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# TOWARDS AN UNDERSTANDING OF HOW BRASS INSTRUMENTS WORK

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This paper is an historical account of how we at Surrey trod our way through a maze of inter-connecting problems. Some of the problems we encountered are usually thought of as psychology or fluid dynamics rather than physics or acoustics, some were not at all difficult to grasp in outline whilst others were remarkably intangible and even now are far from being posed cogently let alone being answered. Since this paper is to be a personal review it seems appropriate to adopt a rather informal style.

The work I am going to describe was largely carried out by an excellent team of research students, Richard Pratt, Steve Elliott, Peter Watkinson, John Goodwin, and Anne Duffield, to name them in chronological order, by Research Officers Richard Bacon, Richard Shepherd and, briefly, Bernard Smith, by a Post-Doctoral Fellow, Robert Edwards, and by a colleague Peter Simpson. They were financed by the Science and Engineering Research Council and by two industrial colloborators: Messrs Boosey & Hawkes (MI) Ltd. and Paxmans Ltd. I was also able to obtain a Research Grant from the SERC, another from the Joint Committee of the SERC and the SSRC, and a minor award from the Interactive Computing Facility of the SERC.

We first turned to measuring the input impedance; the work of John Backus and Arthur Benade in the USA and Klaus Wogram in Germany, to name but three, led us to realise that there was a need for an absolute measurement of this quantity. There were many ways which could be used, but it seemed important to us in Surrey to remember that impedance is a quantity which is defined as the ratio of two others and therefore that it would be better to measure both of them (pressure and volume velocity) rather than have to rely on a measurement of some, perhaps imperfectly, known system to provide a calibration. It is a matter of some argument among those working on brass instruments where exactly one should make the measurement. We measured at the narrowest part of the mouthpiece throat for two main reasons: firstly, that position is well defined, it is reproducible and stable, and secondly, the particle velocity is highest there and thus its measurement is facilitated. I shall turn to the opposing arguments later. We borrowed a device from fluid dynamics to measure the particle velocity of the oscillating air: the hot wire anemometer, and built a special rig to calibrate it every time it was used. The microphone used to record the pressure also had a special calibration rig since we had to use a probe microphone with a very small end and high impedance so that not too much of the volume velocity we were measuring with the anemometer was lost to the microphone.

The instrument under test was excited by using a large loudspeaker to drive it at a succession of single frequencies. Many people criticised this approach on the grounds that it was not a "natural" way for the instrument to be sounded. They usually went on to ask why we chose not to employ artificial lips or similar devices, for example. The answer was, and still is of course, that we were not trying to make the instrument sound in a natural way, we were making a physical measurement of a physical quantity, and that quantity is defined in the frequency domain and therefore should be measured that way. Measurements in the time domain will be mentioned later. There is, though, a school of thought that useful measurements may be made with an apparatus which is adapted by one means or other to simulate the musical behaviour of the instrument. Wogram's siren drives the instrument with a source whose impedance varies during the cycle in a somewhat similar way to the lips, and Dekan uses a pulse source whose impedance is adjusted to give as good a correlation as possible with playing conditions. I am uneasy about such methods because the departures from a known physical condition are complicated and difficult to test over the necessarily wide range of conditions. A device which gives excellent results on, say, a trombone, may be rather misleading when applied to a euphonium because the very different input impedance may interact with the source device in an unexpected way.

All measurements were from the first controlled by an on-line computer. We were the first group in my Department to make measurements using an on-line computer, though it is commonplace nowadays. The reasons we used a computer were many but the principal ones were that we could be sure that measurements were made in a repeatable way, that we could handle all the very tedious calibration and correction curves easily and that the results of the measurement were permanently written away in a form where they could be further processed in ways which may not have been anticipated at the moment of collection.

The impedance was measured 1015 times during a run and each value was fully corrected for a variety of calibrations. The effects of possible distortion of the waveforms being measured were reduced by carrying out a Fourier series expansion of the pressure and velocity waveforms and retaining only the fundamental components. To view the results they could then be put through a plotting programme enabling different presentations to be selected to suit the application. The measurements were made at intervals of 1 Hz or 2 Hz, depending on the upper limit selected, 1024 or 2048 Hz; these figures were a compromise between speed of measurement and resolution. For musical purposes it is necessary to have much greater resolution (an error of 1 Hz in the location of the second peak in the curve of impedance against frequency, for example, is an error of 30 cents), but then the experiment would have taken a long time to run. At first sight, it may not appear to matter if the run time was great, but in fact, it is quite important to ensure that a run does not take too long. If one is trying to locate the frequency of a peak to an accuracy of less than, say, 3 cents, it is not too hard to see that the ambient conditions must remain constant to a similar extent for the measurement to have any meaning. The frequency of a peak is determined by a combination of geometrical factors and the velocity of sound, and to obtain the required accuracy the temperature of the air within the instrument, which affects the velocity of sound, has to be constant to within less than a quarter of a degree. We found that it was very hard in our laboratory to keep the temperature constant within those limits and therefore set out to ensure that each run took as short a time as possible. In Richard Pratt's version of heapparatus, he provided very powerful data averaging which produced excellent looking curves but meant that each run took over four hours. Steve Elliott made a significant modification to the apparatus and enabled one to make a choice between a quite quick run which would produce rather "noisy" curves when plotted and a run which took longer but then was liable to be in error because of drifts in the ambient conditions. The most often used compromise meant that runs took 14 minutes to complete.

