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A few days later than I’d intended – after the wettest June on record, the sun suddenly shone so we took a day or two off to walk the river etc – sorry!

I’m very glad to say that there is a number of corrections to the List of Members in the Supplement herewith – people have actually looked at the List and seen that there are errors (for which my apologies) in their entries, some of them of quite long standing. If you’ve not yet checked your entry, please do! There’s a couple of errors in UK postcodes, but as David Fallows says the post office obviously never bothers to look at them because everything has arrived safely! On the other hand, a member whom I listed a while back as ‘lost’ was only mislaid either by our post office or his, because while Valdis Muktupavels has a new email address and a change in his phone number, his postal address is the same as it always was. The one that I’ve done worst to is Laurent Picault because not only did I spell his name wrongly but I wasn’t even consistent in my error – I spelled him as Picard in the main list and Picaud under traverso and France – please correct all three and, Laurent, please accept abject apologies. Incidentally, his phone and fax numbers have an added pair of figures.

Barbara is now on email so any queries regarding your subscription can be answered more easily: barbara.stanley@cableol.co.uk. She asks me to remind you also of her fax number which is in the main list: 01462-629218

Subscriptions: And talking of your subscriptions you will, as usual, be able to renew them ahead of time if you wish to do so, at the Early Instrument Exhibition at the Royal College of Music, September 5, 6, and 7. I shall be there as usual this year (sorry about last year), probably in some small corner (Jonathan Askey is very helpful to us and fits us in somewhere as we don’t need much space). Whether you want to renew then or not (the usual forms will be with the October Q), I look forward to seeing many of you there.

Reviewer wanted: I asked Eph whom he would like to review this, and he’s not answered, so I’m asking you for a volunteer. The Xunta de Galicia has sent me the five-volume facsimile of Mariano Tafall’s Arte completo de constructor de órganos from 1872. It seem well detailed with many steel-engraving illustrations. It was sent to me as a gift to bring to peoples’ attention as a major contribution to Spanish organ-building, so I asked if we might review it and have received permission to do so. However, I don’t know a great deal about organs and my Spanish is pretty rudimentary – hence this request. If any of you are both organ experts and fluent Spanish readers and, in addition, willing to write a review, let me know. ‘Willing to write a review’ because there are several books out there with various people from whom we’re still hoping to receive reviews. Getting the books to you may be a problem as it’s a heavy enough package to be expensive to post.

Further to: Comm. 1457: Paul Hailperin has sent me a couple of newspaper cuttings with rave reviews of the new display at the Paris Conservatoire Museum. Makes me all the sorrier that they didn’t, as they’d promised to do, send me an official invitation to the opening. It sounds as though the exhibition is a great deal better than I (and they) had feared when I wrote that Comm.,
but it may be, of course, that the remarks that I made in that Comm, and my description of their problems past and present, were why they decided not to invite me.

Comm. 1485 & 1507: From Steve Heavens <steveh02@globalnet.co.uk>: To discourage further correspondence on the matter, could I mention that the method of dividing the circumference of a circle into 5 described in Comm 1485 is in fact mathematically precise. My original attempt to prove this failed, but that was simply due to ineptitude on my part. Eph’s analysis is correct.

Bull. 86, p.7: Marco Tiella among others is in favour of improving communication by Internet, Web sites etc. Nobody, however, has offered to organise one for us yet! Marco suggests that “it would be interesting to set up news groups or forums in Internet news dealing with groups of subjects, like the categories listed in the organological index.” I think there already is a fair number. It might be useful if anyone who is familiar with them could do an index of them – a number of members might be grateful. So often, I gather, one only finds these things by chance. Marco asked also if I knew how many museums have an email address. I suggested that he tried the CIMCIM List, but again someone may know of a better index – if so, please send it here so that we can all benefit.

Bull. 87, p.3: Donald S.Gill: On the question of lubricants raised by Carl Willetts, I have found that a PFTE lubricant works well on the (aptly named) stickers of my portative organs. It is available in aerosol form from Axminster Power Tool Company, Chard Street, Axminster, Devon EX13 5DZ. They also sell a silicone spray which I have not tried.

National Recognition: No, not for FoMRHI, but Early Music, Authentic Performance, Original Instruments, whatever you want to call it, has now gone mainline, mainstream, etc, etc. Roger Norrington was awarded a knighthood in the Queen’s Birthday Honours last month. He is the first specialist in our field to join the other knighted conductors (never let us say benighted) and many congratulations to him. I think that it does deserve a paragraph here, for it puts his and the London Classical Players’ work, and that of all the other directors and ensembles in our field, on a par with that of the major symphony orchestras and their conductors, and shows that we are becoming less often seen as those eccentrics who do funny things to music which people would prefer to hear played properly by the BBC Symphony or whoever.

Information etc available: Kenneth Sparr <kenneth.sparr@swipnet.se>: would like to mention about my catalogue over music for the Swedish lute (c.1780-1830) now is available on Internet: http://home3.swipnet.se/~w-39526/sittral.htm. It contains a historical introduction and a catalogue over all known music for the instrument. I’d like to warn you: the present version is in Swedish! You are welcome to visit it!

Second message received from Kenneth in the nick of time: I’m pleased to announce that my catalogue of music for the Swedish lute now also is available in English. You are welcome to visit it at http://home3.swipnet.se/~w-39526/swelute1.htm

Soundwood is an organisation I’ve mentioned before. They send out a fairly regular glossy news-sheet which is concerned with using ecological sustainable timbers for musical instruments, mainly, but by no means exclusively, for guitars – there’s usually some discussion of
African blackwood and pernambuco and other instrument makers' timbers in their newsletter. They are now contactable on email: info@fauna-flora.org – or soundwood@igc.org, but I warn you that they are also now appealing for donations to help them, even though they seem to have quite generous sponsorship from various organisations.

Laaber Verlag (Regensburger Straße 19, D-93164 Laaber) is the publisher of the new Dutch Double Reed Instruments of the 17th and 18th Centuries, Collection Haags Gemeentemuseum, edited by Rob van Acht with measured drawings etc by Jan Bouterse and Piet Dhont. This follows on from the Recorder volume, which Moeck published a few years ago, and will in its turn be followed by Transverse Flutes and Clarinets. They've not sent a copy for review (perhaps not surprising as it costs DM 380, though Moeck sent us the first one – see Comm.1058), so I'll say no more about it save that it's just as impressive as the first one. The real reason for mentioning Laaber here is that their backlist of other publications is all familiar. If any of you have been wondering what's happened to Frits Knuf and his various series on organs, flutes, bassoon etc, either he has become Laaber or he has been taken over by them – either way, that's where all those books are now.

Requests, Queries, etc: John Downing has become the Historical Tinsmith at Upper Canada Village, and, being an instrument maker by vocation, is making any instruments he can think of out of that metal. He was here recently (it's always a pleasure to meet people whom I've known this way for years but never before had a chance to meet face to face) and I showed him things I had, such as the Dutch midwinterhoorn which was traditionally made of stove-pipe metal before the folklorists got at it and enforced a return to wood, against the true tradition of folk practice, and the Czech valveless stovepipe tuba I bought in Iowa City. He's sent me a photo of a surprisingly convincing looking natural trumpet (he says it works well, though he does intend to make a proper brass one a la Bob Barclay soon). He would be very glad to hear of any other tin-plate instruments.

David Smith asks “Why is it that (to the best of my knowledge) the UK is the only country in the world that does not have a SAL postal service? In case you are unfamiliar (kept in ignorance by Big Brother?) SAL means Surface Air Lifted. It is intermediate in speed and cost between surface and air mail. It means that articles travel within countries by surface and between countries by air. For places as distant as we are [New Zealand] it makes a delivery difference between about 10 days for SAL and 3-4 months by surface. We are able to send parcels by SAL to virtually any country of the world, including the UK, and can receive them from all those countries except the UK...” I have printed this, which was in a letter on other matters, on the chance that anybody may know the answer, or may even know that such a service does exist but, as David suggests, is kept secret to keep up the use of air mail.

From Anthony Elmsly <anthony.elmsly@span.ch> I am looking for pictorial representations of pochette bridges, as reference material to help me make some as near as possible historically appropriate in style, for some instruments in the collection of the Basel Historical Museum, that do not currently have bridges. Three instruments under immediate consideration are: A pochette by Savary, Bordeaux, 1696, A dance master's stick, German, late C18, A walking stick violin, early C19. Can you suggest any sources of information? Historical drawings, paintings, etchings, treatises, collections where instruments thought to have original or near original bridges exist,
from which it may be possible to get photographs, or see relevant details in published catalogues, or other persons who may have relevant information and be approachable?

Concerts, etc: The tenth Suffolk Villages Festival, directed as usual by Peter Holman, takes place 21-25 August with concerts and lectures in Assington, Boxford, and Stoke by Nayland, and an instrument makers' exhibition at the latter village. Anyone interested get in touch with Josephine Person, 50 Halstead Road, Lexden, Colchester CO3 5AF; 01206-767895.

Competition: The Accademia di Musica Antica, Premio Bonporti, Corso Bettini 41, I-38068 Rovereto is offering 6,000,000 Italian lire and a recording for the best chamber ensemble performance on period instruments of Italian music from early renaissance to early classical, with 4 million second prize and 2 million third. Conditions are fairly complex and application forms must be in before October 13, so get in touch with them fairly rapidly if you're interested. Their phone is +39-464-437689 (also fax) or -348-2202808, and email is ama@eclipse.it.

Courses: West Dean have sent me another programme and this time it does include musical instrument making courses (Barbara Stanley tells me that this year's did, too, and indeed looking back I see that I told you about them last October – all that I can say is that the piece of paper I was working from for the last Bull didn't include any). There's a whole clutch January 9-11, when you'd have to choose between making moulds for violin family, viols, or fretted instruments, or working on wind instruments, or a variety of others. If you want to make moulds for both viols and violin family, that's rough – you have to choose. Jan 30-Feb 1 you can sharpen tools for making anything. Feb 20-22 you can make earthenware instruments. And April 8-17 you can make all the instruments that you've made moulds for, sharpened tools for, and so on. That's all next year, of course. This year from October 31 to November 2 you can play recorder, and viol consort music from November 28 to 30, and next year you can play renaissance music from Jan 30 to Feb 1 instead of sharpening tools. The address is West Dean, Chichester, W Sussex, PO18 0QZ, and a weekend costs from £150 up and the nine days for making from £581, both I think including meals as well as bed.

Coda: That's it till I've done the Members List update. I have and have done some updating here, too

Deadline for next Q: October 1st please. Things are a bit hectic round then, with holy days and a couple of conferences, but if I can get it all off to Eph by 5th or 6th we should be OK.

Jeremy Montagu, Hon Sec FoMRHI
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What about the history of tempo standards?

