FOMRHI Quarterly

BULLETIN 69  
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FELLOWSHIP OF MAKERS AND RESEARCHERS OF HISTORICAL INSTRUMENTS  
Hon. Sec.: J. Montagu, c/o Faculty of Music, St. Aldate’s  
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FELLOWSHIP of MAKERS and RESEARCHERS of HISTORICAL INSTRUMENTS

Bulletin 69 October, 1992

RENEWALS: Once again it's time to renew our subscriptions, and I'm afraid that this year I have bad news for you. Printing costs going on spiralling upwards, and we're going to have to put the rates up. The present rate has held since 1988, and five years at one rate isn't too bad these days. As you'll see from Barbara's accounts herewith, we made a loss last year and the year before, and we're pretty sure that we're going to make a bigger loss this year (1992) and, while the idea is always to make a profit for the first year or two, break even in the middle, and make a loss, using up that profit, at the end of each period, that end has now come. Barbara thinks that with a general £2.00 increase, we'll be able to hold for another few years, but of course we're wholly in the hands of our printers (much paper comes from abroad, or the pulp does, and with the pound sinking through the floor, that cost may whizz up) and the post office (who seemed determined to make as high a profit as they possibly can from the worst possible service). For the moment, we think that we can hold the same airmail postal rates; we're making a slight loss on Europe by air, but a slight profit on overseas air, and the two are balancing OK. So no promises, but as always we'll do our best. Rest assured that unlike one or two other societies I could name, there are no honoraria (a posh word for hidden salaries) floating around; what your money goes on is what you are now reading, the envelope it came in, and the stamp on it. Plus the cost of reminding too many of you in January that you've not yet paid; please save us that waste of your money by RENEWING NOW. You'll find an INVOICE herewith. So the rates are:

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 $ 9.50 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 (we've had to put that up, too; the banks are screwing all their customers, including us). Eurocheques work if they are backed with your card number. Unfortunately, as I told you last time, GIRO doesn't work any more, so please don't use it; it's not just that they're charging a lot for it, but much more important that they don't tell us whom the payment is from, so if you do use it we'll never know that you've paid and you won't get your Qs! As far as we know, the GIRO postal orders (the ones that come through the post with a little piece of pink card with your name on it) do work, so you can go on using those. And as far as we know, neither Eurocheques nor GIRO postal orders 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 did this last year), 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 (or restrict by charging grossly excessive amounts for parity; £12 can be a month's income in some places) the export of money, 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 people who'd like to read FoMRHIQ but can't for that reason, and to put them on our lists.

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.

Please remember to pay before the end of the year, we're all getting busier, and people who pay after the January Q has gone out may not get it till April or even July, simply because there is less time available to send out individual late mailings.

LOST MEMBER: Does anyone know where Tim Cranmore has gone to? He was in Stretford-bury, Leominster.

OBITUARY: Ian Gould of Tamworth died in July; he's been a member since 1977 and contributed a number of Comms over the years. His wife Barbara tells me that the Mercian Consort is continuing to meet at their home.

FURTHER TO: Bulls.66 & 68 - Vegetable Ivory: John Downing writes:

You have recently reported in Bulletin 66 and 68 about vegetable ivory from Tagua nuts.

In Canada, these nuts may be obtained from Lee Valley Tools Ltd, 1080, Morrison Drive, Ottawa, Ontario, K2H 8K7 - $6.50 per lb or $25.00 for 5 lb, at about 10 nuts to the lb.

The Lee Valley catalogue has the following information:-

Used as a substitute for animal ivory, Tagua nuts come from the ivory palm. The nuts share the same characteristics of ivory: they are white, very hard, and take a polish well. They can be machined in a variety of ways. They are particularly suitable for turning to make chess pieces, finials, drawer pulls etc. they hold carving detail well and are superb for decorative plugging. They can be sawed into slices and cut into intricate shapes on a scroll saw. Although very hard, they take stain well. These make a very versatile raw material. The nuts vary from 1 1/2" to 2" long with a fairly flat void in the centre. Approximately 10 Tagua nuts to the pound. Instructions for working them included.

Cat. #33K01.60 Tagua nuts, 1 lb
Cat. #33K01.65 Tagua nuts, 5 lb
Paul Hailperin sent the following:

**Die Steinnuss**
(auch Elfenbeinmann genannt)


Bull. 66 brought a report on "vegetable ivory." By chance I shortly afterward noticed the above description, which indicates that the use of this palm seed is quite old. The copy above is from Fritz Spannagel: Das Drechsler Werk, Otto Maier Verlag, Ravensburg 1940 ... second printing 1948, reprint 1981 by "libri rari" Hannover. Spannagel says that this material was imported into Germany, and presumably used as an ivory substitute, already in the early 19th century. He mentions that the material shrinks and warps, which ivory does to an extent also. The biggest limitation is apparently the size.

Comm. 1107: I regret that I’ve hurt Cary’s feelings and apologise to him; see his Comm in this Q, and note that he gives the price, including a good discount to FoMRHI members, for this very useful book. Mine was a short review, which I hope didn’t further offend him; the books that one really finds useful are very difficult to review at any length; what more can one say than you need this, you really ought to have it, buy it. I repeat: more or less whatever you do to instruments, you’ll find much useful reading and relevant material here which you aren’t likely to find in any other source, short of ploughing through years of the main AATA Abstracts.

BACK Qs: There are some Qs which are almost out of print, so if you want copies of any of them, it’s now or never. Number left in (): Q27 (4), 29 (5), 30 (lots), 31 (4), 32 (2), 33 (lots), 35 (2), 38 (lots), 39 (10), 40 (15), 43 (4), 44 onwards Gots); all others before Q44 are extinct. Cost is £1.50 by surface, £2.00 by air, for each Q, and it’s first come, first served.

REQUESTS: Angelo Zaniol wants to get in touch with a specialist on the Cavaquinho, the small four-string Brazilian guitar; he wants to know more about the instrument because he’s fallen in love with it, and is anxious to hear from anyone else who’s a fan of it.

Bruno Gire asks whether anyone can supply him with a drawing or any other data, especially about the soundboard and barring, of the renaissance bass viol by Gaspard Tieffenbrucker in the
Hague Gemeente Museum (inv.no. EC 11-1937); this is the one with "falling shoulders", carved horsehead pegbox, and decorated fingerboard and back.

INSTRUMENTS ON OFFER: One of our members in Poland, Andrzej Kuczkowski, makes reproductions of Polish folk instruments; Ewa Dahlig brought me a copy of his of the mediaeval fiddle which she is publishing in the next Galpin Journal. He makes hurdy-gurdies for $1,300, rebec for $1,000, and zdóboki for $800; all a bit more than I can afford, but some of you may be interested. He's in the List of Members.

CALL FOR PAPERS: Bernard Brauchli is organising a 3-day International Clavichord Symposium next year, September 9-12, in Magnano (Piedmont, Italy). Speakers already scheduled are himself, Clifford Boehmer, Christopher Hogwood, and John Shortridge. Further papers are welcome, 10, 25, or 50 minutes in length. Please send him a 15-line summary by 31st December; he's in the List of Members. Further information available from him.

CODA: That's the lot; a short Bulletin this time. As usual, I'll keep it open till I've finished off everything else. Remember to renew your subs, please, before the end of the year. You could always send a Comm at the same time!

DEADLINE FOR NEXT Q: New Year is a Friday, so let's say 4th January. Allow reasonable time for the Post Office; posts go more haywire than usual over Christmas, and they take as many days off as they can; there won't be a delivery on New Year's Day (which is why I suggest the 4th) and if they've had a heavy and boozy weekend, there may not be much on the Monday either (they won't want to carry a heavy bag with a hangover), so try to get things in before then. Have a good Christmas and New Year.

Jeremy Montagu
Hon.Sec.FoMRHI
Comm 1078 presented problems worth discussing. Jeremy or I could have just written to the author and said "look it up in a standard reference like New Grove". We didn't because most people nowadays cannot afford to buy such books and many don't have ready access to libraries that would have them. The reply in Comm 1095 by John Barnes was just right in length and style. I am sure that at least a dozen other members could have given a perfectly adequate reply, but John's was the only one. Why no others had puzzles me. In these times of recession, I doubt whether being overworked is a believable excuse. Is it just laziness - "leave it to John"? Or is it fear of embarrassment by making a slip when saying in print what all specialists know, with the others looking on? This did happen with John, and his correction Comm is in this Q (it just missed getting into the last Q). Everyone can and does make slips and they appear in every publication, no matter how thoroughly edited and refereed it is. One of the aims of FoMRHI's style is to get away from the printed word as authority (the final word on anything), but rather as communication that can always be improved on. I admire John as having the right teaching spirit. It is our duty to the future to teach, or to put it more correctly, to have the fun of teaching because the process of organising our knowledge for sharing it with others sharpens our own understanding.

I've been sent notice of a course on "Preventive Conservation: Museum Collections and Their Environment" held in London from 18 April - 1 May offered by the Getty Conservation Institute and the Conservation Unit (The Museums & Galleries Commission, 16 Queen Ann's Gate, London SW1H 9AA). It is for senior-level conservators, conservation scientists and conservation administrators working in institutions or private conservation practices. Participants are limited to 20, the cost is £350 and the application deadline is 20 November. Very few of us would qualify and those would surely have heard about this course through other channels. But I felt that notice should be included here simply because we should be informed about what goes on in that field.

Many of us are collectors of instruments, mostly fairly new with perhaps a few older ones we've picked up here or there. They all need good conservation, and we all have much to learn about doing it better than we do. I very much doubt whether the professionals who run and go to courses like the one above would or could run one for us. How about a bootstrap operation where we help ourselves? Perhaps, as a starter, we can have a FoMRHI Seminar on Instrument Conservation at the Bate some weekend. Any interest?

I started to do the T's in the series of DoMI reviews and got stuck on 'Tactus' because it set me off working out the tempi in the 16th century (the Comm is in this Q). I know that to claim to have objectively determined the standard tempi in the 16th and 17th centuries without discovering new compelling evidence seems outrageous. One must be mad to say that the old greats of musicology like Dolmetsch and Sachs got it wrong, and that what authorities like Donington said was an impossible mess is really simple and remarkably consistent. That happens to be exactly what I am saying. I am not any smarter than they were, but my background is different. I was rigorously trained in scientific method as a physicist (that is what my PhD is in). Part of that training is to separate carefully one's subjective preferences (one can't help these) from one's analyses. Researchers in music have not needed this discipline, finding that their aesthetic judgements have consistently helped in their work. Since they have not had cause to question these judgements, they have great difficulty in accepting results that seriously violate their subjective concepts of 'musicality'.

As a musician, I find some of the tempi I've deduced excruciatingly slow (they are half as fast as Donington suggested for the late 17th century and Apel suggested for c.1500). I am just less inhibited about pursuing and reporting findings that are musically uncomfortable. If anyone can find faults in my analysis that would lead to the objective deduction of more comfortable tempi, I actually would be quite grateful.

One reason why I did my chekker Comm in this Q was to demonstrate that I haven't abandoned specific musical instrument research. Another is pique that Chris Page is late again in sending in his 1992 FoMRHI sub. To get back to what I started at the top of the last paragraph, I just want to tell you that I haven't become bored with the DoMi project. Writing reviews (and writing books) are for times when research ideas are not seething.
FoMRHI Accounts for 1990 and 1991

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As you can see, it is now time to put up the subscription rates again. We have not actually gone into debt yet, thanks to earlier years profits, but we must now wipe out the paper deficit for 1991 and 1992.

One of our problems, which I should have picked up sooner, is the appalling exchange rate we have been getting on foreign currency cheques (U.S.$ especially). The banks must make a hefty profit on this, as well as charging us the transaction fee, so we will have to increase our surcharge on all foreign currency cheques. Even so, I plead with U.S.$ cheque writers to be generous to us on your estimation of the value of the $.

Lastly, is there anyone out there who would be willing and able to audit our accounts? Someone living within easy access of south Bedfordshire would be ideal, failing that, we will have to trust the post. I'm afraid there is no fee available for this (but perhaps we could offer free membership of FoMRHI?)

Barbara Stanley
1992 Members List Supplement no.1, p.1

1992 FoMRHI List of Members — 1st Supplement as at 15 October 1992

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

* Anthony Baines, 0252-735489.
* Ewa Dahlig, Universität – Gesamthochschule Essen, Fachbereich 4 – Musik, Postfach 103764, WD-4300 Essen 1, Germany.
Simon Darton, 53 Newfield Drive, Crewe, Cheshire CW1 4AR, UK; Crewe 582184 (harp, str.inst, wind; M,P).
Daniel Deitch, 2025 Clement St #2, San Francisco, CA 94121, USA; (415) 387-4527 (trav, M; fl, fag, sax, recrdr, R,P,coll).
Germanisches Nationalmuseum, Bibliothek, Kartäusergäßle 1, Postfach 9580, WD-8500 Nürnberg, Germany.
* Hendrik Hasenfuss, Kölner Straße 159, D-5067 Kürten-Weiden, Germany; 2268/3549.
Nils Olav Hovden, Ligt.21, N-3660 Rjukan, Norway.
* Renke Lody, Heider Str.12, WD-2242 Büsum, Germany; 04834/4719 (hpschd, clavchd, spnt, bagpp, esp Bohem & Northumbr; M)
* Alec Loretto, Auckland 6304-017.
Robert Lundberg, 3344 NE Oregon St, Portland, OR 97232, USA; (503) 232-0548 (lute, archlut, thrho; M,R,C,coll).
Darryl Martin, 2 Brunswick Place, Edinburgh EH7 5HP, UK; 031-557 0016 (hpschd, virgnl, spnet, clavchd, early pfte; M,R,res).
* Fedor Nekrasov, 127273 Deckabristov Str 20, corp.1, 158 fl. Moscow, Russia; 907-53-73
Daniel Papuga, Kjelsaasveien 51-F, N-0488 Oslo, Norway; 472-229853 (bagpp, ww, perc, M,P; all instrs, res, coll).
Peter Smalley, 30 St Mark’s Road, Salisbury, Wilts SP1 3AZ, UK; 0722-325652 (keybds, ww; M,R).
* Paul White, 16 Abbey Road, Oxford OX2 0AE, UK; 0865-245153.

FoMRHI Comm. 1113

Jeremy Montagu


Arnold only gave me these yesterday. What is available so far is Volume I, the Illustrations, which costs £25.00 plus £3 p&p in UK, £5 abroad, and, of Volume 2, the Descriptive Fascicles:

B ii Lutes, Citterns, and Guitars; £ 3.00
D ii Transverse Flutes; £ 5.00
H i Horns and Bugles; £ 4.00, each plus £1 in UK and £2 abroad. All three are also available on 3¾” disk on DOS, either as ASCII or WordPerfect 5.1, at the same price as above. They are all A4 size, printed in two columns.

