$0 < t_1 < 2\pi$. $K \to \infty$ corresponds to the case where $X(t_1)$ is specified.

The Riccati equation for this revised problem is $\dot{W} = W^2 + \frac{1}{4}$, $W(t_1,t_1) = K$, so that $W(\tau,t_1) = -\frac{1}{2}\tan[-\tan^{-1}(2K) + y]$. Thus $W(0,t_1)$ is finite if $0 < t_1 < \pi + 2\tan^{-1}(2K)$. For $K = 0$ this gives $0 < t_1 < \pi$; for $K \to \infty$, $0 < t_1 < 2\pi$.

Comment on "A Combined Visual and Hot-Wire Anemometer Investigation of Boundary-Layer Transition"

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Knapp and Roache[1] discuss the streakline pattern of the disturbed boundary layer on ogive nose cylinders. With regard to the rolling-up process of the streak-lines in their transition region R_1, they referred to the paper of Hama[2] who calculated the streakline pattern of a free shear layer perturbed by the neutral disturbance due to the linearized stability theory. Hama found that the streaklines roll up, as if to indicate that the flow develops into vortices. Hama assumed that in the perturbed flow there was no vorticity concentration, and it followed that the rolling-up of streaklines cannot constitute a positive identification of the presence of discrete vortices, and Knapp and Roache[1] argued similarly as well.

This statement of Hama, however, was incorrect, since in the perturbed flow used by him, local concentrations of vorticity existed which essentially corresponded to a one-row vortex street configuration as shown in Refs. 3–5. For amplified disturbances, which were investigated by Knapp and Roache, the rolling-up process of streaklines has been calculated in Refs. 5 and 6. The agreement of the theoretical results with the observed smoke pattern in experiments[7] was good. Furthermore, it was found in Refs. 5 and 6 that the rolling-up process of the streaklines corresponds to a local concentration of vorticity.

I therefore suppose that the streakline pattern observed by Knapp and Roache in their region R_1 will, in fact, indicate a concentration of vorticity, since the streaklines look similar to those observed in free shear layers.[7] Another question, however, is whether these concentrations of vorticity can be characterized as "discrete vortices." I think that this is a question of definition. Surely, these local concentrations of vorticity are not of the type found in a potential vortex or in a Hamel-Oseen vortex. But I should prefer to denote an essential local concentration of vorticity as a discrete vortex, since the effect of a local concentration of vorticity is similar to that of a discrete vortex due to the induction law.

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Reply by Author to A. Michalke

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We genuinely appreciate Michalke's Comment[1] and this opportunity to discuss a point that has clouded the literature on boundary-layer transition for years. A long-standing semantic confusion exists between Michalke[1,2] and Hama[3,4] over the imprecise use of the words "vorticity maximum" and "vorticity concentration." Both use the word "concentration" with what they want to characterize as a discrete vortex. But Michalke[2,5] has used the words "maximum," "extremum," "peak," and "concentration" interchangeably, whereas Hama[3,4] clearly has used "maximum" and "concentration" with different meanings. Hama[3] certainly did not "pretend"[1] that there was no vorticity concentration; he plotted out both the mean and the fluctuating vorticity distributions. Also, he says "... the flow ... does not explicitly exhibit singularities. The mean vorticity ... as well as the fluctuating vorticity ... are both broadly distributed in y direction ... and the latter fluctuates sinusoidally; there is [sic] no higher harmonics in the fluctuations or no vorticity concentrations in this flow." Hama uses "concentration" and "discrete vortex" only in the sense of a singularity, at least in Ref. 3. But Michalke[5] quite successfully interprets free shear layer instability in terms of vortex induction, and so refers to the smooth vorticity extrema as "concentrations."

This leads to the problem of definition of a "discrete vortex," the problem being that no singularity can exist in viscous flow; and by Hama's criteria in Ref. 3, no discrete vortex can exist. This is clearly an inadequate definition, as recognized by all parties, including Hama.[4] Thus exists the present argument (also treated by Hama and Nutant,[4] Klebanoff, Tidstrom, and Sargent,[6] and Kovasznay, Komoda, and Vasudeva[7]) over whether the discrete vortex is formed before or after the three-dimensional deformation of the R_1 wave front.

Consider other definitions. One textbook[8] simply states that "a region containing vorticity is called a vortex." (Thus, the stable laminar boundary layer itself is a vortex.) But it seems that everyone involved envisions "vortex" to require some closed streamlines and some sort of vorticity maximum. Now the so-called Tollmien-Schlichting waves exhibit these characteristics (see Fig. 16.14 of Schlichting[9]), but in customary usage they have not been called vortices. Somehow, the vorticity has not been concentrated enough to warrant the characterization as a discrete vortex. And although vague agreement has been reached among different investigators on the distinction, the fact is that no one has ever quantified his criterion.

