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FELLOWSHIP OF MAKERS AND RESEARCHERS OF HISTORICAL INSTRUMENTS
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FELLOWSHIP of MAKERS and RESEARCHERS of HISTORICAL INSTRUMENTS

Bulletin 77

October, 1994

SUBSCRIPTION RENEWALS: Once again it's time to renew your membership, and you very nearly had a nasty shock: Barbara discovered three or four weeks ago, to her horror, that we have spent all the money we've got on the first three Qs for this year (they were all bumper issues, 96 pages, 76, and 88), plus of course the List of Members. As a result, we thought that we were going to have to put the rates up. However, Eph has found what looks like a cheaper printer and we're going to try him, keeping our fingers crossed that he's going to be good enough, and therefore we hope that we can keep the sub the same as it was. For the moment we will chance it. There may have to be some emergency action in the middle of next year, but with luck we'll be OK. We'd have been sorry to put up the rates because we did so only the year before last; the trouble is that paper and printing have always gone up faster than inflation.

So the rates are, as last year:

For UK and for all countries by surface mail:	£ 10.50	
to which should be added if appropriate:		
Supplement for airmail to Europe	£ 1.50 totalling	£ 12.00
Supplement for airmail overseas	£ 3.00 totalling	£ 13.50
Supplement for payments not in £ sterling	£ 5.00 (US \$ 8.50 approx)	

and the address to send it to, is: Barbara Stanley, Honorary Treasurer FoMRHI, 21 Broad Street, Clifton, Beds SG17 5RJ, UK; cheques made out to FoMRHI, of course.

We're happy with your own cheques if your country permits it; don't go spending money on bank drafts in sterling unless that's cheaper than the £5 supplement for foreign currencies. Eurocheques work if they are backed with your card number. The GIRO account number is 27 316 4406. The GIRO postal orders (the ones that come through the post with a little piece of pink card with your name on it) have always worked. And as far as we know, **neither Eurocheques nor either sort of GIRO need the foreign currency £5 supplement**. If it's cheaper for you to pay money directly into our account from your bank by fax or whatever, the account number is 14108260, Bank sort code 77-05-03, T S B Bank plc, 2 Bucklersbury, Hitchin, Herts SG5 1BA. If any of you in one area find it cheaper to get together and send one payment for several people, you're welcome to do so (some of our Dutch members have done this), but for heaven's-sake make sure that Barbara gets a list of all the people concerned; otherwise some people ain't gonna get their Qs. This applies to individuals, too. There are always one or two anonymous payments, which is nice for FoMRHI funds but rough on whoever sent them.

As always, if you can afford to add something for people who live in countries which forbid the export of money, or restrict it by charging grossly excessive amounts for parity (£12 is more than a month's income in some places), they will be very grateful. There are still many countries to which this applies, both east and west, and I'm always happy to hear of more people who'd like to read FoMRHIQ but can't for that reason, and to put them on our lists.

I remind you that Barbara asks PLEASE don't send your subscription registered or recorded delivery unless this is essential at your end; if you do, the postman won't deliver it and she has to trek down to the post office to collect it.

You will find an invoice tucked into this Q, both to serve as an additional reminder and also to make it easier for you to pay if you can just hand it to your bank (or if you're lucky to someone who will pay for you) and leave them to get on with it.

You may be interested to know how the costs have worked out with Beeprint, who have printed us from Q 1 to Q 76. Eph has done some calculations and Qs for this year so far apparently cost a basic £140 plus, for every four pages, £68.75. There is then postage to add, plus the cost of envelopes, plus a certain amount over the year for general postage and other expenses, so that if we were to average 64 pages per Q, our total costs are around £6,000 a year. Add up the number of members and divide that into £6,000 and you'll see why we were panicking and thinking we'd have to put the rate up by a swingeing £3.50 - even at £14 we would have had little in hand. One answer is to have some more members. So maybe you could mention the wonders of FoMRHIQ to friends! Another is to cut costs, as we are doing by changing printer, and another way of doing this would be to have a List of Members every two years instead of annually (each one has been costing about £500); what do you think about that? I like the annual myself, but I'm not sure that 'I like' is worth £500. You would of course continue to get additions and changes in every Q; what you would not get is the drop outs. I'd be interested in your opinions on this. If our new printer really is as much cheaper as his quote would suggest, we could keep the List annual, but it would be useful to have your opinions in hand.

FURTHER TO: Bulletin Supplement on p.5 of last Q, last paragraph: What I mean by a Journal of Record is a journal that can be found in most big libraries and is either refereed (which I think is a lot of nonsense) or whose editor can and sometimes, perhaps often, does reject articles because s/he doesn't like them (a better arrangement than refereeing; if the editor can't judge what's good and what's crap, and can't ask a pal over a beer, s/he has no business being editor). Neither of these criteria applies to FoMRHIQ. We do very occasionally reject material (see recent controversy in this respect, eg Comm.1275) but on the whole we prefer to express our opinions in the next Q (eg see Comm.1278 and a couple in this Q). I prefer to think of FoMRHIQ being on your shelves, and often better in the workshop than the study, rather than in the library. We are in the British Library and the other copyright libraries in the UK, in the New York Public Library and in a handful of university libraries. Maybe I'm wrong there - we've changed a lot since we started (using 'changed a lot' both as a transitive and an intransitive - we ourselves have greatly changed and we have changed many things throughout the early music world). Perhaps we should aim at getting into more libraries (it could help solve any financial problems), but I have the fear that we might then drift towards greater respectability. We are casual and informal, and nobody could call us respectable, and my own inclination is to remain so. Anyway, the main function of a Journal of Record is to be available to the scholarly world as a whole, and with our circulation we aren't - by scholarly I don't just mean academic, but everyone who is working seriously in the relevant field.

Comm.1281 "Forces exerted by bore measuring tools" by Robert Cronin: Charles Stroom writes: he is absolutely right, and if I would have consulted my own index, it would have been easy to check the existence of these articles. I have actually tried to measure the forces and have taken one of my old centre pieces of an alto recorder where the enclosed angle is around $2 \cdot 0.9$ deg. I inserted a Mitutoyo tee-piece with springs loose and let it go by its own weight. As it goes down, the horizontal piece gets more and more contracted and at a certain point it will stop because of a balance of forces. I then fixed the tee pieces, retracted and measured the force of the spring. I have repeated the test several times, it reproduced the stopping point and also the measured force was within reasonable variation. The weight of the t-piece (plus extension) was 60 gr and the force measured 160 gr, well within the predicted range of 5 times of Robert Cronin. Q.E.D.

Comm.1287 & its sequel herewith: David Crookes specifically asks for comments on these.

A Comm from Charles Stroom herewith: (which like his note above came by e-mail) I would endorse his recommendation of MICAT-L - a good deal of useful conservation material has already appeared; it is fairly strictly restricted to that subject and to instrument technology. Charles had repeated the full instructions of how to function on each list, which to save space I've deleted (with his approval) because if you decide to subscribe you will receive those instructions anyway, and if you don't subscribe you don't need them.

A couple of reviews herewith of the Boston keyboard catalogue: For one of the first times that I can remember an unsolicited review has come in from a member to a book that we were invited to review. This is excellent, and since I first wrote this paragraph another has come in, this one for the Liverpool catalogue. To have more than one opinion of something is always worthwhile. Please don't hesitate to do this. It's probably safer (as against risking wasting your time) to wait till after one review has appeared because we can only review things that we are invited to review, so that we would not print a review of a book that had not been sent for that purpose (see a note on the Magnano Clavichord symposia below). The reasons for this are that an adverse reviewer could be sued for libel unless a review had been invited, and if a publisher is too mean to send us a copy, why should we review it for them? But, if like the two members herewith, you don't mind taking a chance on your work being wasted, do by all means send anything in that you like.

SAD NEWS: Those who know him will be sorry to hear that I've had a letter from Arthur Marshall saying that the terminal illness from which he is suffering has reached the stage where all his musical interests are ceasing, so he has resigned from FoMRHI. He was a member almost from the beginning (no 63 in fact), and, as I wrote to him, we have enjoyed his company.

A SCHEME: I've had a flyer, which I'll send to Eph, from the Apprentice-Master Alliance of 100 Southgate Road, London N1 3JB. The idea is to match up people who'd like to teach apprentices with people who want to learn. It could be a good idea; the only thing that makes me dubious is that they are asking quite a lot of money (£30 a year for prospective masters and £15 for apprentices), and insisting on a bankers' order rather than a one-off try-it-and-see. There have been so many bogus directories over the years that one is, perhaps unduly, suspicious of all such approaches. Anyway, you may want to consider it for yourself if Eph thinks it worth including. Just bear in mind that we're not recommending it, just telling you about it.

COURSES, etc: I've already told you of the Bate Weekends in the pipeline: November 5/6 is a **Renaissance Wind Band Weekend** with Eric Moulder. And November 26/27 is an **Alec Loretto Recorder Weekend** with Alan Davis to look after the playing side. Fuller details in Q 75. In the New Year, a **Harpsichord Weekend**, with Martin Souter, January 28/29; fuller details in Q 76. February 11/12 is a **Gamelan Weekend**. Advance booking is essential for this because the number of places is limited by the number of instruments. Probably safer to book ahead for Martin Souter's Weekend, too - we might decide to put a top limit to it for reasons of space. With all the harpsichords there, we can't get more than about 15 people in round them. Cost of Bate Weekends is still £20 (£15 Friends of the Bate Collection and students). To book, send a cheque made out to The Bate Collection, or just turn up for Eric's and Alec's.

Eric Moulder has sent me details of the West Dean courses next Easter, 12-21 April 1995. There are seven tutors and a wide range of instruments. For beginners: flute and recorder, Appalachian dulcimer, flat-back mandolin, rebec, rote, Irish knee harp. For those with moderate ability: celtic harp, viola da gamba, historic woodwind, mandola, cittern. For the highly skilled: lute, guitar, viola d'amore, violone, violin, viola, spinet, troubadour harp, harpsichord, hurdy gurdy, organistrum, northumbrian small pipes, baroque woodwind. Fees last year were £498 res-

idential and £354 non-residential but with meals – they don't give them on the leaflet for this year but tell one to ask for information! To book or for further information, write to The Course Organiser, West Dean College, West Dean, Chichester, West Sussex PO18 0QZ. Don't leave it too long; numbers are limited for all instruments and some book up fast.

They also have Weekend courses. 13-15 January there are several on making moulds or jigs for a variety of string instruments and for either finishing or beginning a hurdy gurdy, a mandolin, a mandola, or keywork for a clavictherium. There's a Renaissance Music Weekend with Nancy Hadden, 17-19 February. Fess for weekends are £139 residential, £89 non-residential but with some meals.

There'll be a **Second Clavichord Symposium at Magnano** 21-23 September. Contact Bernard Brauchli, 19a av des Cerisiers, CH-1009 Pully, Switzerland, and if you want to read a paper or give a recital, do so before 15 January. I told you in the last Bulletin that I'd asked for a review copy of the proceedings of the last one; a copy has now arrived but with it there was a bill, so we can't review it. As I've said above, one can, legally, only review what one is invited to review, ie things that come free as review copies. Maybe I'll bend the rules enough to tell you I was happy to pay for it!

The Metropolitan Museum of Art is arranging a Symposium on The Violin Masterpieces of Guarneri del Gesù, November 22-December 4 this year (1994). Deadline for registration is Nov 15. Send a check for \$200 to Department of Musical Instruments, Metropolitan Museum of Art, 1000 Fifth Avenue, New York NY 10028-0198; perhaps better telephone them first at (212) 570-3919 to see if they have places left. If they'd told me sooner, I'd have told you. I gather that it's in connexion with an exhibition of his fiddles, which should be worth seeing.

BOOK AVAILABLE: Birdalone Music, 508 North College Avenue, Suite 333, Bloomington, IN 47404-3831, have just published an English translation of *Dauprat's Method for Cor-Alto and Cor Basse*. Cost is \$150 + p&p.

CODA: That's about it till I've done the Memb List Supp etc. I have now, and have added one or two bits and pieces. Sorry, this has got held up a bit; a rather rough beginning of term with too much to sort out, and as that's what I'm paid for this has got side-tracked.

DEADLINE FOR NEXT Q: January 3rd I suppose, since that's the first day with post in 1995, but if you can get things in earlier (ie before Christmas), it's safer. Do please get our subscription renewals in by then, too. The more they're spread out from now till New Year, the easier it is for us. Those that come in after the January Q goes out will have to wait till the April Q goes out for both January and April. So do please get them in before January.

Jeremy Montagu
Hon.Sec.FoMRHI

On Comm. 1287 by D. Z. Crookes

No competent physics book would lead anyone to design a psaltery the way Crookes does. The Harmonic Scale (length inversely proportional to frequency, doubling with every drop of an octave), as used for the higher strings on a harpsichord, keeps all strings equally near to their breaking point, but that is not necessarily desirable or convenient. The biblical instrument going by this name apparently was the pillarless harp, and from what we know of such harps then, they had a scaling that was more linear than harmonic. This objection does not argue against his numerological analysis because the numbers could just as well refer to positions on a monochord to form the scale (which is what the physics book would have been discussing).

An objection that does is that nothing has been 'established for certain' by Crookes, in spite of his claim. Scholarship can never be that good. But whether his analysis has any scholarly value at all depends on his demonstrating that the numerological association of this scale with these verses in Psalm 49 is objectively special. We can't take his word for it. All that he has established is possibility, not probability. Applying the same industry and imagination to other verses, trying other ancient scales as well, could perhaps lead to equally impressive results. If he intends to leave this for us as a dare, he will find that we are not motivated enough to bother, and will just think of his paper as a slight of imaginative speculation. For his work to be acceptable as serious scholarship, he must demonstrate that his hypothesis objectively fits the evidence better than alternatives. The obvious alternative here is that it is an edifice built on pure chance.

When I mention scholarship here, I have been referring to the search for historical truth. But musicians and musicologists have a concept of musical truth which transcends historical evidence. Their main evidence is that it sounds 'right'. If Crookes continues the way he has, fulfilling his stated objectives, the resultant melodies, organa etc have a chance of sounding 'right'. The music world would want them to, so 'right' would be liberally interpreted. If they do sound 'right', since they will be covered by copyright (Comm. 1287 was), he could well get rich from royalties. This could happen whether or not he has established scholarly (historical) credibility. Perhaps the rest of us should start thinking about designing up good-sounding 'biblical' harps to cash in as well.

On Early Music and Music History

Changes of fashion are always thought of as improvements in the short term, though in the long term one realises that they rarely are so. So being involved in the arts usually feels like being part of a rolling consensus, being part of progress. This is encouraged by the analogy of progress in technology, giving us greater mobility, easier communication, etc. as time goes on. So it is natural to expect scholarship to develop as a similar rolling consensus. But the conclusions of scholarship can change radically as the result of new evidence or of the successful interpretation of evidence that has not been understood before.

If an art form is supposed to be inspired by a branch of scholarship, and that scholarship has undergone such a radical change, difficulties can arise. The art form has established aesthetic principles that are successful as art, but may no more be supported by the related scholarship. The sensible solution is to distance the art from the scholarship. This is happening with early music, where the appropriately vague term 'historically informed' is steadily replacing terms implying more fidelity to historical scholarship.

Printing Costs

Barbara sounded the alarm - we didn't have the money to pay for this Q! The obvious thing to do was to raise subscriptions, so I calculated costs to make suggestions as to how much to Jeremy and Barbara. By far, the biggest cost was the printing, so I asked for quotes for Q's of various sizes from Roger Harvey (who runs Beeprint, our printers since the beginning of FoMRHI) and from Clayton Press in Huddersfield (who do the Viola da Gamba Society Newsletter and I heard was cheap). When I showed the Clayton Press quote to Roger, he said "I can't do it for that. You have no choice. It will be the end of an era." In fact, it was so much cheaper that we wouldn't have to raise subs. I am convinced that Roger has always been giving us the best deal he could. The biggest cost in the printing has been in photographing our camera-ready copy and converting film images into metal printing plates. The new technology that Beeprint doesn't have is making printing plates directly on a computer printer, with an electronic scanner reading the copy.

Keyboard Musical Instruments in the Museum of Fine Arts, Boston. John Koster. Museum of Fine Arts, Boston, 1994. (Available from The Museum Shop, 465 Huntington Ave., Boston MA 02115. \$85 plus shipping)

A major United States keyboard collection has finally issued a catalogue of its instruments and this publication is state-of-the-art. Its 408 pages provides 16 color plates, 242 duotones, 79 line drawings, a glossary, wood terminology, an extensive bibliography, as well as instrument descriptions. The Museum of Fine Arts, Boston has an excellent collection of early keyboards - 15 of the harpsichord family, 3 clavichords, 4 organs, 5 reed instruments, 23 pianos, a few miscellaneous, and a unique piano action, from c.1550 to 1894.

John Koster, the author, with twenty years experience building early keyboard instruments in the Boston area, is presently Conservator and Associate Professor of Museum Science at the University of South Dakota's Shrine to Music Museum. He is an excellent writer and keeps his own opinions to a minimum. He provides references to substantiate virtually every statement presented that isn't a direct measurement or observation of his own. The endnotes which accompany each of the 54 essays are copious and filled with valuable additional information which, taken with the text proper, provides a superb resource book for anyone interested in early keyboard instruments, their technology, decoration, or social history.

This volume will, no doubt, receive much well deserved praise for its organization, scholarship and production quality. It should be on the bookshelf of anyone interested in keyboard instruments. Perusing this excellent work did, however, bring a number of points to mind which may be worth considering for the sake of possible future work in this field:

In this catalogue, virtually every shape keyboard instrument (square, upright, lyraflügel, grand) containing a piano action is considered to be a piano. Yet virginals and spinets, which have "harpsichord-jack actions", are not considered to be harpsichords. Similarly, the Grove Early Keyboard Instruments (1989) also confuses classification by viewing the virginal as "a smaller type of harpsichord" (p.111) while seeing the spinet as a small keyboard instrument "with a plucking mechanism as in the harpsichord" (p.131). Considering how terminology has changed and evolved over the centuries, this would be a good opportunity to try to reach agreement on terms based on function, purpose and logic for our time, i.e. that keyboard instruments with harpsichord-jack action are all part of the harpsichord "family", and when specific forms need to be indicated, the terms virginal, spinet, clavicymbalum (or upright) and grand will do quite nicely. Failing on agreeing to this, an explanation might be considered necessary as to why virginals and spinets are not harpsichords, but why two instruments as different as a "square" and a "grand" are both pianos.

The front and back views of harpsichord jacks in this catalog are clear, and good information to have. If a side view had also been included it would have been possible to compare plectra angles between various types, periods and building traditions - a significant factor in action set-up which has had little attention paid to it.

Another opportunity for comparisons was missed when no data was collected on the dimensions or materials used in bridge and nut pins. Historically, different size bridge and nut pins were frequently used in different places in the same instrument and different materials were used in different building traditions, all which had significant effects on the tonal color, sustain and articulation of the instruments. I'm surprised that this link in the chain transmitting the string's energy to the air is so consistently ignored.

Every instrument description in this catalog includes keyboard measurements taken from the key front to the "balance point", a term which is not defined in the glossary. "Balance pin" is defined and is surely what is meant for this measurement. The unfortunately-named "balance" pin actually determines the fulcrum point or leverage point of the key lever, not its balance point. The balance point of a lever is that point at which, when the lever is suspended or supported there, the ends of the lever will rest level with each other. This balance point measured with reference to the balance pin hole can be a very useful indication of a keylever's relative weighting. It would seem unnecessary to make a useful term like balance point try to mean something it inherently does not, when a more traditional (if inaccurate) term already exists: balance pin.

The glossary of this catalog calls the board over the tangents of some clavichords a "stop rail" (a term I have never encountered before) and declares an intended purpose which may be in some doubt. This is more usually referred to as a tangent rail or a listing board. Several historical instruments have this rail positioned high enough above the strings that it is doubtful that any reasonable digital pressure could raise the strings high enough to need any further restraint by the rail. Additionally, it appears that quite a few clavichords with a soft action, which might benefit from some restraint, don't have these rails, while others with an already well-controlled action do have them. At best, with a low enough position or additional padding, these removable rails can provide additional resistance to the movement of the strings resulting in a louder sound at some cost to sensitivity, but in no way will they "stop" anything with their typical 5mm thickness.

In the clavichord data Mr. Koster provides measurements of the strings' "afterlengths" defined by him as that portion of the strings between the tangent and the pin on the hitchpin rail. (This distance is an important factor, along with the string

gauge, in determining the string's resistance to key pressure.) Unfortunately, this same term is often used with pianos to describe the extra length at the tuning pin end of the string (p.311, #14). Since this tuning pin portion is also a useful measure in clavichords (it correlates with how much free soundboard there is between the bridge and the wrest plank, and also determines which portion of the keyboard range will be reinforced by the sympathetic vibrations from these string portions [an important area of difference between the Horn design and seemingly "very similar" (p.188) Friederici design]), it would probably reduce confusion if there were two different terms for these extra string portions at opposite ends of the string's sounding length: beforelength/afterlength or prelength/postlength perhaps?

Also concerning clavichords, since there are a few (admittedly unusual) clavichords which don't use pairs of strings it would probably save some confusion if the definitions of "fretted" and "unfretted" did not depend on "pairs of strings" to explain this design element.

Other items of possible interest with page numbers in the book:

51 A plucking point position of 20% for a front plucking c^2 is used as a way of helping to determine the original bridge position in an altered Flemish harpsichord. That the 50% position for a rear plucking c^3 appears to be a common element in many different building traditions in many time periods is probably worthy of mention also.

44 and note#7, p.46 concerning speculation on the position of the 4' register in 17th century French harpsichords. On the harpsichord presently in Stuttgart being discussed it might be worth noting that the layout of the 4' strings crossing at an angle under the 8' strings precludes the possibility of these 4' strings being plucked anywhere but in the front register. The fact that the jacks are not original and the upper keyboard has been altered would not seem to have any effect on this design feature. (Worth noting here perhaps is that inspection of the keyboards of the 17th century Desruisseaux at the Paris Conservatory appears to indicate that originally there was no way of coupling the upper and lower keyboards: the shortening of the upper manual keys and the installation of the round dogs is very crude later work.)

67 Considering the very different crafts of woodworking and metalworking I would not be surprised if "C.E." did the case-work of the regal and "Simon Bauer" supplied the pipes - a not unusual division of labour in organ making today. In his speculation on the presence of the two different inscriptions in the instrument Mr. Koster either ignores this possibility or, for some unstated reason, discards it.

100 Merlin's modification of the Kirckman harpsichord to make both 8' jacks sitting on each key lever pluck the close pair of strings (tuned to the same pitch) would certainly seem

worth a picture or diagram to make clearer this unusual arrangement.

186 Similarly, the notch for each string in the Horn clavichord "functioning to hold it as a back pin would" would seem worth an illustration.

187 The lower photo appears to show a very unusual extra block between the bass hitchpin rail and the first key lever which is not mentioned or explained in the text.

235 The hitchpin plank is described as being veneered with maple with its grain perpendicular to the underlying piece--the photo appears to show otherwise.

333 The belly rails on all but one type of instrument described in the glossary are framing members which lie between the wrest plank and the hitchpin rail, cross the usual soundboard grain direction and have their ends usually attached to case walls. The only exception to this description is the supposed belly rail in the Flemish virginal which does none of these things, but which does function and look just like soundboard liner, except in this case it is not "glued to an interior case wall" as Mr. Koster defines liners. (It isn't clear whether the unmentioned English, German, Italian and French virginals don't have belly rails or if some of these had this framing member turned into a soundboard liner by being glued to the nameboard rather than having a removable nameboard.) I believe it would be less confusing to view all virginals as not having belly rails but as having a liner which is not glued to a case wall, than to say that Flemish virginals have a belly rail that is located differently than in all other instruments which have one. Additionally, to say a liner is "glued to an interior case wall" seems to imply that there is an exterior case wall. Since this is true very infrequently, it would probably be more accurate to state that the liner is "glued to the interior surface of a case wall".

334 Cutoff bar definition. I would have thought that there was enough acoustic research done by now by Kottick, Rawson, Elfrath and others to have sufficiently demonstrated that major resonance areas lie on both sides as well as across the so-called "cutoff" bar, so that this name would no longer be taken literally. In fact, instruments with no "cutoff bar" display lower major resonance patterns virtually identical to instruments that have such a bar.

335 In the discussion of the English grand action the hammer butt is described as being "hinged to the hammer rail by a silver axle wire". This use of the word silver is unusual for this usually precisely worded volume. The word is not marked to indicate that the material has been scientifically analyzed. If a silver-colored wire is meant then this is a rare instance of an ambiguous adjective being used where it serves no useful purpose. (It is not ferrous since it does not respond to a magnet.)

(338) Millboard, so often used in the organ descriptions, is a word I was hoping to find explained here.

341 Strut, definition #2. There may be a tradition, of which I am unaware, of calling this particular piece in German and Austrian pianos a "strut". However, since these pianos often contain struts which fit the first definition, it would seem to be more specifically indicative of location and function to call these pieces "gap spacers" as they are called in English pianos and in grand harpsichords.

While on the topic of German and Austrian pianos, I am also puzzled by Mr. Koster's consistent use of the German terms Auslöser and Schnabel in the action descriptions when escapement and beak seem to be widespread in use and acceptance among English speakers. (Kapsel seems to have been taken into the English jargon for these pianos, although I believe most people would call more than one of them kapselg rather than the German form of Kapseln which Mr. Koster uses.) These seem to be the only non-English words which he does not bother to translate although common translations do exist.