As many of you know, the two parts of my paper reexamining and interpreting the evidence on tempo standards appeared in the May and November issues of *Early Music* last year. In my opinion, it is the most important historical research I’ve done. Most would agree that solving the history of tempo standards in mensural music from the beginning to 1700 would be a rather important contribution to music history. From what I hear so far, there has been no response to those papers. That is not surprising since the tempi implied by the evidence are slower than those which musicologists and musicians need to make the music live for them. Intuition and trust in their judgment tells them that I must be wrong. The composers wrote music to be appreciated, and these specialists do appreciate it, but not at the tempi implied by the evidence, so the evidence must be wrong. But the evidence is pretty solid and very consistent, and they can’t fault either the evidence or my analysis. So they reject and ignore it, waiting for it to go away (i.e. somebody else will find out what is wrong with it some day). At best, it could possibly be mentioned as being controversial (though there is no controversy in progress).

It would be silly for anyone today to perform pre-baroque music in public at authentic tempi. But in the philosophy of today’s music world, to perform with conviction, musicians have to believe that they have discerned (from their training and the music itself) what at least could possibly have been the composer’s intentions. They can’t afford to admit that what they are doing can only be perfectly valid and well-appreciated 20th century interpretations of what the composers wrote.

Musicologists though are supposed to be historians, who should accept what the evidence tells them whether they like it or not. Their problem with my tempo research is that it says that their judgments on original tempo, and the judgments of their esteemed teachers have all been wrong, and this is impossible according to the post-modernist way they operate. The truth they are approaching uses evidence to convince, and that truth is reached when there is agreement of judgments amongst the members of the club. If all agree, they must be right, and the possibility of all of them being wrong is inconceivable. Anyone claiming that they all have been wrong, and an outsider as well, must be exceedingly arrogant. It is a club of gentlemen and gentlewomen, and all of their academic or media careers are promoted by each praising the others at all public opportunities. Someone like me, who cares about quality standards in scholarship, and so criticises shoddy analyses, is not a gentleman, is not properly playing the game, and so can be considered to be rather subversive.

It is therefore no surprise, now that the New New Grove is being written, that the members of the club again agree that I have nothing worthwhile to contribute. As is usual in the field, judgment is supreme and objectivity in what is presented as knowledge suffers. It so happens that this is also good business. Since the knowledge presented only represents current fashion in the thinking of the musicologists in each specialty, and fashions are bound to change because of the ambitions of some to leave their mark and the desire in others for novelty, new editions are always needed to keep up to date.

Cristina Visconta in focus

In Comm. 1518, I wondered about Cristina Visconta, for whom Strad in 1707 made a viol with a neck that was the same size as that for an ordinary violoncello, 27 cm. long. If the proportions of neck length and string stop were the same as in the Talbot ms, this viol would be tenor size, with about 60 cm string stop. Strad called it only ‘viola’ (a term which, if not modified, meant ‘viol’), while bigger ones were called ‘viola da gamba’ or ‘bassa de viola’.

Carlo Chiesa has written to me, mentioning some as yet unpublished information about her: ‘Cristina was the wife of the most important violin player in Cremona, Gaspare Visconti. He came from a wealthy family, and studied the instrument with the best available teachers. To the
point that he came to England, where he played the violin, but also fell in love and married. Thus, Cristina Visconti was not a Cremonese, but an English woman, coming from a well known family of musicians from London. I enjoyed that information. It favours my first speculation in Comm. 1518, that she was a viol player with only fiddles to play with.

Carlo also pointed out to me that though exhibits N. 259 to 265 are listed as one item in Saccone’s book in a chapter concerning a Viola da Gamba dated 1684, there is no evidence that this group of exhibits refers to just one instrument, or that there is any relationship between any of them and the 1684 instrument. So my citing in Comm. 1518 a neck length and body length from these exhibits, and ascribing these to the 1684 instrument, is doubly invalid. I accept this. It doesn’t affect any of my other conclusions. Though there is no direct evidence that the string bits (exhibit N. 309) are related to exhibit N. 308 (the neck drawing of Cristina’s viol), the fact that a maximum-use-of-evidence model for the kind of instrument the strings were intended for leads to it being a viol that was unusually heavily strung establishes a tentative link.

Carlo reads the Strad handwriting differently from Sacconi. With respect to Cristina, Carlo reads ‘Sigra’ (for ‘Signora’) while Sacconi read ‘Signorina’, and Carlo reads ‘Visconti’ while Sacconi read ‘Visconta’. The new information favours Carlo’s readings.

**Tricks in calculating Equivalent Diameters of unusual wound strings**

As an appendix to the Comm. in this Q on wound strings, I would like to mention a couple of tricks I’ve found useful in calculating equivalent diameters of somewhat different types of wound strings. If the core is made of a roped material, I use an equivalent density of the core which is equal to the real density of the material times the square of the ratio of the core’s equivalent diameter divided by the core’s outside diameter.

If the winding is polished smooth, I use an equivalent density of the winding wire, which is equal to the real density of the winding material times the ratio of the volume of winding wire remaining after polishing divided by the volume before polishing. Assuming a circular cross-section (I can’t integrate the equation for the real shape), I calculate that the winding is smooth enough (85% of the surface is flat when the string is tuned up) when that ratio is 0.67, 0.64, 0.60, 0.56 and 0.48 for $D_w/D_c$ of 0.1, 0.3, 0.5, 0.7 and 0.9 respectively, and design accordingly.
West Dean College 10 day Instrument making course

This is a short course [10 days] that takes place during the Easter vacation. The facilities, the accommodation, and the catering are excellent. The grounds of this Edward James [Patron of Dali & Magritte] house are very beautiful.

The course is now in its 25th year and looks set to be a course to be reckoned with in the next millennium. Directed by Gordon Jones, it sets out to introduce instrument making to a group of people of varying abilities and caters for those with little or no experience of handling tools to those who are already fine craftsmen and craftswomen. Having attended the course for 13 happy years, I have seen it grow and develop both in the range of instruments offered and in the quality of making. This is mostly due to the high standards set by the tutors, who include professional instrument makers such as Eric Moulder, Jane Julier, Tony and Louise Padday, to name but a few. A number of 'Easter course' students have undertaken further years of training and are now professional makers themselves.

Some instruments are able to be completed within the 10 days, but many people prefer to work on their instruments over several Easter courses, in order to benefit from expert help and advice.
To the Readers,

This Quarterly carried an announcement of the firm Camwood (UK) Ltd. I think my experience with this firm might be of interest to fellow instrument builders considering doing business with Camwood.

My first experience dates back to a pro-forma invoice of 6 January 1994 in which I was asked to pay 100% advance for boxwood squares; "We will have your order ready to ship in the next 7 - 10 days." I mailed a cheque on 14 January 1994 and received shipment on 15 September 1994.

The second time I ordered sets of squares for oboes, I received a pro-forma invoice for 50% of the order value; "Despatch: 21 - 28 days from receipt of deposit." I sent the cheque on 29 May 1995. Nearly 2 years later I have received zero sets of squares. To be fully accurate: I have received 46% of the ordered squares for upper and bell sections, none for the middle sections. Camwood knows as well as you do that I can neither produce nor sell oboes missing the middle section. I have written to the firm and I have telephoned innumerable times. Each time I succeed in making contact, I receive apologies and explanations and a promise that the wood will be sent within a short term. But no action so far. Anyone considering doing business with Camwood might be well advised to keep this in mind. I will be pleased to report on any further developments.

Yours sincerely,

Paul Hailperin

on 29 April 1997
Donald’s first paragraph is a bit of justified sarcasm because I had presented an erroneously simplistic picture of what he thought. I now have a much better picture of how he interprets historical evidence. Let me try again, expecting again to be corrected.

As an historical researcher, Donald looks for a pattern in the evidence. When he finds the pattern that seems to fit best, he has to contend with bits of evidence that violate the pattern. He will look for evidence suggesting that the violating evidence is wrong. If he can’t find such evidence, he will lay aside each violation as a mystery, satisfied that his pattern is true because it is the best that can be found, whether or not the appropriate evidence will ever turn up. It is with this last stage of what to do about violations that our approaches differ.

When I can’t find evidence suggesting how the violating evidence got wrong, since I feel it is my responsibility to explain all of the evidence, I try to imagine a reasonable scenario which would have made that evidence appear to go wrong and not violate my pattern. My story about inconsistency in the strength of Meuler’s wire is such an exercise. I do think that it is a reasonable enough possibility. Of course I have no supporting evidence for this conjecture. That is not the point. The point about doing this sort of speculation is that if I can’t find such a story that is a reasonably likely possibility, I have no right ever to expect to find any evidence that will correct my rogue evidence, and I have no choice but to accept that the rogue evidence is true. Then I must reject that particular pattern I saw, and look for another.

The reason for this is that we are supposed to be as objective as possible. The evidence is objective. Reasonably explaining all the evidence is more objective than reasonably explaining just some of it. The only objective measure of closeness to truth is fidelity to the evidence. Perceiving a pattern in the evidence to explain it is subjective. The pattern I see in the evidence is no more or less a ‘preconception’ than the pattern Donald sees in it. Any such pattern should have some objective test against the evidence to have credibility as a candidate for truth.

Donald has no objective way of testing his pattern against the evidence. He doubts the evidence from Praetorius and Meuler, without bothering to try to imagine how they could possibly be mistaken (and questions my imaginings of how the contradictory evidence about the strength of Meuler’s wire from Robinson and Praetorius can be resolved). Instead, he asks for more evidence supporting their evidence before he will take any of it seriously. He cannot possibly offer any objective criterion to determine how much of such evidence is enough to convince him. In this way he makes his pattern fireproof, not open to any objective testing.

This approach considerably reduces the probability of his being in the situation (which apparently is to be strongly avoided) of having publicly to admit that he has been wrong about something. He expects that historical writings should be definitive, to be fully trusted, and feels responsibility for that trust. His reaction to my admission of past error (in disbelieving Praetorius’s statement about using Cammerthon) is to turn it into an example of my inconsistency, diminishing the trust that should be given to what I say. He wouldn’t show such inconsistency. What superficially looks like his arrogance (‘my concept of truth is truth, regardless of evidence to the contrary’) is really only standard conservatism protecting the status quo.

At this point, I would like to apologise to Donald. I have been using him as a whipping-post. His approach to historical research is no different from that used by the vast majority of researchers into music history. It follows a long tradition of sloppy scholarship where the opinions of experts have counted more than true fidelity to the evidence. Of course, they have done a lot of great work in advancing the field. My intention here is not to criticise, but to point out how they can do much better, especially in being fair towards new ideas in areas they have written about. I want to teach about more objective methods, and hope that some want to learn. Explaining all the evidence is the way towards truth, not protecting trust in the experts.
Parchment Roses for Harpsichords

Further to Comm. 1515 in April etc., it seems that this is a subject which could well do with more discussion, as there seem to be more questions and problems than there are answers - at any rate in the public domain.

