisellini's "Dizionario", Venice 1770. The way a string can be rolled up gives an empirical suggestion of how stiff it may be and we can not exclude, during this "evolution phase" an influence coming from the spread of overspune bass strings.

From what can be inferred from historic documents from the second half of the 18th c. (See for ex. Crescenzo Ungar workshop's inventory, 1791: "...un ordigno da coprir corde di fil d'argento..."), overspune strings were not made by string-makers but by the lutemakers themselves, who could twist a metal wire around a normal plain gut string (the core). With the spreading of overspune strings, for the string-makers was no more necessary, I think, to know how to make strings of an extremely elastic and pliable nature (and, for the basses, of an increased specific weight). The new wound basses in fact covered perfectly the mid and low registers functions; thus the manufacturing processes unser went, an inevitable simplification expressed by the general increase of elastic modulus, clearly indicated by the necessity of changing the way of making them up: precisely by rolling them up in a circular shape than boundless, like in 17th century. It is true that the description of the manufacturing technique from the scanty documents from the 17th c. do not seem to be far off from the 18th is (See Jan Harwood's article: "String making in 17th c. Padua: an english traveller description", in "The lute", n. ?) never the less something still eludes us, since iconography shows, concretely, that the strings of the 16th and 17th centuries were more pliable than our high twist ones and most probably than those produced in Diderot's and Angelucci's time, too. But in order to obtain strings with such characteristics it is not necessary to bother the sinews of animals, although the question certainly deserves serious investigation. Anyhow, having at our disposal a reel provided with two hooks (exactly like the one employed by the anonymous paduan string-maker described by Skippon), it is sufficient to modify the usual twisting technique of the fresh-gut strands (i.e. to make high-twist strings) in order to obtain a smooth string possessing the same characteristics shown in the 16th and 17th centuries' paints.

It should also be observed that in Diderot's and Grisellini's tables the reels are provided with two hooks, but only one being used for the string being produced (both of them, on the other hand, being used by the paduan string - maker in the second half of the 17th c.).

We should furthermore consider the influence of the "chemical" products they employed in string manufacturing, such as "Potash" (Potassium carbonate), which was obtained by distempering in water the ashes from different plants (like vine) and used to de - grease the raw - material. On the other hand, potash is recommended, under its other name of "oil of tartar" by R. Dowland in his "Varietie" (Bring out from J.B. Besard instruction of the "Thesaurus Harmonicus, Colonia 1603), and by other recipe - books of the time, for softening the skin of the hands, and in it the gut - strands were kept for several days, before being twisted together, without pre - rinsing in running water. The fact is that this compound is still considered, at the beginning of the 19th c., to be the "secret" for obtaining elastic and pliable plain gut strings, as opposed to the function of final oiling so celebrated in Diderot's "Encyclopedie" (And which is, in fact, also important to the same purpose). Here is, for ex., a quote from Jaubert's "Dictionnaire", Lyon 1801, under Boyaudier, p. 319: "...on pense qu'il y a encore une leggere operation a' faire....elle consiste Vraisemblablement a' les frotter d'huile - pour les a doucir et les rendre encore plus souples: mais les Boyaudiers en font un mystere; ils assurent qu'ils ne se servent point d'huile.... mais ils y emploient des sels qui sont extraits de la lie de vin". (We think there is one more light operation to carry out.... which consists of apparently rubbing them with oil in order to make them softer and more pliable: but the string makers make a secret of it, they assure they use no oil ... but employ some salt extracted from the lye of wine...). Concerning the acoustical characteristics of such flexible strings it must be noted that they possess a higher brightness than their high twist equivalent and a higher longitudinal displacement.

Skippon, always about the paduan string maker, descaped "... first they take the small guts of lambs, weathers, kinds, wolves (but no cats - guts)".

Now, why did skippon consider it necessary to point out to the reader that no cat's - gut was employed? Padua is some 90 km. away from Bologna, where the "Venice - Catlins" were produced: may be the manufacturing techniques in use in both cities were not unlike. Undoubtedly it could be very interesting to find - out what the old english string makers called the "Venice Catlins", but that research has been so far unsuccessful.

LIBRO SESTO. 151

A far corde da instrumento. Cap. 123

Piglia li nerui della schiena del cavallo, liquaii farai pistare in un panno lino con un maglio di legno in sino a tanto che siano uenute molle, dopo falli filare polite, e filate lineali con colla forte & secca, e saranno corde fortissime & bone per instrumenti, & pareranno quasi di seta.

A Power Law in the Stringing of Instruments with Varying String Length

The best stringing of an instrument takes into account both the varying resonance characteristic of the instrument and the expectations of the sound, look and feel by the experienced player. This is ideally done empirically by the player trying a range of heavinesses of the type chosen for each string, and choosing the heaviness that is most satisfying. The convenient measure of heaviness of a string uniformly made up of one material is its diameter, and that of a more complicated string is the equivalent diameter of a cylinder of some assumed string material that has the same weight per unit length as the composite string.

For instruments with few strings of the same material and construction, it is fine to do this for each string. But for instruments with many such strings, it is more efficient if this is done on a few evenly spaced strings and interpolating for the diameter (or ED, the equivalent diameter) of the strings inbetween. Effective interpolation can be done by guesswork, but it can usually be done more accurately by assuming that the string tension (T) is proportional to the vibrating string length (L) to some power (p), i.e:

$$T_2/T_1 = (L_2/L_1)^p$$

where 1 and 2 are any two strings, and the exponent p is the same no matter what the choice of the string pair is. The reason why this is probably more accurate is that long stretches of strings on instruments tend to conform to this law with a constant power. For instance, the power law with p about 0.6 fits the modern harp stringing just as well as the formula given in Comm. 1143, and the power law with p about 0.3 fits the top 4 or more octaves of most original harpsichord stringings in the Rose & Law handbook.

Special cases of such a power law are when $p = 0$ and $p = 1$. When $p = 0$ the tension is constant, independent of length. This happens most commonly when one fingers a string up and down a fingerboard. It also happens on some early keyboard instruments. When $p = 1$ the tension is proportional to the length. This is the tension-length principle that seems to apply to corresponding strings of different members of a family of plucked or bowed instruments with parallel nuts and bridges.

In practice, we deal with diameters and not tensions. If we eliminate tension by combining the power law with the Mersenne-Taylor law, for the same string material we get:

$$(D_2/D_1)^*(f_2/f_1) = (L_1/L_2)^{(1-p/2)}$$

where the two empirically found strings are 1 and 2, and the ratio of f 's is the ratio of frequencies characteristic of the interval between the two string pitches.