Obviously I’ve done no more than glance at them; you know that they are available. My comments can wait till next time, and meanwhile all those who, like me, buy any catalogues that they hear of, can write to Arnold at the Collection as above, Reid Concert Hall, Bristo Square, Edinburgh EH8 9AG, with a cheque in sterling only made out to the University of Edinburgh.


All three of these have been sent to us by Bolesław Bielawski of the Polish Historical Monuments Documentation Centre in exchange for membership of FoMRHI. If you want to see them, they are here in Oxford; if you want to obtain copies of them, his address is in the List of Members. I don’t know how much any of them cost, so you’d have to write and ask him.

The first is a description of all the organs in the Kielce Province of Poland; it is the first of a series which will eventually cover the whole country. The introduction, which is a quick historical survey, is in English as well as Polish. The main text is Polish only, but dates and makers’ names are the same in whatever language, and, for example, Dyspozycja, Pryncypal, Oktawa, or even Flet, are not so different from the words we use in English, Dutch, German, or whatever else. Thus anyone interested in organs will find the book fairly useful even without knowing Polish or possessing a Polish dictionary. With such aids, or with the next book, the book will be fully useful. Each organ is described in as much detail as is known or can be ascertained, and each has one or more photographs, even some of those which no longer exist. There are distribution maps for different historical periods, pre-1800, 1800-1914, post-1914, and a fourth which I think means no longer existing; the English version of the introduction does not wholly correspond to the Polish.

The second book is again the first of a series. Every term connected with any keyboard instrument (this includes string keyboards, free reeds of all sizes save the mouth-blown, and organs) is defined; each type of instrument is illustrated with line drawings, with all its parts marked and named, as are all the types of organ pipe, and so forth. The text is only in Polish, but the book would be useful for anyone working seriously with the previous one, for it would act as a technical dictionary for all those who already know what the equivalent parts are called in English.

The third is a similar, and perhaps preliminary, work on historic and folk instruments in general, with a paragraph or so of description and named parts. The drawings are a bit sketchy and not always wholly accurate. If, as I suspect, it is designed as a manual of identification for those who are trying to build up a corpus of surviving historic instruments in Poland, it is more than adequate for such a task.
FoMRHI Comm. 1115

Jeremy Montagu


200 pp. $30.00.

This is likely to become better known as the Betty Bang Mather Festschrift than by its real title. It is a collection of articles, notes, and reminiscences by her friends, colleagues, and pupils. Only about half of them are relevant to us, though all will be of interest to any flautist of any period.

The first article, by Janice Boland, is a useful survey, created initially from necessity, for she needed a tutor to learn to play the thing, 'A Guide to the Best Eighteenth Century Tutors for the One-Keyed Flute'. Brief but informative with useful descriptions, and I doubt that anyone would argue with her choice of which are the best, from Hotteterre to Gunn.

Two articles are on a subject that I suspect leads to ample controversy but that could also be very fruitful. This the linkage between words and music. One, by Patricia Ranum, is 'Do French Dance Songs Obey the Rules of Rhetoric?' (the Rules of Rhetoric are themselves complex enough), and the other, by Sven Hansell, is 'Italian Prosody as a Guide to Musical Structure' dealing mainly with accent and articulation, and carrying it considerably further than the verbal articulations common in, for example, the trumpet tutors.

Anita Breckbill's article on 'The Flute Duets of W F Bach' seems to be full of information but in fact it is mostly information about information instead; one source will be described as differing from another in rhythms or articulations, but no examples are ever cited, so in the end we don't know anything more about the Duets than we did when we started. Since this is one of the longer articles, this is rather a disappointment. John Solum provides one of the shortest, a statistical analysis of the trills in the Trio Sonata from J S Bach’s *Musical Offering*.

Breckbill's is not the only article which is a bit short on concrete fact. Susan Nelson's 'Brief History of the Flute on Record' gives some names, some repertoire, few details, and no catalogue or matrix numbers. She does not even mention those which have been reprinted in CD or LP form, such as the Busch Brandenburgs with Marcel Moyse on flute. I suspect that some of the earlier recordings, too, especially those with a flautist such as Franzella accompanying a singer, are now available again. Articles like this are a wasted opportunity.

Claudia Anderson's 'The Flute in Pre-Revolutionary American Concert Life' has a good deal more precise information and could be a good stepping-off point for an academic thesis were it not a pared-down version of one already.

As you will have gathered, this is something of a curate's egg, good in parts and somewhat ropey in others. On balance, if you're an early music flautist, the good parts make it worth having; if you're not, then it's probably not worth bothering with.

Not a review because the book only arrived on Monday and so far I've only got through the As, Bs, and part of the Cs, plus, as with any book of this nature, a few cross-references and other things that caught my eye. So a full review won't appear till January.

In the meanwhile, I have to say that this book shames and embarrasses me. I think that I can say, without undue vanity, that I am considered to be something of an expert on musical instruments. I would hate to have to confess how often, already, I have said 'I never knew that', when reading these pages. This is a tour de force, the work of a master.

However much you know about instruments, about any instrument, you will learn more here. Also, it's a pleasure to read; unlike the authors of many such books, Tony Baines can write English even when he is having to write short to keep to space. He can also explain and describe with exemplary clarity.

One has, even at this early stage, a few complaints. There are a few misprints, not surprising in such a work (details of any important ones next time). OUP have been very niggardly in the number of illustrations they have allowed. This is a serious blemish, for though there are more pictures, either line or photograph than I can count, there are nothing like enough. Since many of the instruments described are going to be unfamiliar to the general reader, how are they going to get any idea of what they are like without a picture? However, if there had been more pictures, doubtless the price would have been higher, and to produce today a book of this nature and size at so low a price is remarkable.

The price is a tenth of that at which *The New Grove Dictionary of Musical Instruments* appeared, and that was in 1984 (and that hasn't anything like enough pictures either); we've had eight years of inflation since then. The book is about a quarter the size of *NGDoMI*, how the coverage compares, we'll have to wait till January to discover, though I suspect already that it's a good deal more than a quarter of the coverage. One basic difference, though is that while *NGDoMI* is extremely careful to use the correct accentuation, diacriticals, and all the minutiae of spelling, this book has no accents but French, German, and some Scandinavian. This I find sad. It makes an implicit statement that there are major languages, those of Western Europe, and minor dialects, the tongues of the rest of the world. OK, it saves money (it is a wonderful book for only twenty-five quid), but whereas I can put up with a lack of pictures, with some regret, I can't feel that the accepted transliterations of Arabic, Hindi, Japanese, and many other languages can be ignored without dealing them a slap in the face.

So, more detail and a proper review next time.

This time, really just to tell you the book is published, that if you haven't got *NGDoMI* you must have *OcMI*, and that even if you have got *NGDoMI*, there's a good deal of information in *OcMI*, especially clear details on how instruments work, that you may not find in the bigger compilation.
Comments on Jeremy Montagu's review of, "The Conservation and Technology of Musical Instruments", in Comm. 1107

Although I am most pleased that Jeremy feels this work to be of such great value, I am puzzled by his criticism of the decisions made in the selection of its contents. As is clearly stated in its introduction, the publication is intended to be as nearly as possible an exhaustive hard-copy compilation of relevant material contained in two on-line database services: BCIN and RILM (including summary coverage of the latter's neighboring databases on DIALOG). Additional abstracts which had previously been submitted to AATA but not deemed suitable for publication in one of its regular issues are also provided. Finally, some original abstracts were prepared specifically for this publication by myself and a number of interested colleagues.

(For those who may be unfamiliar with the acronyms, AATA stands for, "Art and Archaeology Technical Abstracts", which is the primary bibliographic tool of its type used by museum conservators. RILM is the, "International Repertory of Music Literature", and is the equivalent in the field of musicology. The bibliographic database of the Conservation Information Network, BCIN, contains material provided by a number of conservation institutions, of which AATA is a major contributor. DIALOG is a massive commercial service which hosts RILM among a staggering number of other databases.)

Jeremy feels that the value of the MI-Supp (to use the working name adopted during its preparation) was blemished by the FQ not being among the publications abstracted. He assumes that this was a result of my scholarly judgement having been tainted by a "personal pique" against FoMRHI. Rest assured that I have not "taken against" the organization. Over the years, I have contributed a substantial amount of material to the FQ which easily might be of interest to the AATA readership. However, even during the heyday of my active participation in FoMRHI, throughout which time I was also an abstructor for AATA, I never prepared abstracts of any FQ material.

Why not? Certainly not to slight myself, which following the reasoning in Jeremy's review would be the obvious explanation. I simply took Jeremy at his word when he time and again stated that the FQ should neither be a journal of record nor be kept on library shelves. Given this, it never seemed reasonable to include abstracts of FQ material in a bibliographic tool which required both these conditions to be true. (In fact, not only did I agree with Jeremy, but went on record as not feeling all that comfortable with the notion of the FQ being reprinted in unrevised form, since this could be a deterrent to the submission of truly spontaneous material.)

So much for the sole reason why one of the hundred or so individual contributors to the MI-Supp did not abstract the FQ. Why did none of the others abstract it, either? Without going into every conceivable reason for this, it may suffice to consider what may be the largest single group of contributors to the MI-Supp: the authors of the individual books and articles, themselves.
Both AATA and RILM have always welcomed and encouraged authors to submit abstracts of their material. (Indeed, RILM consists virtually entirely of authors’ abstracts.) If the author of any single item that has ever appeared in the FQ had ever felt it worthwhile to prepare an abstract thereof for AATA or RILM, the FQ would have been among the publications listed in the MI-Supp. The non-FQ publications of FQ regulars, including Jeremy, are represented in the MI-Supp by authors’ abstracts. Why have these people not felt it necessary to treat their FQ material in the same manner?

Among FoMRHI members, at the very least, Jeremy has known full well for as long as I can remember that I have been an AATA contributor. He has also been an AATA subscriber, himself, for some time now. Never -- not once -- prior to the present review has either he or any other FoMRHI member expressed any curiosity about why no FQ material was being abstracted in AATA, or what might be done about it.

I agree without hesitation that the AATA readership could benefit from knowledge of the FQ’s contents. The same goes for material being published in any number of other organological membership organizations’ newsletters and bulletins, as well as in all the popular magazines produced for instrumentalists, to say nothing of truly ephemeral material in the daily press and other mass media. Little of the above has been treated satisfactorily, if at all, in the MI-Supp. By virtue of its vast scope, I cannot imagine how it could have been, either.

Fortunately, the MI-Supp is not the last AATA publication to contain material relevant to musical instruments. On the contrary, one reason the subject was selected as appropriate for the recommencement of the publication of AATA subject supplements was because a need was perceived for enhancing the degree of communication between conservators and the organological community. One result of this will hopefully be an increase in the number of abstracts relevant to the conservation of musical instruments published in AATA. A concurrent broadening of the scope of the periodicals represented will also be most welcome.

To make it as easy as possible for interested FoMRHI members to participate in this process, the AATA editorial office will be happy to provide MI-Supp order slips for inclusion in FoMRHI’s next general mailing. As an alternative, they are equally willing to mail the order slips to all FoMRHI members directly, if provided with a copy of the FoMRHI mailing list. (Presumably preprinted labels would be appreciated. FoMRHI’s corporate response to this offer should be addressed to Jessica Brown, the Managing Editor of AATA.)

The price for the MI-Supp is US$40.00 for a single copy. FoMRHI members will be given a $5.00 discount upon request. While you’re at it, why not request information on how to prepare abstracts of material in the FQ for publication in AATA?
foMRHI Comm: 1118

Copies

Alec V Loretto

Readers of Comm 1111 (What is a 'copy' of an original instrument? E. Segerman) might like to refer to the article 'Which? Some comments on the ways in which recorders are described by their makers, players and others' in Recorder and Music of June, 1976. The article considers programme notes and catalogue descriptions from around the world including:

a) Modelled on Bressan*
b) Incorporating Bressan principles*
c) Copy of Bressan*
d) Bressan Copy* (* or some other well-known maker from the past)
e) Bressan Model*
f) Exact copy of Bressan*

and goes on to suggest what these words might mean, and how misleading they can be. Comm 1111 makes it clear that care is still needed in describing copies of original instruments.

In conclusion the Recorder and Music article expresses the hope that things will improve, but adds that there have always been problems in stating clearly what is actually meant - 'Perhaps in the near future those who write programme notes, make recorders or play them will be a little more accurate in their descriptions, and avoid the ambiguities and confusion found today. But then of course music has been riddled with inaccurate descriptions and misleading nomenclature. As an example, if four recorders together are known as a recorder quartet, and a saxophone quartet is made up of four saxophones, what instruments might one reasonably expect to find playing Mozart's Clarinet Quintet (no, not five clarinets!) or a Beethoven Piano Quartet (no, not four pianos!)?
THE POST-MODERNIST BASSOON: PROBLEMS WITH 'AUTHENTICITY' IN EARLY WOODWIND REPRODUCTIONS (marketing, facade, and the early music industry steamroller)

With the wealth of fine players and abundance of playable period instruments available now I should feel that we have entered a new golden age of historically enlightened performance. One cannot deny that to a large extent this is the case. However, I feel an increasing sense of disquiet when I consider the hype and falsity that has also accompanied the establishment of this relatively new form of artistic expression.

An incident occurred two years ago which forced me to stop and reconsider why I had involved myself with the study of the performance practice of Baroque and Classical Music and the reproduction of early bassoons. A professional musician from Italy who specialised in early bassoon performance asked if I would modify two original late-eighteenth century bassoons which he had recently purchased, a five key bassoon by the Parisian master, Prudent (fl. 1760s-1770s), and a seven-keyed instrument by the highly prolific Heinrich Grenser (b. 1764-d. 1813) of Dresden. Because of the superior sound of these instruments he desperately wanted to concertise on them and stop playing the reproduction bassoons on which he had developed his career up to that point. [For those of you unaware, these two makes of instruments are the most popular 'ancient' models for contemporary reproduction. As a result, between them they populate more period orchestras throughout the world than all other varieties of reproduction bassoons combined. They are, in essence, the Fords and Volkswagens of the modern early-bassoon world.]

His purchases proved a fortunate coincidence for him, because these were the original makes from which his reproductions had been copied. Unfortunately for him, he was finding performance on the original instruments difficult, because they did not respond well to the fingering patterns or reeds which he had always used on his reproductions. It so happens that the most detailed written sources for the performance of bassoons from the eighteenth and early-nineteenth centuries appear to have been written specifically with these two makers' instruments in mind. The 1787 'Parisian' bassoon tutor written by Etienne Ozi (1754-1813) presents us with a fingering chart labelled for use with Bizey and Prudent bassoons, along with descriptions of reeds and other aspects of performance and pedagogic practice commonly in use during the last quarter of the eighteenth century. Joseph Fröhlich's tutor of 1810-11, which revises and updates Ozi's tutor for use on the next generation of Dresdener bassoons, was written presumably with J. F. Grundman's (1720-1807), J. F. Floth (1760-1807), and H. Grenser's instruments in mind. Although there is a wealth of information available on how these instruments were played during their respective periods, my professional early-bassoon specialist had never bothered to consult any primary or secondary sources concerning this during the entire course of his career.