For the rose to have the sort of accuracy of construction that is attractive to the eye, it will probably have to be constructed under magnification. Watchmakers magnifiers may work, better is a low power binocular microscope, provided that it gives about five inches clearance below the objective for using tools.

The material used has to be vellum or parchment, which is tough and fibrous. Paper collapses under the stresses of working. Obtaining suitable materials is not easy.

The parts have to be worked with straight and curved cuts of great accuracy. A scalpel will make straight cuts, punches will make curved ones. Beware of using commercial punches, such as leather punches, bevelled on the outside, as they leave dents all over the work. However, a solid steel punch, provided it is less than about 3mm diameter, will cut through vellum supported on end grain hardwood, preferably boxwood, neatly and cleanly. As a solid punch is easily obtainable - a commercial pin punch - and it can be ground to any shape, including triangular, this will solve most punching problems. Hollow commercial outside-ground
punches have a use however - if you grind them in half they will cut rounded projections on the edges of layers.

Many roses are made of several layers stuck together, which produces a stepped section at the edge of each layer. But vellum is not easy to stick, as its smooth greasy nature acts as a release agent for almost all known glues. One approach is to degrease the surface and gently sand it, which improves adhesion, but it is not easy to prevent the glue squeezing out and messily blocking up the steps.

Sticking the rose to the back of the soundboard is easy, as little wooden blocks can be arranged to hold it in, requiring only a wood-to-wood joint.

Gluing the “walls” to the layers (see p28 in April Bull.) is extremely difficult, owing to the very small glue area. But it was done in the past. Someone must have an answer to this? I cheated and used layers of wood - there is lots of space outside a rose, where you can get up to tricks.

The glue that I found to work best was cyanoacrylate superglue, but it too had serious problems. Certainly it is not an authentic solution. The old books talk about boiling up bits of vellum offcuts.
FoMRH1 Comm. 1525

Harpsichords and "Buntpapier"

In Comm. 1495, Martin Pühringer deals with Georg Christoph Stoy and his "Buntpapier" in an early harpsichord. He amply quotes from the book, Buntpapier. Herkommen, Geschichte, Techniken, Beziehungen zur Kunst, by Albert Haemmerle (2. ed. München, 1977), from which he reproduces part of a plate (pl. 12 on p. 25), without mentioning his source. The mysterious "BEV" which Pühringer interpretes as "possibly Bavaria" has nothing at all to do with Bavaria - at that time Augsburg was an Imperial Free City i.e. a state of its own, situated in the region of the Swabian (not the Bavarian) Circle. We are pretty sure that "BEV" is a misreading for "Bey", in modern German "bei" = (published, sold) "at". Apparently several sheets (or parts thereof) were pasted on the harpsichord (or rather on the inner side of the lid); thus, the correct order of words would be: "Augsburg bey Georg Christoph Stoy C. P. S. C. M." = Augsburg, (to be sold at) Georg Christoph Stoy, with imperial privilege".

Rudolf + Uta Henning
About Neupert harpsichord technology in the 30's.

Some time ago I was asked to see and repair the Neupert harpsichord No 15305, which was built in 1930. From the Neupert Firm I knew that a series of 41 instruments for the Curtis Distributing Corporation, New York was built then.

The harpsichord has a heavy piano-structure, and the bottom board covers only the keyboard box. All the external parts of the structure of beech-wood are veneered with caucasian walnut. The one-manual harpsichord has three stops, 16', 8', 4' operated by pedals and a “Lautenzug” in form of a mute, divided between treble and bass, operated by two front levers. The block slides are of two brass channels soldered together like those even recently in use in Germany and the springs are of the type depicted in fig.5, pag.77 of H.Neupert’s Das Cembalo, Bärenreiter, Kassel u. Basel 1956 with leather quills.

The most impressive detail is the diameter of the strings, many of which are overspun. Really, the diameter of the thickest is of 2,6 mm. At that time Neupert already built the device for holding the 4'-strings (recently still in use) which are attached by pairs to a thick iron pin pinned in a very strong rail and protruding over the soundboard through a round hole. There is one bridge for the bass strings of both the 16' and 8' and the bridges on the soundboard for 16' and 8' are double-pinned. Only the 4' bridges on the soundboard and the wrestplank are single pinned. The strings of both the 16' and 8' pass over a brass rod inserted in the bridge (which has no pins at all) and they are fixed by a piano pressure bar. All the strings run straight from the hitchpin to the wrest pin.

The soundboard is reinforced with many parallel ribs and transverse cutoff bar. The bridges are held firmly by screws. Scaling and other measurements, shown in the diagrams, denotate that the design of the stringings was somewhat approximative.

The instrument is complete in its parts, but was kept in a somewhat poor state. In fact the action didn’t work, apparently from the dirt spread all over the instrument. It was evident that the strength of the strings, together with the action of strong climatic changes, caused the wrestplank to collapse, so that the block slides were forced against each other and inmovable presumably for a long time. It is worth describing how the 1930's technology in making harpsichords reveals great disadvantages. In the sketches there are illustrated the details of the iron structure which should have granted the clearance between the block slides. In fact, the central iron bridge broke through in the wrestplank side owing to its deformations. Notwithstanding the wrestplank had been built in
form of a kind of massive ply-wood, some of the boards became unglued. Then the section toward the bass raised assuming an arcing of about 1 cm. Moreover, the gluing of the wrestplank end toward the bass loosened so that it moved in spite of two big screws, which didn’t hold it firmly. Probably these defects were already once remedied. The instrument was repaired and mechanically it works satisfactorily.

As the quills (cut by different hands) are worn out, it is difficult to have an idea of the sound which should be obtained from its very thick overspun strings. Such strings require more energy than usual to be set vibrating, therefore the quills should be more rigid - but the keyboard becomes very hard. If the quills are cut so weak than the keyboard works well, the overspun strings give out a very unclear murmur.

Therefore I would like to know whether someone came across a stringing like this, so that I could have more information about a possible explanation of the musical use of a 16' stop of this kind.
Neupert harpsichord No 15305
1 - 16' bridge (treble)
2 - 8' (and 16' bass) bridge
3 - 4' bridge
4 - 4' hitchpins
5 - iron braces
6 - brass box slides
7 - 4' bridge
8 - 8' and 16' bridge
Neupert harpsichord No 15305

1 - brass box slides
2 - wrestplank
3 - 4' hitchpins rail $6 \times 6_{cm}$
4 - cutoff bar
5 - wooden bars $6 \times 8_{cm}$
6 - ribs
7 - screws for 8' bridge
8 - screws for 4' bridge
Neupert harpsichord No 15305
1 - nameboard
2 - pressing bar
3 - mute (buff stop)
4 - brass box slides
5 - belly rail
6 - wrestplank
7 - wooden hitchpin bar
8 - wooden bars
9 - bottom

Detail of iron brace
1 - iron brace
2 - iron plate
3 - brass box slides
### NEUPERT HARPSICHORD N.15305 (1930)

<table>
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<th>$\rho_{ps}$</th>
<th>$\phi_c$</th>
<th>$\rho_{pc}$</th>
<th>Pitch</th>
<th>Hz</th>
<th>$A$</th>
<th>$D_{ext}\phi$</th>
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| f      | 789    | 112                     | 0.50        | 0.0075   | ...         | ...   | 192| 9.81| 0.196          | 0.50          | 0.127 | ...    | ...
| c'     | 538    | 93                      | 0.42        | 0.0075   | ...         | ...   | 256| 9.81| 0.138          | 0.42          | 0.173 | ...    | ...
| f'     | 409    | 87                      | 0.41        | 0.0075   | ...         | ...   | 384| 9.81| 0.132          | 0.41          | 0.173 | ...    | ...
| c''    | 273    | 71                      | 0.28        | 0.0075   | ...         | ...   | 512| 9.81| 0.062          | 0.28          | 0.260 | ...    | ...
| f''    | 212    | 65                      | 0.28        | 0.0075   | ...         | ...   | 768| 9.81| 0.062          | 0.28          | 0.207 | ...    | ...
| c'''   | 151    | 61                      | 0.24        | 0.0075   | ...         | ...   | 1024| 9.81| 0.045          | 0.24          | 0.404 | ...    | ...
| f'''   | 128    | 61                      | 0.21        | 0.0075   | ...         | ...   | 1536| 9.81| 0.035          | 0.21          | 0.477 | ...    | ...


**Length**
- 1671 cm
- 1495 cm
- 1268 cm
- 1020 cm
- 817 cm
- 713 cm
- 558 cm
- 387 cm
- 305 cm
- 227 cm
- 192 cm
- 1655 cm
- 1475 cm
- 1242 cm
- 968 cm
- 789 cm
- 538 cm
- 409 cm
- 273 cm
- 212 cm
- 151 cm
- 128 cm

**Area**
- $A$

**Tens.**
- $T$

**Stress**
- $\sigma$
NEUPERT HARPSCICHORD N.15305 (1930)
String Lengths of 16', 8', 4'

Ratios of distance of plucking point to total length (%) 
4', 8', 16'
IN SEARCH OF THE WELL TUNED CLAVIER - II

As Eph pointed out there were some errors in my Comm. 1480 about the well tuned clavier. However while we may both be correct in some respects, we still do not agree about some details. Firstly, my equation 8 which is an exact statement of the difference between two states of the string system is based solely upon Hooke’s law and the Mersenne equation that is customary when discussing metal strings. My reason for writing the relationship in this manner is that both the geometrical change in string length and the elastic length effects are treated in the same terms. My equation can be more aptly written as follows than in Comm. 1480:

$$df(1 + \frac{df}{2f_1}) = \frac{1}{2f_1 K} \left[ \frac{dL_1}{(L_1 + dL_1)^3} + \frac{\Delta L}{(L_1 + dL_1)^3} - \frac{\Delta L}{L_1^3} \right]$$

Written in this way it is obvious that the term $(1+df/2f_1)$ is very near to 1 in value and that the two terms containing delta $L$, are very nearly the same size and so cancel each other. Therefore my crudely linear equation reduces to

$$df = \frac{1}{2Kf_1} \frac{dL}{(L_1 + dL)^3}$$

One can put back in the material properties (in engineering units). Also the effect of $dL$ in the denominator is nil. Eph, being a fine mathematician prefers to solve everything. I am an engineer and our approach is to avoid solving everything, hence I simplified the equation before applying it to my problem.

Because I am interested in the change of tuning of a pair of harpsichord strings of different lengths, initially tuned to the same frequency, that change length by an amount $dL$, I can write the ratio of the sensitivities to tuning for the longer and shorter strings as shown next,

$$\left( \frac{df}{dL} \right)_1 = L_1^{-3}$$
$$\left( \frac{df}{dL} \right)_2 = L_2^{-3}$$

which is why I said that the sensitivities are proportional to the inverse cubes of the string lengths involved (the other factors are the same in numerator and denominator and divide out). To calculate the beat rate produced by the small motion $dL$, we calculate $df_1 - df_2$ by subtracting the equation above

$$df_1 - df_2 = \frac{dL E_g}{8 \rho f_1} \left( \frac{1}{L_1^3} - \frac{1}{L_2^3} \right)$$

In Eph’s Comm. 1511, he refers to an experimental determination that I made to verify the above equation as a sanity check; for these data, my equation differs from his equation by 4 parts per million.