The above points can hardly be considered to make a dent in the amount or significance of the work contained in this superb volume. In fact, except in such small details, it is difficult to imagine what could be done to surpass this work. Since so much information about early keyboard instruments has been given in this book which would not need to be repeated, this might now make it easier for other keyboard collections to issue specific descriptions of their holdings. This would allow the fulfillment of the spirit of public cultural enrichment which motivated so many of the donations to collections as well as the reasons for their foundings as well as reduce the number of requests for information to these institutions and the unneeded wear and tear on the instruments when measurements are repeated every several years, or decades.

Thanks should be extended to John Koster, Sheridan Germann and all the others involved in the production of this catalog which should be on the bookshelf of everyone interested in keyboard instruments.

Review of: John Koster, *Keyboard Musical Instruments in the Museum of Fine Arts, Boston*. Published by the MFA, Boston, 408 pages (A4 size), illustrated; £60 in U.K.

All credit to John Koster and the Boston museum's administrators for the ambitious scale of this project - and for having the resolution to complete what must have been a very costly and time-consuming work. Sam Quigley says of this book, 'I am quite confident that those who become familiar with this volume will agree that it ... sets a new standard for the entire field of organology'. John Henry van der Meer, who has had no little influence on the writing of keyboard catalogues himself, is of very much the same opinion. And Museum Director Alan Shestack believes that 'this work is a highly significant and immensely useful addition to the literature on musical instruments'.

For those who have not seen a copy, and may be thinking that £60 is a high price for a catalogue, let me say that it looks and feels like value for money. With over 400 large-format pages of high quality 'art paper' and a hardback binding, *Keyboard Musical Instruments of the MFA* is a weighty tome in every sense. A preliminary flick through the pages is enough to show that it is crammed with detailed information. It is profusely illustrated with multiple photographs of each instrument; these are supplemented with line drawings, tables of measurements and a very extensive apparatus of footnotes. Seeing the painstaking attention that the author has brought to the task, and feeling the weight of the book, it comes as something of a surprise to discover that the collection numbers only 54 instruments.

The history of the collection is itself worth telling. It begins with the infamous sinking of the *Lusitania* in May 1915. Among those who died was an amateur musician from Boston, Leslie Lindsey Mason, whose father, William Lindsey, was a trustee of the Museum of Fine Arts. He was evidently a man of some wealth. We can only be grateful therefore that he was able to turn this family tragedy, and his fortune, to some lastingly useful account by purchasing, and presenting to the museum, Canon Galpin's collection of musical instruments. Renamed the *Leslie Lindsey Mason Collection*, this was to be her memorial.

Assessing Galpin's collection today, with the aid of John Koster's book, one detects a certain *fay* quality in his choices. He was a man of his time, as we all must be, so his predilections were tainted to some extent by the late Victorian nostalgia for 'Merrie England' and all things ancient that was part of the permanent mind set of men like William Morris and Arnold Dolmetsch; and even more so their clients. The quaint and the curious seem to be too much in evidence so far as Galpin's keyboards are concerned. On the plus side there is an important Andreas Ruckers muselar with its *arpichordum* batten still in place; but on the other hand, a 'Baker Harris' spinet with a screamingly fraudulent nameboard; Galpin apparently chose it, when he might have had dozens of old spinets to choose from, solely for the novelty of its triple-curved bent-side. There

is a curious little keyed metallophone of early Italian workmanship (just such a thing as Papageno might have used except that this one has only 25 notes); but all too predictably, the collector's appetite was also whetted by a clavicytherium in Signor Franciolini's catalogue. Of course Galpin was not the only one to be ensnared by the Florentine faker but the Englishman's taste for the rare and exotic must have made him an easy victim. At the time when this collection was put together interest in the early piano was minimal, so it is no surprise to find that, apart from a token Zumppe (of 1770), his interest extended only so far as a dainty little sewing box in the early Biedermeier style of about 1820, to which someone (I fear not the original maker) has fastened a set of cabriole legs to create a confection of unspeakable prissiness. Galpin's efforts produced a very uneven collection: some gold, but much dross.

All told, there were only 18 keyboard instruments in the original collection of some 300 instruments, but a massive transfusion was given to the MFA's holding by the acquisition of the Edwin M. Ripin Collection; eleven instruments that included several of supreme significance. And to these must be superadded some excellent one-off gifts and purchases. It has become a very significant collection. Now, in John Koster's catalogue, we are offered easily accessible and highly detailed descriptions of such instruments as the Stein fortepiano of 1783 - thought to have been his fabled *Saitenharmonika*; and the Kirckman harpsichord of 1758 to which Merlin added his famous down-striking piano stop. Both of these, and several of the harpsichords (for example the Couchet/Blanchet/Taskin), are specially worthy of attention as they show earlier and later states, each important in its own right. The detailed nature of this catalogue allows John Koster ample space to elucidate such matters and he is certainly equal to the task.

The importance of ample illustration has not been overlooked. The book has 258 well-chosen photographs. Complementing these there are 79 line drawings showing such details as mouldings and bridge sections, soundboard barrings and hammer head profiles. Numerical data is adequately supplied; string lengths, plucking or striking points; critical leverage dimensions of keyboards and actions; and touch weights measured at selected points in the compass - to mention but a few. Timber identifications are given for almost every component, and all in all the descriptions of the instruments set a very high standard. But he does not stop there.

For each instrument there is also a commentary, placing the maker, and the most noteworthy features of each instrument, in its historical context. Here Koster has particularly excelled. His meticulous reading and the refreshing intellectual rigour that he has brought to the source material results in some very informative and reliable biographies which at least equal, and generally surpass, anything previously written; allowing for necessary brevity in some instances. A good example is his monograph on Johann Andreas Stein in which he has synthesised all of the previously known material, sensibly forgoing yet another lengthy repetition of the well-known Mozart letter in favour of three very telling quotes from von Stetten (1788), Christmann (1789), and Reichardt (1789); all apparently relating to the *Saitenharmonika*. The author has himself

translated these passages. Reichardt's is a gem, leaving one breathless with desire to hear this ravishing instrument; and despondent that sadly it never will be heard again. Other entries deserving of special commendation are the very long and well considered description of the John Broadwood piano of 1796 made for Don Manuel de Godoy (the design by Thomas Sheraton including Wedgwood plaques), and also the long and involved reasoning that comprises his consummate analysis of the Couchet/Blanchet/Taskin harpsichord.

A few things I do find irksome. One is the author's decision to persevere with prose descriptions of construction details. Most of it is delivered in a monotonously repetitive sentence structure, which is hard to avoid, consisting of subject, verb, object (in that order) most sentences commencing with the definite article. 'The [component] is/are made from [material] [dimension]' - makes very wearisome reading when the pattern is repeated *ad nauseam*. I find evidence that the author was aware of this too, so that one occasionally finds a curiously contrived inversion; as, for example 'Hinged with leather to the hammer rail, the hammer shanks are of mahogany'. I imagine that John Koster is here expanding on some brief notes made at the instrument: 'hammer shanks, mahogany; hinges, leather'. If so, could we not be better served by reading the notes? An unlooked-for result of spinning out the facts in prose is that, unless one searches diligently, a vital piece of information is apt to be lost under a sea of words. My question is, whether it would not be of more service to display as much of the data as possible in tables or lists?

John Koster is to be applauded for being one of the first to give adequate details relating to keyboard dimensions and touch; it's surprising that it has not been done before. The effective height of the sharps is given as well as their length. Sensibly he quotes the length of the key heads rather than the imprecise and largely irrelevant overall length of the naturals. [Let us hope that this will soon become a universal practice.] In order to make an objective comparison of the touch of one piano with another we also need information about the touch weight and the responsiveness. As a first step towards meeting this need Koster gives 'the finger weight necessary to play the lightest *pianissimo*', which he measures at each end of the keyboard (and sometimes at an intermediate place) together with measurements of the levers that comprise the action. This is a good idea but not quite so useful as it was meant to be. The information is introduced by a verbal formula: 'the proportions of the action are...' leading into what is often a very long set of data requiring careful study. This author's desire is evidently to avoid interposing himself between the reader and the raw, objective data, but what does it mean? In order to find out one must undertake a complex calculation, compounding ratios and sometimes necessarily invoking Pythagoras' theorem, approximating what we hope is something near a right-angled triangle. At the end of it all we should arrive at a simple ratio, such as 8.5:1 — meaning that the hammer head moves 8.5 mm for each 1 mm depression of the key (when in the driving phase). Whether we have arrived at any useful result depends on the accuracy with which the smallest dimension has been measured [neglecting for the moment other, more illusive, variables]. So for example, on a Broadwood grand an error of 1 mm in measuring the 8 mm between the axle pin and the notch of the

hammer butt would result in a 12% error in the final ratio. Would it not have been better if the author had measured such ratios directly? Or, failing that, to print the result of the calculation so as to save readers the tiresome repetitions of mundane number crunching? A few simple definitions, as to touch weight, velocity ratio, touch depth (with necessary cautions regarding wear) would have enabled the author to print concise tables of data giving everyone quick access to some really useful information - and it would have saved a lot of redundant verbal repetition.

In recent years some remarkably ambitious and useful catalogues have appeared which have become reference works for the subject as a whole, owing to the thoroughness with which they describe and illustrate the instruments. Such would be Hubert Henkel's *Clavichorde* of 1981, and Herbert Heyde's catalogue of the *Staatlichen Reka-Sammlung* in Frankfurth an der Oder (1989). John Koster has clearly set his sights on equalling, if not surpassing their standards, and, at least as far as English language publications are concerned, he has a fair claim to have excelled them in several respects. A publication on such an ambitious scale is to be applauded. There is matter here that will make excellent reference and research material as well as interesting the general reader but it is to be hoped that this will not inhibit other museums from going ahead with more limited objectives. There are so many important collections around the world for which not even an accurate checklist is available. It would be unfortunate if every museum felt that this was the standard that must be matched before publication could be contemplated. We can only hope that the appearance of the Boston catalogue will act as a spur rather than a restraint.

Lastly, I find that John Koster has an excellent bibliography and it is plain from the contents of this book that, in his case, it is not just for show. Koster has actually studied these sources with care. Let us hope that all future authors will include his book in their bibliography... and that it will be studied with equal care; it deserves no less.

1994 FoMRHI List of Members — 2nd Supplement as at 13 October 1994

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

- * Gillian Alcock; e-mail *gillian.alcock@anu.edu.au*.
- * Jaume Bosser, Amador Romani 18, E-08023 Capellades, Spain.
- Peter Corneliusson, St Peders väg 21, S-460 10 Lödöse, Sweden.
- Padraig ó Dubhlaoidh, 24 Players Avenue, Malvern, Wores WR14 1DU, UK; 0684-562947 (vln & gmba fams, bows; M,R).
- Alessandra Fadel, Piazza Fontana 14, I-22049 Valmadrera (CO), Italy (plucked instrs; M).
- * Christian Kubli, Fenkernstrasse 17C, CH-6010 Kriens, Switzerland.
- * Darryl Martin, 3 East Castle Road, Edinburgh EH10 5AP, UK; 031-229 8018.
- * Bill Napier-Hemy, 306-1591 West 16th Avenue, Vancouver, BC, Canada V6J 2L7; (604) 730-1145.
- G van Ulsen, Wolterbeekweg 15, NL-6862 BE Oosterbeek, Netherlands; 085-341434 (vln, vla, vcl, M; vlc,P).
- * University of Adelaide, Barr Smith Library, Adelaide SA 5005, Australia.
- * Francis Wood; e-mail *fw@fwood.demon.co.uk* (musette, small pp; M,P).

European Musical Instruments
in Liverpool Museum, Ed. Pauline Rushton

Various contributors have provided details and descriptions of the keyboard instruments and they vary in their coverage as a consequence. Of the stringed keyboard instruments, only Michael Latcham's descriptions of two of the pianos comes up to the necessary standard for organological enquiry. These are two very contrasting items; a sewing box piano with 32 notes, and the well-known Bohemian/Viennese fortepiano reputedly with Beethoven connections. Some of the other entries are perfunctory and very disappointing. We have, for example, a handsome and unusual spinet by Kirshaw of Manchester; a very deep case, an unusual outline, and yet veneered and crossbanded in walnut. One immediately wonders whether this man trained under any of the London makers, and if so, whether his master was of the English school (as exemplified by Hitchcock) or one of the later makers (like Faulkner) influenced by the Tabel tradition. A quick look at the keys would tell us quite a lot. But unfortunately we are told nothing. Are the key levers lime or pine? Are they front guided or rack guided? Is there a solid register or upper and lower guides? We are told none of these things. Who was Kirshaw? This surely would have been the ideal moment to furnish some information; Manchester is only just up the road and a couple of hours in the local records office would have quickly turned out something worth knowing - very possibly enough to date this instrument more precisely. But instead we are treated to only a perfunctory summary from Boalch.

A spinet from Gaveau (1926) appears to merit even less attention. All that we are told about its action is that it is a single manual (which we can see anyway) and that it has a single row of jacks. In view of Gaveau's fantastic, piano-inspired harpsichord actions of the period, with capo tasto bars and overly complex jacks, some further clarification is called for I think. No attempt has been made to place this or any other instrument into any historical or musical context, so the general reader would have difficulty in making any sense of the collection without further reading. The absence of a glossary compounds the problems. Several unfamiliar terms are employed without explanation.

If, on the other hand, the catalogue is intended as a specialist publication, I find it woefully lacking in some of the most basic data. Although the three octave measure is quoted for all keyboards, not one has the length of the natural key heads or the sharps; much less the height of the sharps, or even the style of the keyfronts. Soundboard ribbings or thicknesses are hardly referred to. Information relating to harpsichord plectra materials is overlooked or ignored. One could go on.

Liverpool Museum is to be applauded for bringing out this catalogue, but the job could and should have been better done.

Michael Cole

Review of: Pauline Rushton, ed, *European Musical Instruments in Liverpool Museum, National Museums & Galleries on Merseyside* (POBox 33, 127 Dale Street, Liverpool L69 3LA), 1994. 182 pp, 110 b&w illus, 8 colour. Introductory price (they don't say till when!) £9.95 + £1.50 p&p; rrp £14.95 + £2.00.

This is an excellent catalogue. Each instrument is described in considerable detail by one of a team of authorities. Almost every one is illustrated, a few of them more than once; the only deficiency here is that there are no references to the colour plates from the texts so that one might think at first that an instrument has not been illustrated, and then 'oops - let's check the colour plates' and there it probably is.

The basis of the Liverpool Collection is that of Rushworth & Dreaper - many of us doubtless have one or more of the little catalogues that that firm produced through the 1930s and later with varying amounts of detail about varying numbers of instruments. Now at last we have full details of them all. The amount of detail seems to me to be about right - quite a bit more than my Bate Demi-Catalogues though considerably less than Leipzig or the new Boston (see elsewhere here for reviews of the latter). For keyboards, range of course, 3-octave span, scaling for at least three notes and their plucking point and gauge, inner and outer case dimensions. Provenances are given, often in considerable detail, and references to any previous or relevant publications. One slightly surprising omission from the bibliography at the end of the book, unless I am mistaken about some of the instruments, was Edwin Ripin's book on Franciolini. A number of these instruments were bought from Paul de Wit, and his involvement with the great Italian forger was notorious.

There is less detail for wind and string instruments, with sometimes an overall length or height rather than tube or string length, and a nominal rather than actual pitch. But then the bulk of the R & D Collection was, not surprisingly for an organ- and piano-making firm, keyboard instruments.

As always, there are a few details at which one would cavil. One that constantly niggled

me was the repeated use of the phrase 'foot pedal' - is there any other form of pedal? It's like the phrase 'hollow tube', for the use of which I was once reprov'd - something that comes easily to the lips (or the fingers) but which, once its tautology has been pointed out, one can never use, or see, again without a shudder. A more serious one is naming the great key of a Grundmann cor anglais as the F key. Agreed that that is the pitch which sounds when it's closed, but the cor anglais is a transposing instrument and to the player it's the C key; he reads C, he fingers C. It is the normal rule with large oboes, small flutes, and all sizes of clarinet that keys are given their 'normal' name. All one-key flutes have a D# key, whether they are concert flutes and piccolos, on which it sounds d# and d# respectively, or F flutes (3rd flutes or F basses), Bb flutes (fifes or Bb basses) on which it sounds a variety of F sharps and B naturals respectively. And so on. And, please, on cors anglais, too.

One flute has a more detailed pitch given than usual, a D'Almaine & Co, at D plus 40 cents, which seems to me extraordinarily high for a one-key flute of about 1800. But perhaps that was why it was given that much detail. Similarly, but more misleadingly, a treble recorder attributed to Dolmetsch is given as E - 30 cents; this is a baroque F, and exactly Bressan's pitch (F @ A=408 Hz); it looks to be one of Arnold Dolmetsch's Bressan copies.

With a Clementi clarinet it is remarked that they may well not have made the woodwind that bear their name but 'simply retailed them for other makers'. I think the London scene in the early 19th century was more complex than this and it is high time that somebody tried to sort it out. There were several of these firms, Clementi, Longman & Broderip, etc, and I think above all Goulding, which retailed everything but made little, buying instruments in from all round the trade and stamping their own name on them. Sometimes

we know who made for a firm, Geib making for Longman & Broderip, for example, James Wood making some woodwind for Goulding, but it should be possible for some of those with a better eye than mine for details of wood turning, etc, to allocate names of actual makers much more accurately to some of these instruments. Possibly, too, work on street directories and rate books would turn up more makers who are unrepresented with surviving instruments and whom it might be possible, with further documentary research, to identify as working for one or more of these firms. There is still a great deal that we don't know about the 19th century instrument-making trade. But just as an indication of what went on, and still does, I remember being with Bill Lewington once when a major manufacturer rang up and ordered 5,000

Chinese trumpets without mark on which they could stamp their own name.

In a rather similar connexion, I have wondered for years whether anyone has done any research on the harp-maker Erat (there are two of his harps in this catalogue). Was that really his name or did he take it as a working name in the hope that people would think he was another, rather better-known, maker?

As I said at the start, this is an excellent catalogue. It is important to acquire it for two reasons, one that it is very reasonably priced, and the other that the exhibition of the instruments is over and therefore all or most are now back in store, so that this is the only way to find out what there is in an undoubtedly major collection.

FoMRHI Comm. 1295

Ephraim Segerman

Review of: Eszter Fontana, Friedemann Hellwig and Klaus Martius, *Historische Lacke und Beizen auf Musikinstrumenten in deutschsprachigen Quellen bis 1900*. Germanisches National Museum, Kariausergasse 1 und 12, S500 Nürnberg, Germany, 1992, 158 pp. No price given.

The title says 'historical varnishes and stains for musical instruments in German sources up to 1900'. The core of the book is 91 pages of recipes, usually one to a page, with what the recipe was for on the top of the page, and a list of the materials called for at the bottom. There are two recipes from very late in the 17th century, 30 from the 18th century and 51 from the 19th century. Instruments mentioned include clavichords, harpsichords, fiddles, lutes, organs, harps, viols, citterns, dulcimers and pianos. Musical instruments in general are mentioned in a large fraction of the recipes. Before the recipes is a good introduction for background and practical advice on how to use the recipes, and full references for the 43 sources the recipes came from. After the recipes is a very useful 34 pages of glossary that identifies and discusses the ingredients mentioned.

The cover picture is of the f-hole region of the belly of a violin showing very marked crazing of the varnish. For the majority of makers who consider crazing to be a fault, this could be a warning that all historical recipes are not necessarily what they would consider to be good ones. The minority who consider crazing a virtue (because of its association with particularly fine old-master instruments) can explore these recipes to find clues for getting the look they want.

Everyone seriously interested in historical instrument finishes must be acquainted with this book and at least have access to it. Having very little facility with the German language is not an insuperable handicap. The German names for instruments are easily acquired. One can make a list of the English names of ingredients with the help of the glossary and a German-English dictionary. Then by scanning the tops and bottoms of the pages of recipes, one can easily pick the few of serious interest. Since each recipe is usually less than a page long, translation with a dictionary is not too daunting a task.

A subtitle states that this book was compiled with the cooperation of the *Fachgruppe Musikinstrumente der Arbeitsgemeinschaft der Restauratoren (AdR)*, the study group of the professional association of instrument restorers. Since conservation is nowadays favoured over restoration, the name is rather old-fashioned. Irrespective of what we think of their name, the association and the authors must be congratulated for doing a marvelous job in compiling this collection of recipes. It already is a standard reference.

FoMRHI Comm. 1296

Jeremy Montagu

Review of: Florence Gétrau, ed, *La Recherche en Organologie; les instruments de musique occidentaux, 1960-1992*. Extracted from *Revue de Musicologie*, 79, 1993, as a separate booklet. Available from SEDIM, 151-153 avenue Jean Jaurès, F-75019 Paris, FF 30 + p&p.

There is an extraordinary amount of information in this booklet of 56 pages. There is a series of short articles on research and publications over the last thirty years, general including encyclopædias, by Florence Gétrau, strings plucked by Joël Dugot and bowed by Karel Moens, wind by Manfred Schmidt (all four in French), and keyboards by Howard Schott (in English). Each article is followed by a bibliography of pretty well all the significant publications, books and periodical articles, in that field over that period. And most of them are indeed there – it's difficult, off-hand and off the cuff, to find anything missing which really ought to be there. The lists include significant reprints of earlier publications and are broken down into convenient sections by subjects, including individual instrument types. As a result it's easy to find what you want.

Because this is a French publication, it is a little idiosyncratic to non-French eyes; books that were published in English, or other languages, and which were translated into French appear, logically, under their French guise, which produces not only titles which are unfamiliar but publishers and dates which are also strange. So if the author is anything but French and you need to follow a title up, you may have to check such details.

Nevertheless, I am very glad to have this and may find myself bestowing what I regard as the ultimate accolade – buying a second copy so as to have it accessible both at home and at the Bate. And this despite the fact that there is one whole family of instruments omitted and ignored – there have been a few quite important studies published on percussion instruments (and I don't just mean mine!).

FoMRHI Comm. 1297

Jeremy Montagu

Review of: Gerhard Stradner, ed, *Klangführer durch die Sammlung Alter Musikinstrumente*, Wien, Kunsthistorisches Museum, 1993. AS 445 in the museum shop, or order from Cultur & Management, Kirchberggasse 7/2, A-1070 Wien, presumably with extra for p&p.

This a booklet and a double CD, the booklet in German (perhaps one should say Austrian), Italian, English, and Japanese, and well illustrated with colour photographs; the CD with spoken text (the same as the booklet) in German only and with musical examples.

What we have on the CD is what one hears as one goes round the museum with headphones, picking up with infra-red rays what is being broadcast in each room. This is part of the newly organised display of the musical instrument collection into chronological order and emphasising Austrian musical history.

Many museums nowadays have these soundguides, but I don't know how many publish them on CD (or cassette), and certainly I've not seen before an accompanying booklet which would stand on its own as an illustrated guide like this one. It's particularly useful because if you are not a fluent German speaker you can follow in another language (if you are better with Italian, English, or Japanese).

(continued overleaf)

The instruments chosen for illustration are not always the ones that one might expect; there are none of the superb range of renaissance recorders or cornetts, nor the beautifully carved lira da braccio. It's not always clear what one is listening to. Sometimes we are specifically told that the recording is played on a certain instrument, but quite often nothing is said, and then we don't know whether it's the instrument illustrated or not, nor whether it's a different early instrument, either original or reproduction, or whether it is a modern one. Certainly a number of the ensemble or orchestral recordings are on modern instruments; some seem to have been taken from broadcasts or commercial recordings without any identification being provided.

As soundguides go, it is quite a good one; not enough detail, perhaps, for people like us, but not so much that it would daunt the general non-specialist public (after all, we're meant to know what we're looking at). The speaker's diction is admirably clear (they aren't always in some museums) and there are probably as many musical examples as it is mechanically practicable to provide in each room of the exhibition. Publishing it like this is an excellent idea because it provides a very attractive souvenir to take away, and a good introduction to the collection before one visits it. And, of course, to hear any of a museum's instruments is, nowadays, an unusual treat. There is so much controversy today about putting instruments into playing order that many museums dare not allow us to hear anything. I'm supposed to be a maverick in this area because of the number of Bate instruments that I allow to be played, but I'm not sure that even I would have taken some of the chances that have been taken here if we really are hearing the collection's instruments.

FoMRHI Comm. 1298

Dave Law

A method of bushing wrestplanks....

I was recently given the opportunity to do some restoration work on the instrument pictured in plate 102a of Raymond Russell's 'The Harpsichord and Clavichord', an instrument of Iberian origin which was the subject of a paper given by Beryl Kenyon de Pascual and myself at the Galpin Society Edinburgh Symposium in June this year. The paper (to be published in *GSJ*) however does not cover any actual restoration techniques, and this instrument posed a problem for which I have always wanted to find a good solution, that of efficiently bushing sloppy or oversized tuning pin holes. Bushing with any sort of dowel has always seemed to me to be unsatisfactory, since the grain of the bushing is easily crushed (although I have been told that bushing with ebony dowels works well).

In this case, an original set of tuning pin holes, set out at bichord piano string spacing, had been filled; and new holes drilled in different positions to conform to harpsichord string spacings. This was probably done by Arnold Dolmetsch, who fitted a set of old tuning pins (these were probably taken from an 18th century square piano; they are a shade over 5mm in diameter, filed to a slight taper, with holes for the wires and oblong heads). These pins were removed in 1974 (they are still kept with the instrument) when it was suggested to the owner that to remedy tuning instability a new set of pins should be fitted! This was done by bushing the holes with a soft dowel (it reminded me of modern piano hammer shanks) and re-drilling. The new holes were drilled all the way through the wrestplank, and the instrument was re-strung using modern steel zither pins, which were driven right through the plank, projecting only 12mm above the plank, protruding 5mm below it. These pins did not hold.