Keeping all this in mind, I will relate to you the dilemma I faced eventually when I had the opportunity to examine the two instruments and meet directly with the musician who owned them. Seeing them for the first time it was clear that the Grenser appeared to be in pristine condition, including an original crook, while the Prudent had literally been played to death, and clearly deserved to spend the remainder of its days in the quiet of a restful museum. The opinion I expressed to the bassoonist was that, because of the rarity of the Prudent and the pure, untempered, state of the Grenser, each was an important archival source that should be maintained for future generations and that neither instrument should be played on for anything other than research purposes. Furthermore, I explained that it was in his own best financial interest not to interfere with the physical integrity of this historical evidence, which would only result in a lower resale value. His response was direct. I was made to understand that if I didn't retune and alter the instruments to suit his performance requirements, then someone else would be found who would do the work as he wished.
My fall-back position was to propose a restoration of the Prudent in which nothing would be removed and any additional build-up would use only natural materials, to be obviously marked, and therefore, easily removed. An expensive, but politically correct procedure.

For the Grenser, my only option appeared to be gentle persuasion, realising that this was the instrument he most wanted to use in performance and that there really was little I could do to dissuade him from this. His major complaint was that although the original had a superior sound to his reproduction Grenser, some of its notes were impossible to play in performance, thus rendering the instrument unusable. From his perspective as a musician, this he found unacceptable. His solution was to retune the problematic tone holes, so that the original played exactly as his reproduction, thus making his life much easier.

I sat the man down and pulled out my copy of Frolich’s fingering chart, requesting that he play each of the troubled pitches. On each problematic pitch I asked him to apply the simple fingerings suggested by Frohlich and if those didn’t work, then try something from Frohlich’s extensive list of alternative fingerings. Much to my surprise, expecting an unplayable instrument, I found instead that virtually every simple fingering sounded and responded not only well, but often as near to perfect as anyone should expect. I was dumbfounded; this well-preserved instrument actually worked the way it was supposed to when someone simply took the time to follow the instructions which had been written for it originally. Over the moon with this confirmation, I crashed back to earth with the realisation that the performer was much less thrilled with this than I and not so easily convinced by the solution I had offered him.

Here was a man who played bassoon in many of Europe’s most highly respected and long established period orchestras. And yet, he felt I was asking too much of him. This would involve learning a new set of fingerings and retraining the brain. Admittedly, this would take considerable time and energy to ensure that any change of technique would not come ‘unprogrammed’ in the confusion that accompanies the heat of performance, but then isn’t this all part of the ‘turf’ that one accepts when choosing to be a specialist? Although he agreed that employing the proper period fingerings did solve the acoustical problems he was encountering with the instrument, this solution simply was not viable for him. He reasserted that the instrument should be retuned to play like his reproduction Grenser. Sensing that I could not persuade him otherwise, I told him I could not do this and sent him away with Frohlich’s chart, hoping he might rethink his position and learn to play the instrument on its own terms and within the aesthetic confines of an earlier period of music. In hindsight, what I think disturbed me so much about this incident is that this man rose through the ranks of performers during the last decade, having studied with good teachers, and at a time when much of the evidence for the performance of early music was readily at his fingertips. Yet, somehow, much of the aesthetic that underpins period performance seems to have missed him completely.

What also greatly distressed me was the subtle arrogance of his position. He presents himself as an artist sensitive to the emotional content and mannerisms of music from earlier periods of history, yet he appears to lack any deep interest in understanding and absorbing its underlying principles or practical execution. His stature as a professional musician has brought a financial success that allows him to own and customise instruments made by the great masters. Unfortunately, his assessment of the situation was probably correct: if I wouldn’t fix the instruments to his liking, he would find someone who would. Having bought the Grenser and Prudent for much less than their respective reproductions, to him they were expendable, and could be disposed of as he pleased.

I wish I could say this is an uncommon attitude, but many period instrument performers have defended his position as reasonable. Powerless to do much, if anything, about this, I can only wonder that if the bassoonist owned a Rembrandt, would he have asked Jackson Pollock to retouch it to match his Eames chairs? As ridiculous as this analogy may seem, is it really any different than re-tuning a Grenser to suit modern ears and fingers? Must this original Grenser now be forced to sound or perform like the reproduction it spawned?
The myriad sins of reproduction instrument builders are no less disturbing. Some months ago an incident reminded me of the other side of the problem. I spoke to a colleague with whom I had once co-produced the reaming tools to reproduce our own separate copies of the surviving four-key contra bassoon by Stanesby junior. He casually mentioned that he was intending to build a copy of the surviving A. Eichentopf contra of 1714. Intrigued, I asked him if he had encountered anything interesting when he measured the original Eichentopf. The gist of his response was that he hadn't actually measured it yet, but he presumed that the bore of the Stanesby would conveniently substitute for the bore of the much earlier Eichentopf, thus avoiding the need to retool. In a telling aside, he added his belief that no one would know the difference anyway, so why bother.

Indeed, why bother?

Unfortunately, this attitude remains all too prevalent among instrument makers at a time when we should be well beyond such deceptive practices. There is all too much window dressing going on in this business. We, as a professional community, should know better and be doing our best to produce exactly what it is we advertise: reproduction instruments based closely on historical designs.

There are many other examples of our shortcomings as builders. I notice that there are still three-keyed J. C. Denner (1655-1707) bassoons reproduced with four, five, six, seven, and eight keys, at any pitch you might possibly want. What ever happened to good old fashioned values like: 'Any color as long as its black?' I know of too many J. Eichentopfs (1678-1769) and other makes out there with Prudent bores, purely fictitious 'computer-designed' 392 bassoons, and countless 'Baroque' bassoons that have been tuned with the larger tone hole diameters of the Romantic-era. These alterations result in instruments that sound louder, are more evenly tempered, and fundamentally uncharacteristic of the period from which they are claimed. In fact, I'm hard pressed to think of but a handful of reconstructed early-bassoons which bear a close enough resemblance to the originals they are copied from. I know of no one who has built and tuned an early-bassoon with an historically-based reed design purely in mind — myself included. This flies in the face of the fact that early reed technology is radically different from the modem bassoon reed. Is this not tantamount to ignoring the essential nature of gut strings on fiddles? This is and will remain a crucial problem with the early bassoon, because from the outset it determines virtually all of the instrument's playing characteristics: crook, bore, tone holes, sound qualities, and response.

Another problem stems from makers seeking to 'improve' on the design of early bassoons. It is all too easy for a maker to slap on an extra key to cover a botched tuning or an inability to balance the tuning of an instrument properly. As a result of this practice there are dozens of musicians out there who are unaware that the low Eb-key on their copy never existed on the original from which their instrument is purported to have been copied. To add keys where they never existed or were originally considered unnecessary is the sign of a lazy or incompetent artist. Consider a recent experience at this summer's San Antonio Early Music Festival. There I heard someone hacking away at an early-seventeenth century Mexican continuo line with a brand 'spanking' new, American-made, eight-keyed J. Eichentoph bassoon (the original needed only four)! I wondered out loud if you could also special order this marvellous thing with electric ashtrays, power seats, and lots of shiny chrome. Watch out Heckel. Heinrich Grenser summed things up well when he spoke of the master maker's aspiration to manufacture instruments with the fewest number of keys. Is this not the sort of ultimate skill which makers should be striving to obtain?

One cannot overstress the point that all of the above alterations, well intentioned or not, change the way an early-bassoon sounds, its balance and dynamics; thereby misrepresenting the basic performance characteristics of the instruments from which they have been copied. Everyone involved should be aware of this by now. Unfortunately, all too often, the players aren't interested, the makers don't care, and the public is getting hood-winked by all this pretense of authenticity.
How has all this transpired? Why are alterations like this now the norm on reproduction early-bassoons? Good questions, which deserve further examination, and demand answers. The trend amongst my own community of builders over the last few decades has nothing to do with finding makers unable to build usable instruments, but rather an unwillingness on their part to rid early instruments of the influence of the residual preconceptions and performance expectations cast by the shadow of the modern bassoon. This dominant influence has come to determine how many reproduction instruments sound and respond, which in their own turn have cast expectations of how other reproduction instruments should also sound and respond. The net result of this is that early-bassoons are becoming more like the modern instruments they were originally created to replace.

I perceive the market pressure of this trend away from historically enlightened reproductions as only becoming worse. This is because we are now seeing more and more players who are less interested in closely studying period performance practice than in making as smooth as possible the transition from modern to early instruments. In many cases the motivation has more to do with earning a living, given the declining modern orchestral market, than with the spirit of artistic achievement or interest in the past. This cross-over / band-wagon effect has created a market pressure which dilutes the distinctive 'ancienneté' of instrument design. Instead of stressing the differences between modern and early instruments it seems that we, players and makers alike, are trying to create more similarities.

This is the antithesis of what had guided period performance originally and as early music continues to accept more modern mannerisms it will eventually engineer its own demise, simply because there will no longer be enough perceptible difference between the sound qualities of modern and early instruments. This would be a regrettable loss because, as an experimental art movement, period performance was founded on a secure foundation intent on reinvigorating early music by discovering new forms of expression and sound based on past performance practices through the utilisation of each period's technology, social context, and written sources. I, and those who taught me, have always assumed that anyone attempting historically enlightened performance should understand this fundamental principle. It is only through the application of these guide-lines that one can then begin to apply this to the music with the energy, style, skill, and musicality needed to bring the spirit of early music back to life.

This same philosophy should also underlie the reconstruction of musical instruments from the early periods. I'm no longer convinced that this remains an underlying principle. As a piece of archival material we should consider and utilise original instruments as we would an autograph manuscript. There should be a thorough attempt to measure accurately, understand, and record what evidence was there from the start, what was added or subtracted in subsequent alterations, and how this affected the music that may have been played by that particular instrument. In attempting to reconstruct these instruments we should be adhering as closely as is possible to the original design in order to recreate a comparable tool with which the musician can then reproduce that same effect on the music written during the given period. This of course is an ideal, and although unrealistic as this at times may seem, it should still remain one to which each builder should aspire.

The recent rumour in London is that the early music movement is dead (although recession bound is probably closer to the truth). All of the recording giants have re-recorded all the great 'Warhorses' now, and having refilled their stables with saleable ware, have packed up their bags and are moving elsewhere, perhaps back to the Handel of Nevil Marriner or Von Karajan's Bach. There may remain a few small gems to mop up, but for all intents and purposes the big money is no longer backing period performance on period instruments.

This is obviously not the best of news for the bank accounts of musicians specialising in early music. However it may prove to be a mixed blessing if we lose those who don't care deeply enough.

The last decade began with the gentle trickle of period instrument performance which had turned, by its end, into a torrent. The major record labels watched as sales of tried and true
'Modern' instrument performances were swept away in the fresh tide of historically enlightened Bach, Mozart, Beethoven, and Schubert recordings. Caught with their pants down, suddenly everyone jumped onto the bandwagon, as each and every label needed its own 'house' early band. In a music centre like London, huge numbers of modern instrument players were drafted in to swell the ranks needed to fill the period orchestras, to fill the recording books, to stock the shelves, to play on the CDs in the house that Jack built. These performers were long on technique, but often absolutely ignorant of (and uninterested in learning) period performance practice and any sense of historical style. What was gained with the influx of these players was excellent sight-reading abilities, efficient, cost-effective studio time, and touring bands that could be relied on to produce facsimile CD recording, live, in concert halls the size of aircraft hangers. What was lost was what originally had made period instrument performance so exciting for me, that urgent sense of discovery and intimacy, something crafted by humans who care, and that all important 'le goût' of which Quantz speaks.

Having cynically rambled my way through this list of crimes and misdemeanours without offering any positive suggestions, you may, quite rightly, ask what then can be done to improve the situation. Perhaps the best we can do, as makers and researchers, is to put our own house in order first and trust that musicians will eventually follow. As organologists we must continue to mine all the historical evidence for period performance and present it in a way that is accessible and useful to musicians. We builders can and should re-commit ourselves to making instruments that follow closely the design parameters of those instruments that survive from the past, constantly striving to understand and explain how these instruments differ from their modern counterparts. In many cases we have only begun to scratch the surface of what instruments can tell us about how music was performed and how it sounded in the past. However, given the current market pressures, convincing the majority of musicians of the value of this may be much more difficult to achieve.
SOME FLUTE PITCHES

In Comm No. 1039, Bruce Haynes asked for information about flute pitches. There are several flutes in the Wade Collection of musical instruments in the National Trust's Snowshill Manor (Snowshill, near Broadway, Worcs). A while ago, the Administrator of the property kindly allowed me to check their pitches. This I did using a Seiko TOLV, with fresh battery, accurate according to the makers to better than one cent. Although the actual instruments may not be very exciting, and they are certainly rather late in date for Bruce Haynes, the results may be of some interest. What I have called "A" is the note produced with two fingers covering the top two holes of the flute. All the instruments are in fair to good condition. I do not live far from Snowshill, so if readers not easily able to get there have queries, I could perhaps follow them up.

The instruments (all one key unless otherwise stated) are:

- Sanguinetti  
  A = 430
- Potter (presumably Richard)  
  A = 438
- Astor (79 Cornhill), (8 key, pewter plugs)  
  A = 434
- Anon  
  A = 473
- Wallis (85 St James St)  
  A = 478
- Potter (W.H., Johnsons Court)  
  A = 440
- Clementi  
  A = 440
- Anon (8 key)  
  A = 458
- Delusse (flauto d'amore in B?)  
  A = 302
- Walkingstick Flute  
  A = 415
- Wrede (35, Lower White Cross St., Cripplegate) piccolo  
  A = 439

As regards other woodwind, there are also three bassoons in the collection, two Astors (one with a metal bell), and a Preston, and three 2-key oboes, two Milhouse and an Astor. One Milhouse and the Astor are plain top (Eric Halfpenny's B category) and the other Milhouse cotton-reel and onion top (Eric Halfpenny's C category). There is also a four-key serpent.

There is a thesis which, inter alia, discusses these instruments, a copy of which is held at Snowshill Manor, which bona-fide inquirers are allowed to look at.
FomRHI Comm. 1121
How to design a traverso

Manfred Brach

All data this little study is based on are taken from Jean-François Beaudin's workshop drawings.

