

I am writing with regards to Martin Elste's article 'From Landowska to Leonhardt, from Pleyel to Skowronek' (*Early Music*, xlii/2 (2014), pp.13–22). Although full of valuable information and details, I believe that it is based on a central premise that is not entirely correct. This premise can be summarized as follows: few if any harpsichordists played in an acceptable 'historical' style before Leonhardt; few if any harpsichord-builders made instruments in an acceptable 'historical' style before Skowronek; and little of importance happened in America during the early years of the so-called 'early music' revival.

The fascinating story of the revival is much more complex and diverse than that. For example, Elste concludes from the cover art on an LP jacket (his illus.2) that Fernando Valenti used what he describes as Landowska's 'claw'. I knew Fernando, and I heard and saw him perform on many occasions: Fernando did not play that way, nor did any of us in the 1950s and early 60s. More significantly, Elste exaggerates when he writes that Leonhardt's use of raven quill and historical instruments in the late 1950s 'signalled the beginning of a new approach to the harpsichord'. It was certainly important and admirable, but it was not 'new'. Ralph Kirkpatrick, who was my teacher, performed on historical harpsichords (and clavichords) early in his career, and he also described to me how he quilled his harpsichord in bird quill during the 1930s.

I am delighted that people are beginning to examine and describe the invaluable contributions of those pioneers that led to the growing acceptance of the harpsichord and its music, but I urge them to take a more global view of this phenomenon and dig a little deeper into its history.

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Exactly 50 years have passed since Frederick Neuman's classic article 'The French "Inégales", Quantz, and Bach' (*Journal of the American Musicological Society*, xviii/3 (1965), pp.313–58) was published. His article precipitated an avalanche of scholarly polemic. In a series of publications the author was forced to argue with his numerous opponents. By the end of the 20th century the problem was somehow settled, and a period of stability followed. Now, once again, we must return to the same problem in connection

with the publication last year of the article by Beverly Jerold with a highly promising title 'Notes *inégales*: a definitive new parameter' (*Early Music*, xlii/2 (2014), pp.273–89). It would seem that we should here *de facto* encounter a work in many ways surpassing Neumann's, and presenting convincing/definitive generalizations and solid theoretical definitions of the phenomenon of French *inégales*. Some notes were added by Jerold a year later (*Early Music*, xliii/2 (2015), p.369).

Jerold centres (directly or indirectly) most of the weight in the article on a polemic debate with John Byrt's point of view, and we partly agree with her arguments. This polemic probably explains why (having in mind the title—which is of special importance) we were not able to detect which scholarly or scientific method is applied in Jerold's work. Different national traditions and historic styles (high Baroque, Rococo, Classical) are intermixed there, and instead of providing general definitions ('a definitive new parameter') the article is separated into regimental units such as 'The basic structure' devoted to the discussion of '*notes inégales* outside of France', with Loulié as the main early French author who followed the routine 'taught to beginners'...; next 'Cancelling inequality', with Corrette (1741), Bordier (c.1760), Lacassagne (1766), Mercadier de Belesta (1776), Bordet (c.1755) as witnesses; then 'Unwritten inequality: a French practice' etc.

Jerold begins the first part of her article by stating that 'new documentation clarifies the purpose'. This thesis turns into an unbelievable suggestion to ignore 'nearly 70 French sources' (discovered by Neumann), because they represent 'method books [designed] principally for beginners', where nothing more than 'scraps of information about the practice' of *notes inégales* is given. Our question is: who has ever subdivided early treatises into more important and less important ones? We have often encountered occasions when the 'less' important works with 'rudimentary music instruction' (Jerold) shed more light on questions of theory and performance. From our point of view, to dismiss any early source in scientific historic research contradicts the concept of a historical approach. Take, for example, the rudimentary treatise by M. de Saint Lambert, which is named among French treatises dealing with inequality in Neuman's initial article. Along with the treatises of LAffillard (1694), Montéclair (1709, c.1735 and c.1736) and [Borin] (1722), de Saint Lambert's treatise *Principes du clavecin* (1st edn, presumably 1697) is not even mentioned by Jerold. Does this mean that these (and many other works) are, according to her point of view, inconsequential or marginal?

