impossible C shape to the UVT master. This curve is called "C's used for F. & P. tube calibrations."

This could be done because the entire calibration of the 36" F. & P. tube lay in extrapolated \(R_D\) range. The 30", 0.6058 beta F. & P. tube calibration, however, reached down to \(R_D\)'s near to the range of the direct weight tank calibration (for the ARL Master) and this calibration revealed a difference between new and old lab calibration that seems to have generated the discussion.

As soon as the curve "C's used for F. & P. tube calibrations" (Fig. C1) came to author's knowledge, he knew that its shape could not be right. He could be sure because theory and the "thousands of data" he condensed in the paper gave reliability to his knowledge about the UVT C. Consequently, on 10/9/73 the ARL Master was recalibrated by using a 0.4833 beta, 36" UVT previously calibrated in the new lab (and was found to have a 0.981 coefficient) by using two large bore water manometers which were read simultaneously by four men and by collecting 30 manometer readings for each test point. The results verified the weight tank data indicating no slope in the C of the ARL Master from 0.4 to 2 x 10^6 \(R_D\).

1.5 Discusser's Formula.

\[
C = 0.9739 + \frac{6.55}{\sqrt{R_D}} \quad (D4)
\]

This formula is plotted in Fig. 1(c) in the discussion where discusser shows the calibration data taken on a 6", 0.6 beta UVT in ARL's new lab. The points are copied from Fig. 15 in the paper and essentially substantiate the validity of equation (D4).

The trouble here again lies in the facts which would have become obvious if the 22 calibration data points shown in the paper on the same Fig. 15 at lower \(R_D\)'s had not been discarded by Mr. Head when preparing his Fig. 1(c). Why he did this is hard to understand because the very discarded data clearly disproves his theory showing no change in C as the installation was changed; namely, when the tube was transferred from the new to the old lab (to obtain accurate calibration points at low \(R_D\)'s.)

1.6 Discusser's Fig. 1(d) shows the calibration data of a 0.724 beta, 6" UVT as obtained in the F. & P. oil and water weight calibrating facility. Since the C "shape" seemed to be strange, author called Mr. Head to inquire about the conditions under which the calibration was obtained and the equipment used at the work to evaluate the accuracy of each calibration point. Unfortunately, Mr. Head informed author that the data is confidential. Consequently, the only means left to clarify this matter was to repeat the tests in BIF Hydraulic Lab using a standard cast iron 12" x 6" reducer, a 6", 0.7138 beta UVT and calibrate it at 1.1, 3.5, 23.1 and 53.4 pipe diameter distances after the reducer. The data are shown in Fig. (C2) and disproves Mr. Head's theory on all counts: First—because the C is constant for the full \(R_D\) range of the calibration having no hump as shown by the discusser. Second—the value of C does not increase with increasing distance, once the effect of the decreaser was eliminated by straight pipe length.

1.7 Discusser's Conclusions. The following conclusions are quoted from the discussion: "It appeared from discusser's Figs. 2(b) and (c) that, like all other nozzles and flow tubes, Halmi's invention necessarily has coefficients that "float" when the installation is changed."

"Thus, the admonitions in Halmi's introduction are applicable to all nozzles and flow tubes, including the UVT flow tube."

The facts:
- The term "floating" has not been used in the paper. Discusser uses this term in connection with "floating with installation."
- This usage is new to author who spoke—in the introduction of the paper—about change in C with \(R_D\) when the installation is not changed.

While facts seem to contradict discusser everywhere, the puzzling ultimate contradiction of his theories comes from himself. While he believes that all "good" tubes "float" their C's when installation is changed, he does not accept this "normal" floating for the F. & P. tubes, the installations of which indeed were changed. Rather by making the F. & P. tube C's "unfloating" with installation, he assigns C's to the ARL Master making them float with \(R_D\) as shown on Fig. 1(b).

In this process discusser does not pay attention to the facts; that—first, the installation of the ARL Master was not changed throughout these calibrations; second, ARL used C's that floated with \(R_D\) (Fig. C-1) in a different way than Mr. Head's theory indicates; third, at lower \(R_D\)'s the ARL Master was calibrated in place against a weight tank which calibration sharply contradicts the C "shape" assigned to the ARL Master by the discusser.

---

An Experimental Investigation of Transition of Plane Poiseuille Flow

P. FREYMUTH. As an overall response I would like to say that the authors show convincing experimental evidence that the theoretical stability limit for Plane Poiseuille Flow is approached in an orderly fashion if the background fluctuations in the experimental facility are lowered. In addition, I have some questions for the authors.

1. Does experimental evidence reported so far in the literature fit into Fig. 37?
2. Are the natural background fluctuations in the facility of the incompressible turbulence type, or are they mainly sound waves?
3. Is there no way of lowering the background fluctuations of the facility further? Wind tunnels with quadratic cross-sections often exhibit a fluctuation level by an order of magnitude smaller than reported for the facility.