Naust (Berlin 2667)

1) primum principium: 12 : 19 : 31

\[
\frac{H}{C + F} = \frac{243.55}{279.0 + 106.6} = \frac{243.55}{385.6} = 0.6316 \text{ i.e. } \frac{12}{19}
\]

MODULUS = \[
\frac{TL}{31} = \frac{629.15}{31} = 20.295 \text{ mm}
\]

<table>
<thead>
<tr>
<th>12 MODULI</th>
<th>19 MODULI</th>
</tr>
</thead>
<tbody>
<tr>
<td>31</td>
<td>49</td>
</tr>
</tbody>
</table>

Naust elected for the starting point the 4. numeros perfectus 496:

\[
496 = 31 \cdot 16
\]

So did in a similar way Thomas Stanesby jun. when designing "a recorder in B flat" (see comm. 1007).

The positions of the fingerholes are fixed with the help of MODULUS.
2) principium secundum: is deduced from the square

\[ h = \frac{C}{\sqrt{2}} = \frac{279}{\sqrt{2}} = 197.2 \text{ (Beaudin: 197.15 mm)} \]

Dividing the centre C according to Divine Proportion the minor will give the length of the foot F.

279 \cdot 0.382 = 106.57 \text{ (Beaudin: 106.6 mm)}

The linear measure Naust made use of is

\[ \text{TOISE} = 324.849 \text{ mm (le pied Parisien)} \]

\[ \text{MODULUS} = \frac{1}{16} \text{ pied} = 20.303 \text{ mm (see above: 20.295 mm)} \]

\[ = 9'' = 3/4'' \text{ (pouce)} \]

\[ \text{TL} = 31 \cdot \frac{1}{16} \text{ pied} = 279 \text{ ligne} = 23 \frac{1}{4} \text{ pouce} \]

\[ = 629.39 \text{ mm (Beaudin: 629.15 mm)} \]

As to the longitudinal structure this Naust-traverso seems to be in the original state. I never have seen a traverso proportioned in such a convincing way like this one.
The proportion 11 : 16 : 27 is deduced from the square (see Herbert Heyde "Musikinstrumentenbau" page 62).
I am not sure how to solve the problem of the linear measure used in that case by Bizey. This is my proposal:

279 ligne (à 2.169 mm) for the total length (605.25 mm)

pied = 312.387 mm
pouce = 26.032 mm
ligne = 2.169 mm

That's not far from "Rheinischer Fuß" (312.585 mm).

Where does Bizey come from? Was he an Alsatian, originally named -- Karl Bizey? I don't know.

But remember the total length of the Naust-traverso: 279"! In either case the flute is proportioned according to 12 : 19 : 31!

C Bizey (Edinburgh 12)
1) \[ 1 - 2 - 3 - 5 - 8 - 13 \]

\[ H : (M_1 + M_2 + F) : \text{total length} \]

\[
\begin{array}{|c|c|c|c|}
\hline
\# & H_1 & M_2 & F \\
\hline
1 & 2 & 3 \\
\hline
\end{array}
\]

\[
[212.4 \quad 424.8]
\]

2) \[ 1 : 7 : 8 : 15 : 23 : 38 \]

\[ (M_2 + F) : (H + M_1) : \text{total length} \]

\[
\begin{array}{|c|c|c|c|}
\hline
\# & H_1 & M_2 & F \\
\hline
1 & 2 & 3 \\
\hline
\end{array}
\]

\[ 251.6 \quad 385.65 \quad 637.25 \]

The proportion 15:23:38 is deduced from the square.

3) \[ 13 : 15 : 28 \]

is deduced from the equilateral triangle (see comm.1007)

\[ H_1 = 15/28 \text{ TL} = 341.38 \text{ mm} \]

(Beaudin: 341.3 mm)
D Bizey (Oxford Bate 106; see comm. 1051)

1) 1 : 2 : 3  
2) 5 : 8 : 13  
3) 2 : 3 : 5

\[ \begin{align*}
\text{H}_1 &= 7/13 \text{ TL} = 7/12 \text{ MH} \\
F &= \frac{1}{\sqrt{2}} \\
M_2 &= \frac{2/3\text{TL}}{} \\
\end{align*} \]

E Quantzflöte (Berlin 5076)

\[ \begin{align*}
\text{H}_1 &= 15/28 \text{ TL} = 354.6 \text{ mm} \\
\text{MH} &= 25/28 \text{ TL} = 354.39 \text{ mm} \\
\end{align*} \]
A STRINGING/TUNING GUIDE FOR THE IRISH HARP

This Comm. describes a graphical presentation of stringing data, applicable to the Irish harp, originally developed to facilitate investigation of the various possible tuning arrangements for a given instrument geometry.

Chart #1 and chart #2 were prepared using the Abbott and Segerman string calculator (see Comm 162) incorporating limits and constraints relevant to the brass strung Irish harp dictated by the forms of the surviving harps and the contemporary references to the instrument. The assumptions and simplifications built into the A&S calculator, of course, also apply to these charts.

The limits and constraints assumed are as follows:

- String material - plain brass wire.
- String tension - constant across the full compass.
- Limits of tone range - C (66) in the bass to f''' (1408) in the treble.
- Limits of string length - 120cm in the bass to 5cm in the treble.

The charts comprise of a series of curves #1 to #14 each representing a plot of constant string diameter at constant string tension, the series of curves covering the maximum tuning range for plain brass strings.

Thus, all the possible tuning arrangements for the surviving Irish harps may be plotted within the boundaries of the charts given by the curves for smallest and largest string diameter, the minimum and maximum string lengths and the highest and lowest tone limits.

To use the charts as a tuning guide, the individual string lengths for an instrument must be known. These are plotted as horizontal lines across curves #1 to #14.

The boundary envelope for the instrument under consideration, which is dictated by the length of the shortest string, is then established. For example, if the shortest string measures 6.5cm, this string may only be tuned within the tonal range of a' sharp to f''', and all of its possible tuning configurations must then lie within the boundaries defined by curves #14 to #8 and the max/min. string lengths.

The various possible tuning arrangements may then be plotted starting with the longest string length and working either down a string diameter curve, across the string diameter curves, from a higher to a lower number, or a combination of the two, until the shortest string length is reached. (Movement from a lower to higher string diameter curve is not considered valid as this would mean an increasing string diameter from bass to treble).

Note that the vertical scale of string length for chart #1 is double that of chart #2 in the interests of clarity.
To use the charts as a stringing guide, the A&S calculator is required to nominate values of string diameter to each string diameter curve for a specified string tension. For those who do not have a copy of the calculator to hand, selecting a string tension of 12kg (a good starting point for the Irish harp) will give the following string diameters for curves #1 to #14 respectively: - 0.48mm, 0.51mm, 0.54mm, 0.58mm, 0.61mm, 0.65mm, 0.69mm, 0.77mm, 0.82mm, 0.86mm, 0.92mm, 0.92mm, 0.97mm, 1.0mm.

The value of a graphical presentation of string data is that it allows the complete tuning arrangement to be easily visualised and revised without becoming lost in a mass of calculated figures.

It is hoped that this Comm. might encourage others to explore the various possible combinations of Irish harp tunings for the surviving instruments and to report on any interesting conclusions in a future Comm. Good luck!

[charts on following two pages]

FoMRHI Comm. 1123
John Barnes

SHORT OCTAVES

I want to apologise for making the bald statement in Comm 1095 that the short octave was not used in Britain. John Bence (Comm 1078) was referring to the C/E short octave, as indicated by his reference to bottom F sharp and G sharp being uncommon, and I was thinking only of this form of short octave.

As far as I am aware, no British stringed keyboard instruments are known to have been made with the C/E short octave. However, the GG/BB short and broken octaves were often used in the second half of the 17th century and at the beginning of the 18th.

ADDENDUM FIGURE TO R. K. LEE COMMUNICATION 1124

THE COIL SPRING IS DESIGNED SO THAT THE PITCH CHANGE OF EACH STRING IS THE SAME AS THE BEST TUNING STRING ON THE HARPSCICHORD
IRISH HARP-TUNING GUIDE
(TREBLE RANGE)
BRASS STRINGING ONLY
FOR USE WITH THE ABOTT AND SEDERMAN
STRING CALCULATOR
CONSTANT STRING TENSION

CHART #1
IRISH HARP TUNING GUIDE
(BASS RANGE)

BRASS STRING ONLY.
FOR USE IN CONJUNCTION WITH
THE ABBOTT & STEFAN STYLE STRING
CALCULATOR.

CONSTANT STRING TENSION.

CHART #2
Recently I acquired a CTS-4 digital tuning instrument. This electronic device allows me to measure the pitch of any note on my harpsichord to within 1/200 of a semitone in a convenient manner. Furthermore, the instrument can be programmed for a variety of temperament systems and stretching conditions as well as allowing transposition over a wide pitch range. I have used the instrument to observe the effects of weather (humidity) changes on my Italian style harpsichord that I built in 1952-54 and that has served me well musically for 38 years. An exception, however, is that it goes out of tune much too readily as the weather and seasons change by as much as a semitone. I have applied an idea suggested by Charles Fisher of Hubbard & Dowd in 1955 who thought of using springs to isolate harpsichord strings from the movements of the case. Fisher applied a large spring to one or two notes of his Kirkman harpsichord, but as far as I know did nothing further with the idea. In this communication, I am reporting the results of a two year trial of my spring system.

The rational approach to improving the tuning stability of a sensitive instrument like an Italian harpsichord is to first measure how it goes out of tune. Figure one shows how my harpsichord went out of tune during the seasonal change from May to August, 1991, when the ambient relative humidity went from 40% to values around 60% over a three month period.

![Diagram showing pitch change with humidity for a 1954 Italian Harpsichord](image)

**Figure 1** 1991 Data
Primo stands for the first rank of jacks nearest the player and designates the right hand course of strings; secundo is the left course. The pitch changes are measured in cents (hundredths of a semitone). The harpsichord goes out of tune very little in the bass, and almost a semitone around C'. A few years after the harpsichord was made, it had deformed greatly because of the wide 65 note range on a traditional very light Italian style structure. In order to remedy this, a light tool steel plate was fitted for the top octave that directed the string tension straight to the wrestplank instead of to the hitchpin rail as before. A byproduct of this experiment shows the amount of improvement that an iron frame would contribute to tuning stability. Finally, a trial set of springs was fitted to the upper Secundo range to allow me to compare the instrument fitted with and without springs in the same environment. It is clear that the springs work very well in improving the stability of the instrument, even better than the addition of the steel plate frame. The difference between Primo and Secundo on the plate is explained by the difference in spacing of the rows of wrestpins on the wood wrestplank. For those who would ask, the steel plate does slightly affect the tone quality of the upper notes; they sound more "electronic", i.e., lacking upper partials that are so characteristic of an Italian instrument. Except for a few difficult to find "zings", the steel springs do not appear to affect the sound of the instrument.

![Graph](image-url)
In the winter of 1991, I restrung the instrument in wires from Malcolm Rose which resulted in a further improvement in sound quality. The instrument is now strung for pitch of $a'=417$ Hertz. In addition, I have fitted out a set of strings with spring rates so chosen as to compensate for the structural deformation of the wooden parts of the instrument where steel strings were used. I felt that the brass strings performed quite well enough without further aid. Figure 2 shows how the same harpsichord performed in the summer of 1992; the figure does not show the difference between Primo and Secundo since they are both now fitted with tailored springs. There is evidence of some instability between strings 50 and 55; I believe that this may be a wrestpin problem.

A very practical matter is how well unisons stay in tune, since that is the type of error that we hear most easily. Figure 3 shows the frequency difference between unisons for the data shown above. Sigma is the square root of the squares of the difference between each unison pair, and the mean difference over the whole instrument (sigma). This number is a measure of the random going-out-of-tune of the unison pairs of strings and may be regarded as a kind of dispersion. Most of the stability problem exists in the treble on the steel plate. Indeed, as shown in figure 4, the dispersion of the treble notes increases linearly with time in an instrument that is left alone. In my winter musical season, I can now go up to this harpsichord almost any time and play it without retuning, or at most touching up a few unisons in the extreme treble where they may have gone a little off.

Figure 3 Performance of Unisons
The conservator may be interested in how much the shape of the harpsichord changes under the increased seasonal tension, and how much the tension changed in the instrument. I have computed case motions and force changes for the data in the 1991 experiment, the calculations being based on the frequency changes of the strings themselves. The displacements include the deflections of both the wrestplank and case together. The motions are of the order of 0.2mm in the wood structure, and 0.05 mm in the steel. Surprisingly, the force changes are very small, compared to the overall force levels, so that there will be little improvement in structural safety if seasonal tension variations were to be corrected by tuning with the season changes. Most harm is created by having the tension present in the first place. If one were to retune a tensioned instrument with seasonal changes, the main effect is to wear out the wrestpins and their sockets in the wrestplank. The displacements and forces from the 1991 environment are shown in figures 5 and 6. I believe that these data prove the value of electronic tuning devices beyond their primary purpose by making it possible to disclose the precise structural state of a keyboard instrument in its environment.

[Addendum figure on p. 28]
Figure 5 Length Changes Millimeters

1954 Italian Harpsichord

Figure 6 Kilograms Force
On Italian Harpsichord Jacks and Sympathetic Strings

This note is in response to Robert Webb's Comm #1109, "More Sympathetic Strings," which raised the possibility that a second 8' register was added to Italian harpsichords partly in order to exploit the tonal benefit of the unplucked choir sounding sympathetically to the plucked choir.

I'm not much of a scholar of Italian instruments and certainly would defer to Messrs Barnes and Wraight, who happily have sent us their responses to such queries as Mr. Webb raises. Perhaps they will again. However, in a recent restoration of a late-seventeenth-century Italian harpsichord disposed 2 x 8', I came upon a number of jacks that were not native to the instrument being restored. These jacks may bear upon Mr. Webb's points.

Before describing these jacks, I might best briefly review the history of Italian harpsichords up to the end of the seventeenth century, traversing the year 1609. This year is cited by Mr. Webb from Ephraim Segerman's Comm 1097, on sympathetic strings, referring to evidence for their first application in stringed instruments.

Italian harpsichords probably had only one set of 8' strings during the earlier sixteenth century, as Mr. Hellwig's seminal article suggests. Before 1609, there were many instruments with a second set of strings. Mr. Wraight has told us that this second set of strings was more likely to be at 4' pitch rather than unison (8') pitch. The conversion of many Italian harpsichords to the disposition 2 x 8' occurred after their primary stringing of 1 x 8', 1 x 4'. I don't know if Mr. Webb has experimented on the tonal addition of a 4' choir to the 8' choir or vice versa. I guess that the unplucked choir wouldn't have much effect on the instrument's sound. The two choirs don't share a bridge.

