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FELLOWSHIP of MAKERS and RESEARCHERS of HISTORICAL INSTRUMENTS


Honorary Founders: The Fellows have been consulted as to whether Jeremy Montagu and Eph Segerman might be made Honorary Founders of FoMRHI. Except for one abstention (a Fellow who does not favour such titles but does not wish to oppose the motion) they have voted unanimously in favour of this. This is a modest and apt recognition of their role, not just in founding FoMRHI but in making it flourish over a quarter of a century, and we are pleased that both have agreed to accept. We wish them a long and active retirement and hope that they will continue to contribute Comms and reviews for many years to come.

Two issues merged: Owing to the very slow rate of arrival of Comms in the spring and summer, this single Quarterly takes the place of both 107 and 108. Quarterly 109 should appear normally. There continues to be a particular shortage of Comms on the practical aspects of instrument making, which will be especially welcome. The lack of a fourth issue will be reflected in the 2003 subscription, and we hope compensated for also by the planned Directory of Suppliers.

New FoMRHI Address: Following merger with the University of North London, London Guildhall University has become London Metropolitan University, so our secretarial and editorial address is now London Metropolitan University, 41-71 Commercial Road, London E11LA. Members who remember the London College of Furniture and the City of London Polytechnic will realise that this is the third change of institutional name in eleven years. David’s and my email addresses have become d.armitage@londonmet.ac.uk and ljones@londonmet.ac.uk, though in practice our previous lgu.ac.uk addresses will continue in operation for some time.

Directory of Suppliers to Instrument Makers: In Bulletin 105 I announced our aim of producing a new directory of suppliers of materials for instrument making, broadly following on from Mark Norris’s Suppliers to Craftsmen Musical Instrument Makers (Scottish Development Agency, Edinburgh, 1982). The aim is to draw mainly on information supplied by members, for mutual benefit and, through the sale of copies, to inform and encourage others. Apart from valuable offerings from two of our own students, we have received no contributions of information whatever, and not even one request for the email version of the form, which was intended to make things easier both for members making submissions and for those assembling the list. I have previously noted the difficulty of getting active makers to write for the Quarterly, and Jeremy, before me, remarked upon this on several occasions. David and I had thought of this project as one which would allow maker members to share their knowledge to the general benefit of the fellowship. As a gauge of willingness to do so, this present lack of response is very disappointing. We hope that the idea might not be dead, and that contributions will now flow in, by an extended deadline of the end of this year, please. It is possible, of course, that the idea is misconceived, and that there are reasons why such a directory is not needed or wanted. If this is the case, it would be helpful if members would tell us. If you demonstrate that you want it, the directory will be created. If you don’t, it will not be.

An updated version of the form printed in Bull. 105 is given on page 4 here.

Joint Meeting with the Lute Society: On 25 January 2003 FoMRHI will hold a joint meeting with the Lute Society at the Artworkers’ Guild in London. As far as we can recall, this will be the first FoMRHI meeting for some thirteen years. The emphasis will be upon stringed instruments, mainly plucked, and among those speaking will be Andrew Atkinson, Richard Coleman, Lewis Jones and Alice Margerum. Though time will be limited, it is intended that presentations will be fairly short, so there may be room for more. Please let me know if you would like to speak on a subject appropriate to the day. Fuller details will be given in Bulletin 109.
London International Exhibition of Early Music: This year the exhibition is at the new premises of Trinity College of Music, in the Old Royal Naval College, Greenwich. It takes place on Friday 25th October (11.00-6.00), Saturday 26th (10.00-6.00) and Sunday 27th (10.00-5.30). Access by public transport is via Greenwich station or the new DLR station at Cutty Sark. David and I will be there, this year representing London Metropolitan University, and we will be pleased to see any members exhibiting or attending.

Comm. 1784 - an apology: Comms are increasingly received by email, which has the advantage of allowing a measure of reformatting and editing, but also entails risks of corruption. Jeremy Montagu’s Larigot review, Comm.1784, is a case in point. The three underlines after each figure at the end of the title line were, when they left him, the sign for the Euro, intended to indicate the prices of the publications.

RILM Abstracts of FoMRHI Comms: In Bulletin 105 I commented on the relatively small number of FoMRHI Comms abstracted in the Répertoire International de Littérature Musicale (RILM), the main international database of musicological writings. RILM is simply the best single way of communicating the existence and importance of FoMRHI Comms to the rest of the musical and musicological community. The task of RILM UK is to gather and edit abstracts of writings published in the UK. They urge you to send abstracts for any items, new or old, which have not yet appeared on RILM. To make the task of submitting abstracts easier, RILM have created new forms (in rich text format), available on their website, which can be saved, completed and returned as an email attachment to s.hibberd@rhul.ac.uk; or printed out, completed and returned by post. Any queries can be addressed to Sarah Hibberd, RILM-UK Coordinator, Music Department, Royal Holloway, University of London, Egham, Surrey TW20 0EX, UK; tel.: +44(0)1784 443532; fax: +44(0)1784 439441; email: s.hibberd@rhul.ac.uk. The form for Comms. may be downloaded from: http://pages.britishlibrary.net/rilm-uk/submit.htm. Before submitting abstracts, it is helpful to study the style guide, How to Write a RILM Abstract, at: http://www.rilm.org/abstractinfo.html. If you do not have easy access to the RILM database and are unsure as to whether particular writings are already included, please contact the RILM-UK co-ordinator. The homepage for the UK branch of RILM is: http://www.rilm.org/. I will be pleased to send paper copies of the form on request.

John Storrs Workshop: an update: Peter Foster's Comm. 1788 included out of date information about the John Storrs Workshop, which has moved to Rugby and is now run by Stephen Robinson. The new address is John Storrs Workshop, Unit 13, Arches Business Centre, Mill Road, Rugby, CV21 1QW (tel. and fax. 01788 577134; email: StephenRobinson@JohnStorrsWorkshop.co.uk) and there is also a website: http://www.JohnStorrsWorkshop.co.uk.

Kloster Michaelstein Symposium on the Square Piano: This Quarterly includes an account by Peter Forrester of the symposium on the guitar and cittern held at the Kloster Michaelstein in November 2001. The next symposium there, from 11-13 October, is on the square piano, and addresses the theme 'Is the square piano still respectable today?' It seeks to explore 'the history and importance of a neglected keyboard instrument'. The instrument collection at the Stiftung Kloster Michaelstein has several square pianos, including one by Clementi & Co. The event commemorates the 250th anniversary of Clementi’s birth and celebrates his varied achievements, notably in morning and evening concerts in which his music will be played on the instrument.

A Virtual Sodi Harpsichord: The Royal Albert Memorial Museum in Exeter, which houses a fine harpsichord by Vincenzo Sodi, is making information about the instrument available at the website www.virtual-vincenzo.co.uk. In addition to articles and a forum for comments, the site will make available drawings and measurements, and Martin Grice Peters is creating a ‘virtual’ version of the instrument, using its sampled sounds. As the intention is that the original instrument will no longer be played, the museum wishes also to acquire one of the existing copies of the harpsichord made by Clayson and Garrett, for
wishes also to acquire one of the existing copies of the harpsichord made by Clayson and Garrett, for recitals and performances locally, and is currently trying to raise funds for this. The museum has also recorded a Rewallin Virginal of 1675 and a Kirkman harpsichord, which will also be added to the virtual archive.

Changes of Address

# John Downing, P.O. Box 594, RR#1, Alexandria, Ontario K0C 1A0, Canada

# Ian Harwood, Crosskeys Cottage, Creeting St Peter, Ipswich IP6 8QR. Tel/Fax: 01449 673462.

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**Form for the submission of entries for the FoMRHI Directory of Suppliers to Musical Instrument Makers**

Supplier’s name:

Address (including full postal code and country):

Telephone:
Fax:
Email:
Website:

Is a catalogue or price list published?

Items or services offered

Other comments or notes:

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Not for publication:
Proposer’s name:
Address:

Telephone:
Email:

Please send to Lewis Jones (FoMRHI suppliers directory), London Metropolitan University, 41-71 Commercial Road, London E1 1LA, UK.
The Kloster Michaelstein Guitar and Cittern Symposium: A Report

The Kloster Michaelstein symposium for guitar and cittern, their construction, playing technique and history before 1800, took place at the Kloster on the 16th - 18th November. The symposium benefited greatly from its typically Cistercian setting of country, woods and water, quiet and beautiful, with the only distractions the architecture and the autumnal colours on the surrounding paths. Because it was also residential and completely self-contained - no shops, no television - the informal transmission and exchange of ideas and information between participants was optimised.

As well as two concerts, there were numerous musical demonstrations and the symposium opened with a recital for the participants given by Michael Koch on renaissance and baroque guitars made by Winfried Heitland and Renatus Lechner. The authenticity of his instruments and playing amply demonstrated the value of music still too little heard by the general public.

In addition to those instruments in the Kloster’s permanent collection, an exhibition of nearly forty guitars and citterns was gathered together from fourteen collections especially for the occasion.

The programme was approximately chronological, with the lectures on guitar mostly following those concerning the cittern. Of the two instruments, perhaps the cittern won, in terms of new research, maybe because symposia for the guitar have been held before, particularly in London. This was the first ever for cittern. Both have been undervalued instruments, especially the guitar which has a still largely unexplored repertoire approaching that of the lute in quantity.

The first lecture, by Christian Rault explored nomenclature and the emergence of the two instruments from the fiddle, citole and cetre, illustrated by a large number of contemporary pictures, mostly previously unknown to the audience. He was followed by Peter Forrester who concentrated on the cittern and outlined its history and changes in construction from the renaissance to the 18th century. Florence Getreau discussed five citterns from the Paris collection, their probable provenance and especially the significance of the newly cleaned and exhibited instrument by Girolamo Virchi. She was followed by Andreas Michel who introduced the sometimes strange and amusing history of the cittern during the 18th century in Saxony and the surrounding area.

Louis Grijp and Sebastian Nuñez interwove their lectures to show the importance of the 4 course diatonic cittern in Dutch culture. Only three complete citterns from this period are known to exist, so that the excavation and restoration of the remains of two more, perhaps more typical of the average musician’s instrument, from a 1620 shipwreck in the Zuider Zee, including fragments of the original strings, was certainly the most important item at the symposium. Besides conserving the remains, Sebastian Nuñez has been able to reconstruct an instrument, and this was demonstrated.

Rob McKillop outlined another neglected area - the cittern and so-called English gittar in Scotland, playing on a diatonic cittern and gittar both made by Peter Forrester. The gittar was taken from Britain to Portugal by the wine trade, and its 18th century history there, and the related musical developments culminating in the lado were ably covered by Xose Crisanto Gandara. The symposium now moved to the guitar and back to the 16th century with a talk by Frank Hill about the publications of Phalese, concentrating on his playing instructions and tuning, with octaves on both 3rd and 4th courses.

Lex Eisenhardt introduced 17th century Italian guitar music, describing the different tablature systems and demonstrating a range of music from the alfabeto of Montesardo to the mature mixed tablature of Bartolotti on two guitars, one with wire stringing both made by Sebastien Nuñez. Gérard Rebours carried the guitar’s development further into the century and into France, showing how in this country the mature tablature came not from alfabeto, but was a combination of lute tablature and influences from Italy. Gerardo Arriaga took the guitar into the 18th century, demonstrating its use as
a continuo instrument. The cycle of development of baroque guitar style moved from 'simple' strumming, through a complex mixture of rasguedo and punteado to a 'plain' punteado style.

Steffen Milbradt showed an original instrument by Guadagnini and his copy, which was again expertly demonstrated by Michael Koch.

Two lectures on the acoustics of both guitar and cittern followed. The first, given by Christoph Reuter and Wolfgang Voigt, examined especially the different rates of decay for the overtones of each note and their effect on the tone colour of various instruments. Gunter Ziegenhals also examined damping effects, looking at both good and not so good guitars, and comparing their scientific assessment with assessment by 'blind' tests. He showed how the results of the two methods differed, and continued on to show, scientifically, that they could not agree!

The symposium concluded with two short but delightful lecture recitals. Duo Arte en Parte covered the change from double courses to single strings in Spain. Oleg Timofeyef examined the origin, from a combination of guitar and 18th century cittern, of the chordal tuning of the 7-string guitar which developed in Russia around 1790. His musical examples were played on an anonymous but original instrument.

The two public concerts were long by English standards - around two playing hours each - good value for money! Lee Santana used cittern, jarana and renaissance guitar. The 4 course chromatic cittern used for the first half of the concert was made in 1985 by Peter Forrester, who was hearing it for the first time! Santana's style is highly extrovert and entertaining, and his programme included many of his own compositions. Those for cittern especially worked very well indeed, particularly his 1987 Sonata für Cister. Other modern composers have written for lute with varying success, but 'modern' chords which can sound almost accidental on lute become clear and authoritative on cittern, especially when aided by Santana's ability and familiarity with his instrument. On jarana and renaissance guitar, Santana alternated his own shorter pieces with works by Mudarra and Santiago de Murcia; these too worked well and were well received. A fine and very popular concert.

The second, by José Miguel Moreno, was for vihuela and 5-course baroque guitar, of 16th and 17th century music written for those instruments. Señor Moreno deliberately uses instruments built in a modern manner so that the sound is loud enough for a modern concert hall. This perhaps causes the instruments to sound more alike, and similar to a modern guitar, than those more authentically built. Some of the audience felt that this was a fault. If so, it was more than made up for by his effortless technique. Sitting, to a casual glance, apparently unmoving, he was able to produce a speed and clarity of articulation that this writer has never heard surpassed. Another very successful concert.