To increase resolution we interpolated between the data points using a model of how the impedance varies in small frequency intervals. The interpolation algorithm was based on theLorentz description of the behaviour of the complex impedance near a peak. Its use was justifiable here because the frequency deviations were small and the asymmetrical shapes of the impedance peaks noted by Benade had no signicant effect. The essence of the method was that the portion of the curve near to each peak was presented in the Argand form and the data were fitted to the best circle in that representation. Usually one worked interactively with a graphical display because the computer could sometimes be upset by a noisy measurement or by an unusually shaped curve.

Two more examples of the ways the data could be manipulated are worth describing briefly because they gave us an insight into some of the complexities of the whole procedure of trying to understand the acoustics of a brass instrument. The first example shows how one could make allowances for the fact that the measurement was not made at the edge of the mouthpiece, as suggested by Benade on the grounds that that plane represented the interface between instrument and player. The nature of the source is such that the plane of the mouthpiece was not a very stable datum. The lips take up different positions depending on the note being played and the tone desired and hence we felt that a measurement should be taken at a more definable position. Our later work on tone production has confirmed the correctness of our views. This point will be demonstrated during the lecture. A mouthpiece can be represented by a combination of a series inertance and a shunt compliance or as a short transmission line of properties calculable from the internal geometry. In general, the lumped constant model is valid but it turns out that we have to be careful in certain conditions. If we use either model and do all the associated long calculations, we get a new version of the input impedance which may readily be compared with measurements made by others. When this is done, it is very gratifying to see how closely results obtained in many different countries can agree provided one makes allowance for the fact that few of the others have an absolute calibration referrable to standards.

The other is much more useful, and also relates to the mechanism of note production to be discussed later. Suffice it to say now that the source of sound represented by the lips needs to look at peaks of input impedance whose frequencies are harmonic. A way of describing this was put forward by Wogram and, in essence, meant that we looked for the condition for the maximum transfer of energy from the player to the outside of the instrument as a function of frequency, including all harmonics of the frequency. The calculations were easy and gave a useful insight into the way that played notes could vary in pitch as a function of played dynamic. Richard Pratt, though, applied the technique to that most simple instrument, the straight tube, and reached some worrying conclusions. As our paper of the time showed, the straight tube was predicted to be easy to play whereas the reverse was true in practice. This point will also be demonstrated in the lecture.

Although we did not turn to data collection in the time domain until later, both Richard and Steve used the Fourier transform to obtain a representation of the input reflection coefficient in the time domain. By looking at the data in this form, we could identify prominent features in the bore profile of an instrument, but the resolution was not very high owing to the limited bandwidth of the frequency domain data. John Goodwin and, later, Anne Duffield measured exclusively in the time domain to much higher spatial resolution and used various algorithms to reconstruct the bore profile from the acoustic data.

It is convenient now to move towards the psychology of the perception of sounds. This is a vast subject, and I shall do no more than indicate some of the experiments we did and to show how we made use of them to guide our study of the essential physics. Richard Pratt was very interested in this aspect of the work and did many little experiments to try and sort out his ideas. We reported these in a paper in 1978. They were all psychophysical and required that players and listeners made judgements of tone, responsiveness or ease of blowing under conditions which became increasingly more tightly specified as we realised just how much judgments of musical qualities could be affected by extraneous conditions. It is a matter of much pleasure to me that Boosey & Hawkes have found that some of the procedures developed then were very useful in their quality assurance programme and that they are still being used.