This remark is not meant to disavow Mr. Webb's points. However, if he wants to historicize the second choir on Italian harpsichords, he might best take account that many if not most of these choirs were tuned at the octave. Another matter to consider is the style of cutting the cloth (or other substance) of the dampers.

Today, most makers cut dampers of felt straight across, so that the jack holding the damper is suspended by the damper's bearing on its string, whether the stop is advanced or pushed back to silence. It's not easy to move from guess to certainty about the first styles of damper cutting, when one is now looking at old dampers. They are a ragged lot.

What would aid Mr. Webb's suggestion would be to find some Italian instruments that had jacks with dampers cut in a rounded tip, so that the dampers would be withdrawn from their strings entirely when the stop was canceled. Then, the unplucked string would resonate sympathetically when its unison was plucked. Better still, it would continue to sound after the key was released and the plucked string was damped. To me, this effect of an "after-ring" is what defines the quality of sympathetic stringing. All early Flemish dampers were so cut, but for different reasons that we won't go into here.

For Mr. Webb's suggestion to gain greatest weight, I think he needs dampers that are surely very old (pre-1609) and are round-tipped, rather than cut flat in the form of "flag dampers." Even better for his main point would be some jacks that had no dampers at all.
Let us move back to the harpsichord mentioned above, as reference to it might advance Mr. Webb's argument. The instrument is unsigned. Its nameboard is modern. Originally it had the compass GG–c⁴. An antecedent of the present owners, who lived in New York, purchased the instrument around 1920 in Florence. What had happened to it deserves more spacious remark than will appear here, as a longish footnote to the story of the Italian instrument dealer and forger, Franciolini.

Unfortunately the instrument did not appear in Franciolini's catalogues, or, at least, I couldn't find it in Ripin's edition of them. Probably Franciolini's sons continued their father's trade of supplying musical instruments to unwary purchasers. Surely by 1920 the market would have shifted from English and German purchasers to those from the USA. What this purchaser got was an unsigned but antique instrument used as decorative furniture. The present owners recall seeing it in the family's front hall, used to receive winter coats from visitors.

The instrument is very handsomely decorated—much effort having been put into the cabriole-legged stand and tastefully-done lid paintings. But the action was hardly attended to. The instrument looked like it would play, but couldn't. The present compass is FF–f⁴. The instrument came with the expected number of 122 jacks. However, the jacks with the instrument came from seventeen different instruments. Some of these seventeen different kinds of jacks were well-made, and many were not. All were certainly antique. In short, the remakers of the instrument before 1920 had a barrel of antique Italian jacks from which to draw bunches of them to fill the slots of the expanded registers.

I presume these jacks were used to suggest that the instrument was a harpsichord worthy of high price, in part because nothing was lacking from it. The slides have fairly wide slots. The uniform jacks that fit well in these slots were apparently original to the instrument. They numbered only 30, about ¼ of the total 122. The rest ran the gamut of the expected and unexpected in jack making. Most interesting for this Comm, there were six that were not of the same family but did share one fascinating characteristic—they lack dampers. That is, they have no damper slots. They are small jacks, pieced out by very small dowels in their ends. They probably came from shallow instruments. Their apparent age suggested to me that they were among the oldest in the jack barrel from which the Franciolinis pulled their wares. Upon seeing them I had to think—who would want an Italian plucked keyboard instrument without dampers? Their brass leaf springs were worn out, but their small tongues still swivelled smartly. A loupe turned up no glue-smears on the sides of their tongue forks. They merely were made without dampers. The six damperless jacks fell into at least two distinct groups, differing in such dimensions as the depth of the tongue-slots. All were short.

My report only is that around 1920, at a workshop in Florence, someone had many, many Italian jacks grouped together. Probably, in order to fill empty slots of a harpsichord being sold as a complete antique, disparate and non-functioning jacks were stuffed into the slots. Six of the 122, coming from at least two earlier instruments, had no damper slots. Possibly they came from instruments affecting a "celestial" sound, to use a later British phraseology. There were four other jacks dimensionally similar to them, outfitted with damper slots. These few jacks form slender evidence for any conclusions. But, I thought upon seeing them that some very early Italian harpsichords might have had one choir of damped jacks and a second of undamped jacks. To my knowledge we haven't seen an instrument so outfitted. Such a harpsichord would certainly give the sound Mr. Webb suggests we attend to.
On Spanish Harpsichord Jacks and Sympathetic Strings

Recently I sent a comm dealing with Robert Webb's suggestion that Italian harpsichords grew a second 8' choir, possibly to exploit a fuller resonance from what he considered sympathetic strings, that is, unison strings not plucked but resonating with energy from the first choir's plucks. Sad to say, I've thought a little more about these matters.

Intarsias of clavichords at the end of the 15th Century (dated from a period before we start finding any dated harpsichords) show keyboard instruments with unison choirs. The 2 x 8' disposition may well be influenced by what was already common to clavichords. Their double stringing, of course, is for mechanical reasons not applicable to harpsichords.

Beryl Kenyon de Pascual published a short piece in the Galpin Society Journal, XXXVIII (1985) on "Diego Fernández . . . ." This builder worked for the Spanish royal family during the eighteenth century. One document she has pulled from the cajas of the repositories is a bill for the extensive work on a harpsichord that Diego repaired in 1743. The instrument (after the repair, and probably before it too) had fifty six keys—cinquenta y seis teclas. Besides receiving a new keyboard, the instrument got 112 new jacks (ciento y doce martinetes), as one would expect. Also, it was completely restrung—with 124 tuning pins and 124 strings (ciento y veintiquatro Clabixas . . . ciento y veintiquatro cuerdas).

Where did the "extra" twelve pins and strings go? Diego made many mechanically very fancy instruments such as those of six registers (Kenyon de Pascual, 45). Could he have refashioned an earlier instrument into one with a single octave of sympathetic strings? The idea of an octave of sympathetic strings along Mediterranean (Iberian) regions is interesting.

Aside from sympathetic stringing, I can come up with other reasons for 124 strings. The worked-over harpsichord might have been like the one Burney mentions being owned by Farinelli, one with a shifting keyboard that would thereby transpose automatically (The Present State of Music in France and Italy, entry for 25 August 1770, as cited by Kenyon de Pascual). If there were six empty jackslots in each register, a player could shift the keyboard to one of many desired pitch levels and reinstall jacks from the other end of the compass, from which their absence would be harmless.

Another possibility, more likely than this scheme of transposing, is to have extra strings to provide alternate tunings. The strings would have to exist on separate levels. To have six per register, from GG to d³, would be too many to vary only D'/E³ over four and a half octaves. Supernumerary notes do appear on Italian instruments, but not as separate strings, as they regularly appear on Flemish transposing instruments. I would expect extra keys for adjusting meantone tuning, not just more strings.

Although harpsichords had shifting keyboards at least from the time of Mueller, 1537, it strikes me as somewhat desperate to say that this possible shifting is more likely than merely saying there were sympathetic strings. And if here, why not in Italy earlier? Are there other references to Italian or Spanish instruments with more strings than jacks? To me, that's the clue to sympathetic strings.
English Organs and Transposition Skills

Nethaniel Tomkins, writing in 1665 about the organ made in 1614 for his father Thomas in Winchester Cathedral said "an open pipe 10 foot long" sounded "double F fa ut at the quire pitch and according to Guido Aretine's scale (or as some term it double C fa ut according to the keys and musics)". The English organ contracts we know of in the 16th and first half of the 17th centuries that specify pipe length all indicate that it is 10 feet for CC or 5 feet for C. So it seems that, besides using the keyboard in what we consider the normal way for solos when the full untransposed range was needed, when playing with singers, church organists played a keyboard on which they assumed that the key lever that we usually call C was F. If we assume white 'naturals' and black 'sharps', b flat was a white key and b natural was a black key. Though this practice certainly started as a fifth transposition, Tomkins seems to indicate that organists did not consider it a transposition but learned it as a fingering to play the notes in on its own right. Let us call this 'quire-pitch fingering' on the organ.

This might seem an unlikely thing to bother to learn, but it parallels an apparently even more unlikely practice that Ganassi (1545) described for the viol. Ganassi wrote that if the music is written in 2 flats, one plays it assuming that the tuning of the instrument is a tone lower. This complete relearning of the relationship between the written notes and the fingering on the instrument transposes the music up a tone to familiar fingerings, probably to facilitate improvisational components of playing, namely gracing and division. It seems that Renaissance instrumentalists were well prepared to learn alternative fingerings for the written notes on their instruments if it offered them a desired advantage.

In the case of learning quire-pitch fingering on a 10 foot CC organ, this of course enabled organists to play at a pitch standard high enough so that high treble boys parts can be written in the treble (G2) clef with a minimum of leger lines, usually none. But this could not have been the only reason, since if it were, replacing the keyboard with one that would give a 10 foot FF (as was specified on English organs just after the Commonwealth and was similar in pitch level to many German organs) would be simpler. I suggest that the 5 foot C specification that was common in Italian as well as English keyboard instruments in the 16th century was often associated with a quire-pitch alternative fingering for the keyboard to provide a comprehensive transposition scheme that covers a fifth of possible pitch levels to match different vocal or instrumental requirements.

The medieval invention of clefs automatically made provision for transposition of a fifth by substituting one clef for another, with the adjustment of adding or subtracting a flat or a sharp in the signature to keep the mode the same. Sixteenth century sources concentrated on the more modern reading skill of transposing by a fourth. On keyboard instruments this is the same as transposing by a fifth in the opposite direction and then shifting back an octave. On viols it involves shifting to the next string, with a special adjustment for the third in the string tuning.

During the 16th century, transposition on instruments by an interval other than a fourth or fifth was considered difficult (voices can transpose by any interval). If one had two keyboards a fifth apart, and exercised the transposition down a fourth from the higher-pitched keyboard and the transposition up a fourth from the lower-pitched keyboard, the pitch levels available are:

1. that of the lower-pitched keyboard,
2. a tone higher than 1, being a fourth lower than the higher-pitched keyboard,
3. a minor third higher than 2, a fourth higher than the lower-pitched keyboard, and
4. a tone higher than 3, the higher-pitched keyboard.

Thus the interval of a fifth between the two keyboards is fairly evenly divided into four pitch levels, available to meet varying vocal needs.

The early 17th century Flemish 'transposing' harpsichord could well have largely been used this way. The C key lever of the upper manual plucked the same strings as the F key lever on the lower manual. If we consider the lower manual as the higher-pitched keyboard, then it would be used as is for the highest pitch level in the range of a fifth catered for, and it would be used for transposing down a fourth for the next-to-lowest pitch level. The latter could be the main reason why this manual extends a fourth lower than the upper manual. The upper manual played as is
would provide the lowest pitch level in the range. A fifth higher would provide the next-to-highest pitch level.

This situation can be simulated on a single keyboard if one learns the added reading skill of playing the keyboard the quire-pitch way, thus having the use of a simulated fifth-higher keyboard to transpose down from. It is possible that the high-pitched German organs covered a similar pitch range the reverse way, getting two other pitch levels by transposing a fourth or fifth lower, and learning the fifth-lower one so thoroughly that it was a fingering on its own right, and so one could transpose up a fourth from it to fill the range of a fifth by easy transposition choices.

With this transposition scheme thoroughly practiced by many keyboard players later in the Renaissance, there was every reason for the pitch levels of the instruments made to be at the lower extreme (in the English and Italian cases) or the upper extreme (in the German case) of roughly the same fifth range of possible pitch levels that would make convenient matches between the music and the vocal and instrumental resources available. The actual pitch levels of accompanying Renaissance keyboards could well be poor guides to the pitch levels of performances involving them. In the 16th century, the value of transposing up a tone if the bass part went too low was well recognised. Schlick (1511) had a mechanism on his organ to shift the manual and pedal keyboards to accomplish this. He stated that being able to do this transposition without such a mechanism was not a widespread skill.

In England c.1600, the pitch level a tone above the 10 foot CC (and a fourth below quire pitch, and which was about 2½ semitones below modern) seems to have been the usual pitch for ordinary Renaissance lutes, adult voices (e.g. in madrigals and congregational singing) and sets of viols. As the 17th century progressed, sets of viols with obligato keyboard or lute accompaniment predominated, and since these ensembles were then called 'consorts', this pitch level was called 'Consort pitch'. While most organs would transpose down a fourth from quire pitch to play at this pitch level, some organs were made at it.

After the Commonwealth, when most new organs were made to a 10 foot FF specification, 'Consort pitch' was still an easy fourth-down transposition (consorts had a few decades of popularity left). The old 10 foot CC pitch of 4½ semitones below modern was still an easy fifth-down transposition, but there was little if any need for it. Organ solos can be played at whatever pitch level provided by the pipes and keyboard, and the available music was not written so high and the performing resources pitched so low that there was any use for this pitch level. What was lost was the facility to play a tone lower than quire pitch (as a fourth above the old 10 foot CC pitch) if the music was too high for the voices available. It is here proposed that this became unnecessary because, by the time of the Commonwealth, the skill of transposing up or down a tone at sight had become so widespread that it had become an expected skill for organists, and provision for organists not having it ceased to be a factor in instrument design.

Later in the 17th century and early in the 18th, new organs were built and old ones rebuilt in a variety of pitches. The highest one, a semitone below old quire pitch, had the advantages of both catering for high-pitched church choirs and, with a transposition of a tone down, playing at the most effective pitch for violin-led string groups. Some new organs were made at this lower pitch and old ones converted to it. This pitch grew in popularity during the 18th century, and by the 19th century almost all English organs conformed to it. Talbot (c.1694) called it 'Chappell Pitch' and in the 19th century it was called 'Concert pitch'.

There was another pair of pitches a tone apart at which organs were made late in the 17th century and early in the 18th. This pair was a semitone lower than the above pair. While the above pair seems to have been strongly influenced by the best violin pitch, this pair seems to have been similarly influenced by the very successful French designs of flute, oboe and bassoon. The best pitch for these instruments, when applied to organs was called 'Consort flute pitch'. The other pitch of the pair, a tone higher, was called 'Church pitch in F'. Later the name 'Consort flute pitch' was shortened to 'Consort pitch' since the old 'Consort pitch' associated with viols, a semitone lower, was obsolete as an organ pitch standard, but continued in use under the names 'Concert or Opera pitch'. This latter pitch could be played at on an organ at 'Chappell pitch' (or the pitch later called 'Concert pitch') by transposing down a tone.
If an organ was pitched at the higher of either of the pairs of pitches for primarily vocal purposes, transposition down by a tone seems to have been expected for mixed vocal and instrumental performances, and if it was at the lower (instrumentally preferred) of each pair of pitches, transposition up by a tone for purely vocal purposes would seem appropriate. By late in the 18th century when most of the pitch diversity was dispelled with the one favouring playing with violins the main survivor, we can imagine that transposing up or down a tone to handle particular vocal problems was a useful but not obligatory practice for organists.