The symposium was organised by Monika Lustig and Steffen Hoffmann who should be congratulated on their extremely efficient and unobtrusive organisation - helping the exchange of information and ultimate advancement of learning. Stiftung Kloster Michaelstein hosts a symposium for a different organological subject each year, as well as other related events. This was their twenty-second. It is anticipated that the cittern will receive further attention in about four years time.
Spreadsheet I & F calculation of organ pipe pitch

In my corresponding with several organ specialists concerning Praetorius’s Cammerthon pitch standard, I found that they distrusted I & F (Ingerslev & Frobenius) calculations of an organ pipe’s pitch (see Comm. 1701, Q99, pp.9-12). They had not actually performed such calculations and it appears that they didn’t have the courage to get into the mathematics to do it. Their trust in such calculations might well increase if they could do it easily and compare the results with pipes they have in hand.

The purpose of this Comm. is to show in detail how I have programmed a spreadsheet to do it. Spreadsheets can be found in most computer software packages. Once it is programmed for a pipe in one column, that column can be copied to any number of columns for another pipe in each one, with the programming already done.

For reference, the equation we are trying to solve is \[ \cot(\frac{\ell}{2}) = 1.30 \frac{r}{(S/s)} \left( \frac{K(e)}{F(e)} \right)^{0.89} k \]. In this equation, \( \ell \) is the portion of the physical length that is below the centre of the acoustic length, \( r \) is the radius of a circle with the same area as the mouth, \( S \) is the cross-sectional area of the pipe body, \( k \) is the wave number (defined as \( k = \frac{2\pi}{\ell} \)), \( f \) is the frequency, and \( e \) is the eccentricity of the ellipse (defined as the square root of \( \frac{1}{1 - \text{the square of the ratio between the axes of the ellipse}} \)), and \( K(e) = (2\pi) \times F(e) \times \text{the fourth root of} \left( 1 - e^2 \right) \). \( F(e) \) is the complete elliptic integral of the first kind, found in most tables of mathematical functions. In some tables, the variable is \( m \), which is \( e^2 \).

To avoid looking up \( F(e) \), following is an appropriate range from such a table, which should be entered into the spreadsheet for looking up. There should single columns for \( m \) and \( F(e) \), and this look-up table should be out of the way (generally below) the rows used for the rest of the calculation.

<table>
<thead>
<tr>
<th>( m )</th>
<th>( F(e) )</th>
</tr>
</thead>
<tbody>
<tr>
<td>0.75</td>
<td>2.15652</td>
</tr>
<tr>
<td>0.76</td>
<td>2.17483</td>
</tr>
<tr>
<td>0.77</td>
<td>2.19397</td>
</tr>
<tr>
<td>0.78</td>
<td>2.21402</td>
</tr>
<tr>
<td>0.79</td>
<td>2.23507</td>
</tr>
<tr>
<td>0.80</td>
<td>2.25721</td>
</tr>
<tr>
<td>0.81</td>
<td>2.28055</td>
</tr>
<tr>
<td>0.82</td>
<td>2.30523</td>
</tr>
<tr>
<td>0.83</td>
<td>2.33141</td>
</tr>
<tr>
<td>0.84</td>
<td>2.35926</td>
</tr>
<tr>
<td>0.85</td>
<td>2.38902</td>
</tr>
<tr>
<td>0.86</td>
<td>2.42093</td>
</tr>
<tr>
<td>0.87</td>
<td>2.45534</td>
</tr>
<tr>
<td>0.88</td>
<td>2.49264</td>
</tr>
<tr>
<td>0.89</td>
<td>2.53334</td>
</tr>
<tr>
<td>0.90</td>
<td>2.57809</td>
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<tr>
<td>0.91</td>
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<tr>
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<td>0.93</td>
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<td>2.82075</td>
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<td>0.95</td>
<td>2.90834</td>
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<tr>
<td>0.96</td>
<td>3.01611</td>
</tr>
<tr>
<td>0.97</td>
<td>3.15587</td>
</tr>
<tr>
<td>0.98</td>
<td>3.35414</td>
</tr>
<tr>
<td>0.99</td>
<td>3.69564</td>
</tr>
</tbody>
</table>

In the following, the numbers in square bracket refer to the row of that cell or its contents. Spreadsheets have different symbols at the beginning to indicate that the entry in a cell is a formula. Mine is =, and when that appears below, an = should be replaced by whatever that symbol is. In formulas, the row number is without the square brackets here, and is=int be preceded by the column letter. In my spreadsheet, \( \pi \) is represented by PI().

1. the pipe identification
2. pipe length in mm
3. temperature in degrees C
4. air pressure in mm water column
5. body diameter in mm
6. mouth width in mm
7. mouth height in mm
8. the eccentricity of the ellipse \( e: =\sqrt{1-(\frac{7}{6})^2} \)
9. \( K(e)/F(e): =2^2(1-(7^2)^2)^{1/4}/\pi(1) \)
10. \( m: =8^2 \)
11. the above value of \( m \) will generally fall between values in the table, so we will use a linear interpolation to get a reasonably accurate value of \( F(e) \); we first round down to the table value just below it (the function INT is the largest integer below its value): \( =\text{INT}(100^2[10])/100 \)
12. look up the \( F(e) \) to the right of the value of \( m \) given in [11]: \( =VLOOKUP([11];\text{table range};1) \)
13. the table value of \( m \) just above the one calculated in [10]: \( =[[11]+0.01 \)
look up the $F(e)$ to the right of the value of $m$ given in [13]: $\text{=VLOOKUP([13];table range;1)}$

the interpolated value of $F(e)$: $\text{=((10)\text{[11]}\text{[0.01]}*(14)\text{[12]}+12)}$

$K(e)$: $\text{=[9]*[15]}$

area of the mouth, $x$: $\text{=[6]*[7]}$

the radius of the circle with that area, $r$: $\text{=SQRT([17]/\Pi())}$

the area of the end of the pipe, $S$: $\text{=PI()*[5]^2/4}$

combining all of the terms in the right side of the equation except for $k$

$k$: $\text{=1.3*([18]*[19]*[17]*[16]/0.89)}$

The other terms of the equation depend on the frequency, which is what we want to find. The way to solve this is by a method of successive approximations. We first make a guess at what the frequency is, and from the calculated result of that guess, we get a very good indication of how to modify the guess so the next one is much better. A few cycles of this will get the frequency to any desired accuracy.

current guess at the frequency $f$ in Hz

velocity of sound, c: $\text{=1000*(331+0.6*[3])}$

wave number $k$: $\text{=2*PI()*[21]/[22]}$

effective length $l$: $\text{=[22]/(2*[21])}$

correction: $\text{=0.66*[5]/2}$

upper part of the pipe, $l_I$: $\text{=[24]/2-25}$

lower part of the pipe, $l_r$: $\text{=[2]-[26]}$

difference between [20] and rest of equation: $\text{=(1/TAN([27]*[23])/[23]-[20]}$

This is not necessary, but was the indication of the number to lower by subsequent guesses of the frequency before I derived the following correction formula

correction for the next better guess at $f$

$\text{=[(21)[23]]^2/(1/TAN([27]*[23])-[20]*[23])^2/([2]+[25]/\SIN([27]*[23])^2+2+[20])}$

The formula in [29] comes from the general method where, if $x$ is a variable, $\mathcal{F}(x)=0$ is the function of $x$ and $\mathcal{F}'(x)$ is the derivative of $\mathcal{F}(x)$, then the correction in $x = -\mathcal{F}(x)/\mathcal{F}'(x)$.

corrected better guess at $f$, to be copied to [21]; this is repeated until [29] is as small a fraction of a Hz as one wishes, and stops. $\text{=[21]+[29]}$

frequency corrected for average differences between I & F theory and their real pipes:

$\text{=[21]*1.002661}$

This correction raises the pitch by 4.6 cents. In Comm. 1701, I assumed a temperature of 16° C, while it should have been 16.6° C for the reported velocity of sound of 341 m/sec. Consequently the average calculated frequency error becomes ~4.6 cents, with the r.m.s deviation from the mean of ±12 cents (the full range of errors was ~25 to +19 cents).

$\text{=[31]*((1+0.00000727*(w-27))^{1.934})/1.008257}$

This is the corrected frequency resulting from a power law fit to the evidence from Rayleigh (1926) that raising the air pressure of a pipe from 27 to 107 mm water column raised the pitch from 258 to 267 Hz, and the evidence from I & F that raising the air pressure of a pipe from 27 to 74 mm water column raised the pitch from 401 to 406 Hz. This leads to $\text{(f_27-f_27)/f_27 = a(w-27)^n}$, where $a = 0.0000727$ and $n = 1.934$. The I & F study was at $w = 65$ mm, which produces a $f_65$, so $\text{f_65 = f_65*(1+a(w-27)^n)/(1+a(65-27)^n)}$. The value of $(1+a(65-27)^n)$ is 1.008257. (Many more significant figures than necessary are included here because we want to make sure that no unnecessary rounding-off errors are introduced, and computers don’t mind.)

It looks like a big job to enter all these formulas into the cells of a column in a spreadsheet, but it needs to be done only once, after which that column can be copied to any other number of other columns for different pipes. Then for each other pipe, change the first seven rows to the new pipe’s specification, guess the frequency in [21], keep correcting the guess by adding [29] to [21] until [29] is too small to bother, and [31] gives the final calculated result. The spreadsheet automatically keeps a record of all previous I & F calculations, available for comparison.

Of course, a pipe in an organ is much more significant than a theoretical calculation. Its pitch can be measured more accurately than can be calculated from its dimensions. The calculation is useful when only dimensions are known, or when one wants to estimate the effect of changing a parameter. That is much quicker and easier than building a pipe to find out.
FoMRHI Comm. 1801

More Ancient Varnish Recipes

FoMRHI has published many Comms. dealing with the subjects of wood treatment, finishing and varnishing. Some of the Comms. have included recipes from many centuries ago until the present. Recently I was re-examining an ancient book, a lucky find from my student days, with unfortunately the first few pages missing. It is a medical text book which I had guessed was possibly 16th century. I have to thank the John Rylands Library of Manchester for their persistence in eventually identifying the book. It turns out that it is “The Generall Practise of Physicke” translated by Dr Jacob Mosan, Germane, imprinted at London by Edmund Bollifant in 1598. The original, “Praxis medicinae universalis”, or “Ein new Artzney Buch” by Christof Wirsung (1500 or 1505–1571) was first published in Germany around 1592.

It seems useful and interesting to have available as large a sample as possible of these recipes in order perhaps, to arrive at some sort of consensus when attempting a modern duplication. In setting out these recipes, originally in the black letter gothic type, I have transliterated exactly, keeping to the original variable spelling and punctuation. These recipes are to found on pages 781 and 782 in the eighth part of the book, which is an extensive Materia Medica.

Common Varnish. §. 26.

Vernish is made after sundry waies, and there are also sundry sorts of them, each for an especiall use: the one is made thus. and the other so. But we will here describe onely and teach how that it shall be most fitly made, and for what it shall be requisite for each one. First, take old Linseed oyle, and seethe it so long untill it be as thick that if you do take a drop of it out, and let it be cold, when you presse upon it with your finger it spinneth two or three threads, or glueth to the finger. This oyle being now so sodden, put Rosin into it as much as shall suffice for to make the Vernish thicke enough: then let the Rosin decoct together with the oyle so long as it do stand wholly still. This Vernish is good for the Smithes, for the Trencher-makers, for the Trunck-makers; and further for to Vernish all slight things.

Of Painters Varnish. §. 27.

Take old Linseed oile and seeth the same as is aforsayde; afterwards put amongst one pound of the oyle, halfe a pound of Masticke or somewhat more, and so let it seeth together a good while. This being done, then set it in the sunne, in a leaden Basen, and powre one part of Water upon it, and stirre it well togither; then will the oyle come upon the top, and let it stand for clarifying in the sunne and ayre the space of two or three moneths. But it is to be noted that you must always take good heed of the faire weather, and of the raine, and also the night: for if so be that it did raine, then would your oyle run over, and you leese the same: wherefore must you have alwaise ready a boord or plancke for to cover the same oyle, as well from the raine, as from the night, upon adventure lest that it should raine at night, &c. You may make also a faire Vernish of the oyle alone, to wit, through the seething it away, and then let it clarifie as is before said.

For to make odoriferous Vernish. §. 28.

For to make this odoriferous Vernish you are to take a new earthen pot which is well leaded, and put into it one pound of oyle of Spike, and let the same be boyling hot: afterwards you are to take halfe a pound of Sandaraca, or somewhat more, and strowthe same amongst it being beaten small, always with a little at once,
and stirring it well about; then let this Vernish to clarifie as is said before. This Vernish dryeth very hard with a faire glosse: it hath an especial good savour, and may be used for all things that you please.