I remedied this by enlarging the holes, drilling right through the wrestplank to completely remove the soft bushing material. Then I made plugs of old oak to fit the enlarged holes so that the grain of the plugs lie in the same general direction as that of the oak of the wrestplank. The plugs were 6mm in diameter and were made with a commercial plug cutter. Since they were only about 8mm long and the wrestplank is 26mm thick in total, three of them were glued and inserted one after another with the grain angle altering by a few degrees each time. The uppermost plug filled the remaining few millimetres and was aligned with the grain of the wrestplank veneer; I felt that the use of hard old oak for this layer rather than pine was justified.

The soft plugs which had been used previously were about 5.5mm in diameter already; had they not been, it would have been better to have a 5mm plug cutter made since no more of the original material should be cut away than is necessary to achieve a good glue joint with the wood of the new plugs. If the drill size and plug size are correct there is no stress placed on the remaining wood of the wrestplank, but the joint is tight. Before attempting this on the instrument I experimented by drilling and plugging some pieces of old oak; some not much wider than the diameter of the plug. I cut them apart when they were dry to see just how clean the glueline was, and if the insertion of the plugs into these pieces of wood had made them wider. When I was satisfied with the combination of the fit of holes and plugs and the viscosity of the glue, the whole wrestplank was done.

New traditional pattern tapered tuning pins were made from 3.5mm nails. The pins being quite close to the nut bridge meant that the strings used to ride up the pins; the new holes were drilled at an angle to the vertical to reduce this tendency. The holes were drilled completely within the new plugs so that in the future these wrestpins can be removed, their holes plugged, and the bushings re-drilled if necessary without further disturbing the original material of the wrestplank. After the new strings had stabilised, tuning proved very stable.

We all know that most restoration is impossible without damaging some original fabric. The real problem always lies in assessing whether more good will be done both for the instrument and for future researchers by restoring and documenting properly, or by leaving it alone. This particular instrument has by all accounts not sounded or played as well as it does now at least since it returned to Europe in 1946, and probably for a lot longer than that. It is reprehensible that it should have had an original piano action removed, but if it weren't for such things happening lots of people wouldn't have quite so much to write about. I believe that we must consider carefully the actions we take when considering whether to restore, how far to restore or whether just to conserve, but also shouldn't we spare a thought for the original makers' intentions? I know that I'd be chuffed to bits that an instrument of mine was still giving pleasure and being used for making music in 200 or 300 years, and if it was altered a bit I don't think I'd mind. The arguments to not do any restoration work are persuasive; as the restorer becomes more experienced it becomes more difficult to make decisions such as whether to, and if so how to bush this wrestplank, to take a simple example. However, many of us are lucky enough to do work for clients who do wish their instruments to play and have bought them with that in mind. What we ought to do is to disseminate our techniques so that fewer instruments are badly restored simply because the restorer didn't have one piece of information. We shouldn't have to learn techniques by trial and error on antique instruments any longer; none of us will last as long as the instruments, so we have a duty to record what we do for others, not just in a restoration report seen by a filing cabinet. To address this problem would be of great benefit. What do others think? I know I didn't like seeing restoration information in a coffee-table book on instrument making a few years ago, but perhaps within the 'covers' of this publication we should make a concerted effort to formally record our methods and solutions to restoration problems, since as Robert Cronin pointed out in comm. 1278, 'the FoMRHI membership is almost entirely made up of professionals'?

The Davidic notation-system

David Z. Crookes

Numbers are important in the Bible. Two she-bears tear "forty and two children" in II Kings 2.24. In John 21.11 the disciples catch "an hundred and fifty and three" fish. Revelation 13.18 tells us to "count the number." And Psalm 87.6 (AV) says, "The Lord shall count." So you don't feel uneasy about having found a whole garden of numbers in two verses of Psalm 49. One of your friends is uneasy about the idea of a numerical message in poetry of any kind, but then he's been brought up to believe that poetry must deal only with Eternal Problems, Lofty Intimations, and Metaphysical Yearnings. So when he reads, "...Into the valley of death / Rode the six hundred", he savours the tragedy but ignores the numerical specificity. He doesn't know the dictum of Pope: "...most by Numbers judge a Poet's song." He has never read Milton's words: "...feed on thoughts that voluntarie move / Harmonious numbers." Now of course the word *numbers* in both these cases actually denotes metrical lines of verse, but there is nothing unpoetic about numbers in the mathematical sense. You think of "Four and twenty blackbirds" in the nursery rhyme, "sixty thousand bugles" in *The Song of Roland*, and "Jamshyd's Sev'n-ring'd Cup" in the *Rubaiyat* of Omar Khayyam. Omar Khayyam was rather good at numbers. He was a major figure in the history of mathematics: we still have his treatise on algebra and his commentary on Euclid. In the English-speaking world he is best known as a poet, largely thanks to the translator Edward Fitzgerald, who published his first rendering of the *Rubaiyat* in 1859 and his second in 1868. You recall the opening stanza of the 1859 version:

Awake! for Morning in the Bowl of Night
Has flung the Stone that puts the Stars to Flight;
And Lo! the Hunter of the East has caught
The Sultan's Turret in a Noose of Light.

You reflect how much better it is than the second edition of 1868:

Wake! For the Sun behind yon Eastern Height
Has chased the Session of the Stars from Night;
And, to the Field of Heav'n ascending, strikes
The Sultan's Turret with a Shaft of Light.

You're reminded that many compositions survive in more than one version. Look at Mucha's *Été*—you've seen the pastel design, the preliminary drawing, and the final *panneau*. In the same connection you remember the Bruckner symphonies: two versions of nos. 1 and 2, and three versions of nos. 3 and 4. Then you think of Psalm 18 and its alternative version, II Samuel 22. That brings you back to the 1868 Omar Khayyam. Why did Fitzgerald turn his graceful bird of paradise (the 1859 version) into such a clumsy old crow? "Yon" is uncomfortable as a weak syllable; "Session" is unbeautiful padding; "from Night" is illogical—stars *belong* to night, and once they go night is over; line 3 is unfluent; and "strikes...with a Shaft" is a good bit less exciting than "has caught...in a Noose". What was Fitzgerald at? An utterly frivolous idea strikes you. You think of working it up into a humorous article. He was using the 1868 version to send a secret message. You remember three words of stanza 51:

Yes; and a **single Alif** were the Clue,
Could you but find it, to the Treasure-house
And peradventure to THE MASTER too...

A single Aleph! And at once you realize. Fitzgerald wants you to look at *the individual letters* of his poem. Well, take the last three words of stanza 1. "Shaft of Light" must involve an acrostic—the three initials spell *sol*, the Latin word for "sun". But then you remember stanza 12—the bit about "singing in the Wilderness". *Sol* must have a musical meaning as well, it's *sol* as in *solfa*, the fifth degree of a scale that runs *ut, re, mi, fa, sol*. What-ho!

That's enough frivolity, you tell yourself sternly, but there's still something in your head about alternative versions. What is it? Of course—David's song in II Samuel 22, and the "revised version" in Psalm 18. You decide to look up both passages in English and Hebrew. The first three verses of II Samuel 22, with supplied words italicized, read as follows in the AV:

And David spake unto the Lord the words of this song in the day *that* the Lord had delivered him out of the hand of all his enemies, and out of the hand of Saul. And he said, The Lord *is* my rock, and my fortress, and my deliverer; The God of my rock; in him will I trust: *he is* my shield, and the horn of my salvation, my high tower, and my refuge, my saviour; thou savest me from violence.

The first three verses of Psalm 18 (AV, title plus verses 1 and 2), with supplied words italicized, read as follows:

To the chief Musician, A Psalm of David, the servant of the Lord, who spake unto the Lord the words of this song in the day *that* the Lord delivered him from the hand of all his enemies, and from the hand of Saul. And he said, I will love thee, O Lord, my strength. The Lord *is* my rock, and my fortress, and my deliverer, my God, my strength, in whom I will trust, my buckler, and the horn of my salvation, *and* my high tower.

Four differences between the versions leap to your eye at once. First, the Hebrew word for "him" is spelt Aleph-Tau-Wau in II Samuel 22.1, but Aleph-**Wau**-Tau-Wau in Psalm 18.1. (It's exactly like *judgment* and *judgement* in English: both forms are correct.) Secondly, the introductory verse in II Samuel uses the Lord's name twice, but verse 1 of Psalm 18 goes out of its way to use it *three times*. Thirdly, while in II Samuel the same Hebrew word is used for *hand* each time, in Psalm 18 two different words are used. Fourthly, the words that open the actual song in II Samuel ("The Lord *is* my rock, and my fortress") are preceded in Psalm 18 by a declaration: "I will love thee, O Lord, my strength." Why does David describe himself in Psalm 18 as "the servant of the Lord", you wonder, when he's going to use the Lord's name twice more in the next nine (Hebrew) words? Has he some didactic purpose? Does he want us to focus on the Lord's name *in the actual song*? You look at the Hebrew text. The first mention of the Lord's name in Psalm 18 begins on the tenth character of the psalm's title, and the character involved, Yodh, has a numerical value of 10. That doesn't strike you as helpful in the least, but it prompts you to look at the tenth *sung* word of the actual song. The Hebrew word in question means "I will trust in him", and it appears to be completely undidactic—until you notice how it's spelt. The first three letters are **Aleph**, **Cheth**, and **Samekh**. And at once your attention is caught by the first five *sung* words of the psalm, translated in the AV thus: "I will love thee, O Lord, my strength. The Lord *is* my rock..." Five words, five layers, with the word for "Lord" forming the even-numbered layers of a five-layer sentence. Believing that the first three of these words represent a *deliberate Davidic addition* to the text of II Samuel, you spell out all five Hebrew words as follows:

1. I will love thee — — — — **Aleph**-Resh-Cheth-Mem-Kaph

2. O Lord — — — — — Yodh-He-Wau-He

3. my strength — — — — — **Cheth**-Zayin-Qoph-Yodh

4. The Lord *is* — — — — — Yodh-He-Wau-He

5. my rock — — — — — — **Samekh**-Lamedh-Ayin-Yodh

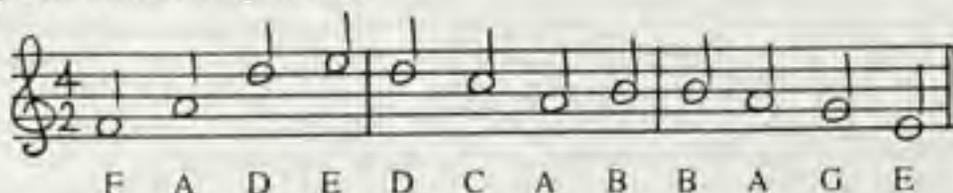
Something is going on here. David's triple use of the Lord's name in the title is meant to focus our attention on the double use of the Lord's name in the first five *sung* words of the psalm. The first, third and fifth sung words, which envelope the Lord's name, begin respectively with **Aleph**, **Cheth** and **Samekh**. These three letters are recapitulated in the tenth sung word (= "I will trust in him"), which begins **Aleph-Cheth-Samekh**. Everything from the Aleph of the first word to the Samekh of the tenth word is contained in 44 characters, and 44 is the greater-numerical-alphabet value of the word Lamedh-Daleth-Wau-Daleth (= "by David", or "of David"—fourth word of the psalm-title). You wonder if these 44 characters contain the key to David's *ut re mi fa sol*, and you decide to investigate the possibility synthetically. That means taking the Hebrew alphabet, pretending you're David, and trying to devise a system of notation. But first you read what Le Corbusier has to say on the subject (*TM*, 15).

For thousands of years men used sound to sing, or play, or dance. That was the first music, transmitted by the voice, no more.

But one day...centuries before Christ...someone first thought of making music permanently transmissible in another way than from mouth to ear—that is, to write it down. No method or tool was available for this. Sound had to be registered at certain determined points. It was necessary to represent sound by elements which could be grasped, breaking up a continuous whole in accordance with a certain convention and making from it a series of progressions. These progressions would then constitute the rungs of a scale.

How to divide into sections the continuous phenomenon of sound? How to cut up sound in accordance with a rule acceptable to all, but above all efficient, that is, flexible, adaptable, allowing for a wealth of nuances and yet simple, manageable and easy to understand?

Some of the Psalms are headed by words or phrases which aren't easy to understand. Thus Psalm 22 is headed *Aijeleth Shahar* (= the deer of morning), and Psalm 60 *Shushan-eduth* (= the lily of testimony). Scholars have tended to construe the mysterious headings as tune-names, but at times the word-combinations are too strange to permit that construction. You imagine a clergyman saying, "Let us sing Psalm 60 to the tune *Forensic Florist*." And it occurs to you now that some at least of the psalm-headings may be notation-mnemonics, giving the beginnings of particular tunes. You wonder if the letters of *the lily of testimony* in Hebrew represent the opening notes of a melody. *Faded cabbage* is a nonsensical phrase in English, but it may bear a musical significance:

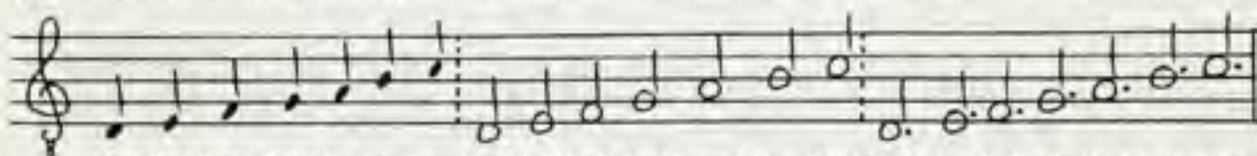


We use only seven letters of the English alphabet in our notation-system, but Psalm 22's word *Aijeleth* contains both Aleph and Tau, first and last letters of the Hebrew alphabet. It seems to you therefore that the *entire* Hebrew alphabet must have functioned as a notation-system. Now English tonic solfa has to put symbols of time, or length, beside its *d, r, m*, etc. And staff notation has to symbolize both the place and the time of each note—it doesn't use letters at all. But you've already seen how one psalmist concealed the 864-432 scale *in the plain letters of his text*. And you've realized that the concealment of the scale is pointless if both notation and melodies aren't concealed in the texts as well. That rules out any symbols additional to the letters themselves. So whatever the Hebrew alphabetical notation-system was, *the individual Hebrew letters must have been completely self-sufficient as musical notes*. Any one letter must have indicated both place (what note of the scale was to sound) and time (how long the note was to sound). You already know that twelve letters bear double *numerical* meanings.

The question now is how to plug the 864-432 scale into 22 letters. Did the Hebrew alphabet cover the musical range of a 22nd, or three octaves? If it did then you can forget about different time-values. You can effectively forget about most of the alphabet as well, since choral singers tend not to have a range of three octaves. You remember that *Aijeleth* in Psalm 22's heading contains the *first* and *last* letters of the Hebrew alphabet. You recall in addition

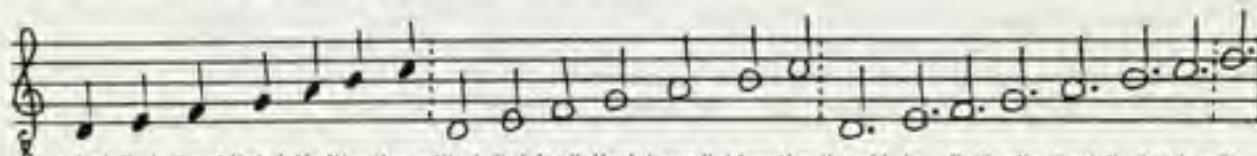
that most hymn-tunes today have a range of about an octave, and (glorious irrelevance) that the three parts of Georgian sacred music are all contained by the range of a major tenth.

That brings you back to the scale: to the scale, and the alphabet. On the one hand you have seven distinct notes plus an upper tonic (top *d'*); on the other hand you have the 22 letters from Aleph to Tau. Any system you devise is going to be limited in some way. (If you wanted to give three different time-values to all eight notes, for example, then you'd need 24 letters.) But the limits imposed by a 22-letter system must simply be accepted as facts of musical life. You remember that the greater *numerical* alphabet is severely limited (it runs out of letters once it gets to 400), and yet you've seen with what tremendous sophistication it can be handled. In the greater numerical alphabet the letters Aleph to Teth denote units, the letters Yodh to Tzadhe denote tens, and the letters Qoph to Tau denote hundreds. Units, tens, and hundreds. You think for a moment of the three temporal units in Daniel 12.7—"a time, times, and an half". What, you ask yourself, if David devised a musical alphabet which would distinctly represent notes of one, two, and three beats? He'd have had enough letters to cover three runs of the seven notes *d* to *c'*, and one letter to spare. (The fact that in Psalm 49's riddle the number 864 precedes the number 432 suggests that the Hebrew scale was conceived of as an ascent.)



Aleph Beth Gimel Daleth He Wau Zayin Cheth Teth Yodh Kaph Lamedh Mem Nun Samekh Ayin Pe Tzadhe Qoph Resh Shin

He might then have used the 'spare' letter, Tau, to represent the three-beat upper tonic, *d'*. You wonder whether the tuned silver trumpets (one-note instruments?) would have played this note in tutti sections, and you remember two Hebrew words that denote the sounding of a trumpet—Tau-Qoph-Ayin, and Tau-Resh-Wau-Ayin-He. Both words begin with Tau. And then something much more obvious strikes you. You recall the fifteen appearances in the Psalms of the word Tau-Beth-Lamedh (= *world*). That three-letter word, which begins with Tau, has a numerical value of 432. And 432 is the scalar number of the note *d'*. So David's complete musical alphabet might have run as follows:



Aleph Beth Gimel Daleth He Wau Zayin Cheth Teth Yodh Kaph Lamedh Mem Nun Samekh Ayin Pe Tzadhe Qoph Resh Shin Tau

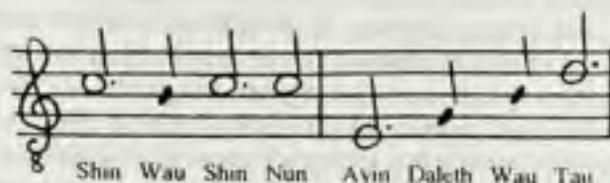
So have you found what Fitzgerald calls "the Clue...to the Treasure-house"? Well, you can only try it out on some of the psalm-headings and see what happens. First of all you transcribe two words involving eight characters from Psalm 22 (= "the deer of morning"). The AV transliterates them *Aijeleth Shahar*, but in Hebrew they're spelt Aleph-Yodh-Yodh-Lamedh-Tau He-Shin-Shin-Cheth-Resh (both the second and sixth characters stand for double letters). You use bar-lines only to indicate the end of a Hebrew word.



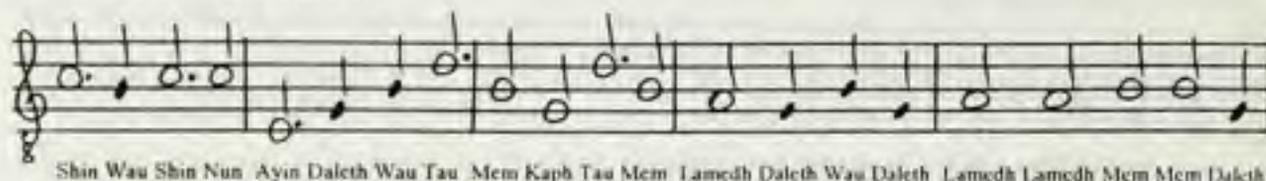
Aleph Yodh Yodh Lamedh Tau He Shin Shin Cheth Resh

This ten-note melodic fragment strikes you as perfectly credible, and even memorable. Its first five notes sound the D minor arpeggio, much as the first four notes of *Morning has broken* sound the C major arpeggio. The interval of a minor seventh between the eighth and ninth notes doesn't give you any problem: *Silent night* has a minor seventh between its eighth and ninth notes! You feel encouraged. Then you remember Psalm 60's floral exhibit in the case,

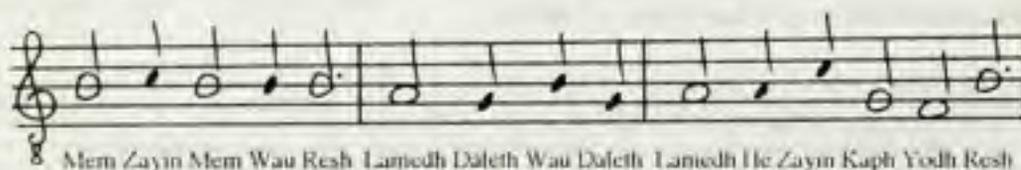
"the lily of testimony", and transcribe the two Hebrew words which the AV renders *Shushan-eduth*. They are spelt Shin-Wau-Shin-Nun Ayin-Daleth-Wau-Tau.



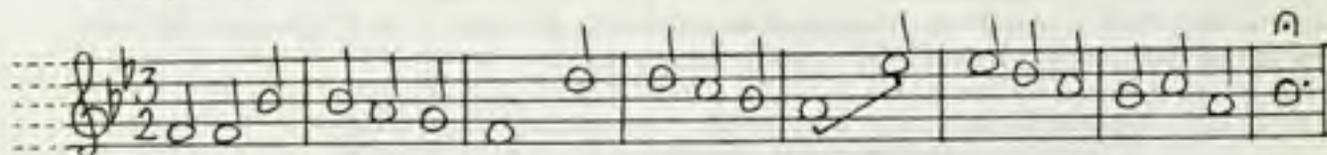
Once again, you think, a credible fragment. The last three notes sound the G major arpeggio. But are they really the last? Is it possible that the next three Hebrew words (AV, "Michtam of David, to teach") continue the notation-mnemonic? You transcribe these three words and add them on to your transcription of *Shushan-eduth*. They are spelt Mem-Kaph-Tau-Mem Lamedh-Daleth-Wau-Daleth Lamedh-Lamedh-Mem-Mem-Daleth (the penultimate character stands for a double letter).



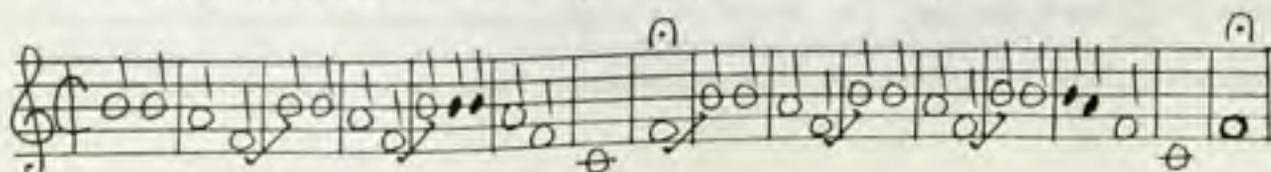
Very plausible, you think, especially since the extended melody contains no less than seven consecutive notes of the G major arpeggio. And now you recall Psalm 38's heading (AV, "A Psalm of David, to bring to remembrance"). It consists of three Hebrew words, which are spelt Mem-Zayin-Mem-Wau-Resh Lamedh-Daleth-Wau-Daleth Lamedh-He-Zayin-Kaph-Yodh-Resh. When you transcribe the three words you discover that the first 13 notes are contained by the fourth *g* to *c'*.



The tritone interval *f* to *b* between the last two notes doesn't worry you at all. You know ten hymn-tunes — *Agapé*, *Alford*, *Bonn*, *Gaudete*, *Hawkhurst*, *Rhuddlan*, *St Audrey*, *St Bartholomew*, *Tyrol*, and *Zenas* — which contain tritone intervals. In fact the composer of *Bonn* (Beethoven, according to the hymn-book) actually reserves the tritone for the moment of melodic climax.



Aside from these hymn-tunes you know 31 Icelandic melodies which contain tritones [Bjarni Thorsteinsson, *Íslenzk Thjóðhlög* (Copenhagen, 1906-09—henceforth *II*), 213, 215, 215-6, 217 (2 examples), 221, 222-3, 227-8, 238, 239, 244, 245-6, 248, 248-9, 252, 255-6, 268, 269, 274-5, 285-6, 291-2, 298, 298-9, 299, 299-300, 348, 357, 366, 380, 389, and 391]. The Icelandic unfeared of the tritone is exemplified by the tune *Kvinnan fráma* (*II*, 215):



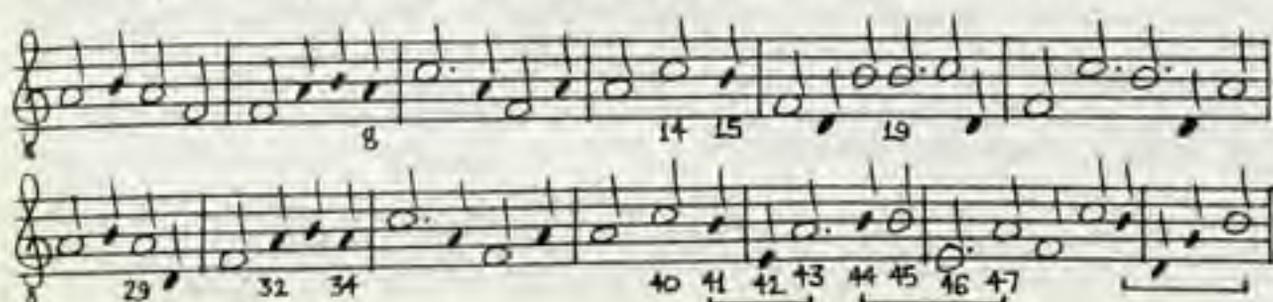
As Le Corbusier says (*TM*, 191), "The gun-dog puts up the game." By now you're beginning to wonder about the psalms which have no mysterious headings. Is it possible that in some cases the tunes are hidden in particular verses? Could such verses both mean what they mean in Hebrew and constitute the notation of melodies? Well, as you happen to know, "Italian Jewish poets of the early modern period wrote poems which could be read as both Hebrew and Italian" [Lawrence Zalcman, 'Death and the Calendar', in *Hebrew University Studies in Literature and the Arts* 16 (1988), 99]. In this connection a phrase from Vergil comes into your mind. The words "it comes" can be construed as a plausible part of an English sentence, but they are also the first two words of *Aeneid* 6.159: *it comes et paribus curis vestigia figit*. And that's a mere accident. In deliberate terms, if it's possible to write Hebrew which can also be read as Italian, then it's certainly possible to write Hebrew which can also be read as music notation. English experiments along these lines are doomed to **be caged in a bad café and fed aged beef and faded cabbage**, but a system employing the whole alphabet would let you have everything from Arbroath smokies to zwiebacks. So what should you do? Learn off your own hypothetical notation-system, and then sing your way through some of the psalms.