This sensitivity number ($df/dL$) is useful in the practical evaluation of the structures and mechanical problems of harpsichords. Consider that at $c^2$, the sensitivity of an Italian harpsichord is 0.3 Hz per micrometer, while that of a Flemish harpsichord is only 0.1 Hz per micrometer. For an acceptable tuning error of 0.1 Hz, the tuning hammer must be controlled to a precision of 0.3 micrometers for an Italian harpsichord compared to the “easy” Flemish instrument of only 1 micrometer. This is a remarkable example of what we humans can achieve when we do not know the numbers involved.
The mathematics of tuning up a string

Following is a new calculation of how frequency depends on how much one stretches the string when tuning it up. It is better than that offered in Lee's Comm 1480 because it is more direct, avoids potentially-dodgy calculus, has no approximations (so it can apply to gut or nylon which stretch much more than iron), and is simpler. It is better than my Comms 1493 and 1511 because I didn't keep the physics of the situation fully in view when manipulating the mathematics, so they were wrong.

From Hooke's law, \( T - T_0 = EA(L - L_0)/L_0 \). With constant \( A \) and \( L_0 \), Mersenne's Law for the difference between two tensions on a string can be expressed as \( T - T_0 = 4\pi AL_0^2(f^2 - f_0^2) \). Equating the two, we get

\[
(1) \quad f = \sqrt{f_0^2 + E(L - L_0)/(4\pi L_0^3)}.
\]

It is convenient to separate the constants \( f_0 \) and \( L_0 \), where one starts measuring the tuning from, and the variables of string stretch \( \Delta L \) and of the resulting frequency difference \( \Delta f \). The relationships are \( \Delta f = f - f_0 \) and \( \Delta L = L - L_0 \). None of these \( \Delta \)'s are necessarily small. Then

\[
(2) \quad \Delta f = \sqrt{f_0^2 + E\Delta L/(4\pi L_0^3)} \cdot f_0.
\]

If we had a string on an instrument of vibrating length \( L_0 \), and marked the string at the nut when it sounds at \( f_0 \), we can plot on a graph the increase in frequency (\( \Delta f \)) that results from tightening the string. The effective movement \( \Delta L \) on tightening is the actual distance the mark moves from the nut times the ratio of sounding length over stretching length to the nut. One would then be plotting equation (2). If one wanted to measure \( E/\rho \) of the string material, it would be better to plot \( f^2 - f_0^2 \) (from squaring equation (1)) vs. \( \Delta L \), which gives a straight line, and extracted \( E/\rho \) from the slope (which is \( E/(4\pi L_0^3) \)).

The tuning sensitivity is the slope of the graph of equation (2), which is its derivative:

\[
(3) \quad \frac{df}{dL} = \frac{d(\Delta f)}{d(\Delta L)} = E / \{8\pi L_0^3\sqrt{f_0^2 + E\Delta L/(4\pi L_0^3)}\}.
\]

If, as in Lee's case, we are tuning a string that is only slightly out, so \( \Delta L \) is near 0 (technically, when \( \Delta L < 4\pi f_0^2 L_0^3/E \)), then equation (3) simplifies down to \( df/dL = E / 8\pi f_0 L_0^3 \). This verifies his claim that the sensitivity is proportional to the inverse of the length cubed if the pitch is the same, and I withdraw all previous objections to that claim.

There has been some speculation about why the string lengths on harpsichords and similar keyboard instruments have been scaled so that as many of the strings as is practical are tuned near to breaking. Indeed, as equation (3) shows, maximising the frequency or \( \Delta L \) does minimise the tuning sensitivity, making tuning accurately easier. A less well known reason is that the strings sound fuller and warmer because of absorption of some of the highest frequency vibrations due to creep, which is greatest near breaking. Brass is preferred to phosphor bronze, and iron is preferred to steel, because there is less 'zing' in the sound, the 'zing' being unwanted higher harmonics. Phosphor bronze and steel, once stable, don't stretch at all, so there isn't the slow stretch (the creep) that provides that absorption. The creep that causes the absorption creates the need for more tuning and leads eventually to string breakage, but the sound advantage is considered to be worth the extra bother.

In Comms. 1493 and 1511, I solved the wrong problem. The one I solved when I differentiated Mersenne's law was, in effect, to find what happened when the nut moved with the mark on the string when one tightened the string. Sorry, I think that I got it right this time.
A short history of the names ‘gittern’ and ‘cittern’, in response to Comm.1514

The names of instruments
I have never heard of the name of an instrument in its culture being any different from what its players call it. We should accept this as a principle, and only use other names when either we don’t know what the players called it or we want to refer to a mutually-agreed generic category that it falls into. Players usually distinguish between one instrument and another by the technique they need to learn to play it, for which the tuning is a very important component. What it looks like is a secondary characteristic. Original adjectives in a name relate to secondary factors distinguishing between it and other examples of that type of instrument (e.g. ‘Spanish guitar’ or ‘bell gittern’). Instrument historians and antiquarians today have to distinguish between instrument aspects that original players didn’t have to worry about, so we introduce other adjectives (like ‘Renaissance’ and ‘baroque’ guitar).

At times, players use instrument names not to distinguish between those that play differently, but to project an image that the audience will recognise and respond to. A sad modern example of this is that a Welsh crwth player I know feels that often he has to call his instrument a ‘harp’ because the Welsh usually only recognise that name as an ancient Welsh instrument.

The cithara
Another example of naming for image is when early medieval players often called stringed instruments of various types ‘cithara’ (the Latin for the ancient Greek ‘kithara’). These included lyres (also called rotes and harps), and when fingerboard instruments first came to Europe around the 8th century, fiddles both wide (vielles) and narrow (giges). They all adopted the bow around the 11th century, without changing names. Since there was no need to distinguish between plucking and bowing, we can expect that there was minimal difference involved in playing technique, implying no change in tuning and a strumming technique when plucked. Bowing also did not alter claims to the ‘cithara’ name. That claim was often reinforced by non-functional bits of lyre arms (or wings) in the designs of fiddles. The name ‘citole’ first appeared at the end of the 12th century. It was a wide fiddle that specialised in being plucked by a large plectrum, as it was known that the ancient kithara was.

Musicians wanted to claim the ‘cithara’ name because everyone knew that music played on that ancient instrument had the magic for the ancient Greeks that all musicians crave, the power to control the emotions of listeners. The citole eventually won the competition for that name, and 14th century music theorists rewrote instrument history accordingly (see the Berkeley ms., Page, G.S.J. (1980), discussed below). The association of this magic with that name was very important in the 15th century revival of the citole, as the cetra, and the 16th century revival of the cetra, as the cittern. Everyone knew that these names were variants of ‘cithara’. They were early musicians of their times, promoting their instruments with the magic of authenticity.

The gittern
In the second half of the 13th century, a group of new instruments introduced via the Arabs were adopted in Christian Europe. Amongst them was a small round-backed instrument with a sickle-shaped pegbox called ‘guitarra’ in Spanish, ‘chitarra’ in Italian and ‘gyterne’ in English. These names derive from the North African Arab name ‘qitara’ that the instrument came with, which of course derived from ‘cithara’. So while the English ‘git’ and ‘cit’ (Italian ‘chit’ and ‘cet’) names both came from ‘cithara’, the latter names were from indigenous Latin, and the former names came via Arabic and Spanish.

There had been another narrow fiddle in Europe for half a millennium before the gittern arrived, often called ‘gige’. They differed visually by the gige having a pegblock with pegs inserted frontally, and the gittern had a sickle-shaped pegbox with pegs going in sideways. More important to the musicians, they differed in tuning and in what they were plucked by. The gige tuning was probably in fifths and octaves, as was usual for fiddles, but the tuning of the gittern was in fourths. That tuning was established by the Arabs for the qitara (and its big
brother the 'ud, which became the lute in Europe) by the end of the first millennium, and they then also introduced playing with a feather quill. The gige was played with a plectrum.

The citole and cetra
The Berkeley ms presented stringed instruments and their development in terms of increasing numbers of strings. The basic stringed instrument was the citole (called cithara), with 4-strings tuned upwards by a tone, a fourth and a fourth. From this the gittern was developed by lowering the fourth string so that the intervals were all fourths. Then came a 4-string harp called 'lyra' inspired by the ringing sound of the quashed corpse of a dead animal dried in the sun. Further developments involved another harp and two psalteries with more strings.

This source gives us the only citole tuning that has survived. What is different about it is the interval of a tone in its tuning. When it went out of fashion and was revived in the 15th century as the metal-strung cetra in Italy, the tuning Tinctoris gave for it was a tone, a fourth and back a tone. He indicated that it was well out of fashion before 1500. The cittern was a 16th century revival of the cetra.

The cittern
Citterns of the 16th and 17th centuries had various tunings. Except for Praetorius's English cittern (which will be returned to), the interval between the first course and the second was always a tone and the interval between the second and third courses was always a fifth. The fourth course was usually tuned higher than the third. We call this violation of a regular progression of pitch lowering for courses from treble to bass a 'reentrant' tuning. The tuning of the 4-course 15th century Italian cetra was also reentrant.

Update on the gittern
The medieval gittern was in decline in Spain around 1500, but a few decades later was revived there using the body of a corner-less 15th century vihuela, also in decline (by that time gittern and lute tuning had acquired a major third in the middle). Outside of Spain it was sometimes called a 'Spanish guitar' to distinguish it from the original round-backed unwaisted gittern design which still survived elsewhere, such as those drawn by Virdung and Agricola. (These large gitterns were more like treble lutes, and 15th century original-size smaller ones were called 'chitarrino' by the Italians).

By the mid-16th century, this waisted version of the gittern (we usually call it the 'Renaissance guitar') became very popular all over Europe. A small original-design medieval gittern was also revived in Spain then to fill a need for a high-treble plucked instrument in plucked-string bands. Since the guitar name was otherwise occupied, the Spanish invented the name 'bandurria' for it, leading to the name 'mandora' for it later in France. Before the French copied and expanded it, it took rebec tuning of a fourth and a fifth on 3 courses (it was then not written about until its mid-18th century revival, when its body was a miniature version of the Spanish type of English guitar, with 5 courses tuned all in fourths). The English were in the same predicament as the Spanish when they invented a large wire-strung guitar and the names 'gittern' and 'gitteron' (this term for large gittern referred to the vihuela) were already in use for other instruments, and so they followed the Spanish example for a guitar that is not a guitar and called it 'bandurion' (i.e. large bandurria), which later became 'bandora' or 'pandora'.