The QavisiEnbalum from the Manuscript of Henri Arnaut de Zwolle, c. 1440: An update to Comm. 1765

Soon after the appearance of FoMRHI Comm. 1765 it was pointed out by Jeremy Montagu (Bull. 106. p. 3) that the roof of Manchester Cathedral had been extensively reconstructed in recent times, the implication being that some of the carvings may not be original fifteenth-century work, therefore undermining the association of the instrument depicted in the carving (Bowles, plate 19) with the clavisimbalum described and drawn by Arnaut de Zwolle in his manuscript. The history of the Cathedral, certainly over the past 200 years, has been one of continual restoration and improvement. In 1815 an effort to 'beautify' the cathedral resulted in much of the interior being 'damaged' by over-enthusiastic improvements when fear that the roof might collapse prompted a full-scale restoration. At this time, undoubtedly, much new work would have been introduced. In 1882 the building was again extensively renovated and in the twentieth century, work has involved restoration after war damage. However in 1815 the harpsichord would still have been a familiar keyboard instrument. With plenty of examples around of how the instrument should appear, the question is: from where did the carver derive the information to put in place this subterfuge for future generations?

Whilst it is possible that the carving is later work (the addition of restorers work is known to have been included and encouraged in modern restorations, e.g. at York Minster and Windsor Castle), the exact arrangement of the harpsichord at Manchester is so similar in appearance to the plucking mechanism and bridge layout of the Arnaut reconstruction, and to other fifteenth-century illustrations reproduced by Bowles, that it is difficult to imagine that a carver could just think it up, without having access to the manuscript or other information about early instruments. It is of course also possible, if the carvings at Manchester are new, that replicas of badly damaged or worm-eaten originals were made.

The unusual arrangement of the keyboard may not have been made quite clear in Comm. 1765. I was referring not to the cutouts at each end of the keyboard (which were probably to allow the key tails to fit between the wrestplank supports), but to the division of the keys. The slight offset is most clearly seen in the treble. The point being made was that the layout, as shown in the manuscript, was unlikely to be how the keyboard actually was, but rather results from an error in the drawing technique. In fact it is quite easy to reproduce this arrangement. See the diagram below.

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Lay out a keyboard using three parallel lines, one for the tail ends, one in front of the sharps and one in front of the naturals. Mark off the naturals along the front section and mark the corresponding naturals and sharps at the tails. Starting with the first bass key division aligned, as Arnaut does, draw lines equidistant and at right angles from each tail division down to the bottom of the diagram to meet the naturals. The discrepancy begins to appear immediately, in the same places as in the manuscript.

The discrepancy results from the problem of dividing up the octave. The octave C to B has seven naturals of equal width. Section C to E has a total of five notes and F to B seven notes. The notes BC and EF are divided by a line at right angles to the front of the keyboard. This makes the tails of the F to B section a little narrower than the tails of the C to B section. As all the sharps are usually made the same width, for the sake of appearance, the difference is absorbed by a slight variation in the width of the [natural tails] between the sharps. This in turn means that the lines joining the backs of the keys to the tails cannot be parallel. On the Arnaut drawing this method has not been followed, and all the key divisions are shown parallel, resulting in the misalignment. This suggests that the misalignment was probably just a drawing error and not an accurate depiction of a clavisimbalum keyboard.

*The History of Manchester Cathedral, www.dws.ndirect.co.uk/mc1.htm*
Bending Vihuela Ribs: A response to Ephraim Segerman's Comm. 1793 on Richard Coleman's Comm. 1753

Segerman's comments on Richard Coleman's communication on rib bending (Comm. 1753) appear to be a little misplaced. Coleman provides a credible methodology based upon technologies available to the sixteenth-century vihuela maker, and Segerman overstates the case for one-off designs and inflexibility. Most importantly, I do not think that bending fruitwood ribs in sixteenth-century Spain with a consistent transverse camber can be compared with the double twist curves found on certain staves of a conifer viol belly in England from the following century.

My experience of examining the latter form of construction has lead to some observations inconsistent with those that Segerman states. The use of this 'searing' multistave method appears to be confined to viols of the Jacobean period. I have noted this during my examination of instruments by Henry Jaye, John Rose and Henry Smith, and of some anonymous viols of this period. In contrast, Richard Meares I (ca.1638-1699) is the earliest viol maker known to me who does not scorch the undersides of multistave bellies, and this is consistent with later instruments that also include Parisian (presumably English-influenced) work by Michael Collichon and others. Segerman's speculation regarding the loss of this process during the 1690s is inaccurate. The last maker known to me who employed a multistave process was Richard Meares II (1671-1725), working in St Paul's Churchyard, London. However all viols by Barak Norman (1641-1724) beginning in 1689 use a 3-piece method, involving a bent centre stave and carved flanks. This is also found in extant viols including those of Francis Baker, and William and Nathaniel Cross. From 1687, instruments by Edward Lewis (1641-1717) and at least one anonymous follower have a two-piece carved front, a method also used in some earlier viols.

Segerman echoes comments by Martin Edmunds ('Venetian Viols of the Sixteenth Century', A Viola da Gamba Miscellany, Utrecht, 1991, p.24) that scorch marks are only applied to form the double twist on staves 2 and 4. This is not the case, and scorch marks can be found on all staves, although they can be difficult to see, particularly when an instrument is closed.

It is fair to suppose that the same technology was applied to bending the ribs of these instruments, yet scorching is uncommon on these. Each scorchmark is firmly and individually impressed into the belly in a manner inconsistent with evenly bending the stave (where the stave would be run along a heated iron). In some cases, notably the 1629 Smith at the Metropolitan Museum of Art, New York, scorch marks have the appearance of crossing over the joints, and the hole cut for a rose does not intersect the scorch marks. Lastly one anonymous treble viol (private collection, London) shows puncture marks (consistent with a Meares-type belly) in addition to scorch marks (see Kessler, D. M., 'Viol Construction in Seventeenth-Century England', Early Music 10, 1982, pp.340-355, for what constitutes this method). If the scorch marks were related to a method of bending they would appear to be incompatible with the presence of puncture marks. I conclude that although these scorch marks give the appearance of being applied during the bending process, they are in fact applied after the belly has been constructed. If this is the case, I can only speculate about the rationale for their existence. The reason that viol fronts are bent and not carved is to achieve optimum strength over a thinner surface. The process of scorching hardens the wood, and this may have provided a significant and desirable acoustical change.

One final thought: Rather than using a form and counterform (costly and time consuming to manufacture), could a maker use a single form constructed rather like a viol fingerboard, with large amounts of rubber tubing or historically acceptable twine to bind a rib against it?
Gut strings: further to Comm. 1683

I thank Segerman for his review of my article 'Italian violin strings...', Recercare IX (1997), pp. 155-203 in Comm. 1683. After reading the first version of the review sent to me, my reaction was not to reply, because of the frankly aggressive terms in which, inexplicably, it was expressed. The official version is decidedly more acceptable and deserves a reply, though a very belated one.

I would immediately like to point out that some of the documents cited in my text did not refer only to Italy, as the title of my work might have suggested, and were included either because they referred directly to Italian string-production (like the English examples, which also provide interesting technical data on strings) or because Italian string-making technology had presumably been exported (see the case of the Savaresse family, who were of Italian origin). In any case, I did not extend the study beyond the end of the nineteenth century.

1. The four age of gut strings

The first era

Gut, and not silk, was the material mainly used in Western civilisation. The mistake was a translating error, differing from the Italian version, which was correct.

The second era

The string-making towns: here I mainly point out that the centres for spinning and those for gut strings were often the same. Segerman, referring more specifically to the early sixteenth century, corrects me by citing towns other than mine, such as Barcelona, Munich and Brussels. Yet even these, ever since the Middle Ages, had been large spinning and cloth-making centres, which further confirms my point. Barcelona, for example, in the fifteenth century became an important centre of production for many Italian dyers from Tuscany who settled in the city at various moments (for more detail, see L'arte della tintura by Franco Brunello, 1968, Ed. Neri Pozza, Vicenza, pp. 125-178).

The third era

As already expressed in my Comms, I cannot accept Segerman's interpretation of the Dowland passage concerning string transparency (incidentally his final resort for contesting the theory of the loading of gut in the all-gut bass strings). The positioning of the punctuation of this passage from Dowland's Varietie, along with the particular use of upper-case letters every time the string typologies are mentioned (Meanes, Trebles, Basses) – an aspect identical also in Mace – indicates a clear separation: on the one hand, the Trebles and Meanes (to which the string transparency refers); on the other, the Basses. Concerning the latter, Dowland says nothing of their physical appearance, but only mentions the area of provenance.

Segerman then carefully (though I hope not intentionally) avoids discussing the seventeenth-century iconographic situation, for here one more frequently observes all-gut basses that are uniformly dark red, brown or blackish, positioned in many of these examples where overspun strings are used today. Clearly this evidence strongly contradicts his interpretation of Mace and the Burwell Lute Tutor: that the basses should also be translucent. But in fact, neither the Burwell Lute Tutor nor Mace have anything explicit to say about the transparency of the basses. Concerning Mace in particular, Segerman strangely forgets to discuss the anything but secondary subject of the dark red colour of the Pistoy-basses or, to offer a plausible opinion about their possible transparency in spite of the dark colouring. It is also worth noting that his catlines, thanks to the special twisting of the paired strings, are in fact opaque and not translucent. Hence not even the modern catlines follow the criteria he hypothesises for Dowland, especially if they were dyed in "a deep dark
red colour' like Mace's Pistoys or with the colouring of the gut basses in seventeenth-century paintings. I ask Segerman to try colouring them dark red and then to express an opinion on their transparency.

The fourth era

I thank Segerman for his correction concerning the da fondamento instruments and the presumed shortening of the vibrating lengths following the appearance of the overspun strings. But it is worth noting that according to certain historical documents, the two lowest strings of the five-string violone were already overspun, as one also deduces from the iconography of the late seventeenth century (see, for example, F. Puget: 'Portrait of musicians at the court of Louis XV', 1687, Louvre: the lowest two basses of the five-string violone are clearly white). In the late eighteenth century the lowest two strings of the five-stringed violone tuned F, A, d, f, a were still overspun (see James Webster, 'Violoncello and Double Bass in the Chamber Music of Haydn...', JAMS XXIX, 1976). It would seem plausible, therefore, that the 6th of the six-string violone was overspun. I don't know where Segerman got his evidence that the bass viols in Germany definitely had an overspun 6th string only, but I agree with him that the three-string contrabasso, like Dragonetti's, used gut alone. The Maugin & Maigne manual (second half of nineteenth century) indicates the number of guts needed to make the first and second strings only. We thus deduce that the bottom two strings of the four-string contrabasso were overspun, not just the last, as Segerman claims (the first and second were used also as cores for the third and fourth strings).

2. Gut string manufacturing technologies in the eighteenth and nineteenth centuries

Mace

Segerman does not agree with my interpretation of Mace's three typologies of lute string, even though I had duly warned the reader that it was indeed purely a supposition unsupported by evidence. I was merely wondering if there could be some acoustic and mechanical explanation for the subdivision into those three classes of string, and that's all. As is known, Mace's criteria for the strings are merely visual and tactile. Both the reader and Segerman are free to agree with my suggestion or not.

Catlines

There is no written evidence documenting that catlins/catlines were made originally in Barcelona/Munich. Equally, there is no written evidence that this same technology then moved to Bologna, Lyon, Nuremberg and Strasbourg. If there is, then it's Segerman's duty to make it known, and not merely to offer conjectures that have become fact as if by magic. In fact, the Catalan connection is a hypothesis that was mysteriously formulated as a certainty by Segerman in 1994 (Comm. 1289) as an alternative to the no longer sustainable idea that the word 'catline' was derived from nautical terminology (a hypothesis he strenuously defended for years without any certain proof and then discarded in the twinkling of an eye to follow the Spanish escape route). Moreover, there is no treatise or document (whether Catalan, Bolognese, or other) that indicates or even suggests that the catline were made like ropes (as he himself admits in Comm. 1698 '... there is no direct evidence that the strings usually called 'catlins' also had roped construction...'). Now that the nautical hypothesis has had to be dropped - which for Segerman heavily justified its manufacturing technique - and the Catalan hypothesis alone is left standing, this explanation needs definitively to be shelved. In other words, it is by no means so certain now that the catlines were made like ropes.

There does, however, exist a clue to the use of this technique in the late sixteenth century, but only for the very thick gut strings (Ramelli, late sixteenth century), though no relationship with the term catline is specified. In the early seventeenth century what (as I recall) only the English called 'catlins/catline' were made exclusively in central-northern Italy (Bologna), according to the documents. However, we still don't know what the Italians called this type of string.

The theory of the loading of gut, on the other hand, boasts a great deal of evidence that - on the strength of the mathematical formulas on strings that Segerman himself has taught us - can with difficulty point to explanations that exclude it. It is incidentally worth noting that even a string intertwined like a rope can be
The two techniques are not perfectly compatible. The most important evidence is the small holes for the low strings in the bridges of historical lutes of various provenance which I have measured (as well as the iconographic aspect mentioned above, which is constantly overlooked by Segerman).

Cornered into accepting the evidence of the small bridge holes of scores of historical lutes (of which only half of those I have measured have subsequently been published) and unable to contradict the objective data of the calculations, Segerman finds himself elaborating the fantastic explanation that the lute basses must have had tensions of 1.0-1.3 Kg only. I ask Segerman what other stratagem he will then excogitate to reconcile this idea with his theory of equal tension between strings: with such values a lute chanterelle would have a diameter of 0.20-0.25 mm only. But in my experience I have found that the lute chanterelles made from just one whole lamb gut (as Athanasius Kircher, in Musurgia Universalis, tells us in 1650 for the chanterelles of Roman lutes) produce diameters of 0.45-0.48 mm, and not half that figure. It's just not possible to obtain thinner strings.