To do the interpolation algebraically, since we know everything except the exponent, we solve for it. We then put it back into the equation as a known quantity and exchange the new interpolated string with one of the original ones. We know the new string's f and L , so we solve for it's D .

It is much easier to do this using log-log graph paper. Multiply the D by the f of each of the empirically found strings. Don't worry about the sizes of numbers or the units but do not change units during the process. Shift the decimal point of the units along one axis of the log-log paper to cover the range of the $D*f$ product, and similarly shift the decimal point of the units along the other axis to cover the range of L , and plot the two points. Draw a straight line between them. Read the interpolated product of D and f off the line where it corresponds with the interpolated L , and then divide by f to get the interpolated D .

On log-log paper, every factor of 10 in a variable is a cycle. The 4 octaves of uncovered modern harp strings cover a factor of about 6 in Df and about 10 in L , so 2x2 cycles paper will always do in this case. One can always multiply a variable by a constant before plotting, but then one must divide by that constant when reading off the graph. Judicious choice of the constant for each axis allows the use of 1x1 cycles paper in this case.

On describing the body shapes of stringed instruments

I have been corresponding with Michael Heale about the variety of shapes in early viols and fiddles, and got interested in the question of how we decide whether two similar shapes are essentially the same or are different. To some extent that depends on the need to distinguish. For instance, to most people, all normal violins have the same shape, but to specialists, there are different shapes for different schools of makers.

I decided to try to define objective criteria that I hope will usually correspond with the way most people distinguish between instrument shapes. For simplicity, I will confine this discussion to body shapes as seen face on, and to symmetric shapes (this can be extended to asymmetric shapes by discussing each half separately). My suggestion is that the distinction is largely based on the sequence of convex or concave curves, and inwardly or outwardly orientated points in the shape.

As the first example, let us describe what we consider to be a guitar shape. It leaves the neck nearly perpendicular to it in a convex curve, blends into a concave curve which forms a waist, and then blends into a convex curve that continues until it crosses into the other half of the symmetrical shape. If we didn't mention that the concave curve formed a waist, this sequence of perpendicular-to-the-neck, convex, concave and convex would also describe a common unwaisted English Guitar shape, which we consider looks different, so we must include an indication of the waist in our description.

Let us shorten descriptions by a notation. The neck will be called N, and it is assumed to be vertical. Then a horizontal line - after the N denotes that the curve comes from the neck in a horizontal direction. Other possibilities of how the shape comes from the neck are inwards at an angle (N\), like with most later viols, or outwards at an angle (N/), like with Ganassi's viols. A convex curve will be called X and a concave curve will be called V. A waist point along a concave curve can be notated as a <W> after the V. Thus the guitar shape would be notated as: N-XV<W>X and the English guitar shape would be notated as N-XVX.

As the second example, let us describe the usual late viol. For this we need an outward point called O (an inward point would be called I). There is also a tail projection (called 'term' by Talbot) that holds the tailpiece, which will be called T. In this case the curve comes in to T horizontally, so the description would end with -T. In other cases it can come in at an angle as it approaches T (as with some early viols), notated as /T, or going out (as with most Kentucky dulcimers), notated as \T. So this viol shape can be notated as N\XOV<W>OX-T.

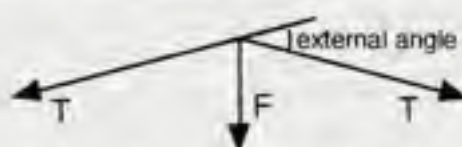
The normal violin shape can be described by the sequence N-XVOV<W>OVX. The shape of Praetorius's bandora, penorcon and orpharion is N-XVXVXIX. A lute shape is N\X. The usual cittern shape is N\X. Some parts of a shape may be straight, which we can call S. So some citterns would be described with N\SX. If we wanted to include both possibilities in the description, we could write N\V(or S)X. If we wanted to say that sometimes (but not always) there is a straight bit between the V and the X, we could write N\V(S)X. A rebec's body includes the neck, so its shape starts from the pegbox P, described by P\SX.

How many different shapes defined this way have been used for musical instruments? Going through Baines's classic picture book *European and American Musical Instruments*, I came up with a little over 50. Then going through Kinsky's classic *History of Music in Pictures*, I came up with less than 10 that I didn't have already. My guess is that the total is under 100.

A more accurate but cumbersome notation would be to put a - or | after each symbol for a curve or a point (considered as a very sharp curve) when the tangent goes through the horizontal or vertical respectively. So a violin would be N-X|V|O|-V-|-O-|V|X|- and a lute N\X|-. A pig-snout psaltery would be -SOISOVO-S-.

On the Hair Tension during Bowing

When the bow hair is pressed against a string during bowing, kinks form in the shapes of both the hair and the string in response to this bowing pressure. Both kinks reduce the clearance angle over which the bow can play that string without touching another. If that clearance angle is small initially because of a rather flat curvature of the bridge (as was with Renaissance viols), and one wants to avoid playing other strings, both string tension (unless one plays very near the bridge) and hair tension needs to be high to keep the exterior angle of each kink (which in radians, is the bowing pressure F divided by the tension T) within the appropriate bounds.



If the bridge is round enough not to present problems with the clearance angle, or if one wants to play an instrument with a flat bridge, there can be advantages in keeping the hair tension lower. If one is bowing a single string, having a sharper kink in the hair at the string increases the area of contact between hair and string, leading to quicker response. This is a factor that has been more important in the late baroque and since than it was earlier. If one is bowing a medieval fiddle with flat bridge near to the bridge, the end strings (the chanterelle and the bourdon), individually or together according to the angle of the bow, can more easily be sounded more strongly than the ones inbetween.

The main issue to be discussed here is rather a more subtle one: that of the increase in tension of the hair as a result of the bowing pressure, and how this is influenced by the flexibility of the stick. The following calculation of this is motivated by reports from players that bow flexibility is essential for quick response. The quick response is presumably the result of the area of contact between the hair and string not decreasing significantly as one applies the bowing pressure, which implies that the increase in hair tension then is small.

The increase in hair tension ΔT is related to a measure of the hair elasticity K_h , the change in hair length ΔL and the original hair length L by: $\Delta T = K_h * (\Delta L / L)$. A measurement of the increase in length of a single bow hair with increments of tension of 100gm leads to the conclusion that K_h equals about 12 times the number of hairs, giving ΔT in Kg. The elasticity of the stick responds to the change in tension with a shortening of the distance between the hair fixings of ΔL_s as governed by the stick's measure of elasticity K_s , so $\Delta T = K_s * (\Delta L_s / L)$. The total lengthening of the hair on applying the bowing pressure $\Delta L / L$ is $\Delta L_p / L - \Delta L_s / L$, where $\Delta L_p / L$ is the lengthening of the hair because of the geometry of the hair shape (triangle hypotenuses and all that), assuming that the ends are fixed. Then $\Delta T = K_h * (\Delta L_p / L) - K_h * (\Delta L_s / L)$, which on substitution = $K_h * (\Delta L_p / L) - (K_h / K_s) * \Delta T$. Thus $\Delta T = (\Delta L_p / L)$ times $(K_h * K_s) / (K_h + K_s)$. The latter term (the effective K) is always between K_h and K_s , about half way when they are nearly the same, and becomes closer to the smaller of the two the more different from one-another they are. So if we want ΔT to be small, we need either to make the number of hairs small (if the bow is stiff) or make the bow more flexible.