By the end of the 16th century in Spain, the Renaissance guitar was so well out of fashion that its original name of 'guitar' (and 'Spanish guitar' outside of Spain) could be appropriated by the newly-invented larger and deeper 5-course instrument that we call the 'baroque guitar'. Renaissance and baroque guitars originally had octave stringing in the bass, with the low octave string called a 'bourdon'. At times the bourdon was not used, leading to reentrant tuning. The tuning is conceived as that of the bourdon whether or not it was actually used. So the 5th course of the baroque guitar is (and was) conceived as always being a fourth below the 4th course. At the same time, the Italians invented their own large guitar (chitarrone), which was a higher-tuned bass lute with reentrant tuning in the treble strings. It could only have been called a guitar if it was strummed, like all guitars were at that time. The later long neck added after 1600 provided another continuo function.
Praetorius

The tunings Praetorius gave for the English cittern were reentrant versions of gittern or lute (top 4 courses) tunings. This is odd because there is very much English repertoire for cittern at about this time, and almost all of it is for the degenerate 4-course Italian cittern tuning. One can easily switch between the gittern or lute and the cittern tunings by crossing the 2nd and 4th courses between the pegs and the nut, and between the tail and the bridge, so it seems that Praetorius’s tuning was just an alternative to the usual tuning used by players wanting to play the treble lute divisions in the duet and Consort repertoire on the cittern. Praetorius probably was aware of the usual cittern tuning but didn’t mention it because, as he wrote, that degenerate 4-course Italian tuning had the vilest of associations, fit only for cobblers and tailors. He admired the English instrument, having copies made for his own use, and apparently wanted to avoid these associations.

In the late 16th and early 17th centuries, citterns came in various sizes, with a string stop of about 45 cm most popular in Italy and elsewhere, and a string stop of 35 cm used in England. Let us call these ‘large’ and ‘small’ here, neglecting for this discussion the ones bigger than 45 cm that were used in Italy and Flanders less frequently.

The 17th century gittern

Praetorius’s English cittern was a small one. By the middle of the 17th century, a wire-strung cittern-like instrument of that size with a non-reentrant version of his tuning was popular in England. It was properly called a gittern because the tuning had two fourths with a major third in the middle. There also appeared a cittern of the large size with degenerate Italian 4-course cittern tuning for amateurs to play simple pieces on. Playford used the same plate to illustrate both on title pages, but on a plate in another cittern book, he showed a man playing a cittern, with a gittern hanging on the wall.

The Talbot ms at the end of the 17th century included measurements of a cittern and some discussion of a guittern and a bell guittern. The cittern was not the usual traditional shape because his measurements of the breadth of the belly were 2 inches ‘at the upper part next to the neck’, 9 3/4 inches ‘at the middle’ and 9 1/4 inches ‘at the lower part’. If the design were traditional, there would be no rational way of measuring a width at the lower part. The shape probably had a lobe at the bottom, in imitation of the lower end of the bandora. The shape of the upper end of a bandora, cut off by a circular arc, is the body shape of the bell guittern, which had 5 double courses. No guittern tuning was given, but the associations with the bandora reinforce our expectation (because of the name) of a similar guitar tuning in fourths with a major third, probably pitched a fifth above the bandora.

Talbot’s cittern was of the large size, and he stated that the guittern and bell guittern were of about the same size as the cittern. But surviving bell guitterns from that period made on the Continent include small and intermediate sizes. We need to keep an open mind as to whether there was a standard size of bell guittern.

The cilhrinchen

The last half the 17th century saw the introduction of a new fashion in tuning, with open strings making chords, including their thirds. Such tunings, with adjacent major and minor third intervals, started on French baroque lute and later spread to other instruments. Playford used a tuning upwards of a fourth, minor third and major third in the final three pieces of his guittern book. Lyra viol tunings with ‘harp’ in the name, had them. Such chord tunings became fairly standard on the viola d’amore, the baryton and on the cithrinchen, the continental version of the bell guittern.

Before then, with the few exceptions mentioned, there had been a close correlation between an instrument’s name and its tuning. A lute (or a 15-16th century plucked viola) was a 1st class instrument tuned in fourths with, on later instruments, a major third usually somewhere in the middle. A guitar was a 2nd class instrument with the same kind of tuning. Any descendant of the citole had at least one interval of a tone between adjacent courses, with the cittern having the
top two intervals a tone and a fifth. To play the game properly, any new instrument with a new tuning should have a new name. Actually that is just what ‘viola d’amore’ and ‘baryton’ were. But the ‘cithrinchen’ proclaimed loud and clear that it was a cittern by its name, by its imitating the Italian cittern by having the bass side of the neck cut away, and by having the cittern’s characteristic tapering body depth. Perhaps the players who used the name felt that their break with tradition was justified because Praetorius (whose book was well known in Germanic countries then) had set a precedent, calling a cittern-like instrument with a non-cittern tuning by a name that was a cittern diminutive.

There is evidence for cithrinchens having 8, 9, 10 and 12 strings in 5 or 6 courses. An 8-string one would have had three doubled treble courses and two twisted-brass single courses. A 9-string one would have an added single twisted-brass 6th course. A 10-string one would have usually had five doubled courses, but could have the lowest of these split into two single courses, and a 12-string one would have six doubled courses. Five courses was the norm, and all of the surviving music is for that number. The dominant tuning used in that music was (from the bottom up), major, minor, and major thirds and a fourth. The tablatures imply playing by quill or plectrum. Another popular tuning was the same as the English bell guittern, which is essentially the same as the baroque guitar, the enormous repertoire of which could be played by plucking and strumming with the fingers.

The ‘English’ guitar
In the second half of the 18th century there was an explosion of cithrinchen-type instruments without the cithrinchen shape. They had a variety of body shapes, including that of a lute. Most were cittern-like in outline, but wider and deeper (without a cittern’s tapering depth), and some had screw-type tuning devices. The ‘guitar’ in England was one of these, with 10 strings arranged in four treble pairs and two single basses on a 42 cm string stop. The English tuning (from the bottom up) was major and minor thirds, fourth, and major and minor thirds. The French made it as a small alternative to the ‘guithare allemande’ using the name ‘guithare angloise’ (both were called ‘cistre’), and their tuning for it was a tone, two fourths, and major and minor thirds. The Norwegian ‘sister’ was the same instrument, and was tuned the same as the cithrinchen, with an added fourth on bottom. The Portuguese ‘guitarra’ was like the English version with a screw tuning device worked by a key, but with all six courses paired. By the 19th century, the device had round knobs for hand tuning, as it does today. It used the English tuning and, for special purposes, the 2nd and 5th courses were raised a semitone, giving a tuning (from the bottom up) of a fourth, a tone, two fourths and a tone. Marcuse cites a 1790 dictionary that mentioned a Spanish cittern that had 5 courses tuned like a gut-strung guitar. A larger instrument (51 cm string stop) with extended basses and 8 courses was called by the French ‘guithare allemande’, and its tuning (from the bottom up) was a tone, two fourths, a tone, a fourth and major and minor thirds. Koch’s Lexikon (1802) mentioned a cittern that appeared in Germany c. 1780 that had 7 single strings tuned a sixth, two tones, a fourth and major and minor thirds.

Why so many of these instruments were called ‘guitars’ is very curious. These instruments were plucked with the fingers, which Playford stated was an advantage of the gut-strung guitar. Yet there was no historical precedent for instruments with these kind of tunings to be called ‘guitars’. This is apparently another case of an instrument’s name being used to claim an image. The baroque guitar was in considerable decline by then, and the time was ripe to displace it from its position as the principal domestic hand-plucked instrument. That position had the ‘guitar’ name. The wire-strung instruments succeeded in this for a while, and using the ‘guitar’ name probably helped.

Another question that must be asked is why these instruments became so much more popular than the cithrinchen had become earlier. One difference is that cithrinchen bass strings were made of twisted brass while those of these later instruments were made of metal wound on metal, silk or gut. That the latter were preferred at this time is indicated by Fouchetti’s stringing of the Neapolitan mandolin, which specified twisted strings for the third course, but the fourth course was an octave pair with a high brass string and the low string a wound-on-gut violin string. All of our early mandolin and English guitar customers prefer the sound of
twisted to wound basses. Of course our musical aesthetics cannot be the same as 18th century musicians, so this is not evidence, but the problem may be not in sound preferences but in the bass strings fretting in tune.

The twisted-string specification given by Fouchetti for the mandolin, and in the Wensler G30 ms for the cithrinchen, give the diameters of the strands to be twisted together. This could well imply that the musician was responsible for either doing the twisting himself or having it done for him locally, and twisted strings were not made commercially by specialists. Similarly, Carpentier’s specifications of wound strings for the guithare allemande give the diameters of both the core and the winding, and doing the winding could well also have been a DIY operation. I can say from experience that it is much harder to put enough twist (without breaking the wires) into a thicker twisted string to get it to fret truly than to wind thicker metal onto a core. So my suggestion is that a major reason why the later instruments spread much more widely than the cithrinchen did previously was because the skills needed to make the bass strings with appropriate trueness were much more readily available.

FoMRHI Comm. 1530

Citt/Gitt again

Eph has very kindly sent me an advance copy of his Comm herewith and has given me permission to comment on it in this Q. In fact, I’ve not much to say, for his Comm provides a great deal of new (to me anyway) and useful information, but it answers few of the more important of my questions, especially those between the Portuguese and the English instruments.

Which came first? It’s not a matter of the chicken and the egg but of, as one might say, the brown egg and the speckled egg.

What was the connexion? Port factors in Oporto nostalgically playing their memories of the English countryside? Cork importers in London musically reminiscing the beauties of the Portuguese landscape? Wellington’s army whiling away the boredom of wintering behind the lines of Torres Vedra?

Where did both come from? It’s all very well to say that ‘In the second half of the eighteenth century there was an explosion of cithrinchen-type instruments’. Sure, that’s the whole point. The questions are: Why? and Whence?

Who first got the idea that the French, the German, the Portuguese, the English, Uncle Tom Cobley and All (we find them from Dublin to St Petersburg), all wanted cithrinchens, they all wanted them the same shape etc, and they all wanted them now?

And why did they want them? What caused the sudden craze for plunking away on this sort of instrument? Who were the eighteenth century Beatles who were so wildly popular throughout Europe that everyone wanted to emulate them?

And if they existed, what did they play? Was there a sudden outburst of music that could only be played on these things? And if so, where is it today?

To my mind it’s these and others like them that are the questions that need answers. So, as I said before:

Any ideas?
Jeremy asked for more ideas, so I am happy to oblige. The term 'English guitar' is used here both as the French 'guithare angloise' and as a modern generic term to describe a variety of instruments with metal strings in the treble and metal-wound strings in the bass that were played throughout much of Europe from the middle of the 18th century to well into the first half of the 19th century. These instruments were plucked with the fingers, and their tunings, at least in the treble, usually included a major chord. Harp-lutes and similar 19th century amateur instruments with gut and wound-on gut strings, which were the successors to the English guitar had similar tunings on their fingered strings. In my collection I have an instrument which has a 16th century lute back with an 18th century cranked theorboed-lute neck which had pins for fixing metal strings on the end clasp. It is likely that in the period we are concerned with, it was used as a type of English guitar. What else could it be?