I was surprised that mention of transposition by a fifth is rare in 16th century sources, and that my postulated transposition scheme only includes it in a disguised form, as quire pitch. Transposition by a fourth was the fashionable practice then and seems to have been relied on whenever possible. In the 18th century my understanding of how the pitch standards were used surprisingly doesn't include a function for transposition by a fourth or fifth, implying that transposition by a tone was not just a new skill to add to other transposition skills, but was actually preferred.

In the above, many suggestions about transposition practices are made with no supporting evidence. Critics could say that this is all a load of nonsense because of lack of evidence. This would be true if we had good reason to expect evidence about such practices to exist. Evidence for some transposition practices does survive, as with instrumental parts written at different pitches for various ensembles J. S. Bach worked with. But this is evidence for less widely experienced musicians who had to have transpositions written out for them because they had not yet learned to do them in their heads while reading. Organists usually had wide experience accompanying voices for which transposing at sight was decidedly advantageous, so they wouldn't need written-out transpositions (unless it was for exceptional intervals), and so they wouldn't have left such evidence.

The complete answer to any scholarly question is the sum total of all possible hypotheses that do not contradict the evidence. The seeking of evidence is for the purpose of eliminating possible hypotheses. The question here is what sight-reading skills 17th and 18th century organists routinely possessed. The evidence is rather scarce, but that does not in any way effect the need for postulating hypotheses. In doing this here, I have not shown that mine are the only ones. Subsequent research should both seek new evidence for testing hypotheses and to dream up alternative hypotheses. I hope that others will contribute in both of these ways.

APPENDIX - A Catalogue of Methods of Transposition

Two methods of transposition do not require learning new reading skills and two do:

1. The first of the first type is copying the music into the new key and then reading it normally. An early example of this is the 16th century lute and vellura manuals that instruct the player to transpose the music from the vocal part books (the general lingua franca of the time) by a variety of possible intervals while intabulating it so that it fits comfortably on the instrument.

2. The second of the first type is mentally transposing while reading. The bulk of this Comm concerns these skills in transposing a fifth, fourth and tone. Transposing by an octave needs to be mentioned. On the voice and some instruments such as keyboards, this is so easy that it is often not thought of as transposition. But on instruments tuned in fourths or fifths, this would need learning. Examples of when this has been learned are Agazzari's instruction for continuo violone players to play the bass line on the lower strings (implying transposition down an octave for notes high on the bass clef) and Praetorius's suggestion that it is attractive in combined vocal and instrumental music for the second violin to transpose its part up an octave.

2a. Scordatura tunings on stringed instruments involve compensating changes in string tuning and transposed copying of the music, leading to the intended pitches (but not the usual ones produced) when the music is fingered as written. This was used for violins in the 17th century to get special effects, and for double basses in the 18th century when they were tuned higher for greater soloistic projection.

3. The first of the second type is mentally transposing while reading. The bulk of this Comm concerns these skills in transposing a fifth, fourth and tone. Transposing by an octave needs to be mentioned. On the voice and some instruments such as keyboards, this is so easy that it is often not thought of as transposition. But on instruments tuned in fourths or fifths, this would need learning. Examples of when this has been learned are Agazzari's instruction for continuo violone players to play the bass line on the lower strings (implying transposition down an octave for notes high on the bass clef) and Praetorius's suggestion that it is attractive in combined vocal and instrumental music for the second violin to transpose its part up an octave.

4. The second of the second type involves learning an alternative relationship between the written notes and what one does to produce the actual pitches. This is equivalent to learning a different tuning of the instrument. Examples already given are Ganassi's way of playing music in two flats and quire-pitch fingering on keyboards.
Fascination with the Chekker is Alive and Well

One intriguing byway in the history of musical instruments is the chekker. It was mentioned in written references mostly from the century starting at 1360. The name 'chekker' usually meant 'chessboard'. Ripin (1975) pointed out that it also meant 'eschequer', the special table of the royal financial court, which was a counting board. He suggested that the instrument was called by this name because of similar appearance to the counting table. Page (1979) showed that this is unlikely. Farmer (1926) suggested that the name was derived from the Arabian al-shaqira. Page showed that this was unlikely as well. A chessboard is the only remaining contender for the origin of the name.

If two letters by John I of Aragon written 10 months apart referred to the same instrument, the chekker was a clavicytherium ('upright' harpsichord). This possibility was proposed by Sachs (1940) and seconded by Bowles (1966) on the basis of matching names of unknown instruments on surviving instrument lists with known ones not mentioned. Marcuse (1975) showed that one example of the supporting pictorial evidence was misinterpreted. Page rejected this identification of the chekker on the basis of there being no reason to link the two letters since John was an avid collector of keyboard instruments. Ripin rejected it by questioning the validity of the matching process. He also rejected Galpin's (1940) suggestion that the chekker had a dolce melos (hammer) action and unstopped strings (ie a primitive piano) on the basis of it being a hypothetical instrument. He was convinced that the chekker had to be another name for a familiar instrument. Ripin presented a comprehensive survey of the evidence and concluded that it was a clavichord. The evidence he trusted most was a document from 1488 recording payment for a chekker or manicordion. Page attacked the weaknesses in Ripin's equating manicordion = clavichord = chekker in his 'demolition job' (called this by Shann (1985)). Page's own theory is that the chekker never existed as a specific instrument but was a generic term for any keyboard stringed instrument that was rectangular in shape, that being the shape of a chessboard (I don't understand this because the ones I know are square). If we only allow instruments we now know the structure of (as Ripon did), Page's conclusion points only to the clavichord, in agreement with Ripon.

If I subscribed to 'the chekker' being a generic term, I would concur with Marcuse in preferring the appearance characteristic that earned it the 'chessboard' name to be that the keys were of contrasting colours as the squares of a chessboard, rather than monochrome as keys of keyboard instruments seem to have previously been. But I wouldn't accept it as a generic term unless this leads to a better fit to the evidence, which it doesn't.

Page's conclusion is based on an analysis of the keyboard terminology used by Arnault de Zwolle (written between 1436 and 1466). Page reports Arnault's initial distinction between the clavisimbalum, as an instrument with quilled action, and the clavichordium, as an instrument with tangent action. Then Arnault writes that both instruments can be fitted with a hammer (dulce melos) action, and that the clavichordium can be fitted with a quilled action. In the text, the term 'monochordium' is not mentioned; but on a fragment of parchment inserted into Arnault's thesis, it is stated that a monochordium can have a quilled action. From all this Page correctly concludes that the type of action did not determine the name Arnault used, and that Arnault's clavisimbalum and clavichordium are categories of instruments. From this Page makes the completely unjustified step of concluding that the chekker, which clearly could be (and probably was) an unmentioned member of one category (like the monochordion), had no specific action. He prepared the reader for this by characterising Arnault's terminology as 'flexible'. Page then goes wild, writing "Thus the chekker as it has been formerly understood [my italics] is a myth; it never existed." This is utter rubbish. The phrase in italics refers to modern expectations that a name was associated with a single type of action. If one accepts this statement, then every keyboard instrument that Arnault mentioned is a myth and never existed. The only myth about the chekker is the one that Page has created.

It is highly significant that fitting a tangent action to a clavisimbalum was not mentioned by Arnault. Obviously the object that strikes the string to get it vibrating is not the naming criterion. Page could imagine no other criterion than the shape of the case. He missed the difference that in the clavichordium the string is both stopped and sounded, while on the clavischimbalum the string is sounded without being stopped. The variations from the standard instruments mentioned only involve the method of sounding of the string. Whether the string is stopped when sounded is what is important (and consequently whether the number of strings or courses can be less than the number of notes). Since a tangent cannot sound a string without stopping it, it cannot be applied to a
clavisimbalum without changing its name. It is easy to conceive of a gadget on the end of a key lever that will include a tangent to stop the string as well as a component to pluck or hit it (though much harder to conceive of ones that would be relatively trouble-free and so last). The action diagrams that Arnault provided are quite overcomplicated according to more recent ideas as to what is practical.

The distinction that I assume Arnault was making is the same as Ripon's understanding of that made by Gerson (1424) (but argued against by Page (1980)*. Gerson's list of instruments of each type includes the chekker and various types of monochord amongst those whose strings apparently were stopped. This interpretation of this list rules out calling Arnault's clavisimbalum a chekker, no matter how one interprets Arnault's classifications. Gerson's criterion can explain why monochords and clavichords were often classed together and yet could be distinct.

Gerson wrote a treatise about interpreting 'the mystical chekker' illustrated by several drawings of a chessboard with various virtues and vices written in one or two columns of squares on the extreme left and right of each diagram. Just below the bottom row of squares are short vertical lines below which is stated that these are keys for sounding the strings of the chekker or 'struck by the fingers of meditation'. Both Ripin and Page dismiss these illustrations as allegorical. This may well be true, or they may be quite realistic. One cannot rule out the possibility that the chekker was useable both as a musical instrument and as a board for playing the game. The chekker could then appear in surviving illustrations but not be recognised for its musical-instrument character. Multi-purpose instruments were popular in the 15th century (see Bulletin Supplement 63). Objects that have a non-musical use as well as a musical one can be attractive too (whenever I see old walking sticks for sale I can't help (vainly) looking for one that is also a flute).

The full answer to any scholarly question is the sum total of all of the possible different answers that don't violate the evidence. The possibilities for the characteristics of what the chekker might have had that I can think of are listed below, with what was called by that name not necessarily being only one or the same at different times: The strings must be stopped. This could be by a simple tangent, a tangent plus a plucking device or a tangent plus a hammering device. In all of these the tangent can stop the string in mid air or knock it against a fingerboard (which could be smooth or have raised frets). The reason for the name could be that the keys (of normal keyboard type) were of contrasting colours, that the top of the instrument was traditionally inlayed with a chessboard, or that the keyboard was a chessboard. Or Gerson's diagrams can be taken literally and keys plucked or hammered strings while pressing chessboard squares stopped them (involving two fingers or two hands per note as in fingerboard instruments). This is the simplest possibility.

REFERENCES:

S. Marcuse, A Survey of Musical Instruments (1975), pp 242-4
N. Meeus & C. Page, Early Music 5/2 (1980), pp 222-6
C. Page, Early Music 7/4 (1979), pp 482-9
E. M. Ripin, Galpin Society J. XXVII (1975), pp 11-25; also 'Chekker', New Grove DoMI
C. Sachs, A History of Musical Instruments (New York, 1940), pp 336-7
R. Shann, FoMRHI Q 39, Comm 614 (1985), p 49

* What Gerson wrote, in Ripon's translation is: "'Chordae', according to the glosses, are placed before any other instrument – other than the psaltery (psalterio) and harp (cithara) – that sounds by vibrating strings, be it fiddle (viella), hurdy-gurdy (symphonia), lute (myra), crwth (rota), gittern (guiterma), lituus, nabla, cittern (sistrum), chekker (scacarum), rebec (rebeila); be it even monochords in general or indeed the dichord, trichord, ..., decachord, and so on to the limit of string segments (termino chordarum) in instruments made in the past or even in the future." Page objects to Gerson implying any distinction other than that the words psalterio and cithara "have already been used in the same psalm". I counter with Arnault's distinction: his names differ by 'chord' vs. 'simbal'. Page's main objection otherwise is that Steger showed (with c.1100 evidence) that the rota was a vertically-held triangular psaltery, which didn't have stopped strings. I support Ripin's crwth by claiming that if this was the instrument Gerson wanted a latin name for, he would have picked 'rota'. My reasoning is as follows: The crwth (or crowd or chrota) had the latin name 'chorus' but also had 'rota' as a linguistic cognate. Gerson knew and described a string drum also known as 'chorus', which wasn't it, so 'rota' had to do. By his time the 'true' rota was almost unknown.
Tempos and Time Signatures in 17th Century England

Praetorius's comment (1619) that 160 breves (tempora) go by in 15 minutes at a moderate speed calculates to crotchet = 35 MM, or on average, it takes 2.8 seconds for a semibreve. Mersenne (1636) assumed 1 second for a semibreve to illustrate the principle of tactus, but he added that musicians take 2 seconds (crotchet = 120 MM) or 4 seconds (crotchet = 60 MM). I applied the fastest speeds of playing as given by Mersenne (1636) and Quantz (1752) to survey the occurrence of demisemiquavers in Robert Dowland's Varieties of Lute Lessons (1610) and the Fitzwilliam Virginal Book (c.1614), calculating that the semibreve takes about 3 seconds in the almain (crotchet = 80 MM) and about 2½ seconds in the galliard (crotchet = 98 MM). We have reason to expect that tempi of the galliard and pavin pair and of the coranto and almain pair were each approximately related by the proportion of 3 to 2, so the semibreve would take about 3½ seconds in the pavin (crotchet = 65 MM) and 2 seconds in the coranto (crotchet = 120 MM). Combining both duple-time and both triple-time dances, the duple-time semibreve took about 3 to 4 seconds (crotchet = 60 to 80 MM), while the triple-time semibreve took about 2 to 2½ seconds (crotchet = 98 to 120 MM). Praetorius's average over music including both duple-time and triple-time passages, at 2.8 seconds for the semibreve, is in the middle between the two ranges, as one might expect.

Relating these tempi to time signatures presents a few complications. In duple time the usual time signatures were C and EF, and their usage varied amongst scribes and repertoires. When the time-signature EF first appeared in the first half of the 15th century, it meant that a part is notated in double the note values that it would be if it were notated in the duple-time tempo standard C. Each note value goes by twice as fast as in C. This usage of EF continued uninterrupted from that time onwards till at least the 19th century. It was the most common time signature and usage late in the 15th century and early in the 16th century. Praetorius (1619) mentioned that the Italians called it 'alla Breve', and Morley (1597) called it 'breve-time'. By their time, its use was largely confined to motets and other religious works.

There is 17th century English evidence for the tempo of minims in breve time. The Tenbury copy of Musica Deo Sacra by Thomas Tomkins (1572 - 1656), published by his son Nathaniel in 1668 (apparently as a memorial), included a comment defining the pitch and tempo at which the music had been performed. The pitch part of the comment, that a 2½ foot pipe sounded f, is related to another of Nathaniel's statements about his father's music that motivated another Comm in this Q. The tempo part indicates that the pulse or the swing of a 24 inch pendulum corresponded to a minim, with four minims in the measure. The length of the measure that we can calculate from this pendulum is 3.1 seconds (minim = 77 MM). This minim rate corresponds with a fast normal pulse.