The term $\Delta L_p / L$ is given¹ as $(F/T)^2 * [L_1 * L_2 / (2 * L^2)]$, where L_1 and L_2 are here the distances between the string and each end of the hair. The T is really $T + \Delta T$, but the difference can here be neglected. This term is small near the ends of the hair (L_1 or L_2 is small), making ΔT small, so the flexibility prevents response delay only when the string is near the centre of the bow.

¹ Abbott & Segerman, 'Strings in the 16th and 17th Centuries', Appendix 3, equ. (2), GSJ XXVII (1974)

J.S. BACH'S "FIAUTI D'ECHO": RECORDERS OFF-STAGE

In recent years there have appeared in print several studies (large and small) claiming or disclaiming the use of a special type of recorder in Bach's fourth Brandenburg Concerto. Because this question still seems to be unresolved, it may be helpful to consider some peripheral aspects, which taken separately may not be important, but added together can form a more substantial base on which to make a decision.

(1) Usage of the term "echo" in Bach's time

Today the word "echo" is commonly used to describe any musical phrase that is repeated softly. However, in Bach's time and earlier, it seems that the term "echo" was used in music much less, and when it was used, it sometimes (or even usually?) had a more restricted meaning, a meaning closer to the ancient Latin original. The original myth appeared in the third book of Ovid's "Metamorphoses". According to this, the nymph Echo loved the beautiful shepherd Narcissus, but her love was not returned, because Narcissus was enamoured only of his own beauty. From sheer grief Echo dissolved into nothing but a voice, which awakened only on being called upon by human sounds, when she was able to answer the caller from the hollows and caverns of mountains and groves. Thus, an "echo" could be reflected sound (as in a mountain valley, or in a building), while if it was used in a musical performance, "echo" could mean not just a phrase repeated softly, but a phrase repeated at a fairly large physical distance, by a performer out of sight (as it were by a spirit). For example, Marini's "Sonata in Echo" Op. 8 for three violins (1626) contains the instruction "the first (violin) to be seen and the other two are not", while Purcell's "Dido and Aeneas" (1689) in its chorus "In our deep vaulted cell" and the following "Echo Dance of the Furies" requires a chorus and orchestra behind the scenes. In Bach's own compositions, there is an "echo" aria in the secular cantata "Hercules am Scheideweg" (BWV 213) of 1733. Here Hercules sings: "Faithful Echo of the glade, if I should soon be betrayed, and I were content to go, where this sweet voice bids, say no." Echo: "No." Hercules: "but if then the warning voice, on through toil and weariness, guides me to a better choice, then I bid thee answer yes." Echo: "Yes." To present these words sensibly, the echoing singer needs to be off-stage. A year later, Bach re-used this aria, with new words, but still with echoes, in Part IV of his Christmas Oratorio.

(2) Cultural life in the Cöthen Court

Bach probably wrote the Brandenburg concertos as entertainments for his employer, Prince Leopold of Anhalt-Cöthen. (There is little doubt that in the autograph known today, that was dedicated to the Margrave of Brandenburg in 1721, Bach took convenient music that he had already composed for the Cöthen Court.) Prince Leopold was a well-trained musician, well-educated and cultured, and he had a very friendly relationship with Bach. It is very likely that Prince Leopold himself played in the

small court orchestra. In this situation, it would have been quite appropriate for Bach one day to introduce a new concerto to the orchestra, and to surprise and delight his Prince part-way by walking out of the room with the two recorder-players, and "answering" the Prince (who played the violin and keyboard instruments) from a distance. Such a pantomime, conceit, and adopting of a character or role by musicians, was common in the entertainments of many courts. (It may even be relevant that an early member of the Bach family, Hans Bach (c. 1555-1615) was a minstrel and "fool" in the Court of Ursula, Duchess of Württemberg.) The somewhat pastoral character that is suggested in any case by the use of recorders, is further hinted at by the realistic imitation of a double echo (overlapping as it would in a mountain valley) in bars 235-9 and 251-5 in the 1st movement. Recorders off-stage in the 2nd movement, would further emphasise this pastoral character.

(3) The form of Brandenburg Concerto IV

The form of this concerto accomodates well an off-stage placement of the two recorders and solo violin in the 2nd movement, particularly because of the following features. (1) The extra \bigcirc at the end of the 1st movement, additional to the \bigcirc at the end of the Da Capo section. This suggests a longer-than-usual wait at the end of the 1st movement, perhaps to allow time for the three players to walk off-stage. (2) The first note of the 2nd movement (isolated in the bass), is almost anachronistic, and does not belong to the first phrase, which begins on the second note. Perhaps the purpose of the first note was to serve as an aural clue for the off-stage (and out of sight) musicians so that they knew when to begin. (3) The 3rd movement begins without the two recorders and the solo violin, and would have allowed them sufficient time to walk back without more than a minimal break between the 2nd and 3rd movements. (The music here even seems to have the character of a fast walk.) Both recorder parts begin with 22 bars' rest, and while the principal violinist has only 10 bars' rest, it is not essential for him or her to start playing for another 30 bars, because this part is duplicated in the 1st Ripieno violin part.

It is clear that, because almost invariably the principal violin alone accompanies the recorders in their echoes, the principal violin should be off-stage if the recorders are off-stage. This leads to the question why the term "echo" was not also applied to the principal violin. The answer may be that the principal violin only had an accompanying role in the 2nd movement.

Of course, none of the aspects I have described are proof that the recorders and principal violin played off-stage in the 2nd movement of Brandenburg Concerto IV, they only describe a situation that was suitable for this. However, it may be effective and illuminating to try performing this way today. Particularly when performed on period instruments, by a small orchestra of little more than one player to a part (as Bach probably had), with normal recorders (which can not artistically and accurately make a large difference in loudness), the effect of echo is hardly heard if the recorders are not off-stage.