The tuning and surviving literature make it clear that the English guitar was primarily used by amateurs with low expectations of musical accomplishment, similar to how the folk guitar is used today. A few chords to accompany the singing of a song was adequate usage. There was very little barrier of technique learning before one could enjoy using it to accompany. The metal treble strings held their tune better and broke less frequently than the gut strings on Spanish guitars, and if the player was not good at tuning (and a servant couldn’t help), out-of-tune bass strings could be left out. For those who found individual courses of strings to be targets too small for their fingers to find, there were broad keys that could move hammers to hit the strings, either on a removable attachment or built into the instrument with the hammers coming out through holes in the rose.

Beside being friendly to beginning players, the instrument was also friendly to first-time makers. With no delicate acoustic optimisations required, an experienced maker of other instruments could make a successful 'copy' after just a quick examination, so local makers could spring up wherever the instrument appeared and was wanted.

There are several factors that point to France as the country of origin. The French aristocracy in the middle of the 18th century led European fashion, and they treated musical instruments as fashion accessories. Consequently, French instrument makers had to become the most innovative in Europe. A new invention is more likely to be made by makers who had to innovate than by those safely following traditions. After the initial invention and popularity growth, they developed the greatest variation in design. Besides the English model (probably the first cistre) and the German model, there was also the lute-body (‘pandore’) model and the sloping-sides model (with the back much smaller than the belly). In my collection, I have a French baroque guitar from this period which has sloping sides copied from that cistre model.

For the French to be the inventors, the model names ‘English’ and ‘German’ would have to be related to fashion images of these countries, and have nothing to do with actual origins. There is no evidence for such origins and clear evidence against them. Bremner in 1758 wrote that the instrument was ‘but lately introduced’ to Britain, and German evidence of a truly German version says that it appeared around 1780. The ‘English’ model was smaller, and in the pictures was mostly played by women, while the 'German' model was larger with an extended neck, and was more of a man’s instrument. Carpentier’s cistre method was for the German model and only mentioned the English model in passing, obviously not taking it seriously (a typical man’s attitude towards a ‘woman’s’ instrument). Evidently Englishness had some feminine connotations and Germanness masculine ones in French culture at the time.

A clue in the design of the French English guitar points to its being the first one. From the type of tuning, we expect that the English guitar developed from the cithrinchen. In the cithrinchen body design, the sides come off the neck perpendicular to it, in a curve creating somewhat of a shoulder, as on the bandora. This is also the case with the French English guitar. This is also true with the Norwegian sister, which was just considered to be an up-to-date cithrinchen. But
in countries such as England, where cithrinchen-type instruments were either well forgotten or never existed, this aspect of body shape was not copied in their versions of the instrument, and a more cittern-like shape prevailed. This can imply that the French instrument was closer to the original than the others, and so probably predated them.

Another such clue is the screw tuning mechanism that was usual on the French English guitar. This would not have been a commercial proposition without the c. 1740 French invention of the lathe with a screw-driven tool holder by Antoine Thiout. It was developed for cutting screw threads on spindles used in clocks and watches, but it soon found other applications, such as screws for bows, the first documentary evidence of which is in the first volume of l'Encyclopédie in 1751. The original screw device on the French English guitar was not to everyone’s liking (perhaps recognising the right screw end to fit the key on presented some difficulties), so the French invented the tuning device involving a worm gear that all modern guitars have. What the fingers turned was a metal ring. Having it look like a normal peg head became fashionable only later.

In France, the English guitar was an instrument of the aristocracy. The high level of decoration has led to a larger fraction of these instruments surviving than would otherwise be the case. One would expect that the initial wave of spreading of use would be to the aristocracies of other countries that imitated French fashions. I would expect that the English aristocracy would be quick off the mark in doing this. The Portuguese aristocracy probably couldn’t afford to. The English also had a wealthy merchant class, many of which followed top French fashions. As Jeremy points out, some had strong commercial ties with Portugal. I would therefore guess that the direction of influence is more likely to be from England to Portugal than the other way around. An argument against both countries independently acquired the instrument from France via their aristocracies is that their instrument designs have the same deviation from the French English guitar (and the cithrinchen) mentioned above.

Of course, documentary evidence is much to be preferred to speculations such as these, but the way forward in history when the relevant evidence is not available is to make the most reasonable speculations we can, especially when there is peripheral evidence that could seem to favour one possibility more than others. Alternative speculations are very welcome indeed. Speculations have also the value of pointing to areas where relevant evidence might be found. The speculations here would suggest that sources outside of Spain and Portugal that deal with serious or folk music are much less likely to be fruitful than sources that deal with fashionable diversions of the affluent.

*****

Jeremy responds: Eph kindly sent me this, too, and it seems to answer all my questions — many thanks to him. About the only thing he’s forgotten or omitted, which would add to his remarks about women’s instruments, is the legend that the added piano-type keys (Smith’s patent box, etc: versions were patented by Clauss in 1783 and by Goldsworth in 1785) were produced because ladies complained that the wire strings damaged their fingernails.
Explanations of ways in which wood absorbs sound vibrations

Instrument makers are concerned about the sound produced by their instruments, and expect that sound absorption by the materials the instrument is made of can be an important factor. They have experience in handling these materials, and what they learn from that experience is the engineering of how to manipulate them to get the desired instrument properties, and also they often form mental images of how the materials respond to different circumstances. These images are often strongly influenced by what others have told them about these materials.

The purpose of this note is to enrich these images by offering my own set of mental images of how wood absorbs sound vibrations. My images are influenced by a background as a materials scientist, some knowledge of acoustics and of the basic structure and chemistry of wood, and some awareness of the large amount of experimental research into the properties of wood.

It should not be assumed that my images of what is going on inside wood are typical of what most wood scientists will tell to non-specialists. Most are more careful than I am to distinguish between ideas that are well supported by experimental studies and speculations that go beyond these studies. Reticence with expressing speculations outside specialist circles is often considered wise since further experimentation could well show some of them to be wrong, and trust in science (and the speculator) can be undermined by this happening. To me, science is led by speculations. They stand or fall on the basis of consistency with experimental evidence, and I feel that many non-specialists are happy to follow how this exciting process operates.

A better understanding of the physics of what is going on in an instrument can possibly lead to making better instruments, but if that happens, it is a bonus. Excellent instruments are made by makers who just do what they have been taught to do, doing it superbly, without any deep understanding of the materials they use. The main contribution of science to makers, and to society as a whole, is not in its practical applications, but in the cultural contribution of the beauty of more objective understanding of our world than is otherwise on offer.

Sound and heat
Sound in a material involves waves of compression and expansion. Transverse waves in a slab of material that transmit sound to air involve the same processes, compression on the inside of a bending curve and expansion on the outside. If during the compression and expansion, every bit of the material stays put in the same position relative to its neighbouring bits, so that they breathe together, the material is 'elastic', and there is no sound absorption. But if there are bits in the material that have enough freedom of movement to rub against or bump into other bits during the vibration, these rubbings (on any scale) or bumpings (if the bits are of molecular size) radiate sound-like waves of expansion and compression with extremely short wavelengths, which is how heat expresses itself in materials. Some of the energy the bits contribute to heat energy came from their riding along with the sound vibrations. The sound vibration energy is thus reduced and we experience this as sound absorption.

Friction
The compression and expansion of the material in sound is in one direction only. If a crack is at an angle to that direction, the material might find it easier to spread the compression or expansion to include another direction by slipping along the crack. Rubbing along the crack will absorb sound. Such cracks can occur because of damage, faults in the original wood, or between detached fibres inside degraded (old) wood.

The effect in the last case is a mild absorption of sound in the high frequencies for bowed instruments, giving a more round mellow sound, but when string excitation stops, the faster fall-off of the high frequencies caused by absorption leads to poor projection on plucked instruments. The same differences occur when a hair unwinds from a gut string and touches the fingerboard, rubbing against it when the string vibrates. It is not a serious problem with
...pluckers need to keep a nail scissors or clipper handy to trim hairs because hairs touching the fingerboard cause serious tone deterioration, especially on thin strings.

It is likely that the effect of friction in large cracks in the wood is similar. Of course, where the two sides of a crack don’t touch, there is no friction, and no sound absorption results.

Creep
When we want to permanently bend wood, to make it pliable, we use moisture (preferably as steam) and heat (at over about 90 degrees C.). When pliable, we bend it, and when it cools, it retains the shape it was bent to. The outside of the wall of each cell in the wood is glued to the walls of adjacent cells by a mixture of lignin and hemicellulose. The heat melts the lignin and the moisture swells the hemicellulose, both of which help the cell walls slip past one-another to relieve the bending force. When the wood is cooled, the new neighbours of the cells get fixed (because the lignin returns to its original highly-viscous state), and there is no memory in the wood of the way things were before. If there is any spring-back, it is because all of the wood wasn’t hot and wet enough for all of the cells to slip and relieve the bending force.

At room temperature, the same process occurs, but at a very much slower pace. Then it is called ‘creep’. Creep slowly relieves stresses in wood when there is a constant bending force:

<table>
<thead>
<tr>
<th>UNBENT, UNSTRESSED</th>
<th>BENT, STRESSED BEFORE CREEP</th>
<th>BENT, STRESS-RELIEVED AFTER CREEP</th>
</tr>
</thead>
<tbody>
<tr>
<td>stretched side</td>
<td>compressed side</td>
<td>top and bottom same length</td>
</tr>
</tbody>
</table>

In arched soundboards, the relevant bending is the difference in arching curvature at each point before and after the strings are tuned up, making the bridge press down. (Makers who believe that their arching curves don’t change on tuning up the strings haven’t seriously tried to measure such changes.) Creep happens mostly in the first weeks and months of an instrument being newly strung up. Sound vibration speeds it up creep and creep absorbs sound vibration, hence the need for playing-in. All of the audible frequency range is affected. Creep also undoes most of the spring when bass bars are sprung.

In creep there are creep surfaces where the cells on one side move relative to those on the other side. Sizeable gaps across the surface cannot develop during the movement, so the shape of all the cell walls on one side has to make an excellent fit to the shape on the other side before and after each movement. The direction of creep in which this condition is most easily met is along the grain. That is because wood cells are very elongated along the grain and the cross-sectional shape of each one changes very slowly when moving along that direction. It is possible that this is the only direction in which creep occurs in wood.

While there is a large-scale shearing force that drives creep, it occurs in very small steps in microscopic regions. When one region has just had its slip, its share of the shearing force is reduced and redistributed to neighbouring regions. As each of them has its slip, its share is redistributed, including a part to our region. Thus our region’s share of the force keeps building up until it gets so big that it has to have a slip again.