How, moreover, does one also reconcile his idea with the concept of 'equal feel' between all the strings, propounded by Mace, Dowland and Burwell? Given that, for Segerman, the seventeenth-century basses were obviously made like his catlines (i.e. rope-like), they inevitably have a very high coefficient of longitudinal displacement, making them even softer to the touch when compared to a normal string with a high twist and of equal diameter, which makes things even harder for him to reconcile. In short, we are talking about genuine (and mute) rubber-bands, which would completely unbalance the lute in relation to the Trebles - just what Dowland and Mace heavily condemn.

I ask lutenists to be so kind as to verify once and for all the modern catlines with such working tensions and to express their opinion. In an e-mail sent to me some two years ago, Alexander Rakov kindly drew my attention to the fact that, while working on the catalogue of a private collection of seventeenth-century musical instruments in Pennsylvania in 1995, he was in a position to observe personally that '...into the remains of original strings from the seventeenth century on the instruments, (...) the basses were actual rope, heavily soaked in some sort of compound, still flexible, while the actual rope material was hard to recognize.' I hope these instruments will soon be located and that it will be possible to verify the nature of this impregnation.

It is not at all true that the only difference between the manufacture of strings in the past and that today is sulphurisation. Segerman's position on this point is too reductive. Moreover, there is no document that proves that sulphurisation also served to make the stocks of gut last longer, as he claims (where did he find this written?), instead of merely to whiten the strings at the end of the construction process. From sources of the early nineteenth century we know that fresh gut was preserved in salt (Labarraque: Nuovo Dizionario Universale Tecnologico, Tomo VIII, Venezia 1823), but we don't know how long this system had been in use.

As regards sulphurisation and its influence on the mechanical and acoustical properties of strings, I suggest Segerman carefully reads not only the results of Labarraque (L'art du bouyahidier, Paris, 1822) and the authoritative opinion of the string-maker Savaresse (see the Maugine & Maigue manual, p. 184), but also the entry 'Cor' (c. 1765) in Diderot's Encyclopedie, and finally Francesco Griselini (Dizionario delle Arti e Mestieri, Venezia, c. 1765). If anything, the modern peroxide-based whitener tends rather to weaken the gut, if the concentrations or durations of treatment are incorrect. This is something that Segerman can check for himself. Compared to the modern salts (like sodium carbonate, for example), the use of potash, acquired by burning wine dregs, has positive effects on gut, as is also underlined by Jaubert's Dictionnaire in the late eighteenth century.

As a string-maker myself, I can also emphasise that there are other differences from present-day manufacturing: how many modern string makers, for example, today rub the almost dry strings with a potash solution? How many immerse the strings in olive oil for some months to undergo tanning? They are simply unaware of the benefits. And this is also true for various other processes that belong (today as then) to the secrets of production. The rock-alum cited by Segerman is just one of the elements that are still used today, though often badly (and the poor results can be seen - and above all heard). In any case, as a string expert, Segerman should know very well that rock-alum hardens the gut and does not soften it, as he instead claims.
I should now clarify the meaning I have given in the text to the term 'elasticity'. In this context it implies flexibility, which strongly depends on the type and degree of twist, which in turn gives the string a greater capacity for displacement under tension. This should be related not so much to the statements of Galeazzi (1792), as Segerman (incorrectly) supposes, as to the seventeenth-century iconography. Here one observes that the excess lengths of the string – even those of thick diameters – are bunched up and somewhat curled. What is significant is precisely the fact that they can be bunched in that way: it is evident to me that only particularly pliable strings can produce this effect.

Segerman’s statement that to a given number of turns a string with fewer guts will have a lower twist than another with a larger number of guts is obvious and I witness this in my work every day. From my experience I have found no perceptible acoustic difference in the trials carried out on strings made from whole guts rather than split guts, except for the additional problems in working (and above all rectifying) them evenly, given that the thinnest ones are slightly conical. But this has already been correctly pointed out elsewhere by the string-maker Dan Larson. But I do point out that the splitting of the gut was already in use at least from the second half of the sixteenth century in Italy, given that the technique was strictly forbidden already in the first Statute of the String Makers of Rome (the document, which I have examined, is dated to 1587 and was rediscovered in 1999 by Marco Pesci of Rome). I suspect that in Munich, Capirola’s strings were made starting from split gut, since they did not suffer from the problem of conicity. I repeat that this is a suspicion, and not a certainty, since no one has written as much. One detail: in Comm. 1698 ('Tensile strength') Segerman states that there is no evidence that a gut lute chanterelle in the seventeenth century was more resistant than those today. If one refers merely to the breaking load, on the whole I agree, but if by resistance he means how long before it begins to fray with use, then I do not. I have long experienced that a chanterelle made from whole gut and only slightly smoothed manually (two working processes typical of the former technique) has a much longer life than one made from gut cut into strips and then rectified by machine. Baron (1727) wrote that there are lute-chanterelles from Rome that last even four weeks.

3. Modern strings and high twist

I do not at all get the impression that all musical strings made today always have a genuine high twist, as Segerman states. Clearly it’s not my business to list the firms that make strings that are too stiff and with a low twist, but I believe that this is before the eyes of any musician. There is room for improvement, as George Stoppani tells us with his very highly twisted strings.

As regards string life, I re-affirm that the strings formerly probably lasted longer than our oiled strings because of the delicate superficial polishing which substantially respected their natural diameters without breaking fibres on the surface. Those of today, on the other hand, are subjected to mechanical rectification to impose a precise calibre, and this, if not well judged, can remove too much material from the string. It paves the way from premature fraying of the material, even though it does remove the problem of falseness, an aspect that formerly afflicted strings. In fact there are violin E’s today that last only a few hours, and sometimes the cause can be imputed to excessive rectification (evidently because natural unrectified strings of a calibre much larger than the required diameter were being used), whereas on average one that has been only slightly rectified lasts even weeks before beginning to fray (even at modern pitch).

As regards the wheel formerly used to twist the strings, it could certainly be provided also with two hooks, as Segerman says, but in the iconography one sees examples with one hook only, like that in Griselini’s Dizionario (Venezia 1768), which corresponds to the one in Angelucci’s workshop described by De Lalande: the one I was specifically referring to, given that I was mainly dealing with Italian string making.

4. String types

The use of the overspun 4th in Germany in the eighteenth century is well documented by various authors, who are also cited by Segerman in his work (Lohlein, Mejer, Quantz, etc.). Yet I accept Segerman’s
correction regarding the period prior to the first decade of the eighteenth century in Germany and England: there is no written evidence for use of the overspun 4th on the violin.

The example of the white 4th string distinct from the first three yellow ones of the violin and cello in the two paintings by Gabbiani (1681 and 1685) strongly suggests a string wound with silver or silver-plated copper, and not a conjectured whitening by sulphurisation carried out just for this one string, a hypothesis that strikes me as too far-fetched to be taken seriously: sulphurisation was performed systematically on all the strings, and in Italy that was certainly done at least hundred years before De Lalande (see Skippon and the Paduan string maker, mid seventeenth century). It is curious, however, how quickly Segerman changes his mind on this point: on p. 53 of one of his works ('Strings through the Ages' in The Strad) the white colour of the fourth string is instead imputed to silver overspinning: 'Overspun strings were usually wound with silver (or silver-plated copper) and that colour can often be seen on a cello C in these paintings'.

**Mozart**

Leopold Mozart wrote merely that... 'the violin mounts four strings of different gauge'. I fail to understand how from this short statement Segerman can draw the conclusion of all-gut stringing. From this passage one cannot deduce with certainty that there is a progressive increase in the diameter of the strings, but only strings of different diameters. The strings have different diameters even in a stringing with scaled tension and an overspun 4th! Any deduction would remain inconclusive, if it were not for a painting Segerman seems to unfamiliar with: the portrait of the Mozart Family (with the famous son at the fortepiano). Here Leopold's 4th string is evidently white, while the three higher strings are yellow – in my opinion an interesting clue that favours the hypothesis of an overspun 4th string wound with silver or silver-plated copper rather than an all-gut stringing.

Again on Leopold Mozart, I frankly have no plausible explanation for his theory of equal tension, achieved by testing the open fifths on pairs of neighbouring strings, though we well know the result of a practical application on the instrument. It is enough to read Huggins's opinion on the matter (which I shall cite below). I also observe that what Leopold writes totally disagrees with the data provided by Conte Riccati and DeLalande/Agelucci, to my knowledge the only eighteenth-century sources, which point to a system of scaled tension, as is confirmed by the commercial strings of the time.

**Stradivari**

I have personally examined the charcoal lines on the mould of the 'theorboed guitar' in the Museo Stradivariano of Cremona, and if Segerman holds that these rough lines – which are anything but precisely graduated, as he claims – can be seen as even a remotely reliable source to confirm the use of an all-gut 4th string for the violin, I think he is wide of the mark. In this mould Stradivari draws very wide lines even for the strings that are unquestionably very thin, such as the first three courses on the fingerboard or the intermediate strings on the extended-neck. What I think Stradivari meant by these marks was to relate a given string of the theorboed guitar with the same string of the violin (an instrument with which string makers and musicians were surely much more familiar), not to indicate its actual calibre. So I repeat that in Italy there is at present no written proof for all-gut stringings on the violin in the eighteenth century. On the contrary, a painting by Gabbiani of 1685 testifies that overspun strings were then already being used on the violin. What astonishes me is that Segerman, who considers himself to be a precise researcher and judges other by those same standards, should take such course marks (which he has never personally examined) as precise.

**Brossard**

I don't at all get the impression that Brossard speaks of an alternative, all-gut stringing, as Segerman claims. Instead I believe that his is a mere example to clarify to the reader the effect that metal overspinning has on the thickness of the string. The passage is as follows. I will let the reader judge for himself: '...Si elle est simplement de boyaux, elle doit estre du moins le double plus grosse que la 3e, mais si elle est toute filee d'argent elle n'est que tres peu plus grosse que la 3e...'. 
The example given is of the lower strings of the Romantic guitar, which, as is known, used silk as its core in the basses, and never gut. My view, therefore, is that the author is referring to the manufacture of a violin 4th string that has a silk core (thereby imitating the guitar basses) instead of a customary gut one, but that many players still preferred the latter (more traditional) material. In which case, we are not dealing with a hypothetical all-gut 4th string. Besides, to my knowledge no nineteenth-century string making source ever speaks of an all-gut violin G string. If there is one, Segerman should produce it. In any case I cannot understand what would prompt a nineteenth-century violinist to use an all-gut 4th string.

5. The string gauges

Nor is it true that I fail to appreciate Mersenne’s method of testing the trueness of a string. It is just that I didn’t discuss the subject. What I find irritating is that Segerman should attribute to me opinions that I have not expressed and should give his readers utterly the wrong impression.

As regards manual polishing of the gut strings, I maintain that if it is not carried out with regularity and precision, it makes a string false. Has Segerman ever performed this operation to discover for himself? It is not at all true that a violin E string must have a low twist in order to have a long life: I regularly make medium/high-twist strings with a high average breaking load and lifetime at modern 440 Hz pitch, and I’m sure this is certainly also the case with other string-makers. One frequently encounters a high twist even in the surviving violin chanterelles of the early twentieth century, in spite of Segerman’s opinion to the contrary.

The average breaking load value he has always maintained (32 Kg/mm²) is too low, if we compare it to the commercial situation, which is of around 34-38 Kg/mm². Anyone can verify it in practice and other string makers can confirm it. It is true that my article did not provide experimental data on the matter, but it is well known that a violin chanterelle with a high twist responds decidedly better than one with less twist, which will be stiffer. Naturally Segerman is already aware of this, and in any case one could directly ask the musicians who have compared the two string types.

Whether Segerman likes it or not, almost all the sources refer to the use of three lamb guts to make a violin chanterelle; under these conditions the range of diameters obtained – as I have verified in my own work – varies more or less within that indicated by Hart for the violin E string, i.e. c. 0.65-0.75 mm. Since, to date, there is no record in the contemporary documents of significant changes either in the choice of gut type or in the standard number of strands used to make violin strings by the leading nineteenth-century string makers, i.e. the French and Italians (who therefore display a strong standardisation of the production process), I think that any increase in working tensions must be imputed to causes other than the strings: perhaps pitch standards, for which the original tables in Hellis (London 1880, ‘The History of musical pitch’) report considerable variations over the course of the century, even in the same geographical area and even over short intervals of time. Though, naturally, this does not categorically rule out the possibility that some had a preference for specially chosen larger diameters (as Hart writes on Lindley, Dragonetti and their followers).

It is not true, as instead Segerman affirms, that we know nothing of the string diameters used in Italy in the first half of the nineteenth century. A highly authoritative confirmation that there was no evident change in the traditional diameters of Italian violin strings in the early nineteenth century is offered by a recent discovery in Genoa: that of Paganini’s strings, which were stored in a closed envelope sealed with the coat of arms of the City of Genoa, part of a bequest (1851) by the violinist’s son Achille to the city, together with his bow, a box of Vuillaume rosin and a violin bridge (as well as the violin itself, preserved separately). I have personally examined and measured the various strings (which were intact and had never been mounted) and I found the following: E: 70-72 mm medium twist; A: 85-88 mm high twist; D: 1.15-1.16 mm high twist (for more details, see Recercare XII, 2000). In other words, the same as those of Ruffini of the late nineteenth century.
I really don’t know where Segerman learned that Spohr suggests stringings using thick strings. The Italian translation of the book (which is practically identical to the original German version) reads: ‘E’ però sempre da preferirsi la cordatura alquanto sottilè perché il Violino non perda troppo’ (the somewhat thin stringing, however, is always to be preferred so that the violin should not lose too much). And: ‘Vi sono corde italiane e corde tedesche, delle quali le prime devono essere preferite...’ (There are Italian strings and German strings, and of these the former must be preferred).