The Instruments of the Consort

In Comm. 1336, I pointed out that the word 'Consort' originally meant an ensemble with contrasting timbres and contrasting movement (i.e. a high level of division expected, but not continuously), and that a secondary, but not obligatory, characteristic was contrasting sonorities between three (or more) part polyphony and the homophony of chordal accompaniment by plucked strings. By late in the 17th century, the term referred only to the contrasting timbres of a set of instruments of one type (like viols, recorders or a fiddle band) accompanied by another type of instrument (such as an organ, harpsichord or theorbo).

The Ideal

All of the surviving Consort music comes from the period from 1585 to 1615, considered a particularly high point in English music. In that period, the most respectable Consort involved a specific group of instruments that fully offered all three types of contrast mentioned above. The two printed editions of Morley's *Consort Lessons* (1599 and 1611) specified the instrumentation as 'the Treble Lute, the Pandora, the Citterne, the Base-Violle, the Flute, and the Treble-Violle. Rosseter's *Lessons for Consort* (1609) specified 'Treble Lute, Treble Violle, Base Violle, Bandora, Citterne, and the Flute'. Before the lists, each title page mentioned that there were six instruments in the Consort.

What was particularly special about this Consort of six was its reputation of pleasing Queen Elizabeth. The published descriptions of the entertainments for the Queen during her Summer Progresses at Killingworth Castle in 1575, at Norwich in 1578 and at Elverham in 1591 all mentioned a Consort of six musicians (sometimes with a singer) which delighted everyone, particularly the Queen. The Morley and Rosseter instrumental specification, with its unusual omission of any alternatives, represented an ideal. Of course in practice, there were many variations.

Deviations from the Ideal

Let us consider Richard Allison's publication (1599), which says on its title page: *THE PSALMES OF David in Meter, The plaine Song beeing the common tunne to be sung and plaide upon the Lute, Orpharyon, Citterne or Base Violle, severally or altogether, the singing part to be either Tenor or Treble to the instrument, according to the nature of the voyce, or for fowre voyces.* Since the word 'instrument' in the above is singular, it was obviously expected that the most common users of the book would be self-accompanying singers, and that the combination of instruments ('altogether') and a vocal quartet would be less popular alternatives. Thus the word 'Consort' is not on the title page. Allison's dedication is largely about justifying performance of religious music with instruments: 'our eyes beholding the words of David, our fingers handling the Instruments of Musicke, our ears delighting in the swetenesse of the melody, and the heart observing the harmony of them: all these doe ioyne in an heavenly Consort, and God may bee glorified and our selves refreshed therewith.' William Leighton's commendation for the publication called it 'These swete Consorts'.

In this book, each piece has an unlabelled tablature part for 7-course lute tuning (with the 7th course tuned to D) under the Cantus (singing) part, with separate Altus, Tenor and Bassus parts, plus a tablature part labelled 'Citterne'. In the 'altogether' or Consort option, we can presume that the lute and orpharion play from the same part, with the lute (as was usual) perhaps doing some quaver divisions initially and mostly doing a mixture of quaver and semiquaver divisions the second time through. The orpharion would play the part as written both times through, replacing the bandora of the ideal Consort. The part they play has no written divisions, and the chords have their roots on the lowest strings that are practical. Thus the orpharion's range goes down to only a tone higher than the lowest the bandora usually goes to, making it a credible substitute. This might explain why the evidence of instruments

mentioned in surviving wills never includes a player owning both a bandora and an orpharion. Besides being able to play the lute repertoire, the orpharion can replace the bandora in Consorts, reading from the undivided lute part. This would be the choice of a plucked-string specialist, while the bandora would be the choice of a more general instrumentalist with limited aspirations in string playing.

The instruments listed by Allison are all instruments that one can accompany one's own singing with. That is the context in which the instruments were mentioned. Everyone was aware of what the ideal consort was composed of, and it is most likely that alternatives (treble lute or bandora) or additions (treble viol or violin playing Cantus, or flute or recorder playing Altus) that approached the ideal more closely in the 'altogether' or Consort performance option would have been welcomed.

Another recorded deviation from the ideal composition of the Consort was listed in the Custom Book written in the earlier years between 1600 and 1617 by the Rector of the English College of St. Omers at Pas de Calais¹. The instruments listed to be 'praised' in the consort were: 'The bass viol or *viola da Gamba*, *Testudo* or the lute or in its place the orpharion, the treble viol, the cittern [and] the flute. If a violin is added, also a *Bajon* [bass curtal?], it adds much to the pleasure and ornament.' Since the bandora is missing from the ideal six, using an orpharion instead of the lute would restore the wire-string contribution to the ideal sound of the Consort, but with the sacrifice of the lute sound in the divisions. I would imagine that the player switched regularly between the two instruments to provide variety. The violin and bass curtal (?) apparently were added to the treble and bass viols rather than replacing them. This would give the Consort a more baroque sound.

The Consort as Processional Music

An important clue about one aspect of the Consort's popularity is a statement in the Custom Book about the use of the Consort. After mentioning viols, the Rector wrote: 'However, the combination of instruments of various kinds, which is called *concentum* or *consortium instrumentarum* (in English, consort), is much more delightful for the reception of guests and persons of distinction, especially if the songs are well chosen and pleasant.' This is the way that Queen Elizabeth was welcomed in her 1575 and 1578 Progresses, and it is thus likely that it had become a fashion, i.e. it was expected for households that had the musical resources to provide Consort music to welcome guests (especially distinguished ones) when they arrived for social functions. The guest plus entourage, when hearing the music on arrival, would process inwards in a formal way. Formal processions then were done to the steps of the pavin. This could explain why pavins are so numerous in the Consort repertoire.

The only surviving English depiction of the Consort performing is in the famous Henry Unton mural, and it is apparently playing for a procession of angels (perhaps a reminder that Sir Henry, the lute player in the Consort, was dead). Arbeau discussed the tabor rhythm for pavins, and it is not clear whether the tabor player in the mural was intended to represent an alternative to the consort or actually played with it for the procession of angels.

It may be relevant to consider that the appearance of a distinguished guest was mirrored in the sound of the Consort, with the shimmer of the metal strings reflecting the jewellery and precious metals being worn, and the filigree of lute divisions reflecting the intricate embroidery signifying 'class'. The sound of the Consort strongly implied richness and substance. Consort music might seem light-weight from today's musical point of view, but at the time and from a social point of view, it seems to have been taken very seriously.

More on Deviations

The lute and bandora parts of an ideal Consort are missing in the Walsingham ms set of part books (1588). The instrument names mentioned on the surviving parts are the same as Morley

¹ LSJ XXI (1979-81), pp 104-6

gave. The treble viol and bandora parts of an ideal Consort are missing in the Holmes ms set (c.1585). The lute part book is labelled 'trebles', which probably implies that the main intention of the book was to include the treble parts to lute duets. A duet treble part has divided variations on the melody. Several lute parts in other sources labelled 'for the Consort' are also divided variations on the melody. I can't find a criterion in the parts themselves to distinguish between them and duet trebles, and wonder why their labelling seems to. So, as far as we can tell, we presume that parts which state the melody in this 'trebles' book were used both for lute duets and Consorts. But this book also contains parts that don't state the melody. Since such accompaniment parts are not appropriate as duet treble parts, they must be for other ensembles, such as Consorts.