You prepare yourself to find three sorts of melody: *definitely not*, *perhaps*, and *definitely*. And straight away you remember another sentence of Corb (*TM*, 34): "There's a lot of dangerous ideas for you!" Because what you're about is very dangerous indeed. Your numerical discoveries in Psalm 49 are indisputable. Your notation-system is plausible. But the question of what is melodic and what is not is too big to be answered by one man sitting at his desk. You're in danger of rejecting genuine melodies (and worse, of identifying ungentle ones) on the grounds of your own notions. That's what it comes down to. So what you're going to do is only a preliminary exercise. You really need lots of people—musicians, not musicologists—to test anything you identify *by performing it*. Ultimately, you need all 150 psalms printed out in music notation for other people to study. But in advance of that, you decide to sing your way through some of the psalm-texts (at crotchet = 84 or so) and see what happens.

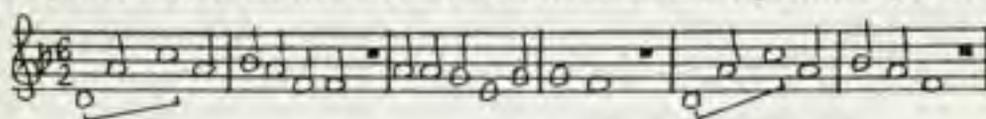
It's laborious at first, and worse—it's tuneless. By the end of Psalm 3 you think you're getting nowhere, so you decide to fire a shot in the dark by jumping into one of your favourite psalms, Psalm 124, which bears the title, "A Song of degrees of David." Psalms 120 to 134 are all "Songs of degrees", mostly anonymous, and while you've no idea what a "Song of degrees" is, you can't believe that the Hebrew words thus rendered (spelt in all but one case Shin-Yodh-Resh He-Mem-Mem-Ayin-Lamedh-Wau-Tau) represent anything melodic. The intervals don't bother you, but the amorphous rhythm does. You reckon that if the Levites whistled *this* sort of thing on the way to work, they wouldn't have needed to keep their music notation cryptic:



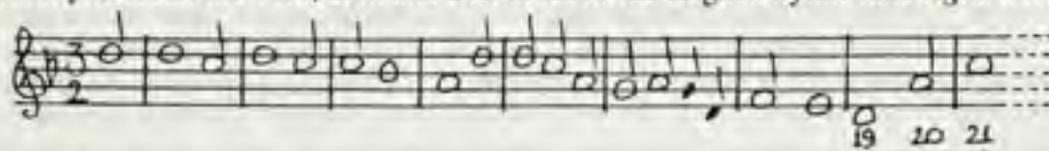
So you set aside the 13 characters of title (the fifth character stands for a double letter), and sing through the text. The first two verses form a unit that starts on the 14th written character of the psalm and contains 14 words (or one hyphenated and 12 unhyphenated words). [You remember that 14 is the numerical value of Daleth-Wau-Daleth (= *David*). The number 14 is so important as a denotation of David that even St Matthew arranges "the book of the generation of Jesus Christ, the son of David" (*AV*, chapter 1, verse 1) in three 14s (verse 17)]. There are eight verses altogether. Now while nothing in verses 3 to 8 of Psalm 124 strikes you as deliberately melodic, you are excited to find that verses 1 and 2 transcribe as follows:



The first six notes sound the D minor arpeggio; there is a deliberate build-up to a medial climax on the 16th note; and the whole tune has a more rhythmical impact than anything you've transcribed so far. The melody is sturdy and memorable, unlike many modern hymn-tunes. Asaph's double use of the pitches *d a c'* (respectively notes 9, 10, 11, and notes 23, 24, 25) creates a feeling of taut melodic construction. There's nothing unlikely or difficult about the *d a c'* phrase: you remember the first six bars of the Icelandic tune *Eljakvæði (II, 519)*:



In addition you recall the 19th, 20th and 21st notes of the English hymn-tune *Agincourt*:



You've no doubt that verse 10 contains the tune, but you wonder whether the fact is indicated by anything apart from its obvious tunefulness. Psalm 124 was written by David; David's name in Hebrew has a numerical value of 14, as even St Matthew expects us to know; the tune of Psalm 124 starts on the 14th character of the psalm and employs 14 words. You can't see anything similar in Psalm 81, but one thing does strike you as rather obvious. The psalm in English begins, "Sing aloud unto God..." If the title is left aside, "*Sing aloud*" translates the psalm's first Hebrew word, and "*unto God*" translates its second Hebrew word. The word for "unto God" begins with the letters Lamedh-Aleph, and so does verse 10 in Hebrew — in fact, it is the *only* verse of the psalm that begins with the letters Lamedh-Aleph. So the opening "Sing aloud Lamedh-Aleph..." may indicate that the tune, the thing that actually has to be *sung aloud*, begins with these two letters. That strikes you as sufficient indication. The Lamedh-Aleph pointer is neat and simple. It's as if an English poem began with the words "Sing loud" and the verse containing the tune began with the word "Lo!"

That reminds you, "Begin, and somewhat **loudly** sweep the string..." What's that? Of course — Milton's *Lycidas*, line 17. Now there's no cryptic melody in any line of *Lycidas*, but the poem is a lament for the death of Edward King, and it makes you think at once of two secular laments written by David. The lament for Abner is found in II Samuel 3.33–34, and with supplied words italicized reads in the AV as follows:

33 And the king lamented over Abner, and said, Died Abner as a fool dieth? 34 Thy hands *were* not bound, nor thy feet put into fetters, as a man falleth before wicked men, so fellest thou.

The lament for Saul and Jonathan, a much more substantial poem, is found in II Samuel 1.17–27, and with supplied words italicized reads in the AV as follows:

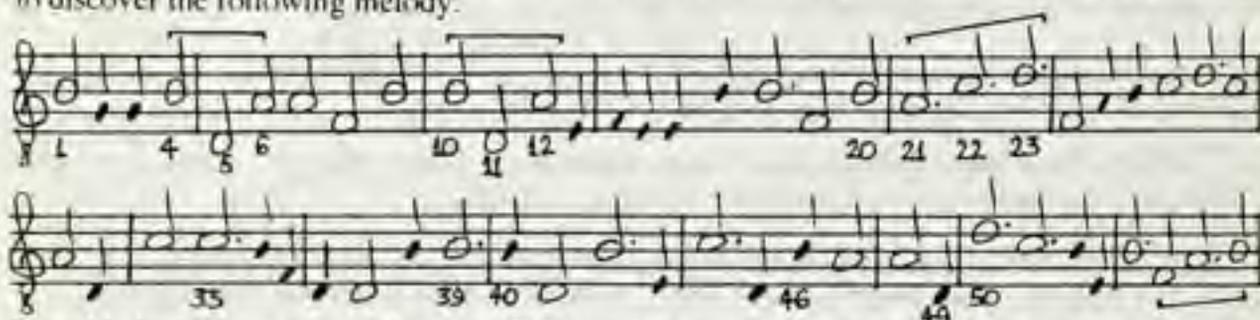
17 And David lamented with this lamentation over Saul and over Jonathan his son: 18 (Also he bade them teach the children of Judah the use of the bow: behold, it is written in the book of Jasher.) 19 The beauty of Israel is slain upon thy high places: how are the mighty fallen! 20 Tell it not in Gath, publish it not in the streets of Askelon; lest the daughters of the Philistines rejoice, lest the daughters of the uncircumcised triumph. 21 Ye mountains of Gilboa, let there be no dew, neither let there be rain, upon you, nor fields of offerings: for there the shield of the mighty is vilely cast away, the shield of Saul, as though he had not been anointed with oil. 22 From the blood of the slain, from the fat of the mighty, the bow of Jonathan turned not back, and the sword of Saul returned not empty. 23 Saul and Jonathan were lovely and pleasant in their lives, and in their death they were not divided: they were swifter than eagles, they were stronger than lions. 24 Ye daughters of Israel, weep over Saul, who clothed you in scarlet, with other delights, who put on ornaments of gold upon your apparel. 25 How are the mighty fallen in the midst of the battle! O Jonathan, thou wast slain in thine high places. 26 I am distressed for thee, my brother Jonathan: very pleasant hast thou been unto me: thy love to me was wonderful, passing the love of women. 27 How are the mighty fallen, and the weapons of war perished!

Three supplied words in verse 18—*the use of*—represent a misunderstanding on the part of the translators. Verse 18 doesn't mean that David ordered the children of Judah to be taught archery, and that his instruction is recorded in the book of Jasher. It really means that David ordered the children of Judah to be taught "this lamentation" (verse 17), entitled *The Bow*, and that the lamentation is also recorded in the Book of Jasher. You think of a modern analogy

"And the poet Edward Thomas wrote this poem: (Also he bade them teach the children of England the trumpet: behold, it is written in the Albatross Book of Living Verse.)

Rise up, rise up,
And, as the trumpet blowing
Chases the dreams of men."

What were the children of England to be taught? Trumpet-playing? No—they were to be taught Edward Thomas' poem *The Trumpet*. And so in II Samuel 1, if Hebrew had capital letters and italics, you would understand verse 18 thus: "...he bade them teach the children of Judah *The Bow*." You notice that in the Hebrew of verse 18 there isn't any definite article before the word for **bow**. And now you wonder why David uses the word for **bow** as title for his lament. It can't be anything to do with Jonathan's signal-arrows in I Samuel 20, since the lament makes no reference to them. But the Hebrew word for **bow** is used once in the song, in what grammarians call the "construct state", without the definite article—in verse 22. May the titular word **bow** be a musical pointer to the only verse which contains that word, verse 22? In other words, does verse 22 constitute the tune? You apply your notation-system to the Hebrew text of verses 19, 20, 21, 23, 24, 25, 26, and 27. None of these verses comes out as an obviously credible melody. Then you look at verse 22, the only verse that contains the word **bow**. In the AV it reads, "From the blood of the slain, from the fat of the mighty, the **bow** of Jonathan turned not back, and the sword of Saul returned not empty." The language is strange: at first sight Jonathan's **bow** seems to be directed against people who are already dead. But after a moment's thought you see that the word "slain" is proleptic, and really means something like *dā der veigen lip* in line 2377 of *The Nibelungenlied*—"those who were doomed to die". That leaves you with "the fat of the mighty". Initially, remembering how the same Hebrew word is used in Genesis 45.18 ("the fat of the land"), you take the word "fat" to denote strength and prosperity. But then you recall that this Hebrew word nearly always indicates the fat of sacrificial animals, and you realize that David is using "fat" in proleptic parallel with "slain", to mean something like "[destined to become] dead meat". Does the taut language of verse 22 point to a musical subtext? You transcribe its 14 Hebrew words to discover the following melody:



It could almost be a pibroch lament. The tritone, occurring no less than five times, is used with great deliberation. There are in addition two obvious pairs of melodic 'pillars'. First, the group consisting of notes 4, 5, and 6 reappears exactly in notes 10, 11, and 12. Secondly, the pitches of notes 21, 22, and 23 are transposed down a third to give the last three notes of the tune. (In this connection you find yourself thinking of Parry's Psalm 84, in which the melody of bars 1-7 is reiterated a third higher in bars 8-14!) The plangent tension created by notes 1 to 20, which are all contained by the interval of a sixth, is released by the notes 21 to 23. These three notes represent the Hebrew word for **bow**, and constitute a medial climax. The pitch-contour of notes 33 to 39—*c'*, descending B diminished arpeggio, and two *hs*—corresponds with that of notes 14 to 19 of Psalm 124, which was also written by David.

The Song of the Bow,
notes 33 to 39



Psalm 124,
notes 14 to 19



A deliberately melodic use of the sixth characterizes notes 40 to 46, and prepares us for the octave leap of the final climax (notes 49 and 50). Overall the melody is a very satisfying piece of musical architecture, and besides that it really does sound like a lament. It sounds *right*, especially at crotchet = 72; and when you consider the emotional effect of the two psalm-tunes, you're struck by the same kind of aptness. Psalm 124 sounds reflectively happy and grateful. Psalm 81 sounds robustly jubilant and admonitory. You feel rather happy and grateful yourself, but it occurs to you now to give your voice a rest, and to see if any hint of David's notation-system appears in the Biblical text. So you leave Ajeleth and look for Aleph; that is, you leave the "female deer" and look for the *doh*.

First of all you have to decide what you're looking for. You believe that each of the notes *d* to *c'* can be represented by three different letters denoting three different time-lengths (**Aleph**, **Cheth** and **Samekh** standing for *d* crotchet, *d* minim, and *d* dotted minim, and so on). You also believe that the note *d'* is always a three-beat note and always represented by the same letter (**Tau**). A parallel suggests itself to you. If a similar *English* musical alphabet used 22 letters, and stopped at **V**, then it could represent the three time-lengths of the notes that we presently call *d* to *c'* as follows: *d* = **A-H-O** (viz **A** = *d* crotchet, **H** = *d* minim, **O** = *d* dotted minim), *e* = **B-I-P**, *f* = **C-J-Q**, *g* = **D-K-R**, *a* = **E-L-S**, *b* = **F-M-T**, and *c'* = **G-N-U**. The note *d'*, always a dotted minim, could be represented by **V**. How might you conceal the three-note groups of such a system in a poem? Consecutive letters? Alternate letters? Initial acrostics? And how would you show the perpetual trinity of **V**? Of course—make **V** the 27th letter (27 = 3 3.3). So you write a three-line neo-Yeatsian poem entitled **All I have, I owe**:

"Ahoy," the **biped duke** rejoined, "invest
Thy gold, **fumatic gnu**, in Budapest
Or **else** in Batley!" **Crazy Jane** quiesced.

As Edgar Allen Poe said in a similar situation, "...upon the whole, the rhythm is very decent—to say nothing of its excellent sense." So you're rather annoyed to find that the Psalter contains nothing to compare with your magnificent didactic terset. You search the Hebrew text long and hard for any cryptic conjunction of 'same-pitch' letters, but with minimal joy. Then suddenly you realize that anything like your terset would be completely useless. Why? Because there's no point in merely displaying a relationship between 'same-pitch' letters: it's essential to show *what the actual pitches are*. Unless your "biped duke" can prove that he descends from the white-note scale of *d*, he hasn't got a leg to stand on. So you start thinking in Hebrew again. How might you cryptically associate two facts: first, that the note *d* is produced by a string whose unitary length is 864, and second, that the three different time-lengths of that note are represented by the letters **Aleph**, **Cheth** and **Samekh**? It's going to need numbers again. It's probably going to mean combining the number 864 with the numerical values of **Aleph**, **Cheth** and **Samekh**. But now the horns of a dilemma sound in your mind: because there are *two* numerical alphabets. One runs from 1 to 400, and the other (the 'place values' alphabet) from 1 to 22. You resolve to call them henceforth simply *the 400 alphabet* and *the 22 alphabet*. Remembering that Psalm 49's riddle explicitly stated only the bottom and top notes of the scale, you decide for the present to focus on these two notes (string-lengths 864 and 432).

In the 400 alphabet the letters **Aleph**, **Cheth** and **Samekh** have values respectively of 1, 8 and 60. If you add those three values to the string-length 864, you get **933**. In the 22 alphabet the letters **Aleph**, **Cheth** and **Samekh** have values respectively of 1, 8 and 15. If you add *those* three values to the string-length 864, you get **888**.

In the 400 alphabet the letter **Tau** has a value of 400. If you add that value to the string-length 432, you get **832**. In the 22 alphabet the letter **Tau** has a value of 22. If you add *that* value to the string-length 432, you get **454**.

Now the numbers **933** and **888** are formed by the combination of two disparate quantities—string-length and different time-lengths—so you're prepared to find each of these numbers represented by a group of words rather than simply by one word. You're prepared to find the

same thing in the cases of both 832 and 454, but you'd like the letters that represent one or other of these two numbers to indicate that the note *d* always has the same time-length.

You begin in 400 alphabet gematria with 933. That number, if you find it, will stand for a notation-system in which the letters **Aleph**, **Cheth** and **Samekh** indicate respectively crotchet *d*, minim *d* and dotted minim *d*. System apart, it will also represent three "deep" or "bottom" notes of different time-lengths. To speak of "deep" notes in the context of ancient Hebrew music doesn't strike you as a fatuous conceptual transfer: for whereas much of the musical theory of ancient Greece was based on a lyre with strings of approximately equal length, and a descending scale, you're sure that the corresponding Israelite instrument was strung in accordance with the numbers of Psalm 49, whose order (864 before 432) implies an ascending scale. Seeing no great conceptual difference between length and depth, you don't think it anachronous to say that in ancient Israel the longest string gave the "deepest" note. Once you've said that, a thought occurs to you. Suppose the Psalter actually does contain (in 400 alphabet gematria) such an example of the number 933 as you hope to find. If the letters **Aleph**, **Cheth** and **Samekh** are neither in nor near that number, then you'll need some clear sign that the number relates to three "deep" or "bottom" notes. A 933 on its own will tell you no more than a 432 on its own, and there are loads of 432s. So how many 933s are there? Or first of all, how many single-word 933s are there?

Well, you search, and it turns out that there's only one. It comes in the strange and powerful eighth verse (AV, verse 7) of Psalm 42. "**Deep calleth unto deep** at the noise of thy waterspouts..." The English words **Deep...unto deep** translate the single (three-part, hyphenated) Hebrew word **Tau-He-Wau-Mem — Aleph-Lamedh — Tau-He-Wau-Mem** with which the verse begins.

In gematria the word is $[400 + 5 + 6 + 40] + [1 + 30] + [400 + 5 + 6 + 40] = 933$. You are inclined to believe that the compound word's *three* parts represent the *three* different time-lengths of the note *d*. You see the word "calleth" in a musical sense. You imagine someone learning to play *d* crotchet, *d* minim and *d* dotted minim—bottom notes, "deep" notes—on a string 864 units long, and it seems to you that it would be hard to do a better job of cryptic representation. "Deep calleth unto deep" means, in musical terms, "*d* calleth unto *d*".

$$933 = 864 + 1 (\text{Aleph}) + 8 (\text{Cheth}) + 60 (\text{Samekh})$$

And there's more. Verse 8 of Psalm 42 contains eight words with numerical values as follows:

$$\underline{933} \quad 307 \quad 166 \quad 376 \quad \underline{622} \quad 69 \quad 110 \quad 278$$

The fifth word (= 622) is two thirds of the first word (= 933), exactly as the fifth note of the scale (*a* = 576) is in linear terms two thirds of the first note (*d* = 864). You take that fact as another musical pointer. And you even wonder if the text contains an oblique reference to sympathetic resonance.

At first you're pleased enough with the 933 that you've discovered, but in time you come to wish that it occurred in an explicitly musical context (as 864 and 432 did in Psalm 49). That wish leads you to look in obvious musical contexts for 933s represented by *groups of words*. You think first of the Levitical music in I Chronicles 15.16. "And David spake to the chief of the Levites to appoint their brethren to be the singers with instruments of musick, psalteries and harps and cymbals, sounding, by lifting up the voice with joy." The six words **sounding, by lifting up the voice** have always puzzled you: they seem clumsy and unnecessary. (The two Hebrew words which represent them are equally hard to account for.) Why bother to tell us that singers make a sound by lifting up the voice? Besides that, since all the singers' instruments are in the plural, why is the word *voice* in the singular? As it stands in English the verse would lose little if it had a full stop after *cymbals*. What then? Do the two "unnecessary" Hebrew words possess a technical sense? Do they have musically significant number-values? You spell out the two Hebrew words in gematria as follows:

1. "sounding"

Mem-Shin-Mem-Yodh-Ayin-Yodh-Mem
 = 40 + 300 + 40 + 10 + 70 + 10 + 40 = 510

2. "by lifting up the voice"

Lamedh-He-Resh-Yodh-Mem—Beth-Qoph-Wau-Lamedh
 = 30 + 5 + 200 + 10 + 40 + 2 + 100 + 6 + 30 = 423

$$510 + 423 = 933$$

The arithmetic speaks for itself. "Sounding, by lifting up the voice" means *playing and singing from Davidic notation*. Of course on the day the Levites might have played their whole programme from memory, like Russian or Ukrainian folk orchestras, but they would have learnt the music from notation in the first place. Likewise, you realize, the children of Judah in II Samuel 1.18 would have learnt *The Song of the Bow* from its own contained notation. And suddenly you remember something odd about II Samuel 1.18. With the translators' supplied words omitted, that verse reads in the AV, "Also he [= David] bade them teach the children of Judah...the bow: behold, it is written in the book of Jasher." Why does the narrator trouble to tell us that the song "is written in the book of Jasher" when he's going to quote its complete text in his own narrative? Normally when Biblical writers refer the reader to another book, they do so in order to save themselves repeating something that has been written elsewhere. Thus I Kings 11.41 reads, "And all the rest of the acts of Solomon, and all that he did, and his wisdom, are they not written in the book of the acts of Solomon?" References are usually given in the form of a negative question: *are they not written?* You know 33 examples of references in that form which contain no *behold* or *lo*: Joshua 10.13; I Kings 11.41, 14.29, 15.7, 15.23, 15.31, 16.5, 16.14, 16.20, 16.27, 22.39, 22.45; II Kings 1.18, 8.23, 10.34, 12.19, 13.8, 13.12, 14.15, 14.18, 14.28, 15.6, 15.21, 15.36, 16.19, 20.20, 21.17, 21.25, 23.28, 24.5; II Chronicles 9.29, 12.15; and Esther 10.2. And you know one example which combines *behold* with the negative question form: II Chronicles 25.26. But you know only 18 examples which combine *behold* or *lo* with a positive statement: II Samuel 1.18 (about *The Song of the Bow*), I Kings 14.19, II Kings 15.11, 15.15, 15.26, 15.31, I Chronicles 29.29, II Chronicles 16.11, 20.34, 24.27, 27.7, 28.26, 32.32, 33.18, 33.19, 35.25, 35.27, and 36.8. So you take note of two things about the reference in II Samuel 1.18 to the book of Jasher. The *fact* of the reference is remarkable, given that the writer of II Samuel quotes the full text of David's song in verses 19–27. And the *form* of the reference—a positive statement preceded by *behold*—is not so common as that of the negative-question reference. So, confronted by a seemingly "unnecessary" reference (cast in a less common form), and not having access to the book of Jasher, you wonder if there's anything going on in the three Hebrew words represented by **behold, it is written in the book**. Do *these* words have musically significant number-values? You spell out the three Hebrew words in gematria as follows:

1. "behold"

He-Nun-He = 5 + 50 + 5 = 60

2. "it is written"

Kaph-Tau-Wau-Beth-He = 20 + 400 + 6 + 2 + 5 = 433

3. "in the book"

Ayin-Lamedh—Samekh-Pe-Resh = 70 + 30 + 60 + 80 + 200 = 440

$$60 + 433 + 440 = 933$$

Once again, the arithmetic speaks for itself. Leaving aside the book of Jasher, you see the purpose of the words **behold, it is written in the book**. They indicate that the song's text *contains its own notation*, and that the children of Judah *learnt the song from notation*. The verse's final word "Jasher", like the final Hebrew word representing the phrase "with joy" in I Chronicles 15.16, may constitute what cryptographers call a "null" or "non-significant": but you're beginning to realize that there are no "insignificant" or "unnecessary" words in the Bible. From now on, when you find a piece of language that *seems* superfluous or even verbose, you'll try to establish its purpose. Especially if it's been written by David. Like the first two verses of Psalm 18 (AV, title plus verse 1):

To the chief Musician, a Psalm of David, the servant of the Lord, who spake unto the Lord the words of this song in the day that the Lord delivered him from the hand of all his enemies, and from the hand of Saul: And he said, I will love thee, O Lord, my strength.

