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Comments on "On the measurement of glottal flow" [J. Acoust. Soc. Am. 84, 888–900 (1988)]

Richard S. McGowan
Haskins Laboratories, 270 Crown Street, New Haven, Connecticut 06511

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This letter indicates the possibility of rotational air motion in regions where rotational motion is not produced. This has application to the region above the glottis, where static models have shown the existence of rotational motion.

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There is a misunderstanding of the role of viscosity and of the persistence of rotational motion in the paper "On the measurement of glottal flow" (Cranen and Boves, 1988). The authors argue that there is no rotational air motion at a significant distance from the walls of a tube of radius 1 cm if the frequency of oscillation is more than, say, 10 Hz. The authors relegate the rotational motion to the Stokes boundary layer, which decreases in thickness as the square root of frequency. From this they conclude that "...only the low-frequency (subsonic) components of airflow during speech production that are viscosity controlled can be expected to produce a rotational flow field" (Cranen and Boves, 1988). In context, they are referring to the existence of a rotational flow field outside the Stokes boundary layer.

The existence or nonexistence of rotational flow in a region that is essentially inviscid can only be deduced on a case-by-case basis. While rotational motion cannot originate in an essentially inviscid region, it can reside there. If rotational motion does exist in an inviscid region, it must have been produced in a region where there were high shear stresses in the air caused by viscosity and then transported into the inviscid region. In the case of the straight tube discussed by the authors, the fact that there is no production of rotational motion outside the Stokes boundary layer is equivalent to the fact that there exists no rotational motion outside the Stokes boundary layer (because the only transport mechanism here is diffusion, which is very slow). The possibility for rotational motion in the essentially inviscid flow regions must be left open in other situations when there is transport of rotational motion.

The authors appear to recognize the possibility of one of these transport mechanisms—that of boundary-layer separation. However, they relegate its importance to low frequencies, again because production and existence of rotational motion have been equated. This equation breaks down in the presence of such a transport mechanism, and the higher frequency rotational motion can be transported into the inviscid region once it is produced in regions of high shear. Also, there is some mention of aerodynamic sources of sound in this context, which is not directly relevant to the issue of measuring the velocity of a flow field, because acoustic and nonacoustic air motions are not measured separately.

In the particular case of the vocal tract, we can expect an unsteady jet to form because of the abrupt area change in the case of phonation. Not only does the jet separate from the vocal folds, thus transporting rotational motion into the vocal tract, but there is a large shear between the newly entered air with the slowly moving or still air of the vocal tract. This region of large shear is also a region where we can expect rotational motion to be produced. This rotational motion can be considered to be embedded in the surrounding inviscid region. (In certain approximations, the vortices associated with the rotational motion are said to be convected by the irrotational flow.)

There is an argument using the momentum and mass conservation equations that shows the existence of rotational motion at abrupt area changes in the case of steady motion (Batchelor, 1970). Also, van den Berg *et al.* (1957) showed rotational motion downstream of the exit of their model glottis indirectly by recording the nonlinear resistance to flow. These findings can be used in the case of phonation if the quasisteady approximation is made. Even if this is considered to be a poor approximation, the possibility of rotational motion in the vocal tract with frequencies above 10 Hz should be admitted.

Equation (3) in the paper is in error if it is considered to be a scalar equation because the curl of a scalar is not defined. Rather, this equation must be taken as a vector equation, and the reason the curl is zero is because the curl of a gradient is zero. Given these changes, it would appear that oscillating rotational motion could not persist in the inviscid region. However, it must be realized that the authors start with the linearized version of the momentum conservation equation in the axial direction [Eq. (1a) in the paper]. Again, the approximations made in linearizing the equations of motion can only be made on a case-by-case basis. The linearization is valid only if the quantities neglected are small compared to the quantities remaining. The approximations must be made explicit, or it is likely that what is assumed will appear as a conclusion. In the case of the vocal tract, there is no *a priori* reason for linearizing the equations of motion. One could study model problems such as small oscillation about a mean flow, but upon linearization there would appear a convective term, making the identity expressed in Eq. (3) invalid.

The above considerations indicate that the relationship between the pressure and velocity cannot be assumed to be an acoustic one. However, it may be possible to subtract out the effects of rotational motion in the same way that the authors subtract out other effects, such as viscous loss and wall vibration. Just as the impedance of the walls is unknown, so is the equivalent resistance due to the presence of rotational motion. The authors hypothesize that the glottal flow derived from the tracheal pressure differences may be more reliable than those derived from the pharyngeal pressure differences. They attribute the discrepancies seen between the two measurements of the glottal flow in the vowels /u/ and /i/ to the finite wall impedance in the pharynx. There is the other possibility that the rotational motion, especially above the glottis, also contributes to these discrepancies. Perhaps there is little rotational motion in the trachea before the glottis, so tracheal measures are, again, more truly indicative of glottal flow. These speculations are interesting because the supraglottal flow in the pharynx for /i/ and /u/ would be expected to be different from that for /a/ because of the differences in tongue position in the pharyngeal region. The authors do not see much discrepancy between the tracheal and pharyngeal measures of the glottal flow in the case of /a/.

In conclusion, there is no reason to suppose the nonexistence of rotational motion in the vocal tract when the distinction between the production and the existence of rotational motion is made. To avoid hasty conclusions, care must be taken in using approximations to the laws of motion. The results derived from the experimental method of Cranen and Boves are seen in a slightly different light when the impedance due to rotational motion is taken into account. It would be an interesting experimental project to determine the flow field in the vocal tract or vocal-tract-like models that are time variable. The existence of rotational motion is well established in static models, but the effect of the inertial term in the momentum equation has yet to be determined.

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