A treble lute would have been preferred for the music in this book (see Comm. 867 for a discussion of what a treble lute was). If an orpharion was used in the Consort instead of a bandora, the missing 'grounds' book for the lute duets would have been ideal to read from. If this were the case, there may never have been a bandora book.

In the Holmes set, instead of a flute part is one labelled 'the Recorder pte'. The second page of music has 'The Recorder parte' crossed out with 'Treble violan' replacing it. Two pages later 'The treble violan' is crossed out with 'Record:' replacing it. In his edition of some of this music in *Musica Britannica* (1977), Warwick Edwards mentioned 'two pages of treble parts accidentally copied into Holmes's recorder book'. He dubiously transcribed two pieces from the second of these pages as recorder parts, and he was probably wrong in writing that the inclusion of these two pages was accidental. Holmes probably copied the music for a particular player, and we can expect players then often to be competent on more than one instrument. This interpretation is supported by the evidence that the list of contents at the end of the part book, which is of the first 8 of the 12 leaves of music in the ms, contains the titles on these two pages as well as on the others.

There is therefore no reason to suppose that the missing book of treble melody parts was more likely to be for treble violan than for treble viol. If there was available a really good fiddler (who could be expressive without being loud) and not as good a treble viol player, the choice would probably have gone to the fiddler. This was probably the case with the Consort depicted in the Henry Unton mural. The judgment that it was more probably a fiddle than a treble viol is based on the string stop being a bit less than that of the cittern (see below). It cannot be based on the way the instrument is held because there is evidence that treble viols were sometimes held against the shoulder. It also cannot be based on the instrument's design because all depictions of English viols and violins from this period have designs strange to us. All of the viols in the set depicted in the mural have tiny bodies and long necks, and there is no evidence to favour artist error over maker's design as an explanation for this. There was an artist's tradition of varying the sizes of faces according to the importance of the subject.

In Campian's *Lord Hayes Maske* (1607), the 'Consort of 10' was obviously a loud version of the ideal Consort. Both the polyphony and the homophony were enhanced. The two violins would most probably have played the treble viol and flute parts (the latter possibly a violino piccolo playing at the flute pitch), the double sackbut the bass viol part, the bandora its own part and the harpsichord a combined cittern and bandora part and perhaps some highly divided parts instead of the lutes. Perhaps several of the three lutes could play the divided lute parts in unison, since they were professionals. At the original tempo for a paven of crotchet = 60 MM,² the speed of four notes per second for the semiquaver divisions requires competence and rehearsal to unify interpretation (dotted rhythms, gracing, etc.) rather than modern virtuosity.

Deviations occurred with the ideal Consort as well. The music gives movement contrast both simultaneously (the lute dividing and the others not) and sequentially (little division on the lute first time through a strain and much division on the second time). Beside simultaneous contrast in timbre, there is sequential contrast in some of the music when the Consort splits and

² My article on the evidence on original tempo standards is currently scheduled to appear in *Early Music* in this year, with a second article discussing it scheduled in a following issue

a dialogue goes on between the fractions. There could have been much more of this sequential timbre contrast with groups of instruments tacet in sections or whole pieces. Possible evidence for the latter is missing pieces in some part books in a set when others have them. Another type of timbre contrast that could have been common was having instruments other than the lute sometimes doing the dividing. Evidence for this is the few divided bass viol parts in the Holmes set. Also, different instruments could have sometimes been produced, like the recorder player picking up a fiddle in the Holmes set. There also could easily have been some sequential contrast between performance solely on the polyphonic instruments and on the homophonic ones.

Modern Problems with Cittern History

The cittern was a new arrival to England in 1548, played by young London gentlemen³. It had four courses, fully chromatic fretting, 40-45 cm string stop⁴ and the 4th course was tuned a tone higher than the 3rd⁵. The tuning was characteristically French, but the fretting was not. A method, most probably a translation of a French one⁶, appeared in 1568. In that same year the London Port Book⁷ listed the import of both normal citterns and 'slight' citterns. It appears that small citterns were popular for a while in Italy then since the publication by Virchi in 1574⁸ has tablature containing stretches that cannot be readily made on a full-size instrument.

When the orpharion was invented in response to the availability of ferrous wire strings that could be tuned higher than gut, the small cittern (with a string stop of about 34 cm) appears to have used this wire to tune about an octave above the large cittern, and to have adopted Italian tuning (with the 4th course a major third higher than the 3rd). By the time Praetorius wrote his comprehensive book on instruments (in the second decade of the 17th century), he only knew of the small cittern as an English instrument, and didn't know of any other English cittern. The question of concern is whether the surviving solo and Consort repertoire in Italian tuning from the period 1585 to 1615 was for the small cittern or the large one an octave lower. It is unlikely that both were used in the circles that produced the repertoire since no source from that time made any size distinction, as they did between the treble lute and (mean) lute.

The Consort repertoire was first explored in modern times using surviving 18th century English Guitars (called 'citterns' then) before reproduction c.1600 citterns were made. The 42 cm vibrating string length of English Guitars led to a tuning with e' being the highest string pitch. When reproduction citterns were made, they were modelled from surviving instruments, which were almost all Italian (none English and none with four courses), and the vibrating string length was also appropriate for a highest string pitch of e'. This is how the attractiveness of the Consort repertoire was first enjoyed by most people in the field, and thus they have become very resistant to any other kind of sound for it.

In the early music movement, once an interpretation of evidence, or an exploration without awareness of evidence, produces a result that is enjoyable, it becomes part of the culture as an historical problem solved. If subsequent scholarship were to produce results that conflicted with that culture but were clearly more enjoyable, no doubt it would be taken seriously. This very rarely happens because we all cherish the sounds that have moved us emotionally when we first heard them. So we can't help but be annoyed by later scholarship that claims that a different sound is more historically accurate, and tend to raise our standard of 'proof' to heights that are objectively unrealistic. This emotional conservatism leads to very little historical scholarship performed that could conflict with the current early-music culture, and each aspect of that culture getting locked into the way that it first developed.