The Hebrew word representing *And he said* is not remarkable in itself, but it has a remarkable function: it constitutes a bridge between the titular verse and the first *sung* verse. In fact, *it joins the two verses and makes them into a unit*. Within the Psalter the usage is unique to Psalm 18. And the word that David uses for **song** is also unparalleled: for while the masculine word Shin-Yodh-Resh appears no less than thirty times in the psalm-titles, Psalm 18's feminine word Shin-Yodh-Resh-He *appears nowhere else in the Psalter*. That's not all. The two Hebrew words representing **this song** are preceded by the first 34 characters and followed by the last 34 characters of verse 1: they seem to have been given a deliberate centrality. [34 is the 400 alphabet numerical value of the word Kaph-Daleth-Wau-Daleth = *according to David, or in the manner of David*.] So two Hebrew words, meaning **this song**, stand exactly in the middle of verse 1, and the noun is a form unique to the Psalter. Are these two words significant? Do they have musically significant number-values? You spell out the two Hebrew words in gematria as follows (the first letter of each word is an untranslated definite article):

1. "song"

He-Shin-Yodh-Resh-He = 5 + 300 + 10 + 200 + 5 = 520

2. "this"

He-Zayin-Aleph-Tau = 5 + 7 + 1 + 400 = 413

$$520 + 413 = 933$$

So here you have a third statement of the Davidic notation-system in an explicitly musical context: the Hebrew words representing **this song** have a numerical value of **933**. And there's more. **933** represents the three time-values of the note *d* (864 + **Aleph** + **Cheth** + **Samekh**) in 400 alphabet gematria. The corresponding number for the note *e* is **849** (768 + Beth + Teth + Ayin), and the prepenultimate and penultimate words of verse 3 (represented in AV verse 2 by the words "my buckler, and the horn of my salvation") add up to **849**. The following three words—final word of verse 3, first two words of verse 4—add up to **762**, which stands for the note *g* (648 + Daleth + Kaph + Tzadhe = **762**). That's enough to be going on with as far as the first seven degrees of the scale are concerned, but you'd like to find the number **832**, which stands for top *d'* (432 + Tau = **832**), and you'd like some indication that Tau always has the same three-beat value. As it happens, there are two **832s** in Psalm 18. One comes in the prepenultimate and penultimate words of verse 28 (represented in AV verse 27 by the words "high looks"), and the other comes in the prepenultimate and penultimate words of verse 23 (represented in AV verse 22 by the words "and I did not put away his statutes"). The letter Tau is part of the spelling in each case. Now while the *two* appearances of **832** may indicate obliquely, so to speak, that "Tau is always the same", the words **I did not put away his statutes** may be taken as a cryptic reference to Tau's unchanging time-

length. In this connection you remember your poem about the biped duke, in which **V**, corresponding to the Hebrew letter Tau, was the 27th letter ($27 = 3.3.3 =$ perpetual trinity), and you're interested to see that the 27th *sung* character of Psalm 18 is Tau. Finally, you wonder whether the psalm contains any numerical hint that the notation-system really is Davidic. **933** plus the authorial word Lamedh-Daleth-Wau-Daleth (which has a 400 numerical alphabet value of 44 and can mean variously "by David", "of David", and also "to David") is **977**. So when you find that the last four words of Psalm 18 add up to **977** (the second of these four words happens to be Lamedh-Daleth-Wau-Daleth!), you feel more than pleased. You've found several cryptic references to David's **Aleph-Cheth-Samekh** notation system. The system is expressed in terms of the 400 alphabet, and that's all there is to it.

Or is it? You think of the riddle of Psalm 49, which has to be read in terms of both the 400 alphabet and the 22 alphabet, and you wonder. At the same time you recall three differences between the opening verses of II Samuel 22 and those of Psalm 18. First, the Hebrew word for "him" is spelt Aleph-Tau-Wau in II Samuel 22.1, but Aleph-Wau-Tau-Wau in Psalm 18.1. Secondly, while in II Samuel the same word is used for *hand* each time, in Psalm 18 two different words are used. Thirdly, the words that open the actual song in II Samuel ("The Lord is my rock, and my fortress") are preceded in Psalm 18 by a declaration: "I will love thee, O Lord, my strength." You've already seen how the Hebrew word for *And he said* joins the psalm's first two verses (AV, title plus verse 1) and makes them into a unit. And now you wonder whether the two verses in question constitute a *numerical* unit. [You can think of no *literary* explanation for David's verbal changes. For example, you can't believe that he uses two different words for *hand* merely in order to avoid repetition, since he mentions the Lord's name *three times* in the title of Psalm 18.] So what happens if you add up the 21 words of the two-verse unit in the 400 alphabet?

$218 + 106 + 26 + 44 + 501 = 206 + 56 + 6(7) = \underline{520} + \underline{413} + 58 + 161 + 413 + 140 + 79 + 60 + 337 + 257 + 269 + 26 + 125 = 4632$

Nothing of obvious significance! Then what happens if you add them up in the 22 alphabet?

$65 + 34 + 26 + 26 + 42 + 26 + 38 + 59 + 61 + 35 + 31 + 71 + 35 + 41 + 52 + 33 + 40 + 50 + 53 + 26 + 44 = \underline{888}$

And at once you see what David has done. While the number **933** represents 864 plus **Aleph** plus **Cheth** plus **Samekh** in the 400 alphabet, the number **888** represents 864 plus **Aleph** plus **Cheth** plus **Samekh** in the 22 alphabet. David has reworked the language of his original version for two reasons. First, he wants to give his two-verse unit a 22 alphabet total of **888**. Secondly, he wants to focus the musical reader's attention on the three letters **Aleph**, **Cheth** and **Samekh** (compare the letters **A**, **H** and **O** in your English poem **All I have, I owe**), so he rewrites the opening *sung* text of the psalm to make its first, third and fifth words begin respectively with **Aleph**, **Cheth** and **Samekh**. These three letters are recapitulated in the tenth *sung* word ("I will trust in him"), which begins **Aleph-Cheth-Samekh** (and corresponds to the word "**Ahoy**" in your poem). Everything from the Aleph of the first word to the Samekh of the tenth word is contained in 44 characters, and 44 is the 400 alphabet numerical value of the word Lamedh-Daleth-Wau-Daleth (= "by David", or "of David"). You remember wondering whether these 44 characters contained the key to David's *ut re mi fa sol*. You know now that they do, but that's only half of the story: for the letters of David's notation-system indicate *both pitch and duration*. (The letter Samekh, for instance, is a combination of *doh* and *taa-aa-aa*—it stands for dotted minim *d*.) Now the two Hebrew words representing **this song** (= **933**) function as a musical cryptogram in both II Samuel 22 and Psalm 18, but the latter's first 21 words, which employ two different number-lines, make David's notation-system unequivocally clear. You've found **933** (= bottom *d*), and **832** (= top *d'*) in the 400 alphabet. You've also found **888** (= bottom *d*) in the 22 alphabet. Can you find any reference to **454** (= top *d'* in the 22 alphabet)?

One word in Psalm 18 has a 400 alphabet value of **454**—the eight-letter (three-part, hyphenated) third word of verse 43 (AV, verse 42). It is represented in English by the phrase "before the wind", and spelt Ayin-Lamedh—Pe-Nun-Yodh—Resh-Wau-Cheth. Did David intend this word to stand for the three-beat eighth note of the scale? You aren't sure. Its actual

meaning doesn't help you much. "Then did I beat them small as the dust *before the wind*..." What you really want is some expression corresponding to the word Tau-Beth-Lamedh (= *world*), which appears fifteen times in the Psalter, begins with Tau, and has a 400 alphabet numerical value of 432; and you would like the *meaning* of such a corresponding expression to indicate Tau's unchanging three-beat value. So you check through the Psalter for words with a 400 alphabet numerical value of 454. The number 454 appears 34 times. It is represented in 23 cases by the Hebrew word **Tau-Mem-Yodh-Daleth**. In the AV Psalter that word is rendered once **always**, once **evermore**, twice **ever**, and nineteen times **continually**. So given the word's frequent occurrence, initial letter (Tau), numerical value (454), and meaning, you find it easy to believe that **Tau-Mem-Yodh-Daleth** was the Levitical mnemonic for a perpetually three-beat top *d'*. And at once you find yourself recalling a strange use of the same Hebrew word in I Chronicles 16. Verses 4, 5 and 6 read as follows in the AV:

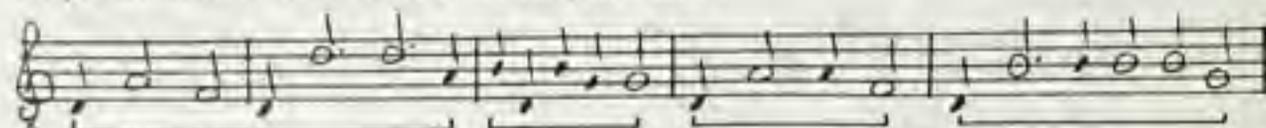
4. And he appointed certain of the Levites to minister before the ark of the Lord, and to record, and to thank and praise the Lord God of Israel. 5. Asaph the chief, and next to him Zechariah, Jehiel, and Shemiramoth, and Jehiel, and Mattithiah, and Eliah, and Benajah, and Obed-edom, and Jehiel with psalteries and with harps, but Asaph made a sound with cymbals. 6. Benaiah also and Jahaziel the priests **with trumpets continually** before the ark of the covenant of God.

The word **continually** here represents the Hebrew **Tau-Mem-Yodh-Daleth**. If the word appeared in verse 4 it would seem much more natural. ["And he appointed certain of the Levites to minister continually..."] At first reading it looks as if psalteries, harps and cymbals played every so often, while trumpets played all the time! But the nature of mouthpiece instruments makes that reading impossible. Lips become exhausted very quickly: a harper can play non-stop for an hour, but you don't know any trumpeter who would like to play non-stop for ten minutes. Then what is the word doing here? Is it meant to cover all four instruments? If so it would make a lot more sense to put it in verse 4. And yet **Tau-Mem-Yodh-Daleth** is stuck right beside the word for "with trumpets" (Beth-Cheth-Tzadhe-Tzadhe-Resh-Wau-Tau). The instruments in question were tuned silver trumpets, not ramshorns, and that fact reminds you of something. In the early stages of synthesizing David's notation-system you wondered whether the tuned trumpets (one-note instruments?) would have played the note Tau, three-beat top *d'*, in tutti sections. It seems to you now that the words of I Chronicles 16—**with trumpets continually**—must actually indicate "454 trumpets", trumpets that play the three-beat top *d'*, or D trumpets. You aren't far away conceptually from "recorders at 440"!

But would trumpeters have been "evermore" content to play only three-beat notes? You doubt it. Might they ever have played **continually** by supplying a long pedal note? Something of the kind, something extended, rather than the occasional dotted minim, seems to have been supplied by 120 priestly players of silver trumpets at the consecration of Solomon's temple (II Chronicles 5, 11-14). Verses 13 and 14 read in the AV as follows:

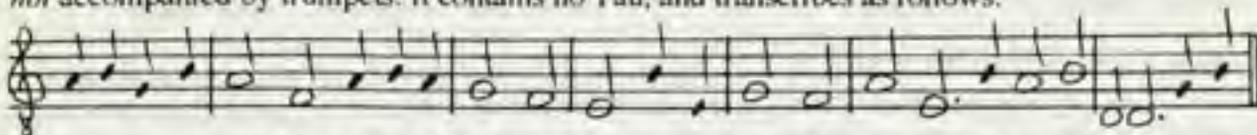
13. It came even to pass, as the trumpeters and singers were as one, to make one sound to be heard in praising and thanking the Lord, and when they lifted up their voice with the trumpets and cymbals and instruments of musick, and praised the Lord, saying, For he is good; for his mercy endureth for ever: that then the house was filled with a cloud, even the house of the Lord. 14. So that the priests could not stand to minister by reason of the cloud: for the glory of the Lord had filled the house of God.

You're not sure what composition was being performed. The Hebrew words represented by **For he is good; for his mercy endureth for ever** appear in Psalms 106, 107, 118, and 136, but only Psalm 118 contains these Hebrew words in both its first and last verses. That fact prompts you to sing through the Hebrew text of Psalm 118. You are amazed to find that the penultimate verse transcribes musically as follows:

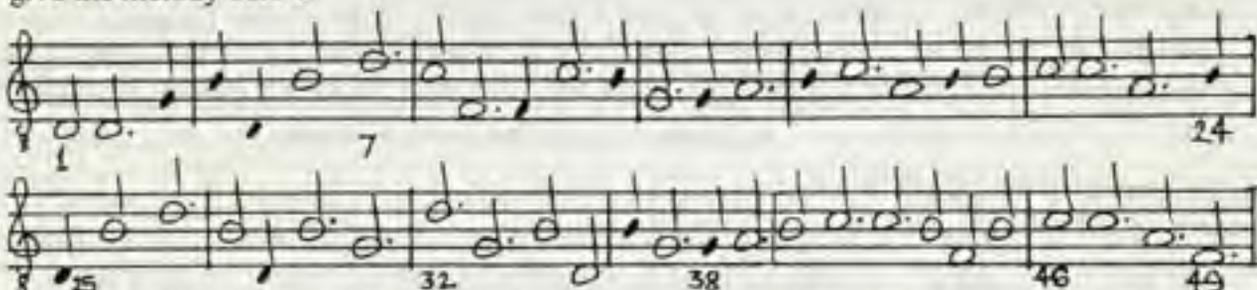


It sounds lively—half the notes are crotchets—and the alternating passages of D minor and G major chord-notes are quite remarkable. The absence of the notes *e* and *c'* means that the

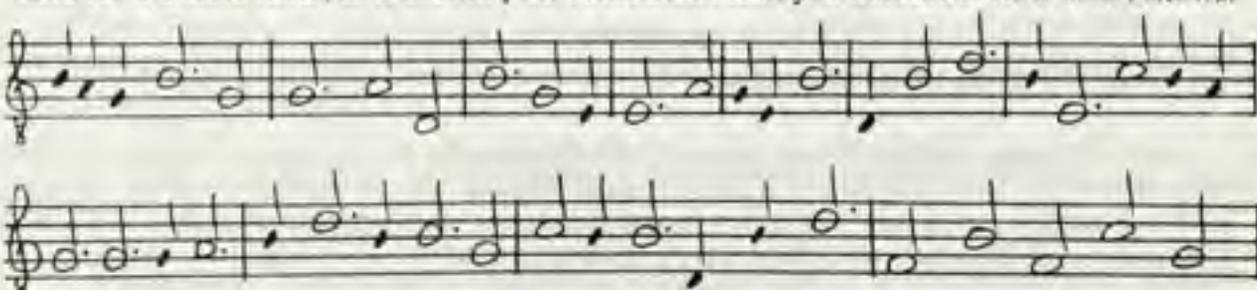
whole melody can be sung and played euphonically over a pedal *d'* (or twelve Taus). Does that explain how "the trumpeters and singers were as one" in II Chronicles 5? You're inclined to take the Hebrew words represented by *one sound* as indicating "one accord" rather than a unison. And you try to imagine the music: performed by enormous forces, and climaxing as 120 priests held a pedal *d'* on silver trumpets. Or did they play different chord-notes, including the note *d'*, on D and G trumpets? Does verse 43 of Psalm 18 (AV, verse 42) contain a cryptic reference to overblowing? Whatever the case, you wonder if the *final* verse of Psalm 118 (which in the Hebrew text is exactly the same as the first verse) constituted part of the music *not* accompanied by trumpets: it contains no Tau, and transcribes as follows:



Once again, it sounds lively (12 out of 25 notes are crotchets), and its last Hebrew word consists of G major chord-notes. That word means *his mercy*, and for no particular reason it reminds you of a passage which *begins* in Hebrew with the word for *mercy*—verses 11 and 12 of Psalm 85. The passage is translated as follows in the AV (verses 10 and 11): "Mercy and truth are met together; righteousness and peace have kissed each other. Truth shall spring out of the earth; and righteousness shall look down from heaven." These two verses transcribe to give the melody below.



Rise and fall are coloured by alternating groups of G and F major chord-notes. The sheer amount of arpeggiation is striking: first seven, and later no less than fifteen consecutive notes of the G major arpeggio (notes 1–7 and 24–38 respectively). You wonder if notes 25–32 and notes 46–49 are intended to paint verse 12's words (respectively truth springing out of the earth, and righteousness looking down from heaven). You go on to ask yourself if the text implies some kind of harmony (*met together / have kissed each other*), based on contrary motion (*spring out / look down*). Then the words *truth* and *righteousness* remind you of another passage—Psalm 45, verse 5 (AV, verse 4): "And in thy majesty ride prosperously because of truth and meekness and righteousness; and thy right hand shall teach thee terrible things." In the Hebrew text verse 5 is preceded by 144 characters, and 144 is the 400 alphabet numerical value of the word Qoph-Daleth-Mem (= *beginning*). Taking the 25th character to stand for a double letter, as it does in your Holzhausen text, you transcribe verse 5 as follows:



Its tunefulness confronts you with a problem. Psalm 45's title begins, "To the chief Musician upon Shoshannim", so if verse 5 is the tune of the psalm, then "Shoshannim" isn't. What does **upon** mean here? Is verse 5's melody played and sung **upon** "Shoshannim"? We shall address these and other harmonic questions in 1995.

הללו-יה

FoMRHI Comm. 1300

From: Charles Stroom (charles@yc.estec.esa.nl or CSTROOM@ESTEC.BITNET)

Further to John Rawson's remarks on mailing lists (C-1277), you may be interested on a few more mailing lists, which do not have such a high level of noise as the instrument makers list (mainly electronic). There exist a lute list (mainly for players), I recently discovered the early-music-list(!) and only just started is the micat-list (on restoration and conservation).

Maybe a brief word on mailing lists (MLs) in general is required. A ML is an email address, which maintains a distribution list. Mail sent to the ML is copied to all participants of the ML. Some MLs are free and open, others are, what is called moderated, i.e. messages are first looked at by the moderators and refereed, before passed on (I guess these are the more serious lists :-). (The micat list is moderated, with Cary Karp and Friedemann Hellwig as two of the moderators.) MLs do usually more, but that depends on who is maintaining it. Most of them keep a repository of the messages, which can be queried by anyone and old messages can be retrieved. Of course, you can always 'unsubscribe' as well.

I append the information messages of the 3 MLs listed above. Be aware that the email address that takes the 'subscribe', 'unsubscribe', 'query', etc message is always a different address than the actual ML email address. It is worth a try, although some of them overflow my mailbox some times.

The Lute mailing list

This list is for messages concerning all aspects of lutes, lute playing, and lute music.

To get on this list, or get off this list, sent mail to: lute-request@cs.dartmouth.edu.

To send a message to the other members of this list, mail it to: lute@cs.dartmouth.edu

Your message will be forwarded to everyone on the list. They will see you as the sender.

There is a complete archive of this list in the directory /pub/lute via ftp from cs.dartmouth.edu

If you really get stuck, you can send mail to me at: wbc@cs.dartmouth.edu, or phone me (603) 646-3198 (days). Wayne Cripps, lute mailing list administrator

The EARLYM-L list (Early Music List).

To send a message to all the people currently subscribed to the list, just send mail to EARLYM-L@AEARN.EDVZ.UNIVIE.AC.AT. This is called "sending mail to the list", because you send mail to a single address and LISTSERV makes copies for all the people who have subscribed. This address (EARLYM-L@AEARN.EDVZ.UNIVIE.AC.AT) is also called the "list address". You must never try to send any command to that address, as it would be distributed to all the people who have subscribed. All commands must be sent to the "LISTSERV address", LISTSERV@AEARN.BITNET (or LISTSERV@AEARN.EDVZ.UNIVIE.AC.AT). It is very important to understand the difference between the two, but fortunately it is not complicated. The LISTSERV address is like a FAX number, and the list address is like a normal phone line. If you make your FAX call someone's regular phone number by mistake, it will be an unpleasant experience for him but you will probably be excused the first time. If you do it regularly, however, he will probably get upset and send you a nasty complaint. It is the same with mailing lists,

with the difference that you are calling hundreds or thousands of people at the same time, so a lot more people get annoyed if you use the wrong number.

Contributions sent to this list are automatically archived. You can get a list of the available archive files by sending an "INDEX EARLYM-L" command to LISTSERV@AEARN.BITNET (or LISTSERV@AEARN.EDVZ.UNIVIE.AC.AT).

MICAT-L: Musical Instrument Conservation and Technology

The Musical Instrument Conservation and Technology Distribution List, MICAT-L, is intended to provide a forum for the discussion of matters of joint interest to the fields of objects conservation and musical instrument technology. Material concerning the theory and techniques of conservation, restoration, scientific examination, documentation, etc., which can be applied to musical instruments is suitable for posting to this list. Similarly appropriate is material concerning the history and technology of musical instruments, including their manufacture, which is of potential use in conservation and restoration. Contributions intended to establish contact within the professional community are also welcome.

is intended to provide a forum for the discussion of matters of joint interest to the fields of objects conservation and musical instrument technology.

The list is open to public subscription but its contents will be moderated. Material describing procedures which would imperil the safety of an object as judged in terms of accepted museum practice will be rejected, as will contributions of potential interest to musical instrument makers and musicians but which lack applicability to conservation or restoration. The degree of latitude applied to the latter consideration will depend on the volume of traffic otherwise produced by the list.

The MICAT-L archives will be maintained as a part of Conservation OnLine -- CoOL. Among a variety of useful features, this service permits searching for individual messages and is available at:
 at: gopher://palimpsest.stanford.edu
 or: <http://palimpsest.stanford.edu/>

CoOL contains much additional information which may be of significant interest to MICAT-L members.

To subscribe, send a message to: mailserv@nrm.se with the ****first**** line in the body of the message ****unindented**** containing nothing except the command, SUBSCRIBE MICAT-L
 Yourfirstname Yourlastname

Your subscription will be acknowledged with a more detailed text about the list and the facilities available to its members.

If you experience any difficulty in accessing this service please contact the listowner at: micat-l-request@nrm.se

Host services for MICAT-L are provided by the Swedish Museum of Natural History (NRM).

FoMRHI Comm. 1301

Further to Comm. 1278

Peter Spohr

As a non-English reader of FoMRHIQ I did not feel to have a mission to make general remarks about the strange style of some disputes in this magazine. Now that I have read Robert Cronin's short communication and especially Ephraim Segerman's (revealing) announcement that "he could be tempted to tear him viciously to pieces" (though this seems to be meant as a joke) I feel obliged to express my agreement with Robert's general remarks.

When I first became a member of FoMRHI I was wondering if the sometimes inconsiderate or even hostile dealing with fellow researchers was an old English or British tradition. Now after many years I have come to the conclusion that for most of it the Honorary Secretary and the Editor have to be blamed. Before I will continue let me make clear that I think it is a stroke of luck for FoMRHI to have these two competent, idealistic and tireless officials who have written many excellent communications and have succeeded in encouraging people to do the same over the years and to think about things they probably would not have done without an inducement. Secondly there can not be any doubt that Jeremy and Ephraim are respected authorities in their fields and - last not least - I think it is important and can only speed up progress in knowledge if you say clearly and right away if you think somebody else is wrong, misinformed, drawing wrong conclusions etc. On the other hand I believe that it will delay this progress if you are treating other people unpolitely, if you do not care much about their labour, engagement, intelligence and feelings and treat them like inexperienced schoolboys (compared to your own knowledge, experience and authority) because this will provoke angry reactions instead of clear thoughts about the topic. And this is only a matter of style and not of "who has the better arguments".

A main reason for some unpleasant disputes is - in my opinion - Jeremy's and Ephraim's (completely unnecessary) tendency towards informing the readers at various opportunities (often latent or between the lines and thus hard to prove) that they are the true experts, the most careful and experienced scholars and all in all important personalities. Continuing this in a dispute with other scholars will regularly lead to a delay of progress as mentioned above. Let me just take two examples for this tendency from the last bulletin: In his obituary to Michael Morrow (Comm. 1272) the first four paragraphs read like an obituary to Jeremy Montagu (I have to admit that it is becoming what you would expect after that). Another example is Ephraim's Comm. 1276 (Occam's Razor), a contribution I find good and important for FoMRHIQ. Looking through the last two paragraphs one may wonder who is meant by the scholar who is still knowing and observing this important principle correctly today and who may be the "inferior scholars"? Or am I too suspicious or sensitive?

Th. 1276 in Com 1276

Two full pages on the purest pursuit of scholarship must have been judged editorially as important and currently needful for the Fellows and Members of FoMRHI. The author draws into the discourse equally the arts and the sciences. (As Stein could have asserted: "A scholar is a scholar is a scholar etc".) The picture emerges from the excellent discourse of a scholar as a seeker specifically after WHY. The author might have mentioned that Aristotle noted WHY as an aim but distinguished as well those seekers who were at a lower philosophic (? -al) level and took HOW for their Grail. Aristotle sketched the two contrasting methods of search and he concluded, rather grandly, that only those who studied WHY were fitted to teach. If so, Praxiteles, Phidias, and Archimedes, or even Socrates, might have been DOERS whilst Plato, his school, and their successors through the ages would merit their being called TEACHERS.

Two millennia onward from Aristotle, if I stub my toe, pain results. There is no puzzle concerning HOW it happened. WHY the stub caused pain takes enquiry into a series of hypotheses, like a nest of Russian boxes, all the way from a trauma receptor in the toe to molecular events in the nervous system and, nowadays, beyond and into particle physics and cerebral cortical architectonics. There lies a teacher's Grove of Academe and one might say the grove offers infinite speculation.

How to ease the pain in the toe, how to prevent stubbing of the toe again, and how to discover if the stub has lastingly damaged the structure of the toe, enters enquiry where hypothesis has hardly any part to play. Proof-of-the-pudding science will suffice.

The begetter of Com 1276, Occam, basically was a theologian, and had a bent towards philosophy, according to the reference books. Of course the question strikes one: Did he apply his frugality with entities, the famous Razor, to the Trinity, or was he too politic to do so? Is the Razor applicable to the endeavours of FoMRHI's readers in a wide or a narrow context? As a sequel to Com 1276 may not one or two examples by way of explication be crisper and helpful of the author?

Response to Comm. 1302 on Occam's Razor

I have R. White's permission for simultaneous reply. In Comm. 1302, he follows a modern tradition in being more philosophical and woolly about Occam's Razor than Comm. 1276 is. Occam himself was concerned with avoiding this. He was a theologian, a type of scholar for whom the nature of the evidence, assumed largely to be true, was the holy books and subsequent revelations of the will of God. Otherwise, he was like any other scholar, creating hypotheses searching for truths based on the evidence. The hypotheses included exploring the implications of the evidence when it was clear, explaining its meaning when it was ambiguous, and when one piece of evidence appeared to conflict with another, interpreting them so that the contradiction was removed. Occam was concerned with an objective way of choosing between conflicting hypotheses. His Razor cut far-fetched flights of fancy away from consideration as answers to scholarly questions when there were straightforward ones available that were just as adequate. His was a back-to-basics approach, keeping the hypotheses from getting too far removed from the evidence.