Il est tres difficile de donner des principes generaux sur l'égalité ou sur l'inégalité des notes, car c'est le gout des Pieces que l'on chante qui en decide, cependant il faut observer qu'en quelque mesure que ce soit, les notes dont il en faut quatre pour remplir un tems, sont toujours inégales la premiere un peu plus longue que la seconde. Ex:



1 Michel Pignolet de Montéclair, *Nouvelle méthode pour apprendre la musique...* (Paris, 1709), p.15

There is also no answer to the question as to why Jerold (along with some other authors) writes only about 'De la Mesure à trios Temps...' from Loulié's *Elements*, yet does not mention his explanation of inequality in other measures. In Neumann's Table II (1965) and in our book (I. Rosanoff and A. Panov, *Essays on problems of rhythm in Germany in the XVIIIth century* (Heilbronn, 1996)) this information is given. It seems that Jerold did not survey the great mass of published literature (French, German, English) on inequality beginning with Arnold Dolmetsch. And why, citing from Loulié, does Jerold not pay any attention to the initial part of his quotation, where he mentions that 'Dans quelque Mesure que ce soit' (in any signature whatever) the 'first half-beats are sometimes made a little longer'? This suggests that inequality is recommended not only in triple measures, as Neumann showed. The same instruction is found in Loulié's previous recommendation (1696, p.32), where he discusses 'De la Mesure à deux Temps'; thus, in 2, C and 2/4 'Le 1. & le 3. quart de chaque Temps sont plus longs que le 2. & 4. quoy qu'ils soient marques égaux, dans quelque Mesure que ce soit' (our emphasis). It might seem that Jerold upholds historical documents only where they are complementary to her new parameter. What does not exactly match is either ignored or replaced with unsubstantiated generalizations such as 'Most of the ... sources'.

The same question arises in connection with the often-quoted §12 on p.105 of Quantz's *Versuch einer Anweisung die Flöte traversiere zu spielen*. We agree in part with the initial section of Jerold's treatment of Quantz's instruction, but as she continues, everything is turned 'upside-down' and Quantz's recommendation to perform even written notes in an uneven rhythm is interpreted as a matter of 'stressed' notes and passing notes. We also do not understand why Quantz's words 'gute Noten' and 'Schlimme Noten' (and not only his, but those of many other German authors) are translated as 'good' and 'inferior' notes instead

of the more generally accepted 'good' and 'bad' notes. In the simultaneous French translation (Berlin, 1752), Quantz uses the words 'notes bonnes' and 'notes mauvaises'! §12, which has become famous, has been analysed in detail by many authors, and not everyone fully agrees with each other. Why is there no reference from Jerold to the literature on this?

The absence of a definitive generalization leads to such questions as: why are the so-called 'gute' and 'schlechte'/'schlimme' notes discussed in the context of Jerold's 2014 article? Why is, for instance, David Schulenberg's (*The keyboard music of J. S. Bach* (London, 2/2006) recommendation to perform J. S. Bach's keyboard music according to the practice of *notes inégales* sometimes partially and sometimes 'throughout' left without comment? Neither Schulenberg nor his writings are mentioned by Jerold. Nevertheless, it is vitally important to consider such a different concept, bearing in mind the main purpose of Jerold's article. As an example, we can cite Schulenberg's treatment of J. S. Bach's so-called 'English' suites, when on p.284 he draws the conclusion: 'The moderate tempo and flowing sixteenth of both passepieds [from the Suite No.5 in E minor, BWV810] make them good candidates for the use of notes inégales, although the latter might well have been expected throughout these suites, as in French music of the period generally' (our emphasis)? Isn't this a question of the tradition of 'notes inégales outside of France' that should be discussed?