After mentioning the breve-time usage of EF, Thomas Morley added "that rule be not so generally kept but that composers set the same sign before songs of the semibreve time". This was the most common usage at the time, making EF equivalent to C. Some theoreticians condemned the ambiguity between the breve-time and semibreve-time usages, but if composers or musicians had found it troublesome, they would have introduced another sign to remove the ambiguity, and they didn't. The main purpose for writing either sign was apparently to distinguish between duple and triple time, and indeed in many sources, no time signature at all for duple time accomplished this. Most manuscript sources (especially those by a single composer or arranger) consistently used only one of these three possibilities to notate duple time. Very many printed sources have C and EF indiscriminately mixed amongst different parts that play together. This could easily result from the printer not having enough of the usual EF type blocks available, so he mixed in C blocks to stretch his supply, knowing full well that this would not introduce any ambiguity.

From the 15th century onwards, the time signature EF was used to indicate twice the speed of EF for each note value. In the repertoire that used the breve-time EF, it was used this way, as C was used to indicate half the speed of EF. In the repertoire using semibreve-time EF (equivalent to C), it seems to have sometimes been used for a particularly fast version of EF, apparently following the symbol usage of Adam of Fulda (1490), where the speed increases by 4/3. The evidence for this is that it appears for all of the almains in the Walsingham and Morley Consort sets. In sources that use this sign, we should expect that EF represented the slower portion of the range of tempi that it represents in the majority of sources that use it for both pavins and almains. This should be
considered when choosing the tempos of duple-time songs in Dowland's *First Book,* where the last song uses $\Phi$. Another application is that since the 'Spanish Measure' in the Walsingham set of Consort music uses $\Phi$, this is evidence that the 'measure' dance was faster than the pavane.

Because semibreve-time $\Phi$ was faster than $C$ in Purcell's time, one naturally looks for evidence for this early in the 17th century as well. If this was the case for some sources, it would complement the above situation with $\Phi$, having $C$ to represent the slower end of the tempo range covered by $\Phi$ in most other sources. Such evidence would be in a source which consistently uses $C$ in all parts for some pieces and $\Phi$ for all parts in others, and it is not a compilation of pieces copied from other sources where it might suffer from copying the time signatures as well without conversion to a 'house style'. I haven't noticed such a source, and it may even not exist.

There is more variety in the time signatures for triple tempi in this period. Since many sources used the sign 3 for every triple-time piece in them, including corantos as well as galliards, it had a generic significance. When 3 appears in the middle of a piece, it means three notes take the time that two of the same time value previously took. A 31 means three crotchets (in black notation) take the time of a white (or normal) minim. A 31 also means that three semibreves or minims take the time of one (I haven't noticed this in the instrumental music which I am concerned with). The time signatures $\Theta$, $\Theta3$, $\Phi$, $\Phi3$, $C$, $C3$, $\Phi3$ and $\Phi3$ all theoretically signify that there are three minims in the usual semibreve tactus or measure, which was standard galliard tempo (the tactus beat of $\Phi3$, being in dotted minims, is the same as the duple-time minim). Signs supposed to mean twice as fast are $C6$ and $\Phi3$. The differences between the various signs for galliard tempo (often called 'galliard time' then) relate to different theoretical paths through which they were derived and to whether there are twos or threes in time values longer than the semibreve, none of which has any relevance to tempo. A time signature $C$ means that the minim tactus beat is the same as in duple time, and $\Phi$ is supposed to mean that it is twice as fast. In the latter, there often are three black notation crotchets in each of these beats, making the tempo the same as $C6$ and $\Phi3$.

One finds demisemiquavers in keyboard music with a $\Phi$ time signature and three crotchets per minim, so the crotchets can't be faster than in galliard time. If the theory was followed, they would go twice as fast. The most likely violation of the theory was in the relationship between $\Phi$ and $C$, and the duple-time practice of having them equivalent applied. Then this $\Phi$ is equivalent to $\Phi3$ in tempo (but not in notation). It is likely that the relationship between $\Phi3$ and $\Phi3$ also followed duple-time practice, $\Phi3$ being somewhat faster than $\Phi3$, rather than twice as fast. It would be surprising if any tempi early in the 17th century was faster than the fastest tempo late in the century.

There probably were subtle differences in performance between pieces using one galliard-time sign and another in some sources, but these are very difficult to discover now. One occasionally finds a sign $C$ for a galliard or a galliard-time sign for a coranto, which presumably means that these pieces are in the form given (beaten by three minims in the galliard and three crotchets in the coranto), but are played slower than the dances usually were danced to.

There is late 17th century evidence from Simpson (1665) and Purcell (1694 ed. of Playford) relating tempo to the pulse, the 'regular motions' or 'little minutes' of a watch, and the 'slow motions' of the pendulum of a large chamber clock. There is also evidence from Mace (1676) involving a pendulum from the ceiling to the floor. The watch and chamber clock evidence is ambiguous between swings (of the watch balance wheel or the clock pendulum) and returns. But the 'returns' interpretations are consistent with the other two types of evidence, which have no ambiguity, and so these are chosen. I have previously reported my analyses of 17th century evidence with respect to duple-time tempi (Comms 753 and 862 and in *Companion to Medieval & Renaissance Music* (1992) ed Knighton and Fallows), and so shall here only summarise it in a table. Also included in the table are triple-time tempi deduced from this evidence, and below the tempi calculated from the evidence are corresponding information from other English sources of the time.

The Playford information from the 1654 edition is not consistent with that from the 1674 edition. The 1654 statement that the name 'Three in One' referred to the faster of the two triple tempi mentioned was changed in the later edition to refer to the slower one (ie the ambiguity in the meaning of 31 was resolved the other way). But an error was introduced by having 'Airy Songs and Galliards' apply to both tempi instead of just the slower one. A combined version including the later meaning of Three in One and the corrected error is included in the table.
Some of the evidence involves relative verbal comparisons between tempi. A remarkably consistent picture emerges if we make these verbal comparisons quantitative according to the following scheme: Quantz (1752) stated that a difference in tempo of 5 notes per minute out of 80 (6.25%) is 'imperceptible' (of course this applies to different tempi taken at different times, not simultaneously). Thus a difference of 10% is a reasonable estimate of what is perceptible enough to comment on by saying 'almost as' fast or slow. In the spirit of the Expert Systems in the currently fashionable Information Technology (and without involving psychoacoustic experiments), I propose that a difference of a quarter (25%) corresponds with the verbal statement of 'a little' slower or faster, and a minimum difference of a third (33%) corresponds with an unqualified 'slower' or 'faster'. Tempi estimated by means of this scheme in the table are enclosed by square brackets [ ]. These estimates work well relating statements from sources listed where no direct evidence of absolute tempi are provided.

The left edge of each entry in the table is placed roughly according to the logarithm of the tempo it represents, so the same horizontal distance between entries always corresponds with the same proportion or ratio of tempi. These are estimated mean values for each defined tempo and include no direct information on the range of tempi within each of these. In cases such as when Purcell defined two tempi that are covered by one of Playford's, the Playford entry is in the middle between the Purcell entries though they most probably cover the same total range.

The first line of each entry includes the time signature given and the note value that the tempo refers to. The second line includes the metronome marking deduced from the evidence and the nature of that evidence. Subsequent lines include other information and tempo usage in italics.

In summary, here is collected independent 17th century evidence from Praetorius, fastest notes in English dances, Tomkins, Simpson, Mace and Purcell, all fitting into a consistent picture of tempos used. It is extremely unlikely that competent scholarship will ever come up with another picture that is significantly different. In duple time, the tempo standard was constant throughout the century, with the measure (a semibreve in semibreve-time and a breve in breve-time) equalling four pulses, a pulse taking \( \frac{3}{4} \) to 1 second (60 - 80 MM). Earlier in the century, the low and high regions of this tempo range were occasionally differentiated with different signs, and at the end of the century Purcell did the same. There was a sign used later in the century for double this tempo. The most commonly used triple tempo (galliard time) had three minims to the same standard measure early in the century. This tempo was used later in the century as well, but the most popular triple tempo was faster, with four minims per standard measure. In the last quarter of the century, it was notated with crotchets rather than minims, the same note value for the same duration as in duple time. A triple tempo at approximately double its speed was the fastest, used for jigs and the like.

A tempo between these two was used for dances such as the coranto and volte, and apparently little else till late in the century.

The existence of tempo standards does not inhibit the varying of tempo according to the nature of the text or of the music or other factors authors mentioned. My guess is that a variation of 25% from the mean of the standard would not be uncommon for this. Much more than this would probably be unexpected, but not unknown. Every rule in an Art is there to be broken for particular artistic effect, including surprise. For this effect to work, it must be exceptional.

There is a fast-pulse tempo standard in modern popular music. 'Serious' music has rejected tempo standards for some time. A letter from Beethoven to his publisher Schott in 1826 stated "we can hardly have any tempi ordinari any more, now we must follow our free inspiration". Those of us who have most of our experience in 'serious' music accept such free inspiration as a matter of course. This paper should help us realise that when we exercise it in our performances of 'early music' (as most feel we must to be true to our own musicality and to the expectations of our audiences), the character of a piece is unlikely to resemble that which the music had when composed. Modern tempi for 'early music' make greater demands on virtuosity and fewer demands on our improvisational skills, imagination, knowledge and musicality than original tempi. Since modern tempi are musically highly successful (in modern terms), and it is not obvious that original tempi will lead to greater success, little interest in exploring original tempi exists. Original tempi may not be important in 'early music', but they are very important components of music history.
### LATE 17TH CENTURY ENGLISH TEMPI

#### Common Time

<table>
<thead>
<tr>
<th>Composer</th>
<th>Tempo</th>
<th>Description</th>
</tr>
</thead>
<tbody>
<tr>
<td>Simpson</td>
<td>1665</td>
<td>C or ( \text{C or } \frac{3}{2} ): crotchets, 70–80: lively pulse</td>
</tr>
<tr>
<td>Mace</td>
<td>1676</td>
<td>C or ( \text{C or } \frac{3}{2} ): crotchets, 60: pendulum from top of ceiling to floor</td>
</tr>
<tr>
<td>Playford/Purcell</td>
<td>1694</td>
<td>C: crotchets, ( \frac{3}{4} ): crotchets, 60: ‘slow motions’ of pendulum of large chamber clock</td>
</tr>
<tr>
<td>Playford</td>
<td>1654/74</td>
<td>( \frac{3}{4} ): crotchets, Songs, Anthems, Fantasies Pavans, Almans</td>
</tr>
<tr>
<td>Purcell</td>
<td>1696</td>
<td>C: crotchets, ( \frac{3}{4} ): crotchets, very slow, little faster</td>
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</tbody>
</table>

#### Triple Time

<table>
<thead>
<tr>
<th>Composer</th>
<th>Description</th>
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<tbody>
<tr>
<td>Simpson</td>
<td>1665</td>
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<tr>
<td>Playford</td>
<td>1654/74</td>
</tr>
<tr>
<td>Playford/Purcell</td>
<td>1694</td>
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<tr>
<td>Purcell</td>
<td>1696</td>
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**Tempo (MM)**

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<tr>
<th>Tempo</th>
<th>50</th>
<th>60</th>
<th>70</th>
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<th>150</th>
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<tr>
<td>C or ( \text{C or } \frac{3}{2} ): crotchets</td>
<td>70–80: lively pulse</td>
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<tr>
<td>( \frac{3}{4} ): crotchets</td>
<td>([75]: \text{little faster})</td>
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<td>( \frac{3}{4} ): crotchets</td>
<td>([135]: \text{almost as fast as regular motions’ of watch})</td>
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<tr>
<td>( \frac{3}{4} ): crotchets</td>
<td>( \frac{3}{4} ): crotchets, twice as fast as ( \text{C or } \frac{3}{2} ): crotchets</td>
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<td>( \frac{3}{4} ): crotchets</td>
<td>( \frac{3}{4} ): crotchets, brisk and airry</td>
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<td>( \frac{3}{4} ): crotchets</td>
<td>( \frac{3}{4} ): crotchets, twice as fast (1654) swifter motion (1674) Corants Sarabands and Jigs</td>
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<td>( \frac{3}{4} ): crotchets, 31: crotchets very brisk Jigs</td>
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<tr>
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<td>( \frac{3}{4} ): crotchets, 3: crotchets brisk Jiggs and Paspys</td>
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<td>( \frac{3}{4} ): crotchets</td>
<td>( \frac{3}{4} ): crotchets, 3: crotchets faster</td>
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**Notes:**
- Tempo (MM) refers to the beats per minute (BPM) for each musical style.
- Tempo (MM) values range from 50 to 150, indicating different levels of speed or pace in music.
Duple Tempi and Time Signatures in the 16th Century

The duple-time tempo picture at the beginning of the 16th century is remarkably similar to that at the end. Both the beginning and end used a time signature \( C \) with a semibreve tactus (semibreve time) as well as a time signature \( \text{f} \) with the same tactus but covering the duration of a breve (breve time or alia breva), where each note value goes by twice as fast as in \( C \). Also used was a semibreve-time \( \text{f} \), but while at the beginning of the century, the note values went by at the same speed as in breve-time \( \text{f} \); at the end of the century, they went by at relatively half that speed, the same as that of \( C \). I know of no interruption in the use of semibreve-time \( \text{f} \) or breve-time \( \text{f} \) during the century (as there was in the use of \( C \)), so it is reasonable to assume that the tempo paths of the two \( \text{f} \)'s diverged in some way during the century.

Before considering the nature of this diversion, it is appropriate to attempt to fix the relative tempi of \( C \) at the beginning and at the end of the century. The tempo at the end has been determined with a high degree of confidence in the Comm on 17th century tempi, and so only the tempo at the beginning needs to be determined. Authors of the 17th and 18th centuries mentioned matches between tempo and the rate of the pulse. Authors writing c.1500 related tempo to the pulse generally but clearly avoided any match of tempo standard with pulse rate. This is likely to be because they didn't match. The worst match would be if the average pulse rate and some note value in the tempo standard were related by a factor of \( \sqrt{2} \). This gives a tempo of about 100 MM, so a fast pulse of 80 is 20% slower and double a slow pulse, at 120 MM, is 20% faster. We now need to find which note value corresponded with this speed at c.1500.

At the end of the century the semibreve took the time of 4 pulses (averaging 3\( \frac{1}{2} \) seconds). For the tempo at the beginning of the century, let us first consider the possibility that 100 MM corresponded with the quaver, with a \( C \) semibreve taking 5 seconds. Then during the century \( C \) and breve-time \( \text{f} \) speeded up by a factor of 1.4 and semibreve-time \( \text{f} \) slowed down by the same factor. If it corresponded with the crotchet (with a \( C \) semibreve taking 2\( \frac{1}{2} \) seconds), then \( C \) and breve-time \( \text{f} \) slowed down by a factor of 1.4 while semibreve-time \( \text{f} \) slowed down by a factor of 2.8. If it was a minim (with a \( C \) semibreve taking 1\( \frac{1}{2} \) seconds), \( C \) and breve-time \( \text{f} \) slowed down by a factor of 2.8 while semibreve-time \( \text{f} \) slowed down by a factor of 5.7. This last possibility, which is the most attractive to modern musicians and musicologists, is most unlikely since slowing down by such extreme factors is unprecedented in the history of music, and is most difficult to explain. Of the first two, the second (with crotchet - 100 MM) is perhaps more likely because there is precedent for the slowing down of the tempi for each note value, but not for speeding up.

