The classical theory of flight is one the most beautiful and subtle achievements of applied mathematics. However, it is no longer of interest to mathematicians, because they know that it has been a solved problem for many decades (although they have forgotten the details). Obviously, it remains of interest to engineers on account of its predictive ability, but it can be employed very successfully without knowledge of the subtleties. Aerodynamics today is therefore almost always taught in a truncated version that retains all of the utility, but has lost much of the profundity. Even the truncated version is no longer as highly respected as it used to be, because Computational Fluid Dynamics delivers, with no requirement for deep thought, most of the practical answers that are needed. In consequence, there are many employed today in the aerospace industry, and even in academia, whose grasp of the basic theory of flight contains many gaps. These gaps are apparent to thoughtful students, who frequently attempt to fill them in for themselves, although the remedy is usually worse than the disease. I believe that the authors of the paper under review would have no quarrel with the orthodox theory if they knew all of the details, although they are right to quarrel with the truncated version that they, like others, have apparently received.

The authors' citation from Hoffman reveals the unfortunate "mathphobia" that many critics display. Understanding flight requires intuiting the behavior of an intangible medium for which our evolution has provided no apt language; it is hardly surprising that an exact understanding requires the use of abstract thought, but the gap is not unbridgeable. The response from the New York Times is merely irresponsible journalism, but undoubtedly an air of mystery does pervade flight, and the attempt to dispel it by simplified accounts does as much harm as good. The present authors may be innocent of mathphobia; nevertheless they unfortunately feed the flames of irresponsible journalism. All of the criticisms that comprise Section I of their paper can be answered, and I will try to do this below.

The authors experience great difficulty with the relationship between potential flow and real flow. This is not surprising because it is glossed over in the great majority of contemporary texts. There is a mathematical subtlety involved because the flow of a fluid at infinite Reynolds number (zero viscosity) is not always the same as the limit at very small viscosity (it is a singular perturbation problem), and so the question is what light can be shed by the former on the latter?

Let us deal first with the issue of how circulation arises. Circulation is simply the integral of vorticity, so first we need to ask how voticity arises. The authors have written down the compressible Navier-Stokes equations, and in suitable textbooks they will find, derived from these, the vorticity transport equation. There is only one term in this equation that accounts for the creation of vorticity, and that applies only to compressible flows. There is no way to create vorticity within a viscous incompressible fluid. Vorticity can be created only at a solid boundary. It travels into the interior solely by diffusion, but once there, it can be transported, stretched, and compressed. So the circulation required for Joukowski theory is an integral of the vorticity contained in the boundary layer. In the limit of vanishingly small viscosity, the boundary layer has no thickness, but is still present as an infinitesimal layer of infinite vorticity and hence making a finite contribution to circulation. The flow outside of the boundary layer is not "potential flow modified by circulation". It IS potential flow because its vorticity is zero (and the circulation around any contour that does NOT enclose the airfoil is therefore zero) but it obeys boundary conditions that allow for circulation around the airfoil. The citation from [20] is absolutely correct. The above is Prandtl's brilliant insight, which explains what Kutta and Joukowsky could only hypothesize. It is of course no criticism whatsoever of any scientific theory that its insights were arrived at gradually.

Many people have difficulty understanding how the apparently local process of vorticity generation can give rise to circulation "at infinity". There is a tendency to suppose that the vorticity must be spread by viscosity, which does not seem plausible, and is indeed too slow, by many orders of magnitude. But by definition the circulations around any two contours, both of which surround the airfoil and are therefore separated only by irrotational flow, must be identical. Again, this strikes people as physically implausible. But what happens is that the circulation at infinity is set up by acoustic waves, and, if the flow really were incompressible, these travel infinitely fast. What acoustic waves cannot do is create vorticity.

It still remains to be explained why that particular value of circulation that forces separation to the trailing edge is observed (the Kutta condition). Again, this is glossed over in contemporary texts, but has nothing to do with any instability of potential flow. It is due to instability of the boundary layer (which in real flows is present at any Reynolds number). Suppose the trailing edge T is sharp, and suppose that the rear stagnation point S is somewhere else. The static pressure is maximum at S (by Bernoulli's Theorem) and so the flow from T to S will be against an adverse pressure gradient. This is now a problem in boundary layer theory, which tells us that the boundary layer is probably unstable. There is no absolute certainty involved, because details may be important ---such as the actual radius of the trailing edge, the structural rigidity, and very importantly the Reynolds number. At the extremely low Reynolds numbers that characterize microbial swimming, the boundary layer is extremely thick and quite stable. At Reynolds numbers that characterize the flight of birds and aircraft, there can be small effects of the radius, as the authors notice. In engineering and in nature, the radius is always made as small as practical, otherwise the stall behavior may be impaired.