Occam's Razor, at least in the scientific tradition of interpreting it that I was taught in my youth, states that simplicity is preferable to complexity, but only if it performs the specified task (at least) equally as well. The apparent simplicity of an hypothesis in itself without equal effectiveness confers no more truth value on it or philosophical preference than the complexity of a rival.

As shown in Comm. 1276, application of the Razor to each piece of the evidence, when there is suspicion of it being untrue, creates rules of evidence which confer objectivity which is fundamental to the whole scholarly process. So hypotheses cannot be compared and evaluated until each includes explanations for *all* of the relevant evidence. Much published research purports to be scholarship but falls short of scholarly standards of objectivity because it ignores relevant contrary evidence that it makes no attempt to explain. This is all very practical (e.g. see Comms 1098 and 1304). It is a matter of methodology in scholarship, not a debate about philosophical implications.

The application is only to the comparison of hypotheses which are offered as candidates for objective truth. Hypotheses that cannot be related to evidence cannot be candidates for objective truth, and are just speculations. It is called 'objective truth' because it is arrived at as objectively as possible from the truth embodied in the evidence. The nature of truth is largely defined by the nature of the evidence, accepted by all involved. For example, in each branch of pure mathematics, the evidence is a set of axioms and postulates. The acceptance of the evidence is unanimous even though each member of such a set is unproven and ultimately undefined. Most scholarship we hear about has to do with our concept of objective physical reality, past and present. Thus scholarship concerned with history and archeology uses surviving artifacts as the evidence, and current science is scholarship using careful observations and measurements in controlled experiments as the evidence.

There is no distinction between HOW (mechanistic) and WHY (motivational) hypotheses, though the WHY questions that can be answered by relating to evidence are usually answered satisfactorily by HOW hypotheses. Take the question of why there is pain in White's stubbed toe. The evidence he presents of how the pain is generated, transmitted and recognised by his brain are mechanisms that do not satisfactorily answer the question. But there appears to be motivation if we consider how biological organisation can evolve so that pain is associated with physical damage. The evidence here is that abnormal animals that cannot feel pain acquire more physical damage to their bodies than normal animals that can feel pain. They should then have less chance to survive and reproduce, and so natural selection would favour having a pain response. This is a HOW hypothesis that can be a satisfactory response to a WHY question. A WHY hypothesis requires evidence with WHY content.

An example more relevant on these pages concerns the hypothesis that since players and instrument collectors are prone to changing bassoon crooks around, the crook that happens now to be associated with a surviving instrument cannot be taken seriously as historically appropriate for that instrument, even when there is no evidence of historical or physical incompatibility. This hypothesis is not acceptable in scholarship because it violates the rules of evidence deduced from Occam's Razor. It is the crook for the instrument's last proper use unless there is evidence otherwise.

For an Open and Honest FoMRHI - Reply to Comm. 1218

Information management is an essential tool for social control and manipulation. Professionals are recognised as specialists in their fields by others whose trust they need. So I presume that Cronin's 'professional courtesy' is the practice of showing consistent approval of fellow members of one's field to outsiders, keeping quarrels private to maintain customer confidence in the field. I doubt whether this works that way. When the customers don't want to know about the differences between practitioners, the professional who complains about the others is the one who is alienated. When the customers are interested in the differences between practitioners and are willing to ask around, they will find out in spite of 'professional courtesy'. I see no reason for being less open and honest about fellow professionals than one is in normal courtesy towards a fellow human being. That should involve showing full respect for the individual in every respect except when a particular position taken is wrong according to objective criteria or generally accepted morality.

Even when it might be appropriate to be economical with our openness and honesty with outsiders (see Comm 1244), it is important to have a place to freely air our differences and findings, and our Quarterly is ideal for that.

In the fields of historical instrument making and researching, formal training has been very variable and often non-existent. FoMRHI has an important role to play in this training. Training in scholarly methods is generally very sketchy, even at universities. Objectivity is emphasised, but how to overcome one's own biases to achieve this is scarcely mentioned (see Comm. 1276).

Some members (who nowadays would be called 'post-modernist relativists') believe that there is no truth other than what people believe is truth. Other members believe that the truth has been revealed to them, and they are unable to let other possibilities compete with that truth in their minds. For both of these groups, (the cynics and the true believers), the only purpose for research is to marshal evidence around which to concoct plausible arguments to convince others to believe what they want them to believe. For the first group, that is the belief that serves their own interests most, and for the second group, that is to get others to see the light that they have seen.

There is no discrimination against members of these groups in publishing their Comms, but since one of the principles of FoMRHI is to be pro-scholarship, we cannot let Comms that claim to be scholarship but do not meet scholarly standards of objectivity to remain uncommented on (the journals 'of record' are supposed to reject them). Readers who belong to either of these groups are only interested in how effectively a Comm argues for or against their chosen position. Most of the other readers with more open minds care about whether the interpretations have been arrived at with proper scholarly objectivity. They need training to be able to recognise the difference. It is for them that I wrote Comm. 1276, and I explained the scholarly deficiencies in the Haynes version of pitch history in Comm. 1098.

In a civilised society all belief systems should be tolerated, but that does not necessarily mean that they should be treated with equal respect. Some beliefs are considered symptoms of mental illness. The laws in the American bible belt that require schools to give as much time to creationism as they do to evolution are considered a travesty by educated people everywhere. When a belief system impinges on areas that scholarship can make objective choices about how well different theories fit the evidence, the winning choices become respectable amongst educated people and the losers not. Anyone (including scholars) can still believe in the losing theory, but unless and until scholars can turn up new evidence that overturns the objective choice, it remains inferior. And if saying so is insulting to its proponents, what else can we say? Scholarship is all about making choices on the way towards objective truth, and is neither a matter of opinions that deserve equal respect nor a beauty contest of ideas and how they are expressed.

For the record, I did not reject any Comms that contained information about CNC machining that I could imagine would be of any use to someone who is keen on getting into it, or could have persuaded any other instrument maker to get more interested. The one request I did get to publish the rejected Comms, if I remember correctly, was from Cronin. I respect his right to have his position, and (within reason) to express it here, but reserve the right not to be guided by it.

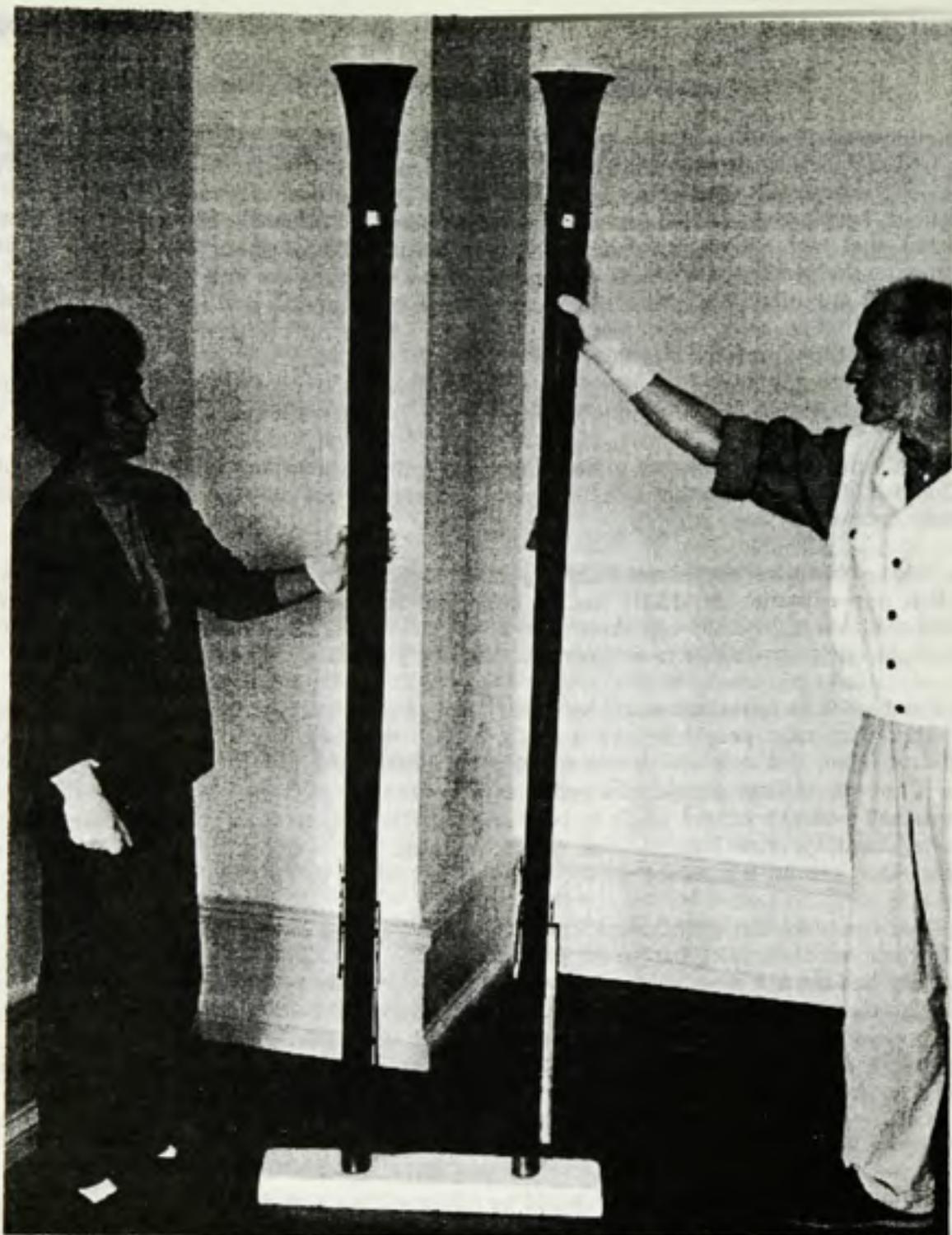


Figure 1. Side view of the Sondershausen instruments.

For the last ten years it has been known that four contrabasses survive, that is three more to add to the Augsburg one depicted in NGDM. (1) These new discoveries were investigated by Herbert Heyde (2) and also by Rainer Weber (3). These instruments have yet to be compared and described in any detail, which is why I went to Schloss Pillnitz, near Dresden, and Sondershausen. It is the purpose of this Comm. to describe and compare all four.

The Augsburg octave bass (Instrument A) is the odd one out, with its three sections glued together. The uppermost has a non-detachable bell, and also carries the up- and down-bores in separate pieces of wood (4). In fact it looks like a one-off with evidence of trial and error on the part of the maker HIERO.S. The Sondershausen ones, instruments B (the black one) and C have detachable bells, otherwise they are of one-piece construction. The Dresden curtal (Instrument D) is in one piece.

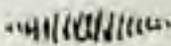
The following table gives some vital statistics:

	A	B	C	D
Maker's mark	HIERO.S.	JB 1681	JB	none?
Location	Augsburg	Sondershausen		Pillnitz
Museum no.	3012	Mu 1	Mu 38	Mö 36
Colour	brown	black	brown	black
Length Overall (mm)	1882	1967	1960	1811.5
Length shoulder to butt (mm)	1595	1661	1675	1656
Bell external diameter (mm)	102	152	145	106
Distance hole 1 to hole 3 (mm)	81.5	76	81	96
Distance hole 3 to hole 4 (mm)	454	435	435	416
Keys	4 (brass)	2 (iron & brass)	2 (iron & brass)	2 (steel)
Weight	8lb 2½oz 3700 g	10lb 9oz 4800 g	7lb 5oz 3175 g	6lb 12oz 3050 g
Diameter of top of down-bore (mm)	10	12.3	10.5	14.5
Diameter of bottom of same (mm)	28	26	27	33

Instruments B and C are both signed JB, apparently Johannes Bohlmann, with the device of a bunch of berries, probably redcurrants (= Johannisbeere). Herbert Heyde (2) quotes from a record which states that he was the Pfeiffenmacher who supplied an OctavBombart to 2 Leipzig churches, however these were delivered in 1671. I don't think this necessarily means extra large bass shawms, but might quite easily refer to octave curtals. It is of interest to note that an ancient label on D describes it as Bombhart. So there may well have been a tradition of referring to contrabassoons as pommers, in much the same way as in England double basses were called bass viols.

Instruments B and C are left-handed. This is actually advantageous when playing with normal hold i.e. lefthand on top. All four possess idiosyncratic features. A and B present "patch it up and try again" areas, but must have been thought to be sufficiently good to merit being branded in both cases over patches where improvements were made. C and D are perfectly made.

A has a distinctively short F key action, the U-bend being "early". Consequently the up-bore holes are all further up and the long bell has a cover to flatten the lowest note. Things went very wrong when JB built instrument B. An up-bore auger seems to have wandered off axis so that when the body was shaped, a panel of wood had to be glued on. The external dimensions are necessarily rather generous, the instrument being much heavier than the more successful C, which he must have built next, to the same pattern as regards length and tone-hole positions (Figure 1). As the bores run true as well as close to one another, it was possible to make this one very thin-walled, and much easier to hold. The other notable difference is the shape of the six fingerholes. C has almost circular ones, while B retains the ellipses that result from their oblique drilling (Figure 2).

Of the four, instrument D is the one I would be most likely to try to smuggle out of the museum. It immediately gives one the impression of being in the mainstream tradition of curtal building. It represents a tour-de-force of boring and shaping, the bell having been lathe-turned on the 6 foot long workpiece of beechwood. It is extremely light and easy to hold. The fingerholes are very rounded, more so even than those on C, in spite of being much more steeply angled (Figures 3 & 4). This is done by over-cutting (the opposite of under-cutting) on the minor axis of the ellipse. Two toneholes, 1 and 9, have brass shields to facilitate closure (Figures 3 & 5). The up-bore holes on D are greatly under-cut, whereas on A, B and C they are not. The Pillnitz instrument is so fine that I feel it must have been signed by the maker, so I looked at the various scratches and superficial damage at likely locations. One scratch differs from the others, in being parallel and not lens-shaped. It is also in the top front typical brand-mark  position. The two close-ups (Figure 6) show this under slightly different lighting conditions. It is deeply impressed, so that some parts of the wood have crumbled off, but it still looks like a row of capital letters: to me it does, but everyone I have shown these pictures to says it is a scratch.

I will gladly send/lend colour photos to anyone who wants to have a go at identification. The bell is fitted with a brass trim; not the first as tooth marks from an earlier one are visible on the inner surface.

I avoided the temptation of playing them; anyway A, B and C have recent crooks and D has none. Herbert Heyde and Rainer Weber disagree on the matter of pitch. I would guess that B and C are a bit flatter than D and A. A clearly belongs to the set of sharp-pitch curtals, and D is likely to have about the same pitch as these. The best, or only, way to find out would be to make close copies, and live with them for some time, trying various combinations of crooks and reeds.

This survey is by no means complete, these being largely qualitative impressions, there being more work to be done on the measurements I was able to make this time round.

Museum staff were very helpful, Herr Wenke at Sondershausen and Dr Jenzen at Pillnitz. I wish to record my thanks to them and to Holle Rohlf who helped with everything: recording measurements, holding things still for photographs, interpreting, and driving the car. The drawings in this Comm. are by Barbara Stanley, Harald Schäfer and myself.

References:

- (1) New Grove Dictionary of Musical Instruments p.182.
- (2) Herbert Heyde *GSM* 1987 pp.24-36.
- (3) Rainer Weber *Oboe Klarinette Fagott* 1991 Heft 2
- (4) Graham Lyndon-Jones *FoMRHIQ Comm.* 1224, Jan 1994

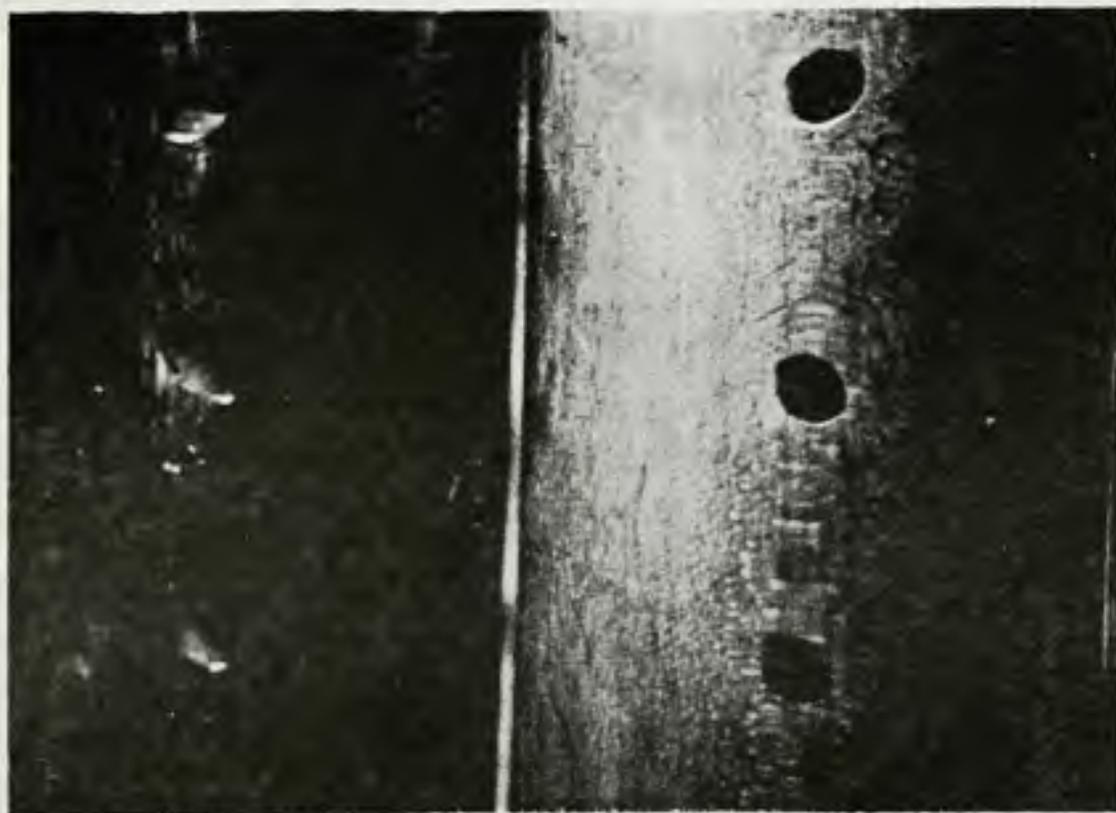


Figure 2. Upper fingerhole group on B and C.



Figure 3. Upper fingerholes of D. Photo: Harald Schäfer.

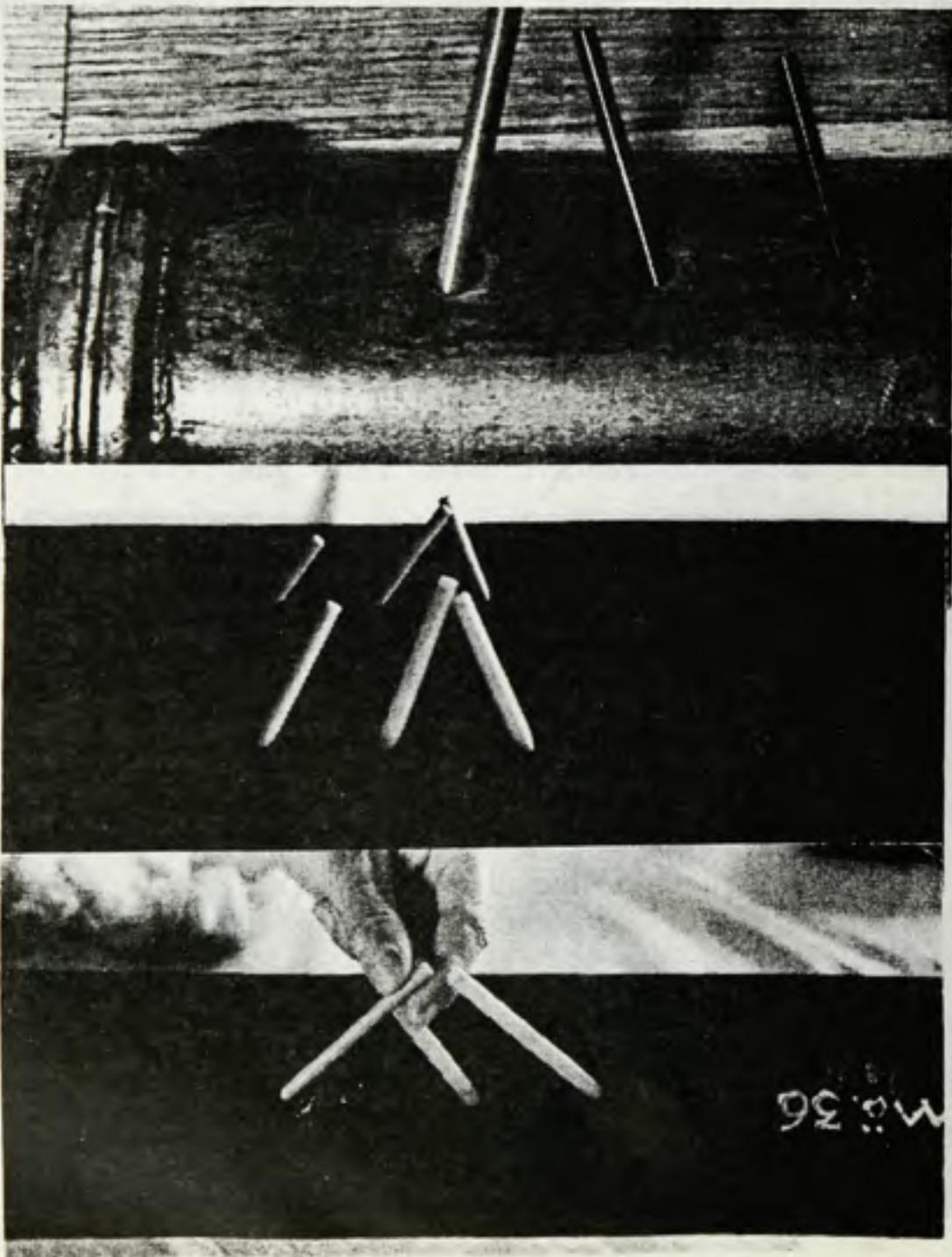


Figure 4. Angles of upper group of fingerholes (A. B. C. D.)

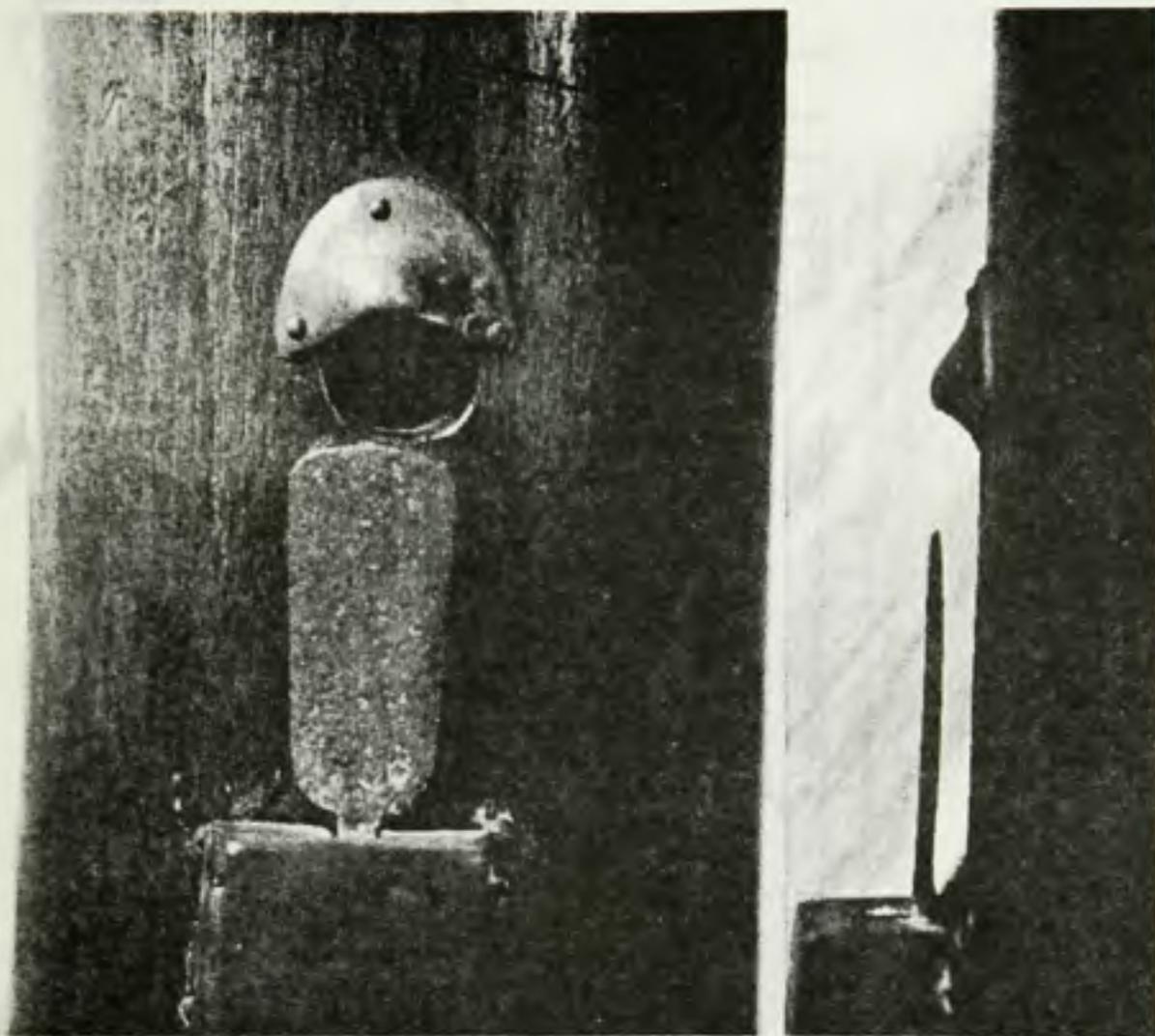


Figure 5. Hole 1 (D).



ht of lettering 5 mm).