³ Thomas Wythorne autobiography

⁴ Eglington Table at Hardwick Hall

⁵ Mulliner Book (c. 1545-85) and Lord Middleton Lute Book (c. 1575)

⁶ Rowbotham

⁷ JLSA X(1977), p 116

⁸ P. Virchi, *Il primo libro di tabulatura di citthara* (1574)

Thurston Dart considered the small English cittern depicted and discussed by Praetorius as an experimental curiosity. Ian Harwood was a pioneer in producing versions of early Italian citterns for use in Consort repertoire and directed his own Consort for many years. He could be a very good scholar (when it did not conflict with his musical judgment), but he just couldn't accept the balance when the small octave-higher English cittern depicted by Praetorius was used in the Consort. When the Abbott & Segerman *LSJ* article (1975) presented an excellent case for this to be the case historically, Harwood always mentioned 'I don't believe it' whenever the paper was mentioned. He was never willing to debate the issue in scholarly publication. His authority as a scholar was highly respected, so his opinion prevailed in early music circles. He was, after all, the leading expert on the Consort because of his excellent research into the origins of the Cambridge (Holmes) part books and his reconstruction of much of the repertoire, which he generously shared with others.

After a few years, Harwood wrote the article 'A case of double standards?'⁹, suggesting that viols sometimes played at a pitch standard a fourth higher than usual by using sets shifted one size smaller for each part, and that the Consort played at the higher standard using the small cittern. He found the balance then acceptable. There needed to be appropriately small treble viols available for this, and he cited such a surviving instrument by Henry Jaye made in the 1630s. Up to a half dozen English instruments of such a size that look like viols survive, and all of them have been 'restored' with 6-string viol necks and pegboxes replacing previous 4-string ones. When they have dates, it is in the 1630s or 40s. In the *GSJ* (1996), I suggest that they could have originally been made as violins or violas with viol-like bodies, in the same spirit as Simpson's preferred division viol being made with the body of a bass violin. There is no early documentary evidence for any English treble viol being smaller or larger than another, and we would expect such evidence if there were different sizes that could not be functionally interchangeable, each in use widespread enough to be commented on.

Harwood did not mention the Consort playing at the low pitch standard in this article. If that omission was intentional, this would imply that Morley exclusively meant the tiny treble viol in his instrumentation without indicating that it was different from the usual treble viol. That is exceedingly unlikely, so we are left with Harwood's speculation being only about a possible minority practice. It is probable that he soon realised this himself because this paper was not included amongst the references in the *New Grove Dictionary of Musical Instruments* article on the cittern that he co-authored with James Tyler, or in the 'Consort' entry by Warwick Edwards, on which he was surely consulted.

Some aspects of the cittern situation have become clearer in the 20 years since the *LSJ* article was published. It is very remarkable that the cittern solo and the Consort repertoires flourished greatly in the first decade of the 17th century and completely terminated during the second decade. The lute solo repertoire continued and shows no sign of any new fashion in music unsuitable for the cittern or Consort. An explanation for this that works very well involves the history and technology of strings. Abbott & Segerman (*GSJ* 1974) pointed out that the availability of a new type of high-tensile steel (that could tune at least as high as gut) is needed to explain the pitch and length of strings of the orpharion first course, and it ceased to be available some time early in the 17th century. Such wire is also needed for the e" high pitch of the small English cittern. In Comm 438 (amplified in Comms 439, 440 and 866), it was found that such wire was probably only made in Jobst Meuler's workshop in Nuremberg, that from 1610 there was another wire maker with powerful connections in the Imperial court trying to stop him from selling it, and that by 1621, he could not fill orders for it unless it was politically expedient (i.e. it had Royal backing) and was passed by a meeting of the Town Council. This explanation only works if the cittern of the repertoire was the small one, with an e" first course, and the desired sound of the Consort was dependent on that small cittern.

It is also quite remarkable that in the last quarter century, when professionalism in early plucked instrument playing has been widespread, no cittern player has emerged who can

⁹ *Early Music* 9/4 (1981), pp. 470-81

effectively perform the extensive surviving solo cittern repertoire of undisputed quality. One of the more powerful arguments in favour of the small cittern presented in the 1975 *LSJ* paper was the large left-hand stretches required by the tablature. Some chords have stretches that can just be managed with considerable discomfort on the large cittern by someone with large hands, and then they cannot be grabbed quickly. It is highly unlikely that the music by Robinson and Holborne would have been published commercially if it were so unplayable by the average purchaser.

With the solo repertoire being ignored, it is unfortunately often forgotten when modern opinions as to which cittern is historically correct for the repertoire have been expressed. This was the case in Comm. 931 (Q56, 1989) by Peter Forrester. He argued that the small cittern at the high octave overpowers the lute in the Consort. This could well be a valid criticism of the performances of this combination that Peter heard (it was probably me playing the small cittern). It was probably a similar musical judgment that impaired Harwood's scholarly judgment and led him to his fourth-higher theory. But in spite of balance being a very important issue to modern early musicians, their perception of it is completely invalid as evidence for investigating the history of instruments.

There is a general reason for this invalidity and specific reasons in this case. The general reason for its invalidity is that we have no independent evidence of what early musicians considered was good balance, and we have no justification in scholarship to project our judgments onto them. Praetorius's vocal ranges, and Gombert's advice reported by Ganassi¹⁰ and the preferred bass string types of modern lute and viol players are evidence that modern concepts of treble-bass balance are bass heavy compared to original judgments. Hearing just the large fraction of the lute's notes in divisions that are played when the cittern has no notes to play might possibly have been fine to them. A clue as to the original importance given to the cittern's sound contribution to the Consort is that when it split for a dialogue, the cittern played continuously through it.

A simple specific reason is that we have no evidence on the way cittern players originally cut their quills. They can be cut to be very stiff making the cittern stridently loud, very flexible making the cittern whisperingly soft, with all degrees in between possible. Thus the way the quill is cut can provide any balance one wishes. And how far from the bridge the quill is applied can give a wide range of sweetness and projection of tone. Thus the player rather than the instrument should have been blamed for any apparent imbalance.

The most important specific reason is that the lute is quite capable of satisfying modern balance requirements if it is played and strung in a more historical way. As Peter agrees, a treble lute that is smaller than the usual mean lute has greater projection. Gut strings have a punchier richer treble sound than nylon, and this aids projection considerably. The main problem though is with modern lute technique. Michael Shaffer developed and introduced this thumb-under technique, and the advantages that he successfully promoted for it are that one can play faster and more cleanly with less of the plucking noise that recording engineers object to. In this technique one plays softly either over the rose or quite close to it. The evidence of wear on the soundboards of surviving lutes where the little finger pressed, and of depictions of lutes being played, indicate that original technique involved playing considerably closer to the bridge. This gives a less sweet sound with more plucking noise. It is a dryer sound with much more projection. One can also play harder there. When Steve Heavens plays lute (with gut strings) in Consorts with either size of cittern, he plays near the bridge using thumb-under technique, and he can always play loud enough to be heard clearly.