Now we can discuss the background to the diversion of \( \text{f} \)'s. Probably the main reason why breve-time \( \text{f} \) was developed and grew in use in the 15th century was a distaste amongst singers for reading fusae (quavers). According to Apel (1953) the Flemish-era music was practically always written in note values from breves to fusae "with the latter value being used only in groups of two as a quick 'cadential mordent'". This was a reading limitation, not a speed limitation, and so using a stroked time signature made quavers into pleasanter-looking crotchets and 'unreadable' semiquavers into just-tolerable occasional quavers. By the end of the 15th century stroked time signatures were more commonly used than unstroked ones, reflecting greater use of the smallest time values. During this period there also was a shift from most music being written in triple time to duple time. So early in the 16th century the breve-time \( \text{f} \) was the most popular time signature used. Semibreve-time \( \text{f} \) had the same tempo but was beaten in minim (rather than semibreve) hand movements. Ornithoparchus (1517) wrote that semibreve-time \( \text{f} \) (called 'lesser' or 'half' tactus) was "allowed of only by the unlearned" and Glareanus (1547) wrote that it was "doubtless easier on the students".

Throughout the 16th century the breve-time \( \text{f} \) maintained its respectability and use for sacred music, but it was eventually abandoned in favour of semibreve-time \( \text{f} \) (and \( C \)) for other types of music.

The fortune of \( C \) was more varied. In the first quarter of the 16th century it lost the little popularity in practical music-making that it still had left. Agricola (1528) didn't even mention it. But the theorists could not forsake \( C \), needing it to provide the tempo standard for proportions to relate to. Switching to \( \text{f} \) would be questionable because it was not properly in the proportion system. Every theorist who numerically described how fast \( \text{f} \) was said that it was twice as fast as \( C \) (others just said it was faster, but never a 'little' faster before the end of the century). Yet many
did not consider the 'twice' as a proper proportion, as its rarely-used equivalent $C^2_i$ was. It was twice as fast from a practical but not theoretical point of view, thus implying approximation (except that $C$ was used proportionally when combined with other time signatures for different voices in one piece).

In the 1540's a school of Italian madrigalists reintroduced $C$, making a point of mainly using crotchets quavers and semiquavers, calling it 'note nere' (black notes). The same school retained breve-time $\text{MISSING SYMBOLOGY}$, calling it 'white notes' notation, and apparently rejected semibreve-time $\text{MISSING SYMBOLOGY}$. This rejection could have been to reform notation to bring practice back into conformity with theory.

I suggest that when $\text{MISSING SYMBOLOGY}$ was the only sign for duple time during or before the second quarter of the 16th century, the range of tempi that the semibreve-time $\text{MISSING SYMBOLOGY}$ represented broadened downwards to include the original $C$ range. The reform of the Italians can perhaps have intended to replace the slow-speed semibreve-time $\text{MISSING SYMBOLOGY}$ with the theoretically-correct $C$. The reform succeeded in making $C$ an alternative for slow-speed semibreve-time $\text{MISSING SYMBOLOGY}$, but failed in the objective of replacing it. At some time in the fast end of the tempo range of semibreve-time $\text{MISSING SYMBOLOGY}$, it no more included the tempo range of breve-time $\text{MISSING SYMBOLOGY}$. This could have been quite early in Spain since in his vihuela book Narvaez (1538) used $\text{MISSING SYMBOLOGY}$ for his slower duple tempo while he used $\text{MISSING SYMBOLOGY}$ for his faster one (in 1546 Mudarra reintroduced $C$, but for the vihuelistas it represented a moderate tempo in between the other two). The reduction in the fastest tempo that semibreve-time $\text{MISSING SYMBOLOGY}$ could represent probably happened at different times in different places for different genres. If there was an ambiguity then as to which type of $\text{MISSING SYMBOLOGY}$ was meant, musicians would have invented a distinguishing mark. Since they didn't, it was clear to them for that genre in their time and place, though it may not be to us now. An analysis of the fastest notes notated could help sort this out, but it will be complicated by the slowing of tempo and the growing comfort in notating faster notes during the century. In late Elizabethan England semibreve-time $\text{MISSING SYMBOLOGY}$ and $C$ seem to have been completely interchangeable, but in Italy then, as Praetorius reported, semibreve-time $\text{MISSING SYMBOLOGY}$ tended to be somewhat faster than $C$.

It would be appropriate to speculate on what mechanisms could have produced the postulated changes. The comment by Ornithoparchus associating semibreve-time $\text{MISSING SYMBOLOGY}$ with the 'unlearned' points to strong amateur involvement. We know that the second quarter of the 16th century saw an enormous increase in amateur activity with musical instruments. Most instruments such as the recorder, flute, fiddle and viol had just sprouted sets of varying sizes and tunings, and the popularity of the lute went from strength and strength. The available vocal music was the main source of repertoire for the sets and for lute intabulations. Amateurs usually have restricted experience with music, and so we would expect them to have a less developed sense of what is the 'correct' tempo. Besides, they were working hard solving problems of reading (possibly involving transposition as well), technique, ensemble and attempting ornamentation (this is the period of Ganassi's recorder and embellishment book), so slower tempi should be expected. Lute intabulations of vocal pieces generally used the note values of the vocal originals, but usually added quick notes one note value smaller than the original. Without these added notes the texture could seem too bare on the instrument, but once they are added there is a tendency to broaden the tempo to integrate them musically into the piece.

A more general point is that the abandonment of $C$ by the vocal composers left vacant an ecological niche of slow tempi that amateur and other instrumentalists valued and occupied in the only ways they could, either by slowing semibreve-time $\text{MISSING SYMBOLOGY}$ down or dispensing with time signatures completely. Their efforts were very popular, and so the vocal composers in the more popular veins soon followed the instrumentalists in using the slow semibreve-time $\text{MISSING SYMBOLOGY}$. The theorists could well have ignored this development when continuing to write that $\text{MISSING SYMBOLOGY}$ was double the tempo of (or at least faster than) $C$. Being of the ecclesiastical establishment, only serious vocal works would be worth their consideration.

The picture of 16th and 17th century tempi presented here constitutes an excellent fit to all of the original evidence that I am aware of. Most of this evidence has been readily available for a long time, but it has been ignored, misinterpreted or ridiculed by the few musicologists who considered it. It apparently conflicted with the tempi dictated by their musicianship, so they projected their own confusion onto the evidence, claiming that it was confused. Now that it is analysed and shown to be consistent, musicologists (and musicians who are interested) should reconsider the question of original tempi as essential and determinable factors in early performance practices.
Appendix: Gaffurius's 'Breathing' and Neusidler's Counting

The 'Tactus' entry in the New Grove Dictionary by H. M. Brown states that Gaffurius (1496) wrote that "one tactus equalled the pulse of a man breathing normally, suggesting that there was an invariable tempo then of MM = c.60 - 70 for a semibreve in integer valo of. This is most probably in error. Bonge (1982) has pointed out that Gaffurius was ambiguous in relating the pulse and tempo. In one passage Gaffurius discussed how physicians considered the pulse as the basic unit of time measurement in medicine, and that it was composed of two components of equal time each, dilatation and contraction. In a second passage, he mentioned that modern musicians considered the regular semibreve in a similar way, as the basic unit of time measurement in music, also composed of two components of equal time each (minims). In a third passage he stated that a dissonance in counterpoint cannot last as long as a regular semibreve, a full measure of time, namely "in modern scilicet pulsus aequi respirantis". Bonge translated the phrase left here in Latin as "in the manner of the pulse of [someone] breathing evenly". This is the most obvious and direct interpretation of the Latin, and essentially agrees with the readings of Brown, Sachs and others.

Bonge's point is that the grammar of these passages strongly implies analogy between the regular semibreve and the pulse rather than equality. This point is strengthened by the comparison being between the semibreve and the full pulse by Gaffurius, and between the semibreve and half the pulse by Ramos (1482) in a similar passage. If equality were implied rather than analogy, then there would have been a disagreement, and this passage was not commented on in the margin of Gaffurius's copy of Ramos's book, as other disagreements were.

The problem with Gaffurius's third passage is why he mentioned breathing at all. The reference to breathing here is unique in the literature on tempo. Brown's 'breathing normally' could imply a normal rate of breathing, but the rate of the pulse is not referred to in any of the passages, only equality between the two halves. Medicine then and now has recognised a close correlation between the pulse and breathing, both going up and down in roughly the same proportion (of about 4 to 1 in rates) as a result of emotion, exertion and illness. Almost every time characteristic of one is mirrored in the other. When for medical reasons the pulse is even or uneven, so is the breathing. Nevertheless, during speech, the breathing is uneven while the pulse remains even. So breathing evenly says no more (and perhaps less) about the pulse than observing the pulse directly. Consequently, there seems to be no motivation for introducing breathing into the discussion according to the obvious and direct translation of the Latin given above.

Young (1969) realised this and has suggested that the use of the word 'breathing' in this passage was as an analogous description of the dilation and contraction (or 'breathing') of the pulse. In Companion to Medieval and Renaissance Music (1992), following the comment by Fallows in his New Grove article on 'Tempo and Expression Marks', I suggested the opposite, interpreting the use of the word 'pulse' as an analogous description of the dilation and contraction (or pulse) of breathing. 'Pulse' would mean both the process of pulsation and that a complete cycle is involved, as it is with the pulse. The motivation according to my interpretation is that in this passage Gaffurius was interested in a practical situation rather than in the theory of the other passages, so he had rather more reason to be specific about the real tempo involved, and he switched to breathing as the analogy here because it actually corresponded much more closely to the regular semibreve tactus than the pulse did.

By a 'regular' semibreve, Gaffurius surely meant a semibreve not reduced in length by a canon or proportion, or by a stroke in the time signature. In fact, a 'regular' semibreve was not the usual one at that time. The most common practice of composers then was to reduce the time of a semibreve by a stroke in the time signature, by 1/4 to 1/4 in $\Phi$ and by 1/4 in $\phi$. I presume that no theorician could accept a diminution as a tempo standard, especially if it is variable (as was the case with $\Phi$), no matter how popular it was. The possibility of confusion between the regular semibreve and the usual one could have been the reason why Gaffurius introduced the breathing rate for further clarification.

It now remains to show that this possible interpretation of what Gaffurius wrote is the most likely. The only other source from this period besides Ramis that mentioned the pulse was Lanfranco (1533). He stated that tempo was governed by raising and lowering of the hand, dividing the semibreve into two equal parts, in imitation of the motion of the pulse. There is no quantitative
information on tempo here, but it is worth noting that this statement applies not only to the regular semibreve, but it can also apply to the faster semibreves resulting from time signatures with a stroke. These latter could be conducted (tacted) at the same beat (or half again as fast when $\Phi$ is interpreted as a reduction of $1/2$) as the unstroked time signature (with a semibreve instead of a minim for each hand motion), or it can be conducted twice as fast as this (with a minim for each hand motion). Lanfranco himself did not sanction this faster conducting (with a semibreve in each tactus) in his discussion of theory, but many others from his time did (see Schroeder (1982)).

The evidence that puts quantitative limits on tempo from this period is the suggestion by Hans Neusidler (1536) that one 'gently' (Sachs's (1953) translation of "Bien gemacht") counts the four crotchets in a semibreve bar "eins zwei drey vier". The questions that arise are how fast his counting was and what kind of semibreve Neusidler's was.

I've repeatedly counted one to four while watching the second indicator on my watch, and invite readers to do the same. One per second (totalling four seconds), to me, could be appropriate for an orator seeking clarity while orating to a large audience (the oft-stated object of imitation of Renaissance and early baroque music style), but it feels decidedly too slow from a conversational point of view. Two per second (totalling two seconds) seems fine conversationally, and all four in one second seems decidedly rushed, quite unlikely to be described as 'gentle'.

There is much counting instruction in late 17th century English sources, all counting four crotchets to a semibreve, just like Neusidler. For the slow duple time (with the time signatures of $C$ and $\Phi$ then, Simpson (1665) mentioned the counting of "One, Two, Three, Four" pronounced "as you would (leisurely) read them". Mace (1676) instructed the counting to be "with deliberation". Playford (1694 edition "Corrected and Amended by Mr. Henry Purcell") used the term "telling distinctly". For the fast duple time (with the time signature $\Phi$), the last reference just used the term "tell". Purcell (1696) first described all speeds of duple time and then prescribed the counting using the term "moderately tell" (without distinguishing between the different duple-time speeds).

The tempi in this period have been determined, and the time for a semibreve in the slow duple time was about 3 to 4 seconds (the time for counting 'leisurely', 'with deliberation', etc.), and that time for the fast duple time was about $1\frac{3}{4}$ to 2 seconds (the time to 'tell' while counting). The suggestion here that the pulse was not used to measure tempo early in the 16th century because they didn't match, leads us to expect Neusidler's counting rate to be inbetween 17th century counting rates. So the choices for his counting of four are about 5, 2$\frac{1}{2}$ and $1\frac{3}{4}$ seconds. The last appears to be too short to warrant the description of 'gently', and the first seems too long, so we presume that it was the middle choice, with crotchet = 100 MM.

Neusidler used no time signatures but it was clearly semibreve-time. From the date of 1536, it would be semibreve-time $\Phi$. If the tempo was the same as breve-time $\Phi$, then Gaffurius's standard tempo was quaver = 100 MM. If it was slow semibreve-time $\Phi$, equivalent to $C$, then the standard tempo was crotchet = 100 MM. These two possibilities, with the time for the semibreve, at 5 and 2$\frac{1}{2}$ seconds respectively, are close on either side to the time of the breathing cycle (3 to 4 seconds). This deduction of Neusidler's counting rate is thus consistent with Gaffurius's comment on breathing. It is also consistent with the two possibilities for c.1500 standard tempo independently deduced above from tempo changes during the century.

The preferred possibility of crotchet = 100 MM would imply that German reduction in the speed of semibreve-time $\Phi$ was as early as in Spain. If this conclusion becomes firm, then my chronology for the slowing of semibreve-time $\Phi$ seems too quick, and so perhaps it started in the first quarter of the 16th century (or perhaps even earlier). Then maybe slow semibreve-time $\Phi$, instead of later fulfilling a function already abandoned by $C$, it pushed $C$ into extinction by replacing it.
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