No Kutta condition applies at the leading edge L, because the flow from a forward separation point S to L involves a favorable pressure gradient. Leading edges are usually rounded because (a) there is no need to make them sharp, and (b) the flow from L in the direction opposite to S is now in an adverse pressure gradient that needs to be kept small.

The authors greatly underestimate the classical theory, most likely because the usual truncated exposition has not shown it to them in its proper light. If they take time to realize how its parts fit together, they will come to see that is a masterpiece of physical modeling.

Section II describes the computer code that is their basis for disputing the classical theory. This is their area of expertise, and it may be assumed that their description is accurate. However, they state that "real flow may thus stay close to potential flow before separation" <u>Most emphatically this is not true</u>. The real flow (by which they mean their computed flow) always contains a boundary layer whose influence is not negligible at any Reynolds number. This is characteristic of singular perturbation problems, and is the reason why Prandtl's insight was transformative to the theory of flight.

Section III gives the authors' "intuitive" version of their account. Taking their numbered points in order,

1. This is true, although its implications need to be based on a sound understanding of what is, and what is not, potential flow.
2. The authors do not give a mechanism by means of which "separation" would avoid "the building up of pressure". Do they have in mind the separation of a finite boundary layer? In that case, whatever the mechanism, the effect surely involves its thickness, so how do they explain the almost total independence of lift coefficients to several orders of magnitude in Reynolds number? (This is different from the scale invariance of the inviscid flow). If they are thinking of some idealized infinitesimal layer, what do they mean by separation? And by what mechanism is its influence conveyed?
3. This merely states a standard definition, and suggests no consequences.
4. I do not understand this sentence. Would not suction from above and push from below cause UPwash? The truth is that the suction, the push, and the downwash (together with upwash ahead of the wing), are all consequences of circulation. This is because that is how Laplace's equation behaves. It is the inevitable consequence of acoustic disturbances having come into equilibrium.
5. I will leave this to the point where it is developed in more detail.

Section IV states that sharp trailing edges are not necessary. This will not come as a surprise to any practicing aerodynamicist. As CFD practitioners, the authors are familiar with the NACA 0012 airfoil that they employ as a test case, and will know that it represents a standard thickness distribution, empirically derived and algebraically described. If they evaluate the formula for this thickness distribution at x=c, taking the formula from an original source, they will find that the thickness there is (I think I remember) about 0.5%, in acknowledgement of practicality. CFD calculations have often been made with the erroneous value of zero. As described earlier, the desirability of the sharp edge lies in forcing the boundary layer to negotiate an adverse pressure gradient before it could reach any other stagnation point. It is not necessary for the trailing edge to be absolutely sharp to achieve this aim. But the shaper the edge is, the more certain the effect, and the more likely to remain effective at high angles of attack.

Section V invokes scale invariance to explain how, within their theory, the lift and drag would be independent of trailing edge radius.

Section VI criticizes the classical solution on these grounds.

1. *There is no mechanism for generating large-scale circulation*. Indeed there is; this was discussed earlier.
2. *The high pressure predicted at the trailing edge is not seen in experiments or computations*. This also deserves an answer. The prediction of potential theory is that stagnation pressure will be

achieved at the trailing edge if the included angle there is non-zero (for a cusped trailing edge there is no stagnation point). Nevertheless, this pressure decays very rapidly (like some very small negative power of distance) even in ideal flows, provided that the included angle is small. In real (or even computed) flows, the boundary layer absorbs most of the change in slope, even at high Reynolds numbers. The pressure distribution is of course dictated by the displacement surface, which is not singular.

3. *The classical solution is a mathematical trick to introduce lift*. Nothing could be further from the truth. It is in full accordance with physical understanding and experimental observations. The "trick", if it deserves to be so called, lies in condensing this to a simple boundary condition, the effect of which is to force the zero-viscosity solution to obey the boundary condition for the small-viscosity solution. The authors are of course familiar with the fact that when one loses the highest order derivatives from a pde, the ability to impose a boundary condition is also lost.

Section VII describes the authors computational experiments, which are three-dimensional as, of course, are real wings. It is well known that it is extremely hard, and probably impossible, to produce two-dimensional flow experimentally. It should be, and usually is, impossible to produce it in a three-dimensional computation. I would have been extremely surprised if the computations had not shown small irregular spanwise variations of the kind presented. In fact, the result is exactly consistent with the expectation that a 3D realization of a 2D flow will behave very similarly to the 2D flow, but with 3D features that are usually small. Although there are examples where the 3D features are not small, a guiding principle of aerodynamic design is to avoid surprises, and this is another reason for designers to prefer sharp trailing edges. There is a computation that the authors should have made, which is to run their code in 2D mode and compare the outcome. I confidently predict that at a low angle of attack there will be almost no difference in the forces.