How sound vibration speeds up creep is not clear. The stretch part of the vibrational cycle could either help slips to start before they would normally happen, or it can induce more slip surfaces to become active. A mechanism similar to that for friction described above is also possible. Regardless of such details of mechanism, it appears general that whenever wood is not in equilibrium with its environment, and it can approach equilibrium faster by using energy from the vibration it is subjected to, it will do so.
Compression and cell-wall buckle
A bowed-instrument bridge is cut so that the grain direction in the wood corresponds with its width, the radial direction in the wood with its height, and the tangential direction in the wood with its thickness. Such bridges often warp in time, forming a curve along its height (the wood's radial direction). There seems to be permanent expansion of the wood along the outside of the curve and/or compression on the inside of the curve. An instant cure for most of the warp is to drop the bridge in boiling water. Since the wood mostly 'remembers' its original shape, the kind of creep we have been discussing cannot be strongly involved in the warp.

When one bends a piece of wood without stress relief, the wood on the outside of the wood stretches a bit and the wood on the inside of the curve compresses. In the compression, the cell walls in the direction of the compression force buckle. They bend like flat springs into the air spaces on the inside of cells. Each buckle involves the walls of two adjacent cells glued together by the mixture of lignin and hemicellulose. If, as a result of time (creep) or heat (moisture helps), the gluing material allows the two cell walls to slide with respect to one another, the stress between the two cell walls (expansion on the outside of the curve and compression on the inside) can be relieved, reducing the force resisting the compressing force to the sum of two buckling flat springs, each of half the thickness. When the compression force ceases, the glue between the cell walls holds them in the buckled shape. They can straighten up again only by heat and moisture, or by time.

If the cell-wall buckling in the wood relaxes by creep over time, that creep absorbs sound. An example is the power sanding of instrument components. Power sanding produces compressed wood (i.e. with buckled cell walls), and the buckling is largely locked in by the heat. The creep as the buckles slowly straighten out makes the components lose a large part of their resonance by sound absorption. In time, that resonance will return, but it can be returned immediately by heat and moisture.

Water and hemicellulose
Of the dry weight of wood, 99% is composed of three chemical constituents, cellulose, hemicellulose and lignin. Their proportions are about a half, a quarter and a quarter, in that order. It is all in the cell walls (and the glue sticking them together), with air in the enclosed spaces where cell contents used to be. Once the wood is initially dried, there is no tendency for water to accumulate in the air spaces. The water content of wood is almost wholly due to the tendency to accumulate water by the hemicellulose molecules, which are located in the cell walls and in the layer between cell walls. Each of these molecules is a highly-branched polymer with many electrical dipoles along each chain. These dipoles are attracted to the dipoles of other chains, but if water molecule are available, their dipoles are preferred, and are absorbed into places between the chains. This swells the molecule, and is the only mechanism by which water is absorbed by and swells wood.

The thermal motion of the chains randomly kicks some water molecules out of their positions between the chains, while others are getting captured elsewhere. When the rate of expulsion equals the rate of capture, we have equilibrium. Beside temperature, the water content at equilibrium depends on the availability of water molecules for capture, which is directly related to the relative humidity outside the wood. The moisture is transmitted between the outside and the air inside each cell by chains of cell insides interconnected by pore holes within cell walls.

When water molecules are moving in and out of being captured by the chains in the hemicellulose molecules, they bump into molecules of the wood. When the wood is vibrated, causing expansions and contractions, the water molecules get extra energy from the vibration, which the bumps dissipate into heat vibrations, causing sound absorption. The effect is across the whole sound spectrum of frequencies.

The more water content the cell walls have, the more sound absorption there is. Instruments don't sound as loudly after a bout of wet weather as after a bout of dry weather. At the same relative humidity, the equilibrium water content of the cell walls decreases some when it is
vibrated (the hemicellulose molecules can't hold as much with all that jiggling).

The equilibrium water content can decrease a lot more if the number of capture sites on each hemicellulose molecule is reduced. That is just what happens when wood is degraded, either naturally with age and the humidity cycling of weather, or artificially by means such as stewing. Hemicellulose is, by far, the least stable of the three constituent materials in wood. Dry wood at 20 degrees C will lose 1% of its weight per century due to the ends of the chains of hemicellulose molecules breaking off into molecules of gas during thermal jostling. This gets faster with higher temperature. Moisture also speeds it up, making sugary molecules break off (which caramelise into a black cloud in the water when wood is stewed).

The hemicellulose molecules in old wood have had their chains shortened, reducing their volume and contracting the wood. These molecules can absorb less water, so the sound absorption due to the water is equivalent to that of recent wood at a much lower relative humidity.

Humidity cycling and moisture gradients
When the relative humidity is cycled, either by the weather or artificially as a treatment, there are moisture gradients in the wood. Neighbouring bits of wood are swollen different amounts, with the less swollen bits compressing the more swollen bits, and the more swollen bits stretching the less swollen bits. The unbalanced forces lead to local creep, which absorbs sound. A moisture gradient will mostly enhance sound absorption by the diffusing water molecules extracting energy from the sound waves to diffuse faster.

The stresses of moisture gradients speed up the degradation of the hemicellulose, as evidenced by the soundboards of old lutes, which show less age contraction where they have been securely glued to cross bars than in between the cross bars. The moisture content changes with relative humidity only if it is free to do the corresponding contraction or expansion. Otherwise, instruments would fall apart as soon as the weather changes.

The basic sound absorption of the wood itself
If we subtract all of the effects described above from the total, there remains the sound absorption due to the three types of molecules that the wood structure is made of. The mechanism involved is essentially the same as with water, i.e. if parts of molecules are free to move in a way that is not correlated with the movement dictated by the sound wave, they will do so, and their resultant collisions with other parts of molecules transfers energy from the sound vibrations to the heat vibrations. Free bit of molecules that can do this are least likely to be in the collagen microfibrils (which are rather crystalline) and most likely to be amongst the hemicellulose molecules (which have the most loose ends). It should happen less when these molecules have shorter chains, which leads to the interesting prediction that the basic sound absorption of old wood should be less than that of recent wood. This adds to the effect of differing moisture-absorbing capacities.

Related issues
Instrument design affects resonance in many ways. The most direct way sound absorption comes into this relationship is that the more wood there is that is vibrating, the more sound absorption one should expect. This has rarely been a factor of overriding importance.

The material factors that affect resonance other than sound absorption are the innate stiffness of the wood (stiffer is better) and the density of the wood (the more air and less cell wall the better).

It is quite possible that a reader will know of a place here where I got the facts wrong, or another way that wood absorbs sound, or a better explanation for how it works. If so, please discuss this with the rest of us. We all have much to learn about the interaction between wood and sound.
The dimensions of windings on a traditional wound string

This Comm. continues the analysis presented in FoMRHI Q13 (Oct. 1978), Comm. 163 pp. 50-2. There we derived the weight, as expressed by the equivalent diameter (i.e. the diameter of the core material that would have the same weight), of a wound string in terms of the diameters of the core and winding materials, their densities and the advance per turn along the string. The variables were represented as follows:

- $D_c$ is the diameter of the core,
- $D_w$ is the original diameter of the winding wire before it gets wrapped around the core,
- $\rho_c$ is the density of the core,
- $\rho_w$ is the density of the winding wire,
- $D_e$ is the equivalent diameter of the string, and
- $p$ is the advance along the string of one turn of the winding.

The equation is: \[
(D_e/D_c)^2 = 1 + \pi (\rho_w/\rho_c) (D_w/D_c) \sqrt{ [(D_w/p)^2 - (D_w/\pi D_c)^2] / [(D_w/p)^2 + (D_w/\pi D_c)^2] } \]. It applies to open-wound as well as close-wound strings.

The novel assumption that was made in deriving this equation was that as the core turns during string-making and rolls the winding over it, there is no slip between the winding wire and the core as soon as contact is made. As the core pulls the winding wire into a curve, the parts of the wire away from the core must stretch, but the part in contact with the core does not.

One superiority of this model of the structure of the wound string over other models previously proposed by us (and proposed before and since by others) is that the derived formula depends on the wire diameter before being wound around the core, and does not depend on the unknown shape of the winding wire after being wound around the core. The other models can be shown to be wrong in detail, but lead to formulas which are reasonably good approximations for practical purposes. They all are the same as the formula above except for the contents of the bracket \([\ ]\) after the $\sqrt{ }$, where they are different (but close to 1) or have nothing. This model appears to be correct because it fits the measurements made so far as accurately as can be hoped from the accuracy of the measurements. Of course, if more accurate measurements were found to deviate from it, the model would need modification.

The measurements that validated the model were made on the string-winding rig during normal production of close-wound strings on gut. The core diameter was the stretched and dried core diameter under tension in the rig, somewhat less than the core diameter in storage. The wire diameter was the actual wire diameter under tension feeding the winding, a bit less than the wire diameter in storage. The advance per turn along the string was somewhat greater than the advance per turn off the rig, since when the core tension was relaxed, the string visibly contracted. The equivalent diameters were calculated using the formula, and compared with measurements of them on another rig that measures vibrating frequency for a standard tension (near to that used for winding) and a standard vibrating length.

At that time, when we measured (at tension on the rig) the advance per turn along the string ($p$), we found that it was smaller than the diameter of the feeding winding wire ($D_w$). The wire apparently contracts on bending around the core. And there is still space to take up between the turns when the string contracts on tension relaxation. It is these details that are missing from other models of the wound string. We also measured the overall diameter ($D_o$) of the wound string. Having no theory giving the cross-sectional shape of the wire after winding, we postulated empirical equations for $p$ and $D_o$ to fit the measurements. That theory is given here.
Let us start with the winding wire before it bends around the core. Its cross-section is circular with a radius $R = (D_w/2)$. The core is a circular cylinder with radius $r = (D_c/2)$, and the length of wire needed for a full turn (neglecting for now the helix advance on a real string) is $2\pi r$. When bent around the core, a ribbon element within the winding wire that is at a constant distance $z$ from the surface of the core is stretched to a length $2\pi r(r+z)$. Its width (across the whole wire) is $x_2$ and its height is a small $\Delta z$. That same ribbon of material before bending had a height of $\Delta y$ at a distance $y$ from the edge of the wire that was going to touch the core, and a width $x_1$. It is the same volume of material, so $2\pi (r+z)x_2 \Delta z = 2\pi r x_1 \Delta y$.

Then $(x_1/x_2)(\Delta y/\Delta z) = 2\pi (r+z)/2\pi r = 1+z/r$. The increase in length causes a contraction in both height and width. Because of isotropy, the proportion of these contractions should be the same, so $(x_1/x_2) = (\Delta y/\Delta z) = \sqrt{1+z/r}$.