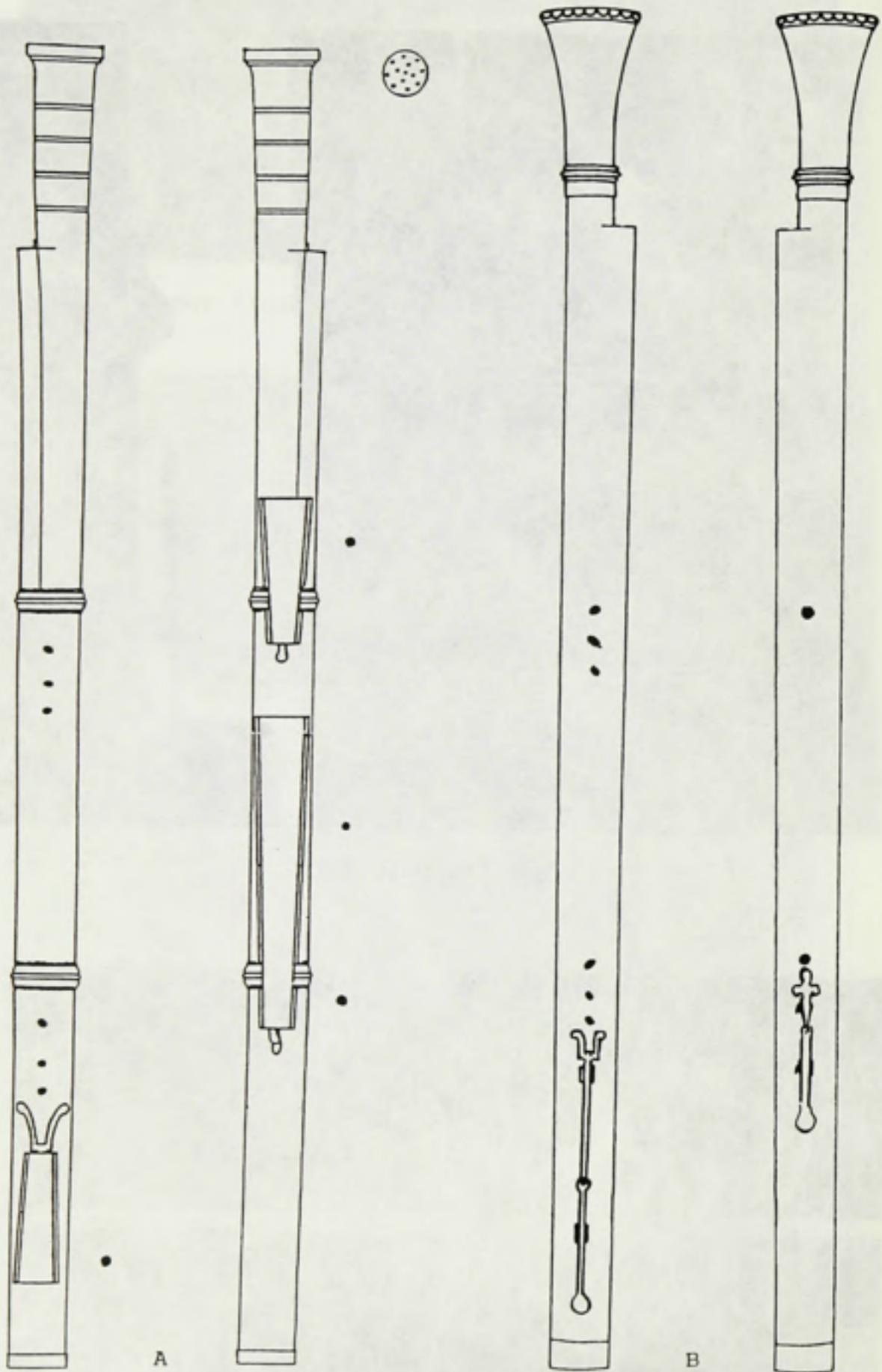
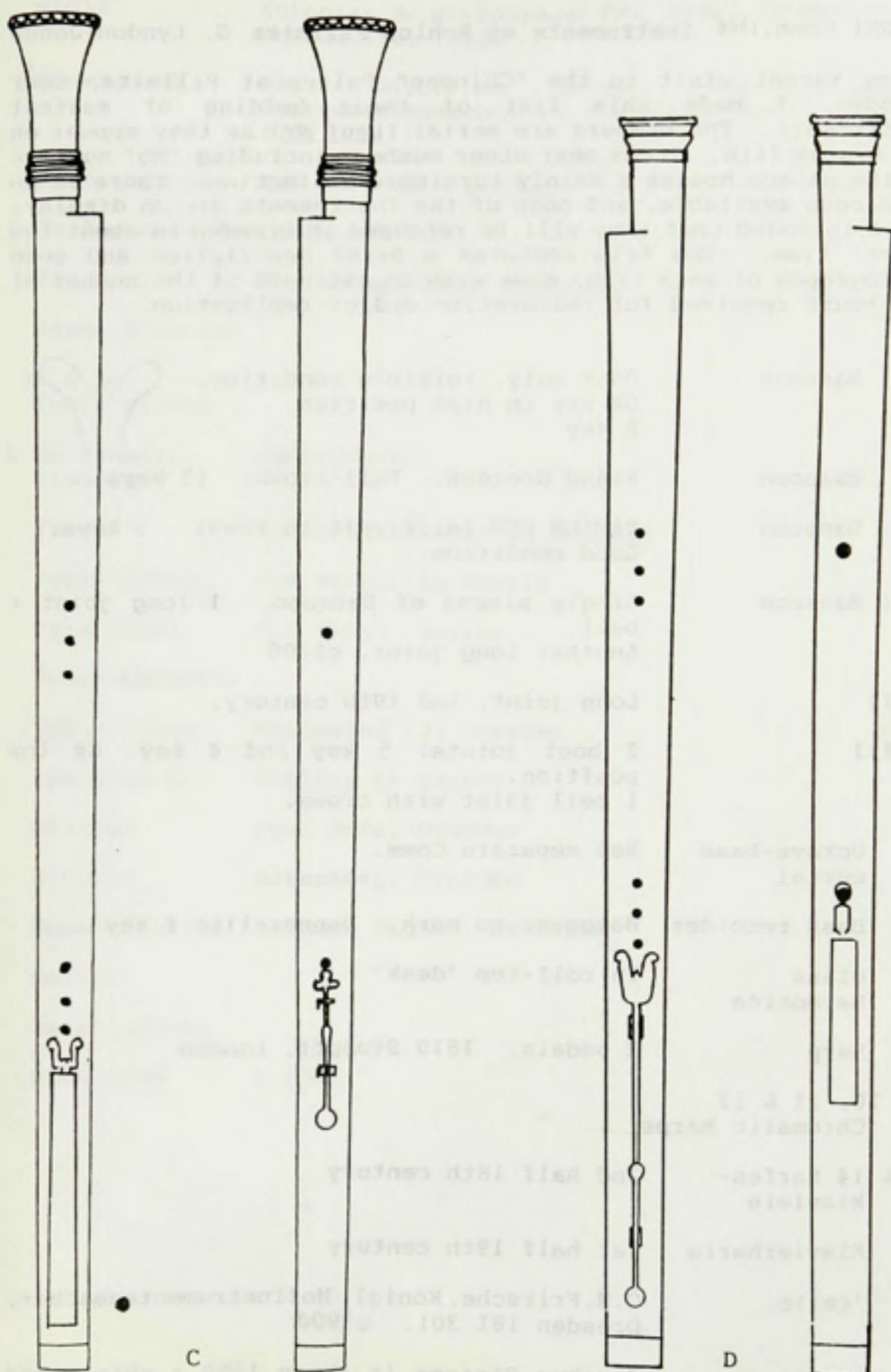


Figure 7.



The four contrabass curtals compared.

FoMRHI Comm. 1306 Instruments at Schloß Pillnitz G. Lyndon-Jones

On my recent visit to the "Chinese" Palace at Pillnitz, near Dresden, I made this list of their holding of musical instruments. The numbers are serial (Lauf Nr) as they appear on the museum file. Items bear other numbers including "Mö" numbers as the palace houses a mainly furniture collection. There is no catalogue available, and none of the instruments are on display. It is intended that they will be rehoused in Dresden in about ten years' time. The file contains a brief description and good photographs of each item, some with an estimate of the number of man-hours required for restoration and/or replication.

- | | | | |
|----------------|-----------------------|---|---|
| 1 | Bassoon | Boat only, terrible condition.
G# key in high position.
F key |  |
| 2 | Bassoon | Finke Dresden. Bell-crown. 13 keys | |
| 3 | Bassoon | TAMICH NEU (difficult to read). 5 keys.
Good condition | |
| 4 I | Bassoon | Single pieces of Bassoon. 1 long joint + bell.
Another long joint, c1700 | |
| II | | Long joint, end 19th century. | |
| III | | 2 boot joints: 5 key and 4 key, G# low position.
1 bell joint with crown. | |
| 5 | Octave-bass
curtal | See separate Comm. | |
| 6 | Bass recorder | Baroque, no mark. Denner-like f key | |
| 7 | Glass
harmonica | In roll-top "desk" | |
| 8 | Harp | 2 pedals. 1810 Stumpff, London | |
| 9, 10, 11 & 12 | Chromatic harps | | |
| 13 & 14 | Harfen-
klaviere | 2nd half 18th century | |
| 15 | Klavierharfe | 1st half 19th century | |
| 16 | 'cello | C.H.Fritsche, Königl. Hofinstrumentenmacher,
Dresden 181 301. c1900 | |
| 17 | 'cello | Jacobus Steiner in Absan 1650 + shield and
crown device. Body only original. | |
| 18 | 'cello | | |

- 19 viola Antonius & Hieronymus Fr. Amati Cremonen,
Andrea fil. F1632
- 20 Clavichord Adam Gottfried Oehme, Orgel- und
Instrumentenmacher in Freiberg, den 15 Mai
1776
- 21 Claviorganum 1st half 18th century
- 22 & 23 2-manual Harpsichords
- 24 Virginal Samuel Bederman, Augsburg
- 25 Hammerklavier
- 26, 27 & 28
Table pianos
- 29 & 30 Hammer- André Stein
flügeln
- 31 Flügel A.Brettschneider in Leipzig
- 32 Table piano F.W.Werner in Döbeln
- 33 Lyraflügel F.A.Klein, Berlin
- 34 Schrankklavier
- 35 Table piano Kühlewind (?) Dresden
- 36 Lyraklavier Schleip in Berlin
- 37 Klavier Carl Marx, Dresden
- 38 Klavier Altenberg, Dresden
- 39 Hammerklavier Solomon Wagner, Nedingen
- 40 Positiv
- 41 Metallophone
- 42 Harmonium c.1900

On Historical Lute String Types and Tensions
(including a response to Comm. 1288)

Historical Background

Late in the 16th century, the open-string range of lutes and some viols expanded from about 2 octaves in the middle of the century to about 2½ octaves. Either the highest strings could then be made stronger so they could go to higher pitches or the lowest strings were modified or used in a way that lower pitches were able to sound acceptably. There is no evidence that hints at any change in the highest string strength, but there is evidence for changes with respect to low strings. The price of 'cattelins' dropped drastically around 1570 (see Comm. 1289), most probably implying that they became much more widely available. In 'Romeo and Juliet', Shakespeare named three servants 'Catlin', 'Soundpost' and 'Rebeck', expecting the members of his audience, most of whom could not afford to have a lute or a viol, to recognise these terms. 'Rebeck' was part of popular culture, but the other two are technical terms which would probably be known about only if they had been in the news, explaining some recent changes in the music they heard.

There was a similar but temporary expansion to about the same open-string range very early in the 16th century. Virdung mentioned 14-string lutes, and Bermudo mentioned that Luis de Guzman (d. 1529) used a 3-octave(!) range on his lute. The Bologna fragment shows a range of 2 octaves and a fourth on a 7-course viola. The Pesaro ms. specified 2 octaves and a third on a lyra da braccio. When Vitali recorded Vincenzo Capirola's compositions, he mentioned the availability of superior strings from Munich which 'give' more (i.e. were more elastic) and did not have thicker and thinner ends (i.e. were uniform). Capirola apparently didn't use them on his 6-course lute since Vitali described his way of mounting strings that were thicker on one end than the other so that they would still be in tune while being fretted.

String Technology Background

There is a limit of how little focus in a bass string's sound is acceptable in a music culture. Focus depends on the amount of higher harmonics in the sound. A bass range expansion results from a change that allows that limit to be reached at a lower pitch. The amount of higher harmonics in the sound depends on the string's elasticity (or 'stretchiness') when tuned up. Elasticity of a uniform string depends on diameter and type of string construction.

Gut strings are made from bundles of ribbons twisted together, each ribbon being a piece of the layer from the wall of a sheep's gut, made up of microscopic collagen fibres. A low-twist gut string, used for high-pitched strings on an instrument, has a bundle of these ribbons twisted just enough to form a cylindrically-shaped string. This maintains maximum strength. The amount of twist can be varied, the higher the twist the lower the strength but the higher the elasticity. A high-twist string has the maximum twist it can take, and so the maximum elasticity for a single-bundle construction. In rope construction, the bundle of ribbons is split into 2 or more bundles, each of which is first twisted on its own and then the bundles are twisted together. This construction has greater elasticity than high-twist strings. The surface bumps characteristic of ropes can easily be polished smooth to feel like the other strings and look like them without close examination. A loaded string is a high-twist string where the ribbons are covered with a slurry or paste of a heavy metal and/or metal compound before being twisted together.

7 Early sources did not distinguish between low and high twist, so it is likely that they automatically put more twist into a single-bundle string the thicker it was (this would happen automatically if they twisted the same number of turns). Because rope construction is innately more elastic than high twist, strings can be thicker and heavier, and thus go to a lower pitch (at the same tension) before reaching the limit. Loaded strings have about the same elasticity as unloaded strings of the same diameter, so the diameter limit is about the same as for high twist. The pitch is lower because of the added weight of the loading material.

The same lower pitch can be reached and still stay within the limit by tuning a plain high-twist string of maximum acceptable diameter down to that pitch. The resulting lower tension reduces the

sound output is produced with the same plucking displacement at the same plucking position. Sound output from a string depends on the product of the force multiplied by the string displacement at the time of release in the pluck. Most (if not all) of this output can be restored by plucking the string closer to the bridge. This increases the loudness of the plucking noise before the string tone comes through, giving a 'thunkier' sound to the note.

Modern aesthetics considers a pronounced transient noise before the resonant note comes through to be ugly in most string playing. The ideal is a purity of tone with minimum interruption by noises such as these and consonants in singing. The ideal note production, both instrumental and vocal, in the Renaissance and French baroque, was generally quite the opposite (like saying words with exaggerated consonants for maximum clarity, like an orator does), as I showed in Comm. 862. So modern lute players generally play largely over the rose, while the iconography and little-finger wear marks on surviving soundboards indicate playing was generally considerably closer to the bridge. Consequently, tension levels considered necessary for adequate tone production by modern lute players has no value in an historical investigation.

String Types and History

The possibilities of how each of the two range expansions happened are the availability of roped strings, the availability of loaded strings, or a change of technique to play closer to the bridge on strings of lower tension. Vitale's mention of 'give' favours the roped choice early in the 16th century. A combination of roped basses and plucking lower basses closer to the bridge could explain Guzman's 3 octaves. In Comm 1289 (most of which is in Comm. 94) I presented a reasonable scenario suggesting that a Spanish monopoly later made these strings available only to the very rich, that they acquired the name 'cattelin' largely because the trickle of supply then was from Barcelona, and that later they became available again after the Spanish monopoly broke down and production started probably in Italy.

If musicians had a choice between roped and loaded strings when string types were named, their properties differ enough to have different names. In his string market survey in *A Varietie...* Dowland wrote that bass strings made in Nuremberg and Strassburg were excellent when new, but the best were 'Venice Cattines'. The differences expressed here are smaller than we would expect between roped and loaded strings. Thus they were all either roped or loaded. If they were loaded, the name 'cattine' was associated with that type of string since the days of the Spanish monopoly. Thin bass strings are certainly indicated by the narrowness of the neck (43 mm) on the only surviving (and enormous) 16th century vihuela in Paris. Consequently either loaded strings replaced roped strings in the days of the Spanish monopoly or Vitale's Munich strings were more elastic only because they were thinner (since they were loaded). But the elasticity of loaded strings is about the same as of ordinary high twist strings of the same diameter, so that would not be the obvious parameter that Vitale would mention to express what is special about them. The only viable possibility involving loaded strings that remains is that they replaced roped strings after Vitale's time.

Two-thirds of a century after Dowland's string market survey, Mace published a similar one. His recommendations amount to using 'Minikins' for the highest octave of strings, 'Venice-Cattins' for the next octave down, and 'Lyons' or 'Pistoy Basses' for the lower still 'Great Basses'. When discussing how they were sold, he wrote: 'Both [Minikins and Venice-Cattins] which, are (generally) at the same Price, and the signs of Goodness, both the same; which are, first the Clearness of the String to the Eye, the Smoothness, and Stiffness to the Finger, and if they have Those two qualities, dispute their Goodness no further.' It is likely cattines were always smooth (polished if roped). Mace mentioned that Pistoy Basses 'are indeed the very Best, for the Basses, being smooth and well-twisted Strings' Lyons (the name obviously implying that they were French) were made in various sizes, and available from rope makers according to Mersenne 4 decades earlier. Mersenne wrote that 'the ropemakers sometimes twist the strings more in one place than another'. Such control occurs in making ropes, but not in twisting single-bundle strings. According to Mace, they were less smooth than the other types. Lyons were most likely of rope construction, unpolished.

The smooth types that Mace preferred could have been loaded or polished roped strings. The thinner Lyons 'which many use, for the Octaves [that Venice-Cattins were used for]; But I care not for Them, they being constantly Rotten, and good for little, but to make Frets of.' This seems to be a statement of physical condition (e.g. poor strength or twisting) and not one that implies that

Venice-Catlines were acoustically particularly different, as we would expect if Venice-Catlines were loaded. The case for Pistoy Basses being loaded is much stronger, especially since their 'deep dark red colour' is that of some heavy metal compounds commonly available then.

Mace's statement about the 'Clearness' of Venice-Catlines (mentioned above) supports the interpretation of the Dowland quote I gave in Comm. 1255: 'This choosing of strings is not alone for Trebles [first course], but also for small [second course] and great [third course] Meanes: greater strings [fourth course and lower] though they beould are better to be bourne withall, so the colour be good, but if they be fresh and new they will be cleer against the light, though their colour be blackish.' Peruffo has pointed out in Comm. 1288 that the part of this passage after the colon could be interpreted as referring to the 'small and great Meanes' mentioned previously, and not to lower strings (assumed in the brackets above). He feels that the punctuation, and Dowland's consistent use of Capital initial letters for 'different registers', supports his interpretation.

The relevant punctuation is the colon. Elsewhere, Dowland did use the colon so that what followed it was to clarify, explain or particularise what was before it, but he also used it so that what was after it was to continue similarly to, or contrast with, what was before it. Thus the punctuation does not prefer one interpretation over the other. Dowland's use of Capitals here is completely parallel to his later statement 'For the greater sorts or Base strings, some are made at Nurenburge...'

In the iconography we usually see the second course as light as the first and the third not much darker. This contradicts Peruffo's interpretation which would make them both dark. That interpretation is therefore quite unlikely, and Dowland's statement agrees with Mace's, and Venice Catlines had a degree of translucency when fresh. Their being loaded is historically ruled out unless they can have this optical property. I doubt whether that is possible with historical materials.

Whether loaded strings were ever made and used before Peruffo has recently produced them may never be known from direct evidence. None have survived (neither has any roped string), but that is not evidence against their availability and use. That is because gut strings not in use lose strength after time, and they are not attractive enough or unusual enough to want to keep once they can't be used. So we have no reason to expect any original gut strings to survive even though many instruments that used them have.

Bass String Tensions on Lutes

There has been a problem about the thickness of lute bass strings since we at NRI first produced roped strings as a solution to the previously unsolved problem of making all-gut bass strings that worked. We called them 'catlines', ignoring the difference between whether they were polished or not. It hurt to polish away all that expensive gut (we've only recently found a way of drastically reducing that waste). In hindsight, we probably should have called them 'Lyons' then. We took Mersenne at his word about equal tension and designed sets of lute strings at the 2½ - 3 Kg. tensions lute players found worked with nylon strings.

Most lute players and makers were upset by the thickness of the low basses, saying that this couldn't be right because the paintings didn't show such thick basses, and original bridges they had seen didn't show such big holes. Over 15 years ago, I was allowed to rummage in the basement of the Smithsonian Institute in Washington D. C., where there were some lutes in various states of disrepair. There I found old bridges with holes of the sizes Peruffo reports in Comm. 1288, and a few with rather larger bass-string holes that our equal-tension all-gut sets could fit into. I wondered whether hole size could be a criterion for determining whether a bridge is original.

The collection of evidence on bridge-hole diameters in Comm 1288 is compelling. It demands the objective comparison of hypotheses to come to a scholarly conclusion. Peruffo's hypothesis is that the strings were loaded and tensions were what modern players are happy with. I once considered the loaded string possibility (see Comm.773) and rejected it. The above analysis shows how historically improbable it is. The only remaining hypothesis is rope construction. The conclusion then is that early bass string tensions on lutes were lower than we have assumed they were.

If we assume that the string diameters for calculations are 85% of the hole diameters reported by Peruffo, that the highest string is tuned a tone below breaking pitch, and that the instrument tunings

were as I have assumed, then the lowest string tension of the strings measured on each instrument, calculated by the method of Comm. 1255 in Kg. follow: With Renaissance tuning I get, 2.0 for the one with 6 courses, 1.6-2.0 (average 1.8) for the three with 7 courses, 1.6-1.8 (average 1.7) for the two with 8 courses, 1.3 for the one with 10 courses, 1.1 for the one archlute with 11 courses and 1.6-1.7 (average 1.6) for the three archlutes with 14 courses. With d-minor baroque tuning, I get 1.1-1.6 (average 1.5) for the eight with 11 courses and 1.0-1.5 (average 1.2) for the two with 13 courses. If an estimated string diameter is a certain percent different from what it really was, then the calculated tension would be in error by about twice that percent.

Tension Profiles

In the above evidence, there is a tendency for lower tension low strings on lutes with more courses, but no correlation with lute size. Dowland would lead us to expect a correlation with size for corresponding strings. The differences could be masked by tension variability from instrument to instrument or from string to string on the same instrument. The latter questions the validity of the assumption of equal tension across the strings of lutes. If that principle applied strictly, it would lead to first string diameters from .24-.32 mm. (9.6-12.5 thou) on the lutes measured. It is questionable whether gut strings that thin were available at that time:

The English tradition, according to Dowland and Mace, apparently was to use German high strings (Minikens) and mostly Italian or French low strings (Venice-Catlins or Lyons). The French tradition, according to Mersenne and the French lute teacher of Mary Burwell, was to use Italian high strings (Roman) and France low strings (Lyons). We have no information about the manufacture of German high strings, but we have a bit of information about Italian ones:

In Comm. 325, from 19th century information given by Maugin and Maigne, I calculated that the violin first string described there had a diameter of .594 mm. (cross-sectional area of .277 mm²), and it was stated to be composed of 4-6 half guts or 3-4 very thin whole guts. The thin whole guts are relevant here since Peruffo tells me that 17th century Roman statutes governing the selling of strings indicate that firsts were made of 2 or 3 guts and they could not be split.* The cross-sectional area is proportional to the number of guts if there is the same degree of twist. The diameter of 3 of the thinnest would be .51 mm. (20 thou), and of 2 of the thickest would be .48 mm. (19 thou). These probably represent an average of the smallest string diameters sold, with the smallest size (2 of the thinnest of the very thin guts) having a diameter of .42 mm. (17 thou).

Thus lute first strings had higher tension than the lowest ones. This would be expected when lutes had a single first course, with most of the others doubled. But strings for the second course couldn't be any thinner either. Using the formula in Comm. 1255 (with the first course tuned a tone below breaking pitch), a .48 mm. (19 thou) second-course string on a lute would have a tension of 2.6 Kg in Renaissance tuning and 3.2 Kg. in d-minor baroque tuning. The single second of the latter should be at a higher tension than each string of a lower doubled course. This calculation indicates that there were higher tensions on the highest strings than on the lowest basses, and that modern lute players got the tensions on the highest strings generally right.

Dowland's instructions for mounting strings imply 'equal feel'. This probably means that the same plucking force causes the same string displacement. This implies equal tension only if the plucking distance from the bridge stays the same on all strings. When tensions varies, equal feel requires varying plucking distances. Most early lute players apparently preferred the percussive effect of playing low basses closer to the bridge and/or didn't like the sound persistence of very thick strings.

* * * * *

*A string made from a single gut is not strong enough since it needs too high a twist to be cylindrical. The restriction to whole guts could be because the small variations of weight along the length of a gut string effects trueness more seriously the thinner the string is. In our experience with hand-polishing untrue strings to trueness, this is much more difficult with thin strings than thick ones (modern machine polishing with centreless grinders provide a level of uniformity never available before). The natural uniformity of whole gut ribbons was probably greater than could be achieved by splitting them. The gut starts as a tube, and for all musical strings, it is made into a ribbon by a cut opening it up for scraping clean inside and out. Then splitting can happen. This cut is not made when gut is prepared for sausage skins, where cleaning standards are lower.