In Comm. 931, Peter commented on the considerable variation in the 'standard of musical attainment' amongst the Consort parts. His concept of 'attainment' is the modern one of how fast one is able to play. At original tempos this variation vanishes. Evidence for original criteria for attainment usually involved 'sweetness' of playing, which seems to have been mostly about invention in gracing.

¹⁰ see second paragraph on p.33 Q 46 in Comm. 773

Peter presented three points as 'some evidence' that I 'was wrong to give the small cittern an octave tuning'. First he cited the Tabley ms (1658) where the gitterne (a surviving version of the small English cittern) was called a treble cittern when, if it was at the octave (of the large cittern), it would have been called something different. I have no idea of what it would have been called, but it wasn't at the octave. Jobst Meuler's strong strings were no more available and it was probably tuned then as Peter suggests. Peter's second point is that at Praetorius's pitches for the cittern strings and his string gauges, the string tensions are much higher than his experience with citterns would suggest, and they would be acceptable at Peter's tuning.

Peter's experience in stringing citterns has tremendous value to modern musicians because it efficiently gives them what they want from their own hearing and playing experience. This has nothing to do with music history. The principle of stringing followed by the early music movement has been to choose the lightest stringing that sounds acceptably. Subsequently found evidence on historical stringing has indicated that this is probably not light enough for the basses of lutes played domestically, it is usually half the original tensions on the violin family, and about right for the late 17th century large English cittern. This cittern and the gittern (descended from the small English cittern) appear exclusively to have been domestic solo instruments, a function for which the sound of the clavichord was considered adequate. A cittern used extensively in ensembles (like in the Consort) could well have had heavier stringing.

Peter's experience is with modern playing and stringing of citterns as citterns. Praetorius's Englishman played the cittern like a modern mandolin, and the tensions of Praetorius's string gauges at the high octave are similar to the modern mandolin. That tension level wouldn't collapse a properly made cittern with the back half open if the half is chosen wisely. Praetorius used the instrument in his own music, mostly doubling with a violin. Peter is not the first to assume that Praetorius made a mistake because what he wrote did not fit a cherished idea.

Peter's third point is that 'Maister Birds Galliard' for cittern and bass viol in the Holborne cittern book implies that it was for the small cittern with Peter's tuning. I discussed this piece thoroughly in the 1975 *LSJ* paper and showed why and how the viol transposed. Holborne wasn't asking the cittern player to pick up another instrument for one piece of music.

Peter's last point was meant to establish that though the small cittern existed, the large cittern was the usual size. He used the term 'usual size' meaning 'large cittern' several times in his *Comm*. We have disagreed about this for over 20 years and I don't expect to convince him this time. I wish that he (or anyone else) would some time come up with just one piece of historical evidence favouring the large cittern as the historically correct instrument for the 1585 - 1615 surviving solo and Consort repertoires, instead of either ignoring or trying to discredit the evidence favouring the small cittern or its implications.

Of course Peter is absolutely correct as far as the early music culture is concerned. The large cittern has always 'worked' in the Consort. It is plentifully available because of Early Music Shop kits designed by Harwood. Its contribution to the sound of the Consort feels 'right'; and the idea of the small cittern doesn't seem to (few have experimented to get it to 'work' for them). The cittern players feel that there is no advantage in considering any change, and the case for the small cittern is not 'conclusively proven'. No-one seems to mind the loss of the solo repertoire because they have never played it comfortably enough to appreciate the music.

Peter is a leading researcher, writer and maker in the cittern field. The players look to him to provide justification for what they feel should be historical truth because it agrees with their musical judgment. I consider that historical truth is only approached by objective choice based on historical evidence, and that the musical judgments of today's musicians and musicologists (no matter how widely agreed) is irrelevant in pursuing this truth. This sadly makes me rather unpopular. I am sorry for both of us that I have to argue with him here.

Wood Treatment to Improve Sound

Part 1: From well-seasoned new wood to antique wood

Most musicians consider that, in general, original violins and viols made in the baroque sound better than modern ones. Modern instrument makers tend to avoid such comparison, competing for reputation in the newly-made instrument market. Many of them believe that their instruments are as good as the old ones, so they would not be interested in a treatment of the wood that would eliminate the main advantage the old wood has, age. Others might.

The theory behind this treatment was presented in Comm. 775. A friend of mine has been using it for about five years on all of the instruments he makes. He calls it his 'naughty secret'. I don't identify him because any process that differs from current traditional practice in the fiddle world is treated with great suspicion, and reputations can easily suffer from the prejudices of respected 'authorities'. As other makers do, he sells his instruments on the basis of craftsmanship, tone, and recommendation. His customers do remark about how much like old-master instruments his sound like. There are now more of his instruments being played regularly in one of the top U.K. professional orchestras than those of any other maker.

It all started when I was writing Comm. 775, considering Stamm's research as a possible basis for wood treatment to reproduce the sound of baroque instruments. Hemicellulose degradation is the main consequence of wood aging. The effect of time can be enhanced or replaced by elevated temperature and/or moisture. Vuillaume had apparently already done it. Flesch's report that Vuillaume ruined instruments is likely to have been biased opinion of those who mistrusted Vuillaume and feared instability in the fiddle trade. Vuillaume deserved mistrust, but he was too good a researcher, maker and businessman to ruin instruments.

When I discussed Vuillaume with Reg Lawrence, he remembered a statement in an art history book saying that to stabilise the wood for painting on, an early source indicated that one boiled it for 4 days. This seemed more controllable than the baking Flesch said Vuillaume did. In our test treatments we stewed the wood at a few degrees below boiling point to reduce the amount of water topping-up needed. Weights held the wood submerged initially (it didn't need weights after a day or so). We added vinegar to speed up the hydrolysis of the hemicellulose. Black clouds came out of the wood while it was stewing, showing the sugars (produced by the breakdown of the hemicellulose) caramelising.

We measured dimensions, dry weight and resonant frequency of a square piece of flat soundboard wood with stewing time, and found that the elastic modulus and density dropped steadily, but their ratio (the velocity of sound) remained essentially constant. This seemed promising behaviour for simulating aging, and we passed the project on to the above-mentioned friend. We knew that no-one would take this seriously until superior instruments were made of stewed wood. The friend is a first-class maker, and in return for his risks in wasting time and materials if it didn't work, we promised to withhold telling others about it if it did work, letting him enjoy a competitive edge for some time. That time is now up. The treatment has worked, fulfilling all expectations. Since there is no accepted objective test for violin quality, all that we can actually claim to have proved conclusively is that stewing the wood beforehand does not in any way prevent the production of instruments that are considered to be top class. We believe that we have reproduced the sound of antique wood.