**Gauges**

In the German version of Spohr’s book the drawing of a string-gauge is given. My hypothesis turns on the fact that the scale of this figure is 1:1 so that it could be of some practical use to the reader. Hence, by measuring the width of the mouth opening and its length in the drawing (which is surely more reliable than the charcoal lines of Stradivari’s theorboed guitar), one can obtain the measurements of the strings by a simple proportion with the position of the gauge markings. The measurements lie perfectly within the ordinary standards of the time, showing both customary diameters (hence contradicting the use of absurdly large calibres like those given by Segerman, who in practical terms proposes 0.90 mm for the E string!) and also a system of scaled tension. That the scale of the string-gauge is most likely 1:1 can also be deduced from other similar examples in the cello manuals of Dotzauer (1810, Paris) and Quarenghi (1915). That said, it is not at all necessary to know the unit of measurement, as claimed by Segerman, who applies one in his articles on violin strings merely because it was also used in the nineteenth century, even though the string measurements obtained are implausible. No string-making document of the time speaks of violin E strings made from 5 guts, i.e. those otherwise needed to obtain an E of 0.90 mm, and hence in proportion a D of 1.5 mm. The sources speak exclusively of 3 guts for the E (exceptionally 4, if they are a little thin to start with). Spohr’s preference for Italian strings obviously refers to the calibres generally in use in that country: those derived from his string-gauge are in fact remarkably similar to those of Ruffini and Paganini, whom Spohr repeatedly met.

6. **Working tension and ‘feel’**

Segerman’s explanation of ‘equal feel/equal tension’, as expounded in his various writings, is strongly contradictory and the moment has come to point this out:

- Comm. 632, p.50: ‘An important factor that seems crucial in maintaining equal tension is that pluckers seem to require that each string “feels” the same, i.e. it moves sideways the same amount for the same plucking force.’

- ‘Strings through the ages’, The Strad, 1988, p. 55: ‘A more real advantage of equal-tension stringing is that the “feel” of each string is the same in the sense that the same force at the same relative position on the string pushes aside (or depresses) each string the same amount’. In both cases the condition of equal tension would coincide with equal feel that would occur when strings pressed at the same point and with the same force deflect by the same quantity. But then in Comm. 1307, at p. 57 we find the new version of ‘equal feel’, again in relation to working tension:
  ‘...lute first strings had higher tension than the lowest ones’. The condition of ‘equal feel’ now means a lower tension in Kg in the basses. So equal feel and equal tension no longer coincide.

It is evident that the version of Comm. 1307 (immediately following my own Comm. 1288, which formulates, with data, the theory of the loading of gut in the basses, giving as main proof the smaller diameters of the bridge holes of lutes in museums) remains Segerman’s only course if he wishes to salvage his catlines. But in 1683, p.31 he again suddenly changes his mind: ‘equal feel’ is no longer what he had earlier formulated in The Strad and then modified in Comm. 1307: ‘Consequently, for true equal-feel stringing, thicker strings should have higher tension than thinner ones’. In conditions of equal feel the thicker strings would thus have a greater tension (in Kg) than the higher strings. In practice what Segerman does is to demolish all by himself his own laboriously confectioned theory of the lower working tensions of
lute basses and thus indirectly confirms the theory of he makes a final gyration: ‘the tension in the lute bass strings would be less than in the treble strings. This is contrary to equal-tension stringing, which works very well on modern lutes and viols, and has much historical support...’ Now what would Mace, the Burwell Lute Tutor and Dowland make of this statement? At this point one seriously wonders which is Segerman’s right version and what credibility one can give his arguments on the subject.

Tests

I’m sorry to disappoint Segerman, but naturally I cannot change the results of my experiments: in the case of strings attached to one end and with weights applied to the opposite end, the thick strings distend less than the thin (the weight applied and length being equal). The graphs in my work (which he claims not to understand) serve to show the reader the variety of longitudinal displacements of strings of equal length but different diameter (with an equal load). By ‘feel’ I mean the quantity of lateral displacement obtained in strings of different diameter mounted, for example, on a violin (the applied weight, length of string and point of application being equal). Given that a lateral displacement is nothing but an additional lengthening of the string subjected to a weight that deflects it; and given that we find that a thicker string distends less than a thin one; then one would obtain a lesser degree of downward displacement (the applied weight, and point of application being equal). To obtain the same degree of displacement with the (increasingly) thick strings, more weight is needed: in conditions of equal tension the consequent tactile result is that these strings are ‘harder’ to finger pressure than the thin ones. So in order to recover the same tactile sensation with the increasingly thicker strings depressed with the same force, one merely progressively reduces the tension (= diameters) of the thick ones. ‘Equal feel’ therefore involves scaled tension, though only on condition that the strings considered have similar manufacturing qualities, as in the first three gut strings of the violin.

I did not take kindly to Segerman’s effort to pass me off as dishonest: I repeated the test to verify my theory a number of times, and if I wrote what I wrote, it is because those were the results I got. Indeed I invite anyone to verify them experimentally by equipping a violin with a set of strings of scaled tension: if one loads the various strings with the same weight and at the same point (to simulate the pressure of the fingers), one obtains the same downward deflection, which can be measured with a ruler.

In 1833 Huggins wrote that ‘By means of a mechanical contrivance I found the weights necessary to deflect the strings to the same amount when the violin was in tune. The results agreed with the tensions which the sizes of the strings showed they would require to give fifths’. Now, this condition is reached in the commercial calibres with scaled tension, like Ruffini’s. In fact, shortly before making the above statement Huggins commented that ‘A violin strung with strings of the theoretical size was very unsatisfactory in tone’. So he mentions as valid the diameters of the strings sold by Ruffini, saying that that they have the same relative proportions and dimensions as those marked on the gauges of various merchants: they are all strings that show a system of scaled tension. Here we must not overlook the problem of the fifths on the fingerboard, which is fundamental on a bowed instrument: Huggins thus experimentally demonstrated the inapplicability of equal tension because of the fifths that come out wrong, which is hardly a secondary matter. It is worth observing that significantly even on the modern guitar there is a strongly scaled system of tension for the first three strings in all commercial sets, presenting a similar curve to that of the violin. The result is that the three top strings feel equally tense under the pressure of the fingers.

Equal tension

It is not true that I avoided considering equal tension in my work so as to avoid problems that would disturb my conclusions: a gratuitous jibe on Segerman’s part. I dealt with all the cases he lists except for Mersenne (who in any case deals with the subject at a level of theoretical speculation, as even Segerman recognises in some of his Comms) and Hepworth; since they are too distant from the historical period and regional ambit I was considering. It is simply that I do not consider the hypothesis of equal tension to be plausible in the following cases (apart from Mozart, who for me remains an open problem), which I list below with comments:
- **Tartini (1734):** the fact that we are informed of the total tension of the four strings of Tartini’s violin by Fétis does not at all give us any certainty that that the system used was either of equal tension or scaled. For where is it written that Tartini had an all-gut G string? One simply cannot draw any certain conclusion (though Segerman does).

- **Conte Riccati (1767):** the count does not formulate any new theory, as Segerman claims in *The Strad.* He merely introduces a mathematical explanation that justifies the reason for the scaled tension of the commercial strings found on his violin.

- **Mozart (1756):** about Mozart – who considers equal tension – I point out the contradiction mentioned earlier regarding De Lalande and Conte Riccati, who provide data on strings present on the market. Incidentally, if one follows Mozart, one would have an all-gut D that was as much as 1.6 mm in diameter (E = 0.70) and the problem of the fifths cited by Huggins.

- **Di Colco (1690):** in agreement with Patrizio Barbieri and also Segerman, I think that the author is indulging in purely theoretical speculation.

- **Stradivari:** there are no certain elements that allow one to assert either equal tension or scaled tension.

- **Fétis and Savart (1840 and 1856):** if their strings had equal tension, as Segerman claims, what reason was there then to write that the chanterelle accounts for 20-22 pounds and the rest of the strings up to 80 pounds? I would therefore prefer to consider a system of scaled tension, also considering what is written by the contemporary Delezenne.

- **Delezenne (1853):** he first formulated the theoretical hypothesis of equal tension, but after examining a dozen assortments of commercial strings shown him by the instrument maker Lapaix, he realised that they all instead adopted a system of scaled tension.

- **Maugin & Maigne/Savaresse (1869):** the tension values indicated in the text for the four strings are in decided contradiction with the number of guts needed to make them, which instead point to a system of scaled tension similar to all the other examples.

A blatant error should be noted: the chanterelle presents a lower working tension for the second string (7.5 Kg against 8.0 Kg of the A string); the correct value is probably 8.5 Kg. After deriving from this an estimate of the diameters (at a vibrating length of 33 cm and a pitch of 415 Hz), and relating these values with the breaking tensions given in the text for each string, one notes another fundamental incongruity: the breaking tensions of the gut are too low, outside every acceptable standard: 33-36 Kg/mm² for the E string (which is acceptable); only 21 Kg/mm² for the A string and 17-19 Kg/mm² for the D string. This makes the whole example absolutely unreliable as regards drawing a definite conclusion in favour of equal tension. If, instead, one bases oneself on the number of guts indicated by Savaresse (which give a system of scaled tension), the breaking loads are again wholly reasonable.

- **Huggins (1883):** after calculating the theoretical diameters in accordance with a system of equal tension, he shows the validity of Ruffini’s commercial calibres with scaled tension by asserting that the theoretical values give neither the fifths nor a satisfactory acoustic response and cannot therefore be used in practice. One cannot help wondering how Leopold Mozart solved this problem.

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**7. The fourth string**

I thank Segerman for teaching me the correct way of handling machines I have been using every day for a number of years. I also confirm that the machine for making overspun strings that rotates at one end only is not as manageable as that rotating on two hooks (with two rotating hooks). Indeed it subsequently fell into disuse and was supplanted by the latter.
The hypotheses of equal tension, which Segerman claims were omitted in the table, were in fact there and were shown to be either of the scaled type, or unreliable or incapable of offering a certain conclusion (as in the case of Stradivari). The table gives information relative to the diameters of real strings; as a rule theoretical speculations were not considered. Though my work (centred above all on the Italian situation) did not encompass the period of the Lullists and the Restoration in England, I do not agree with the conclusions Segerman gives in his works on Talbot’s indications (c. 1692) for the violin (where Talbot wrote: 2nd violin = 1st bass viol): in *The Strad* Segerman remarkably relates the second string of the historic violin to the diameter of the chanterelle in use on a modern (!) bass viol and by such a comparison obtains violin string diameters that are exaggeratedly thin (0.46 mm for the violin E string). How can a serious researcher, as Segerman credits himself to be, come to a conclusion about late-seventeenth-century violin strings from a comparison with those used on a twentieth-century copy of a bass viol? Shouldn’t research perhaps follow the opposite course? In Comm. 1675 he even goes so far as to deduce that a chanterelle of 2-3 guts made in Rome (early seventeenth century) has an average diameter of 0.48 mm. From what technical data does he get this value? In the statute of the Cordai di Roma – which I have repeatedly examined – there is certainly nothing significant.

Strangely, in the case of Angelucci/De Lande, c. 1760 (Comm. 743) three Neapolitan guts lead Segerman to 0.70 mm; two guts to 0.57. Perhaps we are dealing with a case of lambs swollen by hormones.

Finally, I find it somewhat hasty to conclude that tensions on the violin in early eighteenth-century France were certainly lighter on the strength of a general opinion expressed by just one man (Raguenet, 1702) or because according to Le Blanc (1740) the tone had to be ‘delicate and even a bit nasal’. Such a tone can be obtained in many other ways, not only by acting on the string diameters. From the France of that period in fact we have no information on strings that is even vaguely technical. The first available source is Diderot, in the second half of the century only.

I do not intend, as Segerman dryly concludes, to propose using just one set di violin strings, suited to all musical periods, and to solve every problem merely by replacing a loaded 4th with an overspun string when necessary. But whether he likes it or not, he cannot ignore the strong standardisation that one objectively notices when examining the old documents that deal with violin strings and their manufacturing processes: the guts of animals always coming from the same area and of the same age; the same number of guts used to make the strings; the extensive standardisation of a technological cycle that was jealously and secretly transmitted – at least in Italy – from father to son for generations; and nations (like France) that rewarded those who succeeded in copying the Italian production. All of this could not help but lead to a certain degree of standardisation in the diameters produced. I believe, therefore, that the choice of diameters for each note was probably achieved within the range of calibres offered by the strings made from the same number of strands, a range that was in any case sufficient to cover a range in working tension of a tone either higher or lower, as Hart’s data suggest with his Small, Medium and Thick tensions. I repeat, probably.

I do not understand why, at the end of his review, Segerman feels the need to hint (with poorly concealed animosity) that I may be artfully furthering purely commercial aims, all cunningly disguised behind a barrage of graphs and learned references. As any reader can verify for himself, it is Segerman himself who rarely misses an opportunity to cite the NRI in his works on strings, thereby laying himself bare to the charge of deplorably confusing marketing and research.