There has been another criterion widely used by experimentalists[6,7] to distinguish wavy disturbances of the Toll-

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mien-Schlichting type from vortices, and that is a phase-reversal of the longitudinal velocity fluctuation across the critical layer for the vortex. But Hama[3] does not consider it to be sufficient, nor even necessary. Also, Michalke's calculations[2] of wavy disturbances in a free shear layer show that the existence of a phase reversal at the critical layer depends on whether the wave is neutral or amplifying, and whether its amplification is temporal or spatial. Further, even for the Tollmien-Schlichting wave in a Blasius profile, the phase reversal criterion is vague. In application, experimentalists have more or less implicitly looked for a phase reversal about the critical layer as the wave passed directly over a hot-wire location. But streamline calculations of a Tollmien-Schlichting wave (Fig. 16.14 of Ref. 9) actually do show a phase reversal about the critical layer, for the velocity profile at $\frac{1}{4}$ wavelength ahead of the wave center. The extent of this phase change will depend on the eigenfunction solution, and is thus dependent on frequency, Reynolds number and the shear-layer profile. Thus, the phase-reversal criterion also appears inadequate.

As for Michalke's characterization of a vorticity extrema as a vortex, consider Uchida's solution[9] for oscillating pipe flow. Here, maxima of vorticity appear periodically, yet all streamlines are at all times identically straight and parallel. It therefore appears that Michalke's criterion is also inadequate.

Granted the lack of a completely adequate objective distinction between a wave and a vortex, the fact remains that various investigators do distinguish subjectively. We will now hypothesize some kind of consensus about particular cases and consider the applicability of Michalke's comments[1] to our work.[10]

First, with regard to the identification of a discrete vortex by the roll-up of a smoke line, we note that Hama[3] repeated his calculations for a u component of velocity identically equal to zero, and still obtained the characteristic roll-up of the smoke lines. There is no phase reversal of u in this case, only a fluctuating v component. It is clear that a Tollmien-Schlichting wave would also cause the smoke lines to roll up, and, as we have said, customary usage has been to not call these waves vortices.

But suppose we now adopt Michalke's view of a maximum in vorticity being legitimately characterized as a discrete vortex. Then Michalke has argued[2] that since a (smoke) streakline is approximately a line of constant vorticity, that a smoke line roll-up constitutes a positive identification of a vorticity maximum (or concentration, in Michalke's sense). But streaklines are constant vorticity lines only in inviscid flow. In some regions of even high Reynolds number flows, significant viscous production of vorticity may occur. That Michalke's approximation is not entirely valid for a wall boundary layer may be easily demonstrated. If vorticity production is negligible, then the maximum vorticity in the waves can nowhere exceed the maximum vorticity in the undisturbed shear layer[2]; but Kovasznay et al.[7] have experimentally measured maximum vorticity equal to twice the maximum in the undisturbed Blasius profile. It is clear then that our original claim[10] stands unaltered; the roll-up of a streakline cannot constitute a positive identification of discrete vortices, and the interpretation of region R_1 is not possible from the smoke technique alone.

Finally, we consider another hypothetical consensus. Suppose that it is agreed that R_1 is a region of Tollmien-Schlichting waves, which all parties agree not to call vortices. Suppose also that all parties consider the characteristics of Michalke's spatially growing disturbances in a free shear layer,[2] noting the phase reversal and the vorticity extrema, and agree with Michalke that these may be legitimately characterized as discrete vortices. What is the relation to our work?[10] Michalke's discrete vortices develop in a free shear-layer *inflexional* profile, not a Blasius-type profile. It is well known[6,7,4] that such an instantaneous inflexional profile does develop in region R_2, and that this instantaneous inflexional profile develops a *secondary* instability, which ultimately cascades[4] into region R_3—a region that we and all other investigators have characterized as *discrete vortices*.

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Correction to Tsien's Listing of Ideal Gas Flow

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In his article[1] on gasdynamics, Tsien states that there are eight consistent types of frictionless, adiabatic flows for an ideal gas in the absence of body forces, and he tabulates the flows. However, careful examination shows that two of the flows have identical classifications (4 and 8 in Tsien's table). This raises the question of whether there is an error in the table, or whether there are, in fact, only seven consistent flows. A correct listing of fluid motion of the type being considered here is not available in the literature.

By using the energy equation with stagnation enthalpy as an independent variable, Crocco's theorem in both the steady and nonsteady form, and Eqs. (7–21) from Tsien's article, one can establish criteria for flow consistency. There are 16 possible adiabatic, frictionless flows with no body forces, nine of which are inconsistent with the equations of motion. In fact, only seven classifications are consistent with the equations of motion, not eight. Flow number 8 in Tsien's table should be deleted.

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