We are now in the position to calculate the relationship between the $y$ position of the ribbon before bending and its $z$ position after bending. Integrating $dy/dz = \sqrt{1+z/r}$ gives $y = r(2/3)\{(1+z/r)^{3/2}-1\}$, and solving for $z$ gives $z = r\{(3/2)(y/r)+1\}^{2/3}-1\}$. For the total contracted wire height on bending, we put $y_{max} = 2R$ into the last equation and get $z_{max} = r\{(3(R/r)+1)^{2/3}-1\}$. To get the final width, we first have to find the $z$ that the ribbon at $y = R$ ended up at, which is $z_R = r\{(3/2)(R/r)+1\}^{2/3}-1\}$, and plug it into $(x_1/x_2) = \sqrt{1+z/r}$, with $x_{1max} = 2R$, and get $x_{2max} = 2R\{(3/2)(R/r)+1\}^{-1/3}$.

These equations are accurate enough for most purposes. But it is an insult to our mathematical slaves (computers) not use them to get as high an accuracy as can conceivably be asked for. For this we include the advance per turn ($p$) in the calculation of the original length per turn of the wire, which becomes $\sqrt{(2\pi r)^2+p^2)}$, and the elongated length of the ribbon when bent, which becomes $\sqrt{(2\pi (r+z))^2+p^2)}$. The ratio of these can be expressed as $\sqrt{[(u^2+d)/(1+d)]}$, where $u=1+z/r$ and $d=(p/2\pi r)^2$. The square-root of this ratio is $[(u^2+d)/(1+d)]^{1/4} = x_1/x_2 = dy/dz$. I couldn't integrate $dy = r(1-d)^{-1/4}(u^2+d)^{1/4}du$, so I replaced $(u^2+d)$ by $u^2(1-d/u^2)$ since $(1-d/u^2)$ could be replaced by a rapidly converging binomial series. Since $p < 2r$ and $u > 1$, $d/u^2 < 0.1$ (or $0.1\%$), so stopping here is accurate enough. Integrating from $u = 1$ to the ribbon $u$ gives: $y/r = (1-d)^{-1/4}\{(u^{3/2}-1)-((d/2)(u^{-1/2}-1) + (3/5)(d/8)^2(u^{-5/2}+1)\}$. Call this equation (1)
Before we can do any calculations, we need to have \( d \) or \( p \). Let us consider the conceptual cylinder coaxial with the core at which the neighbouring windings are closest to touching. Its radius is \( z_R + r \). Now let us conceptually cut it parallel to the axis and flatten it into a ribbon. Its width is now \( 2\pi (z_R + r) \), and the centres of the winding wires are parallel lines crossing the width at an angle \( \theta \) to the axis edge. Then \[ \cos \theta = \frac{2\pi(z_R + r)}{\sqrt{\{2\pi(z_R + r)\}^2 + p^2}}. \] This also equals the perpendicular distance between wire centres (call this distance \( h \)) divided by \( p \). Thus \[ p/h = \frac{\sqrt{\{2\pi(z_R + r)\}^2 + p^2}}{2\pi(z_R + r)} \]. Solving for \( p^2 \) gives \[ \frac{1}{p^2} = \frac{1}{h^2} - \frac{1}{\left(\frac{3}{2}(R/r)+l\right)^2}. \]

When winding, the straight uncontracted wire is pressing against a contracted wire of the previous turn and beds into the core. As it is bent around the core, the point where it is embedded stays put, and as it bends, the wire contracts, leaving a space between each winding which is half the difference between the width of the contracted bent wire and the uncontracted straight wire. Thus the perpendicular distance between wire centres is \( h = x_{\text{max}}/2 + R \). Since \( d \) is used here in rather small corrections, very high accuracy is not required, and from above, we can approximate \( 1 + z_R/r = \left(\frac{3}{2}(R/r)+l\right)^{2/3} \), and \( x_{\text{max}} = 2R\left(\frac{3}{2}(R/r)+l\right)^{1/3} \). Thus we finally get \[ \frac{1}{d} = \frac{4\pi^2}{R/r} \left(\frac{3}{2}(R/r)+l\right)^{1/3} \left(\frac{3}{2}(R/r)+l\right)^{2/3}. \] When we calculate \( p \) properly, we'll use more accurate values of \( z_R/r \), and of \( x_{\text{max}} \) that is derived from it.

Equation (1) gives \( y/r \) from \( z/r \), but we sometimes need to get \( z/r \) from \( y/r \), for which we need to use successive approximations. We start with a known \( y/r \) and use the approximation \( z_0/r = [(3/2)(y/r)+1]^{1/3} - 1 \), which we plug it into equation (1) (where \( u = 1 + z_0/r \)) and get \( y_0/r \). We next use \( (y-y_0)/z-z_0 = dy/dz = [(u^2+d)/(1+d)]^{1/4} \), (where \( z_0 \) is still used in \( u \)). We solve for \( z = z_0 + (y-y_0)/[(u^2+d)/(1+d)]^{1/4} \), calling it \( z_1 \), put it into equation (1) and get \( y_1/r \), which should be much closer to the original \( y \). If it is not close enough, do another cycle. This method of successive approximations is only needed for finding \( z_R \) and \( z_{\text{max}} \).

To show the contractions on a graph, it is convenient to get everything we can on the same scale, so we plot \( z_{\text{max}}/2R \) and \( x_{\text{max}}/2R \) as functions of \( R/r \). These are shown in Figure 1, with the lowest curve being \( x_{\text{max}}/2R \) (contraction of height above the core). To get the final cross-sectional shape as an equation, we start with the original circle \( (x_1/2)^2 + (y-R)^2 = R^2 \), replace \( x_1 \) by its function of \( x_2 \) and \( z \), replace \( y \) with its function of \( z \), and get \( x_2 \) as the wanted function of \( z \). The cross-sectional shapes are shown on Figure 2. To get them on the same scale, we plot \( z/R \) against \( x_2/2R \). The origin is the point of contact with the core. Different shapes come from different \( R/r \)'s, and the ones shown, from outside in, are \( R/r \) equal to very nearly 0.0 (exactly 0 is indeterminate, so I used 0.001), 0.2, 0.5, 1.0 and 2.0. The outside circle represents the original cross-section before winding, and the other shapes are the contracted cross-sections after winding. All shapes have the same curvature near the origin, so the cross-sections are near round with \( R/r \) small, and they become egg-shaped when \( R/r \) gets big.

I am sure that it is because the cross-sections look so circular that the contractions haven’t been noticed, and people have assumed no difference between the wire before and after winding.

With properly calculated \( z_R \) and \( x_{\text{max}} \), we can now calculate \( p/2R \). This gives the curve on Figure 1 that starts out below the fourth curve at low \( R/r \)'s and then crosses it to be the highest curve at high \( R/r \)'s. A blown-up version of this theoretical curve is shown with the measurements (as open circles) in Figure 3. The agreement is excellent amongst the heavier strings where the accuracy of measurements is greater.
We usually measure wound strings when they are not tuned up on an instrument. We can measure the advance per turn (p') and the core diameter (D_c or 2r), and usually want to know the original winding diameter (D_w or 2R) to calculate the weight (as the equivalent diameter). Then the perpendicular distance between wire centres is h'' = x_{2\text{max}}. We can calculate p'/2R as before and multiply by R/r to get p'/2r as a function of R/r. This relationship is shown in Figure 4. This graph should be very useful in analysing an unknown close-wound string. To use it one measures p and D_c and calculates p/D_c. Find that value along the vertical axis of Figure 4 and then go across the graph and find the point where the curve has that height. Drop down from that point to the horizontal axis, and the reading is the D_w/D_c that one then uses in the calculation of equivalent diameter (or multiplies by D_c to get the original wire diameter D_w).

The measurements were all made on the winding jig with a gut core. As one winds, the embedding squeezes the core and it consequently lengthens visibly. When one takes the string off the winding jig, it visibly contracts, closing up the little spaces between windings. They open up again when the string is tuned up on the instrument.

We are now in the position to study the embedding by comparing measurements of the total outside diameter (D_O) of the wound string in Comm. 163 with a theoretical calculation of D_O without embedding. In the measurements, the relative contraction is D_O/(2D_w+D_c), and in the theoretical curve, the relative contraction is (2z_{\text{max}}+2r)/(4R+2r). That curve is the remaining one on Figure 1. A blown-up version of it is shown on Figure 5 with the measurements as open circles. Let us assume that all of the difference between the measured and theoretical points are due to embedding into the core, decreasing D_c. Then the difference ΔD_O = ΔD_c, so ΔD_c = D_O(ΔD_O/D_O). Consider the two close-together measured points (circles) on the right, where the accuracy is greatest. D_O is around 70 thou and ΔD_O/D_O is around 0.015, so ΔD_c is about 1 thou, and the embedding into each surface of the gut core is half that, or about 0.01 mm. The amount of embedding depends on the tension in the winding wire during winding, and that has to be less if the wire is thinner to avoid breaking it.

Measuring p (or p') is a strain on the eyes. A magnifying lens helps. One has to very steadily hold the string next to the scale of a ruler and count the number of turns in a particular distance along the ruler, trying to avoid parallax inaccuracies. The advance per turn is that distance divided by the number of turns. The larger the distance and the number of turns, the more accurate is the measurement.

What is often preferred is to derive the equivalent diameter purely from micrometer measurements of the overall diameter (D_O) and the core diameter (D_c). If one is willing to ignore the effect of embedding, this can be done from Figure 6, which plots D_O/D_c = (2z_{\text{max}}+2r)/2r = 1+z_{\text{max}}/r as a function of R/r. This graph can be used the same way as the p/D_c graph above, calculating D_O/D_c from the measurements and then look up D_w/D_c.

The final possibility easily catered for is when the core is inaccessible, in which case one can get D_w/D_c from a measurement of p/D_O by using Figure 7, which is the curve of Figure 4 (p/D_c) divided by that of Figure 6 (D_O/D_c). It must be kept in mind that these curves are only relevant to close wound strings made in the pre-mid-20th-century traditional way with round windings directly onto the core.

In conclusion, until more accurate measurements of wound strings are made that could challenge it, I believe that the theory presented here represents the most accurate account of the winding shape and dimensions of a traditional wound string that has been offered. The surprise is how much the windings contract when bent over the core, and that how similar to a circle the cross-section remains.
FIGURE 1

- advance ($p/2R$)
- overall diameter
  \[\frac{(2z_{\text{max}} + 2r)}{(4R + 2r)}\]
- height
  \[\frac{z_{\text{max}}}{2R}\]
- width
  \[\frac{x_{z_{\text{max}}}}{2R}\]

$R/r$ or $Dw/Dc$
FIGURE 2 shows the cross-sectional shape and size of winding wire after bending around the core. The different curves are for different ratios of the original winding wire diameter divided by the core diameter ($R/r = Dw/Dc$). The original wire diameter is represented by the circle on the outside.
Dots are theoretical, circles experimental.

FIGURE 3

Dots are theoretical without embedding, circles experimental.

FIGURE 4
FIGURE 6
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* in left hand margin = change of address or other change

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