Another class of study which was done was the Social Survey; we did two types of survey: in the first we asked a large population of trombone players (we wrote to every trombone player in the London area of the Musicians Union) what they thought about their instruments. We sent out a circular and got over a 50% response to a range of questions about present and past instruments, mouthpieces, influences on their choices, etc. The results which come from a survey like that are a guide to the popularity of different instruments and mouthpieces and helped in our attempts to find a relation between what players think and the physical measurements we could make. In fact, I wrote a computer programme which, when provided with the input impedance, was able to make quite a fair prediction of the responsiveness and richness of tone of trombones. This work was not published because the heuristic approach I used could not be justified in a general sense. In the second we interviewed selected players in depth and analysed the interview to find out how they used language to describe instruments and the sounds they produce. What words are associated with "good" or "bad" features of instruments? what do conductors want? etc. etc. The words used by players are necessarily rather informal and it was not easy to describe the results of this work succinctly. Robert Edwards made a most ingenious approach here and unearthed the statistics of the words used by players; which words were used in association and which in opposition. In this way he built up a matrix of meanings which could be handled by the techniques of multi-dimensional scaling and thus gave us a picture of a space where the distances between the words is a representation of the difference in perceived meaning between them.

How can we begin to put together what I have talked about so far and the knowledge of musicians? This paper is following a chronological approach and therefore started with basic physics and simple psychophysics. It is now appropriate to enlarge the horizons and start to discuss how the player and the instrument interact - input impedance is but one of the aspects of an instrument; there are many others which fit together into a complex pattern. The transfer function is the ratio of the pressure at the output of the instrument to the pressure at the input and it therefore is of importance in considerations of the sound heard by the player as well as in the physics of the instrument viewed as a transmission line. Peter Watkinson made use of the fact that we could measure impedance and transfer function fairly easily. By solving the equations governing the propagation of sound in tubes, he extracted data for the loss of energy and the velocity of sound in the tube as functions of frequency. The technique has turned out to be a useful addition to the amoury of methods one has for measuring energy losses in sound propagation. With the help of Dick Shepherd, we combined these measurements with some psychophysical ones on the acceptibility of instruments having various amounts of additional attenuation and were able to establish that the apparatus was at least as sensitive as the players.

A different source of energy loss is that caused by the vibration of the walls of the tube. It was very difficult indeed to calculate how a structure like a trombone bell could vibrate until the computer-based technique known as Finite Element analysis came along. It is not necessary to describe this technique in detail since it has now become widely known. Briefly, it is a computer programme which gives us some pictures of how the structure can vibrate, and a vast amount of data describing the details of the vibrations and of the frequencies of the modes. Pete was able to show that the modes of a trombone bell are very sharply tuned and the frequencies depend critically on fine details of the structure like the precise positions of stays or the exact nature of the rim, and not too much on the material of construction. It was much more important to work out how the air column is coupled to the vibrating bell walls. We could do this because we were lucky enough to be situated in an Institution which was very well endowed with computer facilities. Peter Watkinson combined the output from the finite element programme on a machine at the Rutherford and Appleton Laboratory with data collected with the aid of our NOVA on the distribution of pressure inside the instrument as a function of frequency. He then carried out all the necessary numerical integrations on a University PRIME and calculated the coupling between the structure and the air column. As well as working out the coupling between the internal air column and the vibrating bell of the instrument, we could in principle go on to work out the radiation to the outside world from the bell walls. This would be a fairly difficult calculation but I have not been able to do it because it has not been possible to persuade grant giving bodies that the work is not only of interest to musical acoustics but would also advance understanding of sound radiation from complex structures. The external radiation affects the sound heard by the player, particularly if he is a trombone player, and therefore has an effect on his perception of the quality of sound radiated by the instrument, as mentioned again later.

The radiation from the instrument walls is much less than the radiation from the end of the air column, but surprisingly few people have studied the latter in detail. There is some work in the literature from 40 years ago in America and there are occasional papers from East Europe, but by far the major contributor to this aspect has been the PTB in Braunschweig under the direction of Meyer. But even they have not done the very detailed analyses of the wavefronts which are necessary for a complete description of the near-field and its effects on the perception by the player and on the terminating conditions of the standing waves inside the instrument. Instead they concentrated on the more general aspects of the far-field radiation. Unfortunately, it is not possible to make deductions of the radiation impedance terminating the instrument and of the near-field radiation from such results and one of the things it would have been satisfying to have completed at Surrey is a detailed examination of the near-field radiation of brass instruments.

Towards an understanding of how brass instruments work

I have already mentioned John Goodwin in the context of time domain measurements, he designed a spark source and some signal averaging techniques which enabled him to locate the positions of quite small features inside an instrument. His work has been continued and much developed by Anne Duffield and her paper in this meeting is on the meticulous care one needs to take when attempting to construct an apparatus which will help the production staff in the factory compare a newly finished instrument with its prototype.