The stewing of wood is very different from ponding, which was often used on timbers for ship building. Ponding involves immersion of the wood in water for some years at ambient temperatures. This process will probably degrade the hemicellulose by hydrolysis, and as a result would confer stability (as well as improved sound). This is how we understand the mechanism today, but we must remember that in early times, they usually assumed that they had to add something to the wood to give it a property it didn't have before.

In ponding, bacteria eat away the edges of the pores between cells. When taken out of the water, moisture moves in and out of the wood much faster than before. This is a desired property of wood in ship-building. Recent research on old-master wood (Woodhouse, *The Strad*, '91) looking for bacterial action on the pores, shows no sign that the wood was ponded.

Part 2: From new wet wood to seasoned wood

The difference in sound produced by fully air-dried new wood and similar wood that has matured for several years is much greater than the difference in sound discussed above (between wood that has matured for a few years and wood that has matured for a few centuries). The modern tradition is to mature for five years. Sibyl Marcuse (*Dictionary*, 1964) cites Fioravanti (1564) writing that wood for harpsichord soundboards must be dried for several years and be as old as possible 'so that it will not swell in the humidity'. This illustrates how makers were concerned with the physical properties of the wood without necessarily realising that what was needed to stabilise the wood also improved sound.

There are several pieces of evidence indicating that old-master violin makers often did something else rather than just wait for the wood to mature before using it. One comes from the Guarneri exhibition recently held in New York. Professor Peter Klein of Hamburg University was invited to make a dendrochronological (tree-ring dating) study of all of the instruments in the exhibition. He concluded that Guarneri's soundboard wood was as young as one to three years old when incorporated into the instrument. Other evidence is the chemical analyses (by Nagyvary and Schnur) mentioned in Comms. 793 and 881, where instrument parts apparently had deliberately been impregnated with salts. Remy Gug has shown that this was a very natural thing for a craftsman at that time to do to his wood to give it favourable properties (e.g. to be resistant to woodworm). Only one of Gug's sources (Palissy) associates the treatment with improved sound, so it could well be that this usually was not a major motivation for doing it, but it happened to be a major byproduct of the treatment.

Impregnating wood with salts is not easy. It needs time and high temperature just to get the salt solution to penetrate all the way. The amount that got in doesn't seem to be enough to have significantly affected acoustic properties (this was probably not the intention), but the stewing process involved in getting it in could well have given the wood sound-improving hemicellulose removal equivalent to a number of years of maturing and aging.

Leopold Mozart indicated that the practices of makers varied widely. One good violin that he knew of had been made of wood that had originally been 'smoked'. In this case it was probably the heat rather than the smoke that contributed to the good tone.

Conclusion

Stewing wood gives it good properties. It relaxes internal strains, making it very unlikely to warp when cut to final shape. It reduces subsequent wood dimension changes with changing humidity. So blanks for necks, pegs, blocks, etc. can be thrown into the stew with advantage. It also makes the wood sound better. It is a safe and practical process. It also is probably authentic for many original makers.

A possible estimate for calibrating the process could be the Stamm extrapolation for spruce, where 1% loss of dry weight is equivalent to a century of aging at 20 degrees C. Maple may be different. We need to compare air drying plus 5 years maturing with the stewing of fresh wet wood. My guess is that the latter can replace the former, and that this is what many early makers did. This needs to be experimentally checked out.

Finally, I would like to remind readers that evidence for old instruments sounding better than new ones is confined to bowed instruments and mandolins. I would not recommend extensive stewing for soundboards of lutes or guitars, but a shorter stew of new wet wood could well replace the initial drying and maturing.

1995 FoMRHI List of Members — 3rd Supplement as at 21 January 1996

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

- Maria Bania, Box 22086, S-500 02 Borås, Sweden; +46 - 33 - 515001 (trav, str 4tet, hpschd, fpno; P).
- * Tjeerd Bosklopper, +31 - 50 - 4095332.
 - * Jan Bouterse, t&fx +31 - 172 - 445957.
 - * Christophe Brass, 27 Löhstr., D-78647 Trossingen, Germany.
 - Leo D Cloma, 230 Cristina Street, Guinhawa Subdivision, 3000 Malolos, Bulacan, Philippines; +632 - 814 8241 (bar ww, keybds; P).
 - * David Freeman, 252 82 Kamenny Přivoz 274, Czech Republic.
 - Tetsu Ito, 80 Limegrove, Newark, Notts NG24 4AH, UK; 01636 - 700285 (ob; M).
 - Julia Victoria Morley, 34 Engate Street, Lewisham, London SE13 7HA, UK; 0181 - 318 5838 (hpschd, spnt, virgnl, clavchd, pft; M,R).
 - * Museum of Welsh Life (change of name from Welsh Folk Museum).
 - * Sean Rawnsley, Le Rial, F-81140 Vaour, France; +33 - 63 56 32 38; fx +33 - 63 56 39 11.
 - * Carl Rennoldson, Chester Cottage, 131 Dover Road, Walmer, Deal, Kent CT14 7JH, UK.
 - * Peter Roovers, +31 - 50 - 3140852.
 - * Wiebke Schneider, Termaar 5, NL-6289 AE Margraten, Netherlands.
 - B Taylor, 17 Elizabeth Gardens, Southsea, Hants PO4 9QZ, UK; 01705 - 751982 (hpchd, spnt, virgnl; M,C,R,coll).
 - * Rob Thornton, Woodlands Raod, Hertford - my apologies; I misspelt his name in the last Suppl.
 - * Marco Tiella, (0)464 - 420706.
 - * Michio Tominaga, Chuo University, 742-1 Higashinakano, Hachioji, Tokyo 192-03, Japan.
 - * David J Way & Zuckermann Harpsichords, e-m rdaub@ix.netcom.com.

Many of the Dutch are now going through what we suffered a year ago - telephone numbers are changing. There are some here, but more to come. Old numbers will work till April; after that...

As you see here, many members are going international on their phone numbers - should we all? Your opinions will help me decide between now and the next main list. The differences are: putting a + before the international code (+44 for UK, for instance), and dropping the 0 which is only used inside the country. The advantages are that it saves people abroad looking up the country code, and that everybody within the country knows that Oxford is 01865, whereas foreigners may not know that they should dial only 1865, and there are some countries where you do have to use the zero from abroad, so that showing it only where necessary from abroad does have its advantages.

Please let me know what you think.

Also please let me know what you think about the List of Members. Since we recovered from the financial panic a while back, I'm inclined to keep it annual, instead of the threat to make it every two years. On the other hand, if you don't use it, then there's no point in spending your money and my time on it.

Jeremy Montagu