Segerman forgets that what is going here is just an exchange of ideas, not a struggle for life.
Eph Segerman cannot be faulted for criticizing my optimistic choice of words when I speculated about catgut etc in Comm. 1751. However, as Eph himself is inclined to create mountains of probability out of molehills of slight possibility, I consider myself to be in good company! In responding to his remarks, I shall - to avoid repetition - omit material that is already covered in Comms. 1795 and 1796.

In his Comm., Eph finds it necessary to invent a theory of “terminology transition” in order to introduce his idea that lines made from sheep’s intestines - not silkworm gut - replaced horsehair for fishing leaders.

Horsehair was used by anglers – according to historical record – for making fishing lines, as well as leaders, from at least the fifteenth century until the mid nineteenth century, when silkworm gut and spun silk lines eventually gained general favour. Silkworm gut and silk lines were first advertised for sale on a commercial basis during the eighteenth century but these products were initially slow to gain in popularity over horsehair.

Silkworm gut, however, was reported as being traditionally used by anglers in the Alpine regions of Europe by Saunders in his *The Compleat Fisherman*, 1724. The use of this material by anglers, therefore, must predate (by an unknown period of time), its introduction as a commercial commodity. Saunders also mentions that silkworm gut was being used by anglers in England but that it was not as strong as its European counterpart. There does not appear to be any evidence to support Eph’s conjecture that sheep’s intestinal fibre replaced the use of horsehair for angling leaders as silkworm gut eventually did.

Gut (or cat gut) to an angler meant silkworm gut.

It is doubtful if Eph’s assertion - that sheep’s gut strings were lower in cost than silk strings – can be supported by historical evidence, for one would expect the reverse to be true. Silk is easier (and less messy!) to make into uniform strings than sheep’s gut and hence the labour costs to make silk strings must have been lower. While, no doubt, finished silk fabric was very expensive (making it was a skilled process and labour intensive), raw silk filament used for string making may not have been a costly material (especially during the periods of overproduction that the silk raising industry was subject to from time to time). Also sheep’s intestines – as a valuable commodity for making into strings and cords – would not have been free for the taking.

Eph tries to “blind ‘em with science” when he tells us that sheep’s gut, having a smaller elastic modulus than silk, gives a brighter sound. Silk lute strings – at least those made by Alexander Raykov - produce a bright sound with good sustain. Clearly there is a lot more to making viable instrument strings than the magnitude of Young’s Modulus for the string materials.
Eph reckons that silk bass strings could not have been Mace’s catlins because silk strings are not ‘smooth’. Early Chinese strings of larger diameters were wrapped with thin silk filament gauzes or ribbons, soaked in fish glue, to make them smooth.

The manufacture of smooth braided silk lines was mentioned by Diderot (mid eighteenth century) but was likely known in much earlier times. (Braided silk lines were later used by fly fishermen – because they were smooth and flexible - making them easier to cast. Braided silk cord is still used today for surgical sutures – its smooth surface reduces friction during the stitching procedure). The braided cords of silk (and other fibres), described by Diderot, were known as “ganse”. In the Southern regions of France, this word is pronounced as ‘ganza’ (see Comms 1351 and 1393). As regions of the South of France were also major silk producing areas, it is possible that lute strings known as ‘gansars’ came from this region, were of braided construction and possibly made from silk filament at some time (they were also made from sheep’s gut during the first half of the 16th C – see the Capirola Lute book).

Making smooth silk strings should not be an obstacle to a string maker.

Eph claims that Mace makes it clear that his catlins were made from sheep’s gut. Mace does not! Mace only mentions “gut” strings - comparing them to wire strings - in the context of fret placement. He does not specify what kind of “gut” he was referring to – it could have been sheep’s gut or Catts gut (silk strand or silk filament?) or sinew fibre – among some of the possibilities. (The only direct historical reference that we have, concerning the material that Catalans were made from, is from Shakespeare – see Comm. 1442 - and he tells us that they were made from sinews – but that is another can of worms!).

One has to wonder if Eph’s mysterious vegetarian anti-gut brigade are not against sheep’s gut strings at all but have yet to be persuaded – despite all of the theories and rhetoric – that the efforts of the ‘historical’ string makers, to manufacture viable imitation lute basses from plain sheep’s gut, have yet to meet with success? Perhaps they are not convinced that the flaccid ‘thud’ produced by some modern ‘catlins’, made from plain sheep’s gut twisted like a rope, comes even close to matching historical descriptions of lute basses that sounded too loud and had a sustain of 10 to 20 seconds (Mersenne, Burwell)? Perhaps these anti gut rebels still have some difficulty in accepting the modern theory that ‘catlin’ strings were named after and constructed like a nautical rope that never existed! Perhaps there are more ‘proper historians’ among the ranks of those disbelievers than Eph would care to imagine? Just speculating!

“Oh had the monster scene those Lilly hands.
Tremble like Aspen leaves upon a Lute,
And make the silken strings delight to kisse them,
He would not then have toucht them for his life ..”
(W. Shakespeare, Titus Andronicus, Act 2, scene 4, lines 44-47)
Some relationships involving string displacement

When a string of length \( L \) and of tension \( T \) is displaced an amount \( D \) by a displacement force \( F \) at a distance \( X \) from the bridge, its length increases by an amount \( \Delta L \). Assuming that the displacement is much smaller than the distance from the end of the string, (i.e. \( D \ll X \)), the formulas relating these variables, as given in our paper 'Strings in the 16th and 17th centuries', *G.S.J.* XXVII, pp.48-73, are:

\[
\frac{\Delta L}{L} = \frac{D^2}{2X(L-X)} = \frac{[X(L-X)/(2L^2)][FT]^2}{2X}, \text{ so } DLT = X(L-X)F.
\]

I don't know of these relationships being published elsewhere, and since the derivations were not published in the *G.S.J.* paper, they will be given here.

Let us consider a triangle ABC with the horizontal AB representing the undisturbed string with length \( L \), and the displacement being at the vertex C, with the length of the perpendicular from C to AB being \( D \). The distance along AB from B to the perpendicular is \( X \), and from the perpendicular to A is \( L-X \). Let us call \( b \) the length of the triangle side AC and \( a \) the length of the side CB. Then

\[
b^2 = (L-X)^2 + D^2 \quad \text{and} \quad a^2 = X^2 + D^2.
\]

Let \( \Delta b = b-(L-X) \) and \( \Delta a = a-X \), which results in \( \Delta L = \Delta a + \Delta b \).

Doing straightforward algebra with these equations leads to

\[
\frac{\Delta L}{L} = \frac{D^2}{2X(L-X)} \left( \frac{1}{1+\Delta L/2L} \right) \left( \frac{1}{1+\Delta b/2(L-X)} \right) \left( \frac{1}{1+\Delta a/2X} \right).
\]

When we assume that \( \Delta L/2L \), \( \Delta b/2(L-X) \) and \( \Delta a/2X \) are each negligibly small compared to 1, the second term becomes 1, so

\[
\frac{\Delta L}{L} = \frac{D^2}{2X(L-X)}.
\]

Let us now consider the same triangle, and call \( 2\theta \) the angle at C between AC and CB. From the resolution of forces at C,

\[
F = 2T\cos\theta. \text{ From the cosine law, }
\]

\[
\cos 2\theta = (a^2 + b^2 - L^2)/2ab.
\]

With the relationships

\[
a+b = L+\Delta L \quad \text{and} \quad \cos 2\theta = 2\cos^2\theta - 1,
\]

straightforward algebra leads to

\[
(F/T)^2 = \frac{(\Delta L^2 + 2\Delta L)/ab}{ab}.
\]

With \( b = (L-X)+\Delta b \) and \( a = X+\Delta a \), we get

\[
\frac{\Delta L}{L} = \frac{[F(T)^2][X(L-X)/(2L^2)][(1+\Delta b/2(L-X))(1+\Delta a/2X)]}{[(1+\Delta L/2L)(1+\Delta b/2(L-X))(1+\Delta a/2X)]}.
\]

With the same assumptions that \( \Delta L/2L \), \( \Delta b/2(L-X) \) and \( \Delta a/2X \) are each negligibly small compared to 1, then

\[
\frac{\Delta L}{L} = \frac{[F(T)^2][X(L-X)/(2L^2)]}{ab}.
\]

In the above formulas, the term that can not be negligibly small compared to 1 is \( \Delta a/2X \) when the displacement is very close to the bridge. That term is

\[
\Delta a/2X = (D/X)^2/4 = [(L-X)/X](\Delta L/L)/2.
\]

To get a more accurate calculation of \( \Delta L/L \) in this case, do it first with the simple formula and then insert that value in the full formula retaining only the \( (1+\Delta a/2X) = (1+(L-X)/X)(\Delta L/L)/2 \) term, and recalculate \( \Delta L/L \).

Let us consider how the displacement affects the tension. By Hooke’s law

\[
\Delta T/A = EAL/L, \text{ where } E \text{ is the elastic (or Young’s) modulus and } A \text{ is the cross-sectional area of the string. If we consider the string stress } S = T/A, \text{ then}
\]

\[
\Delta T = E(S)(\Delta L/L).
\]

In the case of a violin 1st e” string, the value of \( E/S \) is about 140 for a steel one and 25 for a gut one.

To find the effect on vibrating frequency, we differentiate the Mersenne-Taylor formula and get

\[
\Delta f/f = \Delta L/L + \Delta T/2T = \Delta L/L + (E/2S) \text{ where } f \text{ is frequency, } \Delta f \text{ its change, and } \Delta L \text{ and } \Delta T \text{ are averages over a vibrating cycle. By doing the integration, it turns out that the maximum displacement } \Delta L_{\text{max}} \text{ is twice the average in both plucking and bowing. Ignoring the 1 compared to } E/2S, \text{ the result is}
\]

\[
\Delta f/f = (1/4)(E/S)(\Delta L_{\text{max}}/L). \text{ The maximum tolerable amount is about 0.02, a third of a semitone.}
An analysis of the bridge hole data on lutes in Comms 1288 and 1350

In those Comms, Mimmo Peruffo presented measurements of some bridge-hole diameters on 27 lutes from the 16th to the 18th centuries. The objective of this Comm is to explore aspects of historical lute stringing that might be inferred from this evidence. At first sight, it seems that these diameters vary so much that no quantitative conclusions can be made, but if we group them sensibly into types of strings on types of lutes, the averages make more sense, and can give us reasonable indications of what to expect historically. To broaden the the basis for comparisons of types, besides averaging the hole sizes, we also average a measure of the inharmonicities of the strings and the string tensions.

As mentioned in Comm. 1766, the inharmonicity constant equals \( (\pi^2/128)(E/p)(D^2/f^2L^4) \), where \( E \) is the string's elastic modulus, \( p \) is its density, \( D \) is its diameter, \( f \) is its tuning frequency and \( L \) is its vibrating length. For the moment let us consider only the case where the relevant strings were always made of the same material, so they had the same \( E \) and \( p \) (this applies to all of the lutes except the 6-course one). These strings were called Catlins or Lyons or Pistoy basses, and probably were of 2-strand rope construction. The square-root of the inharmonicity constant is then proportional to \( D/fL \).

Let \( f = f_1(f/f_1) \) with the 1 referring to the first string, so \( D/fL \) is proportional to \( D/(f/f_1) \). We expect that \( f/f_1 \) is constant since that is a necessary consequence of the instruction to tune the highest string as high as it can go without breaking, so the proportionality is with \( D/(f/f_1) \), which we define as the 'inharmonicity factor' where the \( D \) is the diameter of the hole (in mm), \( L \) is in metres and \( f \) is in Hz. The real string inharmonicity factor, not specifically referred to below, is the one defined here multiplied by the fraction of the hole diameter that we assume represents the string diameter.

The tension can be found from the Mersenne-Taylor Law which, for the density of gut, can be expressed as the tension (T) in Kg being \( T = (D_SfL/48.09)^2 \), where \( D_S \) is the string diameter. Now \( D_SfL = D_S(f/f_1)(f/f_1)L \). We have found from Praetorius's information that the maximum \( fL \) for gut was about 210, with his lute at that maximum. Most surviving lutes which apparently had the same tuning as Praetorius's one have a somewhat smaller string stop, so we assume \( f/f_1 \) typically to be 200. Also assumed is that the string diameter is 0.9 times the hole diameter (\( D_S = 0.9D \)). Then we can estimate the tension to be \( T = [(0.9D)(f/f_1)(200)/48.09]^2 = 14.0[D/(f/f_1)]^2 \).

The first group to be discussed is the ones where all of the strings go over one nut and which we expect were designed to be played with Renaissance tuning. Then the 6th course was 2 octaves below the first, so \( f/f_1 = 1/4 \). On 7-course lutes, it is assumed that the 7th course was tuned 2 octaves and a 4th below the 1st, so \( f/f_1 = (1/4)(3/4) = 3/16 \). On 8-course and 10-course lutes, it is assumed that the lowest course was tuned 2 octaves and a 5th below the 1st, so \( f/f_1 = (1/4)(2/3) = 1/6 \). One lute has a 7th course tuned 2 octaves and a tone below the 1st, so \( f/f_1 = (1/4)(8/9) = 2/9 \). It is presumed that we are generally dealing with the low-octave string of octave pairs.