It is now appropriate to consider some of the details of the mechanism by which notes are produced: how does a steady stream of air from the lungs become changed into a musical note? It is probably wise to admit that, although people have worked on this for about a century, there is still no clear answer. We, at Surrey, felt that we were on the right lines, but this is another problem remaining unsettled. Helmholtz is generally recognised as the father of this area of study and his ideas have been the foundation of most of the more recent approaches. For example, Webster's simple model from 1919 includes in essence the ideas of Helmholtz from 1895, In it the note is produced by the action of a variable valve which adjusts the flow rate of the air passing into the instrument from a reservoir. The assumptions made in all theories were that the pressure in the reservoir was much higher than that in the instrument and remained constantduring the flow cycle. Thus the flow would always be into the instrument and would consist of a fairly large steady flow with a small alternating component.

Surprisingly, it appears that no one actually made careful measurements of the pressures in the mouthpiece and in the mouth of a player until Steve Elliott did a few years ago. It was very striking that the assumptions of constant and high mouth pressure were not met. One thing which we then predicted was that since the mouthpiece pressure sometimes exceeded the mouth pressure, perhaps the flow might reverse. We found that it did - this important result was entirely unanticipated and Steve was able to build up a much more valid model of behaviour making use of this observation and others reminding us that the relationship between the flow and the lip opening was not linear and hence the characteristic waveform changes as the frequency of the notes changes; the detailed analysis showed that the waveform depended on the relative values of the "average" resistance of the orifice and input resistance of the instrument. It was illuminating to look at these results in the frequency domain. Ordinary Fourier transforms were not very useful here and we used a pitch synchronous analysis written by Dick Bacon and based on the ideas of Risset when he was in the Bell Telephone Laboratories. Most of the models of lip behaviour predict that the oscillation should take place on the sharp side of the impedance peak for an outward beating reed. All players knew that this was nonsense - oscillation could take place on either side of the peak, and it was in fact easier to go to the flat side than to the sharp side. As an example, studies of the waveforms experienced when a player did a downward glissando on a fixed instrument revealed a complete confirmation of Steve's model and also indicated how well Bouasse's ideas from 1930 fitted the generation of "privileged" notes on brass instruments. These points will be demonstrated in the lecture.

Since we had a hot wire anemometer attachedto our apparatus, Steve was able to measure p and u simultaneously and confirm that the source resistance did indeed have the predicted effect and also that the flow reversed during each cycle. A snag was that the anemometer could not measure particle velocity because it actually measures speed. Thus we had to interpret the waveforms rather carefully. Apparently fluid dynamicists are used to doing this all the time, but we were a little taken aback at first. Theory demands a simple model on which to work and Steve Elliott's was indeed simple and based on a variant of Flanagan's model of speech production. I am constantly reminding people that speech production and note production in brass instruments are closely analogous processes.

The easiest way to apprehend the general conclusions was to present the physical situation around the lips of a brass player in terms of an electrical circuit diagram and our 1982 paper did just that. Many conclusions can be drawn from this work and many avenues of further research were opened, however only a few comparisons could be made for lack of data. As an example, though, when we predicted the variation of vibrating mass with frequency we got results which compared well with those from a short paper by Hoza in Czechoslovakia. The agreement was very good; perhaps better than one should really expect, but nevertheless most of the predictions of the theory remain to be tested thoroughly.

Inspired by Steve's ideas, I constructed a more general model of the control structure governing the player/instrument interface. This structure consists of a nest of interlocking servo loops, and recent experiments have been directed to examining some of its specific features. I have discussed these ideas in Jablonna and Paris and in my view this approach is the one most likely to yield significant progress in the future. To begin to test the ideas, we performed one of the classic experiments one does with a feedback circuit: the loop was opened and what happened when we upset the response of that branch was studied. The loop worked on was that from the bell of the instrument back to the player's ear and we interrupted it by giving the player some special headphones with insert microphones. By changing the frequency response of the amplifier joining the insert microphones to the headphones, we changed the sound quality of the instrument as it appeared to the player without changing the instrument itself. By changing the time delay in the loop, we could change the auditory size of the room without changing the apparent size, but this experiment is still in the future. So far, these experiments are in their preliminary stages; there were many problems associated with the rather poor response of the headset, but these were overcome and the real work will be described by Peter Simpson in his paper.

I hope that this survey has been of use by showing the way that we tackled what turned out to be a very complex problem. We were well on the way to understanding what goes on when a note is produced on a brass instrument, but there are still many factors to be explored by those interested and more lucky than I in being able to raise the necessary finance. A prime factor has emerged: all those interlocking servo loops within the control structure so obscure any simple ideas one might have about the relationship between any physical aspect of the instrument and a player's or listener's opinion that I think that it is clear why one cannot answer the question about "good" or "bad" easily. Obviously, this begins to explain why one player's "good" instrument is another's "bad", and why musical acoustics is such a rich and fascinating field of study.

## Bibliography

There has been a considerable number of papers published about this work by the author and his colleagues and rather than give references in the body of the text, I have chosen to append a selection of papers from the last few years as illustration.

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