Section VII fails almost every test for the proper reporting of computational results. The description of the code omits many details that might be important. Additionally, for these particular tests, the following questions should have been answered. What was the radius of the trailing edge as a fraction of the chord? What was the Reynolds number? What was the Mach number? What was the mesh size in the trailing edge region? Have they estimated the errors or run convergence tests? Remarks elsewhere in the text suggest that this may have been run with the dissipative terms turned off "to simulate a potential flow". It should be realized that the resulting first-order system is mathematically different from the second-order potential flow equation. This is because the first-order system permits vortical solutions but the second-order equation (by definition) does not. However, In the first-order system, the numerical dissipation will remain, and will serve a function, similar to that of the physical dissipation, of removing energy from the high-frequency modes. Calculations of this kind are often referred to as Implicit Large Eddy Simulation, and are a recognized, but somewhat controversial, approach to modeling some aspects of turbulence. Is that what is being done? In any case, the mere fact of vorticity being observed means that the code did not simulate a potential flow.

If the authors do believe that they are modeling potential flow, it would explain why the observed drag is said to be accounted for by the separation effect. They think that the drag should theoretically be zero and they need to provide an explanation. They need look no further than the numerical dissipation. It is notoriously hard to create an Euler code that does not predict drag at subcritical conditions, especially when compressible codes are run at low Mach numbers. They should make the test suggested above, of running their code in 2D mode, which would eliminate their explanation but leave other explanations in place. What happens to the drag? I very strongly recommend that they do this experiment.

The thrust of the paper so far is that classical explanations of flight, based on two-dimensional potential flow, are wrong because they are thought to be self-evidently inconsistent. Now in section VIII the line is taken that the classical explanations are wrong because the true explanation involves three-dimensional flow. I cannot see how this helps. If the authors had been correct previously--that the classical explanation offers no mechanism to generate circulation--there would still be no such mechanism. If the authors deny that circulation exists, let them calculate it from their simulations; it will be there.

However, the authors are correct that separation might be fundamentally different in 3D than in 2D. One of the best-known examples is the industrial aerodynamics problem of flow past a tall cylindrical chimney. This might naively be supposed to be two-dimensional in planes parallel to the ground, but in practice is always three-dimensional, asymmetric and unsteady, resulting in variable forces perpendicular to the oncoming wind. It is customary to place a spiral band around such chimneys to prevent the flows at different heights from being phase-locked. In the present case, the intuitive expectation would be that such 3D perturbations would appear, but only at the small scale of the trailing edge radius, at least for small incidence. Since the streamwise vorticity must vanish in the mean, the velocities “induced” by it must substantially cancel. (Incidentally, the matter of induced velocity is also frequently misunderstood. To forestall that possibility, it should be realized that induced velocity does not need mysterious mechanisms to explain it, but is a necessary consequence of vector calculus. The name is unfortunate).

There follows a stability analysis of the linearized Euler equations. This is of doubtful validity because it assumes that a perturbation with non-zero curl can be introduced into an irrotational flow. Physically this cannot be done; I have already explained that there is no mechanism even within the Navier-Stokes equations for vorticity creation, merely the evolution of vorticity already present. Creation must take place at solid surfaces and involve viscosity, or must require external body forces. There is nothing at all wrong with Kelvin’s Theorem.

Regrettably, it is my conclusion that publication of any of this material, in any form, would be highly retrogressive. The authors have put their fingers accurately on many of the defects in the truncated versions of aerodynamic theory that are now current. However, they have not realized that all these difficult issues were struggled with years ago by the founding fathers of the subject, and resolved in completely satisfactory ways. Sadly, the outcomes of those struggles have since been simplified or discarded in modern presentations to create a pragmatic treatment focusing on utility. Undergraduate textbooks these days all too often simply omit anything that students find difficult.

The authors have then sought their own explanations, stimulated by interesting results from their Navier-Stokes code. However, they have failed to ask questions that would have been suggested by any experienced practical aerodynamicist. Consequently, they have simply added to a proliferating literature of "theories of flight" that serves only to confuse students and mislead the public. I wish it were possible to retract what has already been written.

This review is very much longer than I would normally write, because I believe that serious issues of substantial public interest are involved. Even so, it may be too brief to carry conviction with the authors, so I am going to recommend some reading books. One is *History of Aerodynamics* by J D Anderson, and another is *An Informal Introduction to Theoretical Fluid Mechanics* by M J Lighthill. These are both accounts intended for a non-specialist but technically-literate readership. A book intended for specialists, but that is old enough not to have succumbed to the almost universal dumbing down, is *Theory of Flight*, by R. von Mises. All of these sources should be consulted before the authors attempt any response to reviews. If there are any statements about two-dimensional flow concerning which they feel skeptical, they have only to run their own code and retrieve the appropriate data. Finally the authors have respected colleagues in the aerospace department at KTH, with whom they should consult freely.