The calculations are displayed on Table 1. The 'number in Peruffo's papers' is not actually given in his papers, but numbering the sequence of his entries will help to locate the origins of the numbers listed here. For each average, the root mean square (r. m. s.) deviations from the average is given as an indication of the range of values involved in the average. The averages of the equivalent hole sizes, inharmonicities factors and string tensions are given are not only over all of the lutes of this type, but also just over those with short and those with long string stops, to see the effect of length on the data. Mace (p.65) wrote 'And as to the size, if it be a Large Lute, it must have the Rounder Strings; and a Small Lute, the Smaller.' We would thus expect that the hole sizes of the longer lutes would be greater than those of the shorter lutes. The hole-sizes averages do not show this. They indicate that on these types of lutes in the length ranges covered, lute players tended, on average, to pick the same diameter for the same course function (either 6th or lowest). There seems more consistency in string diameters than in inharmonicity or string tension.

At the bottom of this Table is the data on the one 6-course lute measured. The 6th course hole diameter happens to be the same as the average of the other lutes. Since this lute was probably from
the 16th century, designed to use a type of lowest string (of high-twist gut) different from the later lutes. This suggests that in the transition from 6 to more courses, the diameter and tension of the 6th course did not change, even though the type of string used for it changed. We would expect that in the transition, the inharmonocities of the lowest strings would be roughly equal. According to our measurements of elastic modulus, that of high-twist gut is about 3 MPa, while that of a 2-strand gut rope is about 1 MPa. To take elastic modulus into account, for two strings with the same inharmonicity, the hole inharmonicity factor times $\sqrt{E}$ is constant, so to get the hole inharmonicity factor of the high-twist string to be comparable to the others, one has to multiply it by the square root of the ratio of elastic moduli, which is $\sqrt{3}$. The 6-course lute 6th inharmonicity is 10.1, and multiplying it by $\sqrt{3}$ results in 17.5. Since the relevant average inharmonicity factor, that for the short lutes, is 17.3, that expectation is supported exceptionally well.

As an application of this information, let us now try to reconstruct the stringing of Praetorius's lute, which was tuned in his preferred Chorthon pitch standard with $a'$ = about 380 Hz, and which had a string stop of 61.8 cm. The tuning pitch range was 2 octaves and a 5th in 9 courses. We assume that the 6th course and lowest course string diameters were 0.9 times the average hole diameters given in Table 1.

We can consider that the diameter of the 1st string was the thinnest string that the statutes of Italian string makers allowed, made out of two whole guts, which is about 0.43 mm in diameter when made, and which becomes about 0.40 mm when stretched. But the 1st string is single while doing the same job as is otherwise done by a double courses, so to balance with the second course, it must be thicker than it would be if it was a double course. We would expect that the higher strings, except for the 1st, should be close to being in equal tension, and we have found that for balance with the second string, the stretched 1st string should be 15-20\% thicker than in equal tension with the 2nd course. We thus assume a virtual 0.34 mm diameter for the 1st string to calculate a smooth tension distribution with the already defined diameters of the 6th and lowest strings.

With the three defined diameters, we assume a power law relating the tensions, with the difference in tension between any string and the tension of the virtual 1st string (T - T_1) equalling a constant ($a$) times the number of tuning semitones between that string and the 1st ($n$) raised to a power ($n$), or $T - T_1 = a n^n$. From the three diameters, we calculate that $a = -2.6 \times 10^{-17}$ and $n = 11.0$, from which we calculate the other tensions and diameters. These are shown in Table 2. We can see in the Tension column that it drops only 2\% over the top 6 courses, when the total tension drops 29\%. We can then understand how lute stringing got the reputation, as Mersenne repeated, that the ratio of diameters was the inverse of the ratio of frequencies (actually, instead of frequency, it was the ratio of string lengths on the monochord for the interval difference). It is probable that Mersenne noticed that this relationship didn't work on the lower strings, didn't have an explanation for this, and so obscured the situation by lumping the lute and archlute together in his statement.

The next group of lutes to be discussed is the 11-course lutes in $d$-minor tuning. Peruffo gave diameters for the 10th course hole for three of the lutes and the 11th course hole for eight. The 10th course was tuned 2 octaves and a minor third below the 1st, so $f_{10}/f_1 = (1/4)(5/6) = 5/24$, and the 11th course was tuned 2 octaves and a 4th below the 1st, so $f_{11}/f_1 = (1/4)(3/4) = 3/16$. The results are shown in Table 3. The average inharmonicity factor on the lowest string of 13.3 is decidedly less than the 16.3 average for the earlier lutes with Renaissance tuning. Thinner lowest strings on the later lutes could be related to a shift in the balance away from power towards richness and clarity as the lute. No measured later baroque lute had an inharmonicity factor greater than 15.

Let us next try to reconstruct the stringing of the 11-course 'French lute' mentioned by Talbot (Comm.1592) with a string stop of 71.0 cm and $d$ minor tuning. The French pitch standard of $a' = 375$ Hz is assumed. Both the 1st and 2nd courses are single, so we must have virtual diameters for these with that of the first assumed to be, as with Praetorius's lute, 0.34 mm. From Table 3 we take the average hole diameters of the 10th and 11th course strings of 1.60 and 1.75 mm, and applying the power law, we get $n = 2.35$ and $a = -2.01 \times 10^{-4}$. From these, we calculate the other tensions and diameters, which are shown on Table 4. There is reason to question this solution. It is very sensitive to the difference in diameter between the 10th and 11th course strings. The 0.15 mm difference in the averages is not reflected in the three actual lutes that had both these strings measured, where the
differences are 0 mm in two, and 0.52 mm in the third. The last one clearly cannot relate to the real
difference in string diameter. A solution that is more likely has the smaller difference in string
diameter of 0.06 mm that results from assuming that n is the same (11.0) as that found for
Praetorius’s lute, which makes a = -4.6x10^-17. Both of these solutions are shown in Table 4. With
either, as with the Praetorius case, the deviations from equal tension on the strings other than the
diatomic basses is insignificant.

The remaining groups of lutes have the strings going over two nuts. These include archlutes with
apparently Renaissance tuning, 13-course baroque lutes with a 2-course bass outrider on the pegbox,
and late German 13-course archlutes. The last two groups apparently were designed for the d-minor
tuning. For these lutes, we have to modify our inharmonicity calculations to make them comparable
with those made above on lutes with a single nut.

We defined the inharmonicity factor as the square root of the inharmonicity constant with constant
factors being ignored. The ignored constants were a numerical constant (\(\pi^2/128\)), a materials
constant (E/p), and 1/(f L) which is constant because this results from tuning the highest string as
high as it could safely go without breaking. What was left was D/f (f L). When a string is on a nut
with a string stop (L2) that is different from that with the highest string (L1), we have to replace
D/f (f L) by something that when divided by f L equals D/(fL^2). That something can only be
D/f L (f L)/(f L) L2. Thus the equivalent inharmonicity factor is the same as the usual one but where
f L appears it must be replaced by (f L)/(L2/L1). The same modification is needed in the calculation
of string tension since the kernel factor Ds f L = Ds (f L)/(L2/L1) (f L).

The calculations for archlutes with Renaissance tuning are shown on Table 5. The tuning range
interval that has not previously been encountered is the 2 octaves and a 6th for an 11th course, where
f L = (1/4)(5/8) = 5/32. Strings in the courses on the short necks are a bit thinner than corresponding
ones on the lutes with that tuning in Table 1, making the inharmonicity factors smaller and the string
tensions smaller yet. Holes for strings on the long neck unfortunately were measured on only two of
the archlutes. These two diameters are typical of those on the lowest strings on the one-nut lutes, but
on the archlute, they reduced the inharmonicity below that of the lowest strings on the short neck.
What is surprising is that on both, the long-neck strings seem to have almost double the tension of the
short-neck ones.

Only one 13-course baroque lutes with a 2-course bass outrider on the pegbox was measured.
Information on it is shown at the bottom of Table 5. The top 11 course are in all respects, including
stringing, similar to the 11-course lutes with the same d-minor tuning in Table 3. The 13th-course
hole is smaller than the 11th-course hole. This can possibly make sense if, when going from the
11th to the 13th course, players then were more concerned with keeping the inharmonicity from
increasing than with keeping up the tension.

The final group considered is the late German crook-necked archlutes with d-minor tuning.
Information on six of these is shown in Table 6. Notable about the resulting averages of these is that
both the inharmonicity and string tension of the lowest strings on the short neck and the long neck are
the same. We can theoretically derive the ratio of string stops that gives this result. Equal
inharmonicity factors gives D L/(f L)/(L L)/(L S) L L = D S/f (f S/f L) L S where the subscript L refers to
long and S refers to short. Equal tensions gives D L/(f L)/(L L)/(L S) = D S/(f L). Dividing the second
equation by the first, and we get (L L)/(L S)^3 = (f S/f L)^2. So L L/L S = (f S/f L)^2/3. The lowest string on
the long neck is a minor 6th lower than that on the short neck, so f S/f L = 8/5, resulting in L L/L S =
1.37. This is remarkably close to the average of 1.36 for the ratio of string stops on these archlutes.

Of course, the makers and players of these lutes didn’t know about string inharmonicity and knew
little about string tension, but these are physical manifestations (that can be discussed and measured
today) of what they could sensitively hear and feel when they played. We cannot know what
language (if any) they used to explain their aesthetic judgements about strings, but studies like this,
using modern scientific language, are a good way to find out what those judgements were.
Table 1: Lutes with one nut and Renaissance tuning

<table>
<thead>
<tr>
<th>Number of courses</th>
<th>No in Peruffo's papers</th>
<th>Course</th>
<th>String stop L (m)</th>
<th>Frequency ratio of range f/f1</th>
<th>Hole diam D (mm)</th>
<th>Inharmon factor D/[f/f1)L</th>
<th>String tension T (Kg)</th>
</tr>
</thead>
<tbody>
<tr>
<td>7</td>
<td>1</td>
<td>6th</td>
<td>0.583</td>
<td>0.250</td>
<td>1.60</td>
<td>11.0</td>
<td>2.2</td>
</tr>
<tr>
<td></td>
<td></td>
<td>7th</td>
<td>0.583</td>
<td>0.188</td>
<td>2.00</td>
<td>18.3</td>
<td>2.0</td>
</tr>
<tr>
<td>7</td>
<td>3</td>
<td>6th</td>
<td>0.590</td>
<td>0.250</td>
<td>1.50</td>
<td>10.2</td>
<td>2.0</td>
</tr>
<tr>
<td></td>
<td></td>
<td>7th</td>
<td>0.590</td>
<td>0.188</td>
<td>1.80</td>
<td>16.3</td>
<td>1.6</td>
</tr>
<tr>
<td>7</td>
<td>8</td>
<td>6th</td>
<td>0.681</td>
<td>0.250</td>
<td>1.70</td>
<td>10.0</td>
<td>2.5</td>
</tr>
<tr>
<td></td>
<td></td>
<td>7th</td>
<td>0.681</td>
<td>0.167</td>
<td>1.80</td>
<td>15.9</td>
<td>1.3</td>
</tr>
<tr>
<td>8</td>
<td>2</td>
<td>6th</td>
<td>0.672</td>
<td>0.250</td>
<td>1.40</td>
<td>8.3</td>
<td>1.7</td>
</tr>
<tr>
<td></td>
<td></td>
<td>7th</td>
<td>0.672</td>
<td>0.222</td>
<td>1.50</td>
<td>10.0</td>
<td>1.6</td>
</tr>
<tr>
<td></td>
<td></td>
<td>8th</td>
<td>0.672</td>
<td>0.167</td>
<td>2.12</td>
<td>18.9</td>
<td>1.7</td>
</tr>
<tr>
<td>8</td>
<td>7</td>
<td>8th</td>
<td>0.905</td>
<td>0.167</td>
<td>1.90</td>
<td>12.6</td>
<td>1.4</td>
</tr>
<tr>
<td>10</td>
<td>5</td>
<td>6th</td>
<td>0.678</td>
<td>0.250</td>
<td>1.30</td>
<td>7.7</td>
<td>1.5</td>
</tr>
<tr>
<td></td>
<td></td>
<td>10th</td>
<td>0.678</td>
<td>0.167</td>
<td>1.80</td>
<td>15.9</td>
<td>1.3</td>
</tr>
</tbody>
</table>

Average 6th all r.m.s. deviation ±
Average 6th short r.m.s. deviation ±
Average 6th long r.m.s. deviation ±
Average lowest all r.m.s. deviation ±
Average lowest short r.m.s. deviation ±
Average lowest long r.m.s. deviation ±