Strings and their Names

Eph Segerman writes (Comm. 1289) that "an appropriate transformation [of cat-rope] would be 'cat line'". This I believe has been termed the conditional subjunctive form of argument, which can be elaborated in favour of any proposition, however fanciful. It is not evidence. Cat-c(h)ord would be more *appropriate*. It is perhaps significant that dictionaries often define "catling" as a *small* lute or fiddle string, never as a large one, as they commonly are nowadays. The many citations in the OED do not show a single spelling as "...line": there is not a shred of evidence to support any connection with mariner's cordage.

I have not seen the Capirola lute book, but "expecting" that catlings were "probably" made in Munich does not justify a positive assertion that "the name 'mynekin' means Munich" (München). Has Eph any other evidence for it? or that "bressell" means Brussels/Bruxelles? Derivations based only on some similarity to an English word (cf Geneva, journeyman, boot and saddle, slug-horn) do not warrant such positive assertions. "Minikin" has a long entry in the OED with many cited examples from 1541 onwards. There is nothing in it to suggest anything to do with Munich (and the long and well documented entry on "catling" makes no reference to Catalonia). Perhaps the OED is wrong, but anyone challenging their etymology needs to justify the challenge on better grounds than are apparent here. We badly need more facts about historical strings. Will not someone, a good linguist, with easy access to the major dictionaries of Europe, make a start with a solid comparative study of the terminology in various languages? Do (for example) the terms Lyons (for gut strings) and Pistoys occur elsewhere than in Mace? I never heard that they do, but I may well be simply ill-informed.

On a detail of Comm. 1288, p. 73; it is unsafe to assume that Talbot had no knowledge of overspun strings. When I examined the original mss. some years ago I found a partly-illegible note on an odd leaf, which ran as follows, the parts in square brackets being uncertain: "in low[est] Basses [mixed] with Copper or [Silver] Wire in lowest [...] of Bass Violin or Viol". This note seems to have escaped the editors of the printed texts.

Mace (pp. 65-6) lists "*Minikins, Venice-Catlines, Lyons (for Basses) and Pistoy Basses*, which I conceive [a hint of uncertainty here; did he not know for sure?] are none other than *Thick Venice-Catlines... Smooth and well-twisted...*" (this last could mean either highly twisted or skilfully twisted). He adds that Pistoy basses were not readily obtainable in England and that his readers would often have to be content with Lyons for their basses. Later he writes of "a *small sort of Lyons*" which are of no use except for fretting.

Minikin is a dictionary word and the accepted derivation and meaning of "a little one" make sense. Catlin is also a dictionary word. The place names may be those of manufacture or marketing only. But they *may* denote methods of fabrication. If they do, and "Lyons" strings were all of the same type, it seems to me that Lyons basses must have been plain gut, for it is hard to believe that any of the other proposed fabrications would make for satisfactory frets; and that would imply that very many players in England in 1676 used plain gut for basses.

But it may be that the place names do not denote a particular method of fabrication. If so, any argument which turns on the association of place-name and type is suspect. Mace would be using the terms used by the vendors of strings, and there was no Sale of Goods legislation in those days. I can buy Cheddar cheese from half-a-dozen different countries.

"What I should hope to teach" said Howard Mayer Brown in 1993 "is how little we know and how difficult it is to know it. One should be exceedingly sceptical about whether we know anything and how we know it and of the way dogmas arise".

On Melodic and Continuo Bowing of Early Fiddles and Viols

We perceive mysteries about the playing of early fiddles and viols because we expect similarities with the melodic playing of these instruments that is fashionable today, and the evidence does not support that expectation. The similarities with the modern instruments are there, but they are mainly in the function of continuo playing, a function we somehow forget because so few bowed-instrument players do it today. Chordal playing on viols has been common throughout their history, from before Ganassi to the French baroque masters, and it has often been done on the violin family (the famous 19th century cellist Robert Lindley did continuo for the arias of Don Giovanni).

Continuo playing can be defined as providing an environment of other notes for one or more melodic lines, without that environment being fully specified in the music. The simplest continuo is a drone (plus perhaps a fifth), as one plays on the bagpipe or hurdy-gurdy. This practice goes back to medieval times. The only possible way of playing medieval bowed instruments with low flat bridges and wide bodies is to play all strings simultaneously. This provides a drone environment for a melody either performed by the voice or another instrument or played on the fingerboard amongst the drones. Instead of the melody, a counter-melody could be played instead amongst the drones. If the melody or counter-melody is confined to one or two strings, it can be enhanced by these strings having higher tension. The drone environment could have constant pitches, oscillating between two (or more) chords or otherwise moving in time, and it could be with or without rhythmic interest.

Parameters Affecting Bowing and Early Fiddles

The geometry of bowing was discussed in Comms 714 and 715, Q 43 (1986). In the second of these Comms, I presented evidence for a 3° clearance angle per string at the bridge of a lira da braccio and that the bit of surviving repertoire for the instrument involves playing a minimum of three strings all of the time. Other evidence indicates that a minimum clearance angle at the bridge of about 8° per string was needed for playing each string individually on a Renaissance viol. It seems that about 8° or 9° divided by the clearance angle per string at the bridge gives the number of strings bowed simultaneously under the bowing conditions of these instruments. These conditions apparently included equal tension for the strings and equal clearance angle per string. In the baroque, they apparently also included stronger bowing and isolated playing of inner strings at higher positions since Simpson's bridge offered a 13° clearance angle per string.

The clearance angle at the bridge is what is readily measurable in some carefully executed illustrations that clearly show it, but what really matters is the clearance angle at the bowing point while bowing. This is always less than the clearance angle at the bridge because the shallow concave angles made on the string and on the hair by the bowing force between them both conspire to reduce it. The bowing force and the tensions in the string and hair are involved here, as well as how close to the bridge that the bowing point is. Bowing at 1/8 the distance from the bridge to the nut is typical for playing a single string, and if one bows further up the string at the 1/3 position, with everything else being equal, the bow depresses the string twice as much. If the clearance provided by the bridge curvature is enough for playing a single string when bowing at the 1/8 position, it would allow playing 3 strings at the 1/3 position.

If a string has higher tension than its neighbour on an instrument, it is depressed less by the bow pressure. It then sticks out, acquiring a larger clearance angle. Thus, if one wants equal clearance angle for them in the bowing position, one gives it a smaller clearance angle than the neighbour at the bridge. That is why bridge clearance angles on the modern violin decrease as one goes from bass to treble strings.

Bowing much further from the bridge than at the 1/8 position, which is so often seen in medieval and early Renaissance illustrations, produces less sound volume and there are fewer higher harmonics resulting in lower projection of the sound. If one wants to restore some of this deficiency, one would have to bow faster with more bowing force, which further increases the number of strings bowed. It thus is most likely that bowing closer to the middle of the string than the bridge on an instrument set up for playing individual notes when bowed near the bridge, was largely for the purpose of bowing several (if not all) of the strings at one time.

There are various aspects of bowing near the strings ~~whose centre lines are unimportant to us~~. Bowing faster means that if one wanted continuous drones (as could be provided by a symphony), longer bows would reduce the number of changes in bowing direction. Shorter bows could imply that droning was not continuous and the bow was lifted after each stroke, letting the strings ring on their own before the next stroke.

One often sees the bow being held at a non-perpendicular angle to the strings. What is necessary to properly sound a string with a bow is for the hair to move perpendicular to the strings wherever they touch. If the bow is at an angle, the arm moves it perpendicular to the string (not along the length of the bow), resulting in a changing distance from the bridge during the stroke.

Many medieval fiddles had a 'bourdon' string that went over the bridge just like the other strings, but then missed the fingerboard and went over a knob on the side of the peghead, after which it went through a hole in the peghead to a tuning peg amongst the others. An octave pair of such bourdons was very common on the lira da braccio. The illustrations usually show the thumb of the player's fingering hand touching the bourdon (or bourdons). That could be for the purpose of plucking, of damping (to avoid discord) or of stopping in mid-air (like Agricola said Polnische Geigen were stopped, and how the Indian sarangi and Moroccan rebab currently are stopped). It is likely that the thumb did all of these at different times.

There is no clearance angle at all for an inner string when one bows very near a flat bridge when the string surfaces available for contact with the bow are accurately in a plane. Then the bow will not 'bite' and the string will not sound. The first and last string (the ones that have a clearance angle) will be the ones that sound. When bowing at the 1/8 position from the bridge, the end strings depress enough to give enough clearance angle for the inner strings to just sound. The end strings still dominate. When bowing much further from the bridge, the available clearance angle is distributed much more evenly amongst the strings. Thus if smoothing out the contributions of the different strings were the objective, bowing far from the bridge would be favoured for continuo bowing on flat-bridged instruments.

If the bridge is high enough so that the bow can be tilted to favour the higher strings, one can play closer to the bridge and play tunes on the first string that stand out amongst the neighbouring continuo strings. A slightly curved bridge (an example was given in Comm. 715 with a 1½° clearance angle per string at the bridge) allows bowing all of the strings (at equal tension) with equal loudness near the bridge. A bridge could appear quite flat but this slight curvature could be given to the tops of the strings just by carefully varying notch depths.

In the bowing region, bridge height (plus arching height if there is an arch) interacts with the the difference between soundboard width and bridge width to determine the total clearance angle for all of the strings. This is the sum of the clearance angles for each string. Narrowing the body or raising the bridge increases the total clearance angle. That could be distributed amongst the strings when either there is a rounded bridge or a flat bridge bowed away from it, allowing stronger continuo bowing. The increased clearance angle could otherwise be concentrated on the first and last string with a flat bridge when bowed closer to it.

A waist cut-out in the body (usually with sharp corners) at the bowing position apparently first appeared amongst early- or mid-15th century Spanish fiddles. It combined the warmer sound (lower-frequency resonances) of a wider soundboard with the greater total clearance angle of a narrow soundboard. In Woodfield's plate 27 (*The Early History of the Viol*, 1984) the bridge is close to the cut-out for single-string playing either of end strings on a flat bridge or all strings on a rounded bridge. In Woodfield's plate 28 the bridge is further away from the cut-out, allowing stronger continuo playing than otherwise possible with that bridge height. It is quite likely that in many cases, the same instrument could be used either way by just moving the bridge.

Shifting or switching bridges for changing between melodic and continuo functions seems to have been common in the 15th century. One type of fiddle had two soundboards. One continuous with the fingerboard was flat, and the other one below was arched. There was a step between them. The body shape of the bottom end below the step was rounded, like that part of what we call a 'rebec', while that of the upper end was rather rectangular, like that part of some wide fiddles. We see this instrument either with a high round bridge on the rounded lower end below the step, or a low flat

bridge

The instrument mentioned above with a $1\frac{1}{4}^\circ$ clearance angle per string (to play continuo on all strings near the bridge) has a very unnecessarily high bridge for this purpose, and there was most likely an alternative rounded bridge for playing individual strings on it.

There are some pictures showing a second bridge half-way between the normal bridge and the nut. They have been used by some to show how silly medieval artists could get. This setup could simply be a way of playing constant drone continuo, with interesting acoustical interplay (possibly a thobbing) between the bowed half of each string and the sympathetically-driven other half. Fingering the unbowed half could vary that interplay. Such an instrument could be considered to be a bowed version of a string drum (one can see a fiddle set up like this but plucked in Woodfield's plate 17).

We usually can tell from the geometry of a fiddle illustration whether playing single strings with a bow is impossible, but there is no situation where more than one string is available but bowed continuo is impossible. The 2-stringed Moorish rebec seems to be set up ideally for melodic playing. The two strings are a 5th apart, the soundboard is particularly narrow, and the bridge is not particularly short. Yet the traditional style of playing on the instrument was (and still is) to provide drones (with a countermelody often played on one string) to accompany singing.

Continuo and the Development of Viols

By the second half of the 15th century, consonant chords included thirds as well as fifths, and such chords are more easily playable on a tuning in fourths, like the lute, than on the previously usual fiddle tuning in fifths. Lutes then usually had 9 strings in 5 courses. The Spanish, in the region of Valencia, modified the wide fiddle to perform the functions of either the lute or the fiddle. Since it was probably invented by Moorish 2-string rebab players, when it was bowed, it was played downwards, like the rebab. Melodies could possibly be bowed on the first and last strings, which would be tuned a fifth apart if the octave string of the 5th course was the last string. It had a wide body with waist cutouts to make it narrow in the position of bowing. The bridge was flat and low in height, often attached to the soundboard, like on a lute. The bridge was also positioned low on the soundboard, making the bowing at about the $1/3$ position. It was the first type of viol. When bowed, this was obviously primarily a continuo instrument, with the cut-out function primarily to allow stronger bowing than normally allowed by the low bridge. Whether the end strings could be bowed singly is very unlikely, but they probably could be emphasized by bow tilting or higher tension.

There is one painting of a viol before 1500 (c.1470-80, plate 38 in Woodfield's 1984 book) that was set up for playing individual strings, like later viols. It has a high rounded bridge, further up the soundboard than usual (close to the waist cutout), and a raised fingerboard. Nevertheless, the bow is playing much further from the bridge than the waist, at about the $1/3$ position, indicating that continuo playing was being depicted.

The minority of early fiddles and viols that were set up to be able to play isolated melodies seem mostly to be depicted playing in an alternative continuo mode. Both modes were obviously used (perhaps in the same performance), and we should consider that the continuo mode served the purpose of the painting more closely. That purpose usually was to praise the glory of God, and this could imply that the continuo mode of performance was considered more forceful or dramatic. This could also be the reason why the plucking of fiddles was so rarely depicted. They are most suitable for playing in this way, and they probably were quite often. The plucking of early viols was depicted more often, probably because by then, the lute had made plucking much more respectable and emotionally meaningful.

The 15th century Spanish viol continued in Germany into the 16th century. Virdung (1511) depicted it with the original 9 strings and an attached low bridge. Later, Agricola (1528) illustrated 4 sizes of 4-string 'grossen Geigen' and 4 sizes of 3-string 'kleine Geigen with frets', all of the same design as Virdung's. In his 1545 edition, we don't see the kleine Geigen with frets any more, but the large-viol family (with different tunings and the bass now with 5 strings), was called 'grossen welschen Geigen', illustrated with the same plate as before.

In all arts, clarity is essential to communicate the expected since that makes it more interesting. For instance, since what is happening at cadences was clear, there was great freedom of embellishment at these places in the music. Similarly, when the chord sequence was clear, instruments such as the cittern and the guitar could play continuo without the bass notes of the chords being in the right octave. Tutors for these instruments in the 16th century mentioned that some chords (e.g. second inversion ones) sounded acceptably though they were frowned on by the theorists. This surely would have applied to continuo viols as well.

Before the 15th century, the only evidence of polyphony being played on instruments or a single instrument that I know of was in private as an aid to composition. Their functions seem to have been to play tunes, continuo for tunes, or both. This changed during the second half of that century. Amongst the most popular instrumental performers were lute duos, largely playing 3-part polyphony, with one lute playing a highly decorated version of the discantus with a quill, and the other lute playing the tenor and contratenor parts either with the fingers or a quill. The latter player was called a 'tenorista'. Playing vocal polyphony on instruments had become fashionable.

Also newly fashionable was the bass voice, called then 'contratenor in bassus'. Probably the first instrument to be designed to play a vocal bass part at its proper pitch, appeared late in the century. It was a double-sized viol, with a string stop of about a meter, set up to play individual notes. The original size, set up the same way could play the discantus part. Intermediate sizes were made to play the tenor and altus (or contratenor). Imitating the voice was their primary function, so they could be called 'vocal' viols. The new large instruments had attractive sounds, and continuo viols (such as those Agricola depicted) adopted these sizes as well.

With viol (or lute) tuning, no more than 4 strings are needed to cover a vocal range. Yet, vocal viols had 5 or 6 strings (Agricola mentioned these in the 1528 version but not in the one of 1545). Instructions for transcription of the vocal music for viols indicated that the high strings were the ones used, leaving the lowest strings with no apparent vocal function. With the types of strings then generally available, the low strings would have had a rather rough non-vocal tone quality. One possibility of how they were used is to use them for chords, as on a lute, whenever it seemed appropriate in the music, most likely at cadences. Since these viols were set up for playing individual strings, the notes of the chord could either have been played by arpeggiation at the usual bowing position, or simultaneously by bowing further from the bridge. Another way of bowing them simultaneously was described by Ganassi:

Ganassi (1542) published a 3-part madrigal to be performed with a singer accompanying himself on a viol playing the two lower lines. He also mentioned using a long bow with somewhat slack hair for doing this with pieces in 4 or 5 parts. Paintings of the 16th century that depict the shapes of viol bridges generally show them with shallower curvature across the top than we might expect (a clearance angle per string of about 9°, while in the baroque it could go up to 13°). Ganassi wrote that normal viols with not too much curvature of the fingerboard, bridge and tailpiece were used for playing 4 or 5 part music, and the slack long bow used could be tightened with the finger when one wants to play fewer strings (even down to one). He said that it was permissible to add notes or subtract them from a composition when adapting it to the viol, and a different adaptation would be needed when playing it on the bowed bass instrument he called 'the seven-stringed Lyra', presumably because of a different tuning. These adaptations would mainly involve playing the chords, with those additional notes taken from the faster-moving polyphonic parts that one can manage.

The 'seven-stringed Lyra' probably was the precursor to the lira da gamba or lirone, the usual exclusively continuo bowed instrument in Italy during the second half of the 16th and first half of the 17th century. The lira da gamba was called 'lyra' in the 17th century since the lira da braccio was not played any more so there was no ambiguity in just using that term. It is likely that the term (spelled with an l or a y) was always associated with continuo playing in Europe.

Tractotius called the English lyra viol (a new instrument with sympathetic strings in the second decade of the 17th century) a viola bastarda. This implies that they were used similarly. This is supported by most of his viola bastarda tunings being the same as known lyra viol tunings. We know that the lyra viol was largely a chordal instrument, yet the surviving viola bastarda repertoire involves only divisions in mensural notation. Since we can infer from Ganassi that chordal playing of viols was an old tradition in Italy, we can presume that the viola bastarda was also played chordally,

but then could be that the viola bastarda players read from the notes for each tuning had to be memorised independently of the way as well. The way of using mensural notation like tablature that violinists used for scordatura was thought up later than when viola bastarda players developed their methods. We can easily imagine performances where the chordal and division styles alternated, as with the English lute and virginals solo repertoires.

The accompaniments to most 17th century English songs were presented with a bass line only, and bass viol alone was one of the favoured options. The vast majority of the surviving bass viols (the models for modern bass viols), and probably a large fraction of those made then, were too small to play the repertoire for sets of viols in consort, but were intended for accompanying the voice. Mace warned against acquiring them for sets. They tended to be called 'viola da gamba' or 'viol', while the full-sized basses were always called 'bass viol'. These smaller viols also tended to play the solo music written in tablature. So the viol players accompanying songs usually knew about chordal playing and so could provide a proper continuo.

There seems to have been varying approaches to the number of continuo instruments to use in the late 16th and 17th centuries. One extreme, which Praetorius identified with the English consort, was to have roughly as many instruments playing continuo as there were performers on the melodic lines. The other extreme was to have a single continuo instrument regardless of the number of melodic lines being performed. The difference seems to be in the relative importance of rhythm and harmony vs. polyphony in the music. The practice of accompanying a single melodic line by multiple continuo instruments is probably more popular today than it originally was.

There are many baroque chamber pieces that specify only a bowed bass instrument, or a bowed bass instrument as one of the alternatives for the continuo, with no indication that more than one continuo instrument was expected. These parts usually have figures for the harmony associated with the bass part. The usual practice today is to play the bass line as single notes on the bass bowed instrument and to add another instrument like a keyboard or lute or guitar for the harmony. This certainly is historically possible, but not necessary. There often were players that could get a bass bowed instrument to provide an adequate continuo on its own. Experience in improvising makes one more aware of the harmony implications of the notes being fingered.

Scholarship is about getting the most objective answers from the evidence available, and not about accepting mysteries until the evidence we would like to have (but don't know how to get) happens to appear. Relevant evidence that has no contrary evidence other than that it seems bizarre, must be accepted. Though medieval artistry does not invite trust in the accuracy of detail, in lieu of other evidence on performance practices, the evidence available must be interpreted. Indeed, the quantity showing the same practices easily makes up for any lack of quality in individual examples. Medieval illustrations of instruments being played have thus not had the respect they deserve for the information on playing style they offer. Objective interpretations of what the bow is doing in the vast majority of these illustrations have been offered above. This approach takes all of the evidence seriously and uses our knowledge of the geometry of the situation and the mechanics of bowing.

The conclusion reached is that bowed instruments primarily played continuo (with or without a melody or counter-melody included) primarily before 1500, and this remained a minor mode of playing through most of the baroque. The way of playing them imitating the voice developed rapidly around 1500, and has been dominant since, with instruments following changes of style of note production and phrasing as that changed vocally. These styles apparently were like spoken clarity of an orator in the Renaissance and French baroque, like stylised noises of emotional expression in the Italian baroque, a synthesis of these in the Classic and Romantic periods (with power becoming an increasingly important consideration) and powerful vibrant tone smoothly floating on and on (with the musical phrase as long as possible) in recent times.

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Masters and Apprentices wanted!

Apprentice-Master Alliance, launched in July 1994, is a non-profit association with initially a main focus on the London region. Based on a San Francisco model which has been operating successfully since 1977, it is a scheme linking would-be apprentices – mainly young people who have just left school or university – with 'masters', women or men who have a mastery of their craft or who have a long-standing expertise in a particular skill or small business, and who are willing to pass this on through the apprentice working with them in a one-to-one relationship. The masters may be expert at crafts, or computer programming, or desktop publishing, or guitar-making, or photography, or gardening or cooking – whatever the skill, as long as they are prepared to take the apprentice on for a period from three months to a year, and, in return for work helping the master, the apprentice is taught a real skill, leading to possible permanent employment with the master. The apprentice would not normally be paid by the master during the initial apprenticeship period but might receive expenses or board or food, depending on the circumstances. The scheme is seeking special arrangements with government bodies so that apprentices on the scheme receive financial help if needed.

Each year Apprentice-Master Alliance will publish about 5,000 copies of a directory of masters. This directory will give a biographical sketch of the masters, a description of their work, and the master's conditions for apprentices – length of service, minimum and maximum hours per week and any special aptitudes required. The directory will be distributed through schools, universities, bookshops and direct to would-be apprentices. It will be priced at £4.95 including postage and packing and the first edition will be available by mail order from approximately December 1994 from the above address.

As a special launch offer, masters and apprentices will be able to become members of Apprentice-Master Alliance by filling in an annual bank standing order form for £30 (masters), and £15 (apprentices), half yearly, but with the first payment not until November 1st 1994, so allowing a free trial membership period, during which time the standing order can be cancelled if desired. A standing order can be made out by a parent or friend, if the apprentice has no bank account. Masters and apprentices are expected to remain members for the duration of the placement. The fee works out at less than 58p a week for apprentices and less than £1.16 a week for masters. Apprentice-Master Alliance needs year-long memberships to cover the costs of the scheme. A limited bursary fund may be available for those apprentice applicants unable to pay the full membership fee.

Apprentice-Master Alliance will offer each apprentice applicant who desires it, a personal review session in which their strengths and weaknesses, skills, needs and long-term ambitions are looked at, aiming to point them in the direction of a suitable master, or towards other training if required. Where no suitable masters are already in the directory, the scheme will endeavour to track down a suitable expert. Applicants will be given Apprentice-Master Alliance membership cards which must be shown to masters at their interviews. Each master will be vetted by Apprentice-Master Alliance for the quality of their work, for their teaching ability and for the safety and suitability of their work environment, with references, including references from any past employees, taken up. Each placement will be formalised by a signed agreement between apprentice and master and will be evaluated by both master and apprentice on completion. Apprentices successfully completing their courses will receive a certificate from Apprentice-Master Alliance. There will be an annual meeting and other 'networking' social occasions for masters and apprentices to meet and review their progress and that of the organisation as a whole, and to share experiences.

Apprentice-Master Alliance director & co-ordinator is Robin Dean who recently ran the The Times/RIBA Community Enterprise Awards Scheme and founded the original Brass Tacks recycling workshops in London. The project is a non-profit association administered by the Institute for Social Inventions, chaired by Nicholas Albery, and whose directors are Margaret Chisman, Nicholas Saunders, Guy Dauncey and Tanya Schwarz.

It is hoped that the LETS group in Stroud may also start an Apprentice-Master Alliance scheme to run in conjunction with the London one. At the end of the three years, the intention is to seek government support to set up bases in all the major cities. Apprentice-Master Alliance is relying for some of its start-up costs on grants from trusts for the first two years but expects its London office to break even in its third year and thereafter to put any surpluses towards expanding the services provided.

The San Francisco model has about 180 masters and 300 apprentices on its books at any one time. The organisers there report: *'When you match up someone who really wants to teach with someone who really wants to know, it is one of the finest one-to-one relationships.'*

Please ask for membership forms and bankers order form.

Consultants: Tyrrell Burgess, Ken Campbell, Dr David Chapman, Dr Alice Coleman, Dr Alec Dickson, Norman Glenn, Oiva Oksanen, William Hall, Professor Charles Handy, Keith Hudson, Professor Keith Jackson, Roger Kelgish, Michael Norton, John Piggford, John Southgate, Hugh Stapleton, Professor Meredith Thring.

Apprentice-Master Alliance is a non-profit association administered by the Institute for Social Inventions, and is a project of the Tower World Educational and Research Association Trust. Registered Charity No. 263820.

Mr Richard Body, John Scym