I. WYGNANSKI The major portion of this paper is devoted to observations of hot wire traces in transitional flow between parallel plates. Throughout the paper the authors seem to be obsessed by a critical Reynolds number of 7700 and try to explain what they see in relation to this number.

Natural transition from laminar to turbulent flow is often characterized by a region in which the flow is intermittently turbulent or laminar. The extent of the intermittent region may vary from one flow to another but it seems to be a universal phenomenon accompanying transition. Turbulent "spots" occurring in boundary layer transition increase the skin friction locally but their occurrence does not influence the velocity outside the boundary layer. Since the momentum and displacement thickness both increase in the interior of the turbulent spot the local \(R_D\) or \(R_T\) increase as well, nevertheless transition is intermittent. Elder observed that turbulent spots can occur over a wide range of Reynolds numbers.

Slugs in a pipe flow occur at various Reynolds numbers de-

2. Department of Aerospace Engineering Sciences, University of Colorado, Boulder, Colo.
pendent on the disturbance level in the entrance region. The experimental apparatus was so designed that the increase in the friction factor had practically no effect on the mass flow in the pipe. This may be deduced from the enclosed oscillogram (Fig. 1) showing that the velocity just before the leading edge of the slug is identical to the velocity after its trailing edge. Since the measurements were made at the exit of the pipe, the velocity just prior to the arrival of the slug was measured while most of the pipe flow was turbulent. After the slug left the pipe the entire pipe contained laminar fluid, nevertheless the oscillogram indicates identical velocities. I thus find it difficult to believe that there is a magic Reynolds number below which the transition process ceases.

It is unfortunate that in many figures shown the authors used capacitors to block the mean velocity, because the shape of the traces depend critically on the capacitor used. I would much rather see the DC voltage subtracted in another way.

I would still like to comment about the spikes shown in Fig. 11. This is a very peculiar phenomenon which is observed when the mass flow in the channel changes with skin friction. We have observed similar spikes in initial stages of the investigation before we managed to control the constancy of the mass flow (see Fig.

2. Consequently it is important to control the constancy of the mass flow in order to reduce extraneous effects.

Some of the qualitative conclusions drawn by the authors indicate that the transition process in the plane Poiseuille flow are similar to the axisymmetric case.

Author's Closure

Response to Professor Wygnanski:

The initial motivation of this experimental work was to observe instability as near the limit predicted by linear theory of 7700 as possible. Since several previous investigators attempted this and failed and since much of the theoretical stability work involves this linear prediction of 7700 our obsession appears justified.

Response to Professor Freymuth:

In response to the questions:

1. No experimental evidence exists except at the low Reynolds numbers corresponding to high fluctuation levels.

2. The fluctuations entering the channel are of both incompressible, turbulence type and low intensity sound waves. The low intensity sound waves (normal conversation) can result in transition. Experiments were conducted at night when the background sound level was low.

3. Additional efforts have resulted in a reduced fluctuation level of 0.12 percent with an associated critical Reynolds number of 7500. This activity is reported in a paper submitted by the Journal of Fluids Engineering and is presently undergoing review.

Design and Construction of a Wind Tunnel for Mass Transfer Studies in Incompressible Boundary Layers

D. M. BUSHNELL

Paper contains a useful, critical survey of experimental problems encountered in previous low speed mass transfer experiments. Of particular importance are the statements concerning the sensitivity of \( C_f \) to variations in plate permeability. Based on this discussion in the paper the ± 2 percent permeability variation quoted for the University of Waterloo apparatus, although better than corresponding values for previous experimental setups, may still yield considerable uncertainties in local \( C_f \).

While the first paragraph states that one of the major purposes of the paper is to report reference data for prediction models and methods, the data provided consist of integral quantities only. No detailed profiles are supplied with which to start, or check, "mean field" or "mean turbulence field" models. Also, one would hope that fairly complete hot-wire data will eventually become available for the flows examined in this investigation, where considerable care has been taken to ensure valid measurements. Such data, including Reynolds stress profiles, would also aid in the evaluation of \( C_f \) accuracy.

As a final comment, the Re range for these data seems somewhat low for a "fully developed" condition to exist.


\*Bushnell, D. M. Paper contains a useful, critical survey of experimental problems encountered in previous low speed mass transfer experiments. Of particular importance are the statements concerning the sensitivity of \( C_f \) to variations in plate permeability. Based on this discussion in the paper the ± 2 percent permeability variation quoted for the University of Waterloo apparatus, although better than corresponding values for previous experimental setups, may still yield considerable uncertainties in local \( C_f \).

While the first paragraph states that one of the major purposes of the paper is to report reference data for prediction models and methods, the data provided consist of integral quantities only. No detailed profiles are supplied with which to start, or check, "mean field" or "mean turbulence field" models. Also, one would hope that fairly complete hot-wire data will eventually become available for the flows examined in this investigation, where considerable care has been taken to ensure valid measurements. Such data, including Reynolds stress profiles, would also aid in the evaluation of \( C_f \) accuracy.

As a final comment, the Re range for these data seems somewhat low for a "fully developed" condition to exist.