<table>
<thead>
<tr>
<th>Number of semitones below the 1st</th>
<th>Nominal tuning pitch</th>
<th>Frequency a'=380 Hz (Hz)</th>
<th>Tension with virtual 1st (Kg)</th>
<th>Diameter with virtual 1st (mm)</th>
<th>Diameter if in equal tension (mm)</th>
</tr>
</thead>
<tbody>
<tr>
<td>0</td>
<td>g'</td>
<td>338</td>
<td>2.19</td>
<td>0.34</td>
<td>0.34</td>
</tr>
<tr>
<td>5</td>
<td>d'</td>
<td>254</td>
<td>2.19</td>
<td>0.45</td>
<td>0.45</td>
</tr>
<tr>
<td>10</td>
<td>a</td>
<td>190</td>
<td>2.19</td>
<td>0.61</td>
<td>0.61</td>
</tr>
<tr>
<td>14</td>
<td>f</td>
<td>151</td>
<td>2.19</td>
<td>0.76</td>
<td>0.76</td>
</tr>
<tr>
<td>19</td>
<td>c</td>
<td>113</td>
<td>2.18</td>
<td>1.02</td>
<td>1.02</td>
</tr>
<tr>
<td>24</td>
<td>G</td>
<td>85</td>
<td>2.15</td>
<td>1.35</td>
<td>1.35</td>
</tr>
<tr>
<td>26</td>
<td>F</td>
<td>75</td>
<td>2.09</td>
<td>1.49</td>
<td>1.53</td>
</tr>
<tr>
<td>27</td>
<td>E</td>
<td>71</td>
<td>2.05</td>
<td>1.56</td>
<td>1.62</td>
</tr>
<tr>
<td>29</td>
<td>D</td>
<td>63</td>
<td>1.88</td>
<td>1.68</td>
<td>1.82</td>
</tr>
<tr>
<td>31</td>
<td>C</td>
<td>56</td>
<td>1.55</td>
<td>1.71</td>
<td>2.04</td>
</tr>
</tbody>
</table>

On the real 1st, with a diameter of 0.40 (unstretched 0.43) mm, the tension is 3.03 Kg.
Table 3: Lutes with one nut and baroque $d$-minor tuning

<table>
<thead>
<tr>
<th>No of courses</th>
<th>No in Peruffo's papers</th>
<th>Course</th>
<th>String stop L (m)</th>
<th>Frequency ratio of range $f/f_1$</th>
<th>Hole diam D (mm)</th>
<th>Inharmonicity factor $D/[f/f_1]L$</th>
<th>String tension T (Kg)</th>
</tr>
</thead>
<tbody>
<tr>
<td>11</td>
<td>12</td>
<td>10th</td>
<td>0.699</td>
<td>0.208</td>
<td>1.50</td>
<td>10.3</td>
<td>1.4</td>
</tr>
<tr>
<td>11</td>
<td>11</td>
<td>11th</td>
<td>0.699</td>
<td>0.188</td>
<td>1.50</td>
<td>11.4</td>
<td>1.1</td>
</tr>
<tr>
<td>11</td>
<td>13</td>
<td>10th</td>
<td>0.674</td>
<td>0.208</td>
<td>1.80</td>
<td>12.8</td>
<td>2.0</td>
</tr>
<tr>
<td>11</td>
<td>11</td>
<td>11th</td>
<td>0.674</td>
<td>0.188</td>
<td>1.80</td>
<td>14.2</td>
<td>1.6</td>
</tr>
<tr>
<td>11</td>
<td>15</td>
<td>10th</td>
<td>0.718</td>
<td>0.208</td>
<td>1.50</td>
<td>10.0</td>
<td>1.4</td>
</tr>
<tr>
<td>11</td>
<td>11</td>
<td>11th</td>
<td>0.718</td>
<td>0.188</td>
<td>2.02</td>
<td>15.0</td>
<td>2.0</td>
</tr>
<tr>
<td>11</td>
<td>16</td>
<td>11th</td>
<td>0.716</td>
<td>0.188</td>
<td>1.60</td>
<td>11.9</td>
<td>1.3</td>
</tr>
<tr>
<td>11</td>
<td>17</td>
<td>11th</td>
<td>0.690</td>
<td>0.188</td>
<td>1.80</td>
<td>13.9</td>
<td>1.6</td>
</tr>
<tr>
<td>11</td>
<td>18</td>
<td>11th</td>
<td>0.690</td>
<td>0.188</td>
<td>1.80</td>
<td>13.9</td>
<td>1.6</td>
</tr>
<tr>
<td>11</td>
<td>20</td>
<td>11th</td>
<td>0.725</td>
<td>0.188</td>
<td>1.80</td>
<td>13.2</td>
<td>1.6</td>
</tr>
<tr>
<td>11</td>
<td>21</td>
<td>11th</td>
<td>0.715</td>
<td>0.188</td>
<td>1.70</td>
<td>12.7</td>
<td>1.4</td>
</tr>
</tbody>
</table>

Average 10th:

<table>
<thead>
<tr>
<th>String tension T (Kg)</th>
</tr>
</thead>
<tbody>
<tr>
<td>1.60</td>
</tr>
<tr>
<td>11.0</td>
</tr>
<tr>
<td>1.6</td>
</tr>
</tbody>
</table>

Average 11th:

<table>
<thead>
<tr>
<th>String tension T (Kg)</th>
</tr>
</thead>
<tbody>
<tr>
<td>0.14</td>
</tr>
<tr>
<td>1.3</td>
</tr>
<tr>
<td>0.3</td>
</tr>
</tbody>
</table>

r.m.s. deviation $\pm$

<table>
<thead>
<tr>
<th>String tension T (Kg)</th>
</tr>
</thead>
<tbody>
<tr>
<td>1.75</td>
</tr>
<tr>
<td>13.3</td>
</tr>
<tr>
<td>1.5</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>String tension T (Kg)</th>
</tr>
</thead>
<tbody>
<tr>
<td>0.15</td>
</tr>
<tr>
<td>1.1</td>
</tr>
<tr>
<td>0.3</td>
</tr>
</tbody>
</table>

Table 4: Conjectural stringings of Talbot’s French lute

<table>
<thead>
<tr>
<th>Number of semitones below the 1st</th>
<th>Nominal tuning pitch</th>
<th>Frequency $a'=375$ Hz (Hz)</th>
<th>Tension with virtual 1st &amp; 2nd (Kg) $n=2.35$ $n=11.0$</th>
<th>Diameter with virtual 1st &amp; 2nd (mm) $n=2.35$ $n=11.0$</th>
<th>Diameter if in equal tension (mm)</th>
</tr>
</thead>
<tbody>
<tr>
<td>0</td>
<td>$f'$</td>
<td>298</td>
<td>2.23</td>
<td>0.34</td>
<td>0.34</td>
</tr>
<tr>
<td>3</td>
<td>$d'$</td>
<td>250</td>
<td>2.23</td>
<td>0.40</td>
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</tr>
<tr>
<td>8</td>
<td>$a$</td>
<td>188</td>
<td>2.21</td>
<td>0.54</td>
<td>0.54</td>
</tr>
<tr>
<td>12</td>
<td>$f$</td>
<td>149</td>
<td>2.16</td>
<td>0.67</td>
<td>0.68</td>
</tr>
<tr>
<td>15</td>
<td>$d$</td>
<td>125</td>
<td>2.12</td>
<td>0.79</td>
<td>0.81</td>
</tr>
<tr>
<td>20</td>
<td>$A$</td>
<td>94</td>
<td>2.00</td>
<td>1.02</td>
<td>1.08</td>
</tr>
<tr>
<td>22</td>
<td>$G$</td>
<td>84</td>
<td>1.95</td>
<td>1.13</td>
<td>1.21</td>
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<td>24</td>
<td>$F$</td>
<td>74</td>
<td>1.88</td>
<td>1.25</td>
<td>1.34</td>
</tr>
<tr>
<td>25</td>
<td>$E$</td>
<td>70</td>
<td>1.85</td>
<td>1.31</td>
<td>1.41</td>
</tr>
<tr>
<td>27</td>
<td>$D$</td>
<td>63</td>
<td>1.77</td>
<td>1.44</td>
<td>1.52</td>
</tr>
<tr>
<td>29</td>
<td>$C$</td>
<td>56</td>
<td>1.69</td>
<td>1.58</td>
<td>1.82</td>
</tr>
</tbody>
</table>

With the real 1st and 2nd, with diameters of 0.40 (unstretched 0.43) and 0.48 mm, the tensions for both are 3.16 Kg.
### Table 5: Lutes with two nuts other than late German archlutes

<table>
<thead>
<tr>
<th>Number of courses</th>
<th>No in Peruffo's papers</th>
<th>Course</th>
<th>Ratio of lengths L2/L1</th>
<th>String stop L (m)</th>
<th>Frequency ratio of range f/f1</th>
<th>Hole diam D (mm)</th>
<th>Inharmonicity factor</th>
<th>String tension T (Kg)</th>
</tr>
</thead>
<tbody>
<tr>
<td>6+4</td>
<td>27</td>
<td>6th</td>
<td>1.59</td>
<td>0.584</td>
<td>0.250</td>
<td>1.60</td>
<td>11.0</td>
<td>2.2</td>
</tr>
<tr>
<td></td>
<td></td>
<td>10th</td>
<td>1.59</td>
<td>0.930</td>
<td>0.167</td>
<td>2.00</td>
<td>8.1</td>
<td>3.9</td>
</tr>
<tr>
<td>6+5</td>
<td>9</td>
<td>6th</td>
<td>1.50</td>
<td>0.542</td>
<td>0.250</td>
<td>1.10</td>
<td>8.1</td>
<td>1.1</td>
</tr>
<tr>
<td></td>
<td></td>
<td>11th</td>
<td>1.50</td>
<td>0.812</td>
<td>0.156</td>
<td>1.60</td>
<td>8.4</td>
<td>2.0</td>
</tr>
<tr>
<td>7+7</td>
<td>6</td>
<td>6th</td>
<td>1.38</td>
<td>0.640</td>
<td>0.250</td>
<td>1.40</td>
<td>8.8</td>
<td>1.7</td>
</tr>
<tr>
<td></td>
<td></td>
<td>7th</td>
<td>1.45</td>
<td>0.640</td>
<td>0.222</td>
<td>1.50</td>
<td>10.5</td>
<td>1.6</td>
</tr>
<tr>
<td>7+7</td>
<td>11</td>
<td>6th</td>
<td>1.38</td>
<td>0.580</td>
<td>0.222</td>
<td>1.50</td>
<td>11.6</td>
<td>1.6</td>
</tr>
<tr>
<td></td>
<td></td>
<td>7th</td>
<td>1.38</td>
<td>0.670</td>
<td>0.250</td>
<td>1.38</td>
<td>8.2</td>
<td>1.7</td>
</tr>
<tr>
<td>6+8</td>
<td>10</td>
<td>6th</td>
<td>1.38</td>
<td>0.670</td>
<td>0.250</td>
<td>1.38</td>
<td>8.2</td>
<td>1.7</td>
</tr>
</tbody>
</table>

Average 6th r.m.s. deviation ± 0.16 1.1 0.4
Average lowest on short neck 1.42 9.9 1.6
Average lowest on long neck 1.80 8.3 3.0

<table>
<thead>
<tr>
<th>Number of courses</th>
<th>No in Peruffo's papers</th>
<th>Course</th>
<th>Ratio of lengths L2/L1</th>
<th>String stop L (m)</th>
<th>Frequency ratio of range f/f1</th>
<th>Hole diam D (mm)</th>
<th>Inharmonicity factor</th>
<th>String tension T (Kg)</th>
</tr>
</thead>
<tbody>
<tr>
<td>11+2</td>
<td>14</td>
<td>11th</td>
<td>1.06</td>
<td>0.716</td>
<td>0.188</td>
<td>1.70</td>
<td>12.7</td>
<td>1.4</td>
</tr>
<tr>
<td></td>
<td></td>
<td>13th</td>
<td>1.06</td>
<td>0.760</td>
<td>0.156</td>
<td>1.60</td>
<td>12.7</td>
<td>1.0</td>
</tr>
</tbody>
</table>

### Table 6: Late German archlutes with d-minor tuning

<table>
<thead>
<tr>
<th>Number of courses</th>
<th>No in Peruffo's papers</th>
<th>Course</th>
<th>Ratio of lengths L2/L1</th>
<th>String stop L (m)</th>
<th>Frequency ratio of range f/f1</th>
<th>Hole diam D (mm)</th>
<th>Inharmonicity factor</th>
<th>String tension T (Kg)</th>
</tr>
</thead>
<tbody>
<tr>
<td>8+5</td>
<td>22</td>
<td>13th</td>
<td>1.36</td>
<td>0.950</td>
<td>0.156</td>
<td>2.00</td>
<td>9.9</td>
<td>2.5</td>
</tr>
<tr>
<td>8+5</td>
<td>23</td>
<td>8th</td>
<td>1.35</td>
<td>0.738</td>
<td>0.250</td>
<td>1.50</td>
<td>8.1</td>
<td>2.0</td>
</tr>
<tr>
<td>8+5</td>
<td>24</td>
<td>8th</td>
<td>1.28</td>
<td>0.705</td>
<td>0.250</td>
<td>1.90</td>
<td>10.8</td>
<td>3.2</td>
</tr>
<tr>
<td>8+5</td>
<td>25</td>
<td>8th</td>
<td>1.36</td>
<td>0.700</td>
<td>0.250</td>
<td>1.40</td>
<td>8.0</td>
<td>1.7</td>
</tr>
<tr>
<td>8+5</td>
<td>26</td>
<td>8th</td>
<td>1.39</td>
<td>0.690</td>
<td>0.250</td>
<td>1.40</td>
<td>8.1</td>
<td>1.7</td>
</tr>
<tr>
<td>8+5</td>
<td>19</td>
<td>13th</td>
<td>1.44</td>
<td>1.045</td>
<td>0.156</td>
<td>1.40</td>
<td>6.0</td>
<td>1.4</td>
</tr>
</tbody>
</table>

Average 8th r.m.s. deviation ± 0.21 1.2 0.6
Average 13th r.m.s. deviation ± 0.22 1.5 0.5