We also cannot agree either with the following argument nor with its conclusion: Jerold begins by stating that in France the application of *notes inégales* 'was routinely taught to beginners in music' (p.273), 'in reality, this form of unwritten mild dotting was employed in large part to teach beginners the rhythmic organization of beats in the various time signatures' (p.274), and so the phenomenon of *notes inégales* turns out to be a pure pedagogical method of teaching meter: 'Without the modern metronome to assist their pupils

in reading music, teachers needed a substitute. Elsewhere in Europe, this was accomplished by defining beats as “good” and “inferior”, or “strong” and “weak”, but without altering note values’ (p.274). Thus *notes inégales* are equated here with ‘gute’ and ‘schlechte’/‘schlimme’ notes of the German ‘Taktenlehre’. The conclusion to these arguments in Jerold’s work reads: “This adds a new dimension to the question of unwritten inequality. Instead of being an arcane artistic device, it was for the most part a means for helping beginners and less-accomplished musicians learn the music’ (p.274). Both this argument and the conclusion are false. To prove this it is sufficient to cite Montéclair (*Nouvelle méthode pour apprendre la musique*, 1709, see [illus.1](#)):

It is very difficult to give general principles on the equality or inequality of the notes, because it is the character of the pieces which one sings that determines this; however, it is necessary to observe that in whatever measure it be, the notes of which there are four to fill up a beat, are always unequal, the first a little longer than the second.

In almost the same words, Stephen E. Hefling (*Rhythmic alteration in seventeenth- and eighteenth-century music* (New York, 1993), p.28) quotes this passage from Montéclair. Were French music teachers—as Jerold puts it—so incompetent, and did they make such fatal mistakes by teaching their pupils to play unevenly, because it would take years to retrain these poor beginners if in future they needed to perform music evenly/*égal*, and only considered music texture in terms of stressed and inferior elements? What is the reason for this defamation (qualified as ‘a new dimension’) of French music pedagogy of the 18th century?

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It is a pleasure to cross swords with Beverly Jerold (Correspondence, *Early Music*, xliii/2 (2015), p.369) once more on Italian inequality. To start with: I concede that I may have misunderstood Hotteterre’s remark about Lully’s *Tempo di gavotta* on his p.57. As far as Corrette’s and Hotteterre’s ‘tabular systems’ are concerned, however, I do not agree with Jerold that these authors made any distinction between ‘French’ and ‘Italian’ time-signatures (they regarded them as ‘shared property’) and I certainly do not accept that the position in which they placed the note-value eligible for inequality had anything to do with

whether the Italians used inequality. I think this is a figment of her imagination. My view of the matter is quite different: I believe that these authors were trying to show their readers that the French adopted many Italian metres and that this explains how semiquaver inequality finds its way into the Neumann/Hefling Meter-Inequality Relationships Table (see S. Hefling, *Rhythmic alteration in seventeenth- and eighteenth-century music* (New York, 1993), p.8).

I am afraid I still consider that her ‘essential parameters for *notes inégales*’ has nothing to do with my work on Scarlatti and Handel, for I do not think the use of inequality by these composers shows any French influence, unless they were writing in the French style (which Handel did occasionally but Scarlatti did not, as far as I know). Muffat certainly tried to publicize the use of inequality in French music but he also referred more than once to the necessity to play quavers equal in Italian music.

Jerold believes that her ‘finding that *notes inégales* are cancelled when notes smaller than those eligible for inequality are present’ (p.287) removes much of Byrt’s musical-score evidence. (I do not follow this at all—surely it only applies to French music where the quaver was normally unequal. In Italy the quaver was emphatically always equal!). In any case Jerold seems to misunderstand the French sources on this point. Saint-Lambert states in his *Principes du clavecin* (1702) that in allemandes ‘à cause de la lenteur du mouvement ... l’inégalité tombe sur les doubles croches, s’il y en a’. I would argue that what she is describing here is what Hotteterre wants his readers to do when he writes ‘croches égales’ over certain preludes in his *L’art de préluder*: in other words, to let the inequality descend to the semiquavers. I would refer her to Hefling, p.27.

I still insist that when Rousseau says ‘croches’ he means ‘croches’. Jerold writes that Couperin offers no instructions for the use of inequality, but what about his ‘doubles croches un tant-soit-peu pointées’? Finally, I shall never believe that inequality was essentially a French convention and would point to the fact that the earliest evidence we have for it comes from a Calvinist musician from Geneva (1550) and a Spanish writer on improvisation (1565). This is not to mention the writings on the subject by Bovicelli (1594), Caccini (1602) and Frescobaldi (1615)—Italians to a man. As to France’s contribution—I side with Professor John Butt, who writes that ‘the French practice was a particular standardization of an existing, European-wide, ornamental practice’.

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