Stockholm, August 21, 2012

Rebuttal of review report for article "New Theory of Flight"
Johan Hoffman, Johan Jansson, Claes Johnson

In our paper we question the classical theory for subsonic flight currently being propagated, based on 2d potential flow with the circulation theory for lift with drag resulting from skin friction in the boundary layer. We present instead a flight theory based directly on the fundamental laws of conservation of mass and momentum, in the form of the 3d Navier-Stokes equations, without any assumption of 2d irrotational flow. Based on the development of theory, computations and experiments over the last decades, we find that it is now possible to describe the basic physics and mathematics of flight in 3d turbulent flow.

Both reviewers defend the classical theory, even if it is clear that the real 3d flow observed in experiments cannot be reproduced with any 2d theory. In particular, we show that potential flow is not stable in 3d, which means that it cannot exist as a stable physical flow, and that the classical proof of Kelvins Theorem is incorrect from the point of view of wellposedness with respect to small perturbations. We show this in computational experiments where the flow is initialized as potential, and we show this with a linear stability analysis. Both the computational experiments and the linear stability analysis show that streamwise vorticity develops at the trailing edge, which is also observed in physical experiments with a close match in lift, drag and pressure coefficients. In this new flow configuration observed in computations and in physical experiments, the main part of the drag results from the change in the pressure distribution at the trailing edge, which dominates any contribution from skin friction in the boundary layer.

As was pointed out in the cover letter to this article, the computational experiments are described in detail in the related paper:

J.Jansson, J.Hoffman and N.Jansson, Simulation of 3d unsteady incompressible flow past a NACA 0012 wing section, available as:
http://www.csc.kth.se/~jhoffman/Johan_Hoffman_KTH/Publications_files/kth-ctl-4023.pdf

Even the report of Reviewer 2 gives us support in that the current state of the classical theory is problematic:

- *The classical theory of flight 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.*

Reviewer 2 then attempts to clarify the orthodox theory, giving the standard argument that 2d circulation comes from vorticity generated in the boundary layer.

We finally address some detailed questions and misunderstandings by the reviewers:

Reviewer 1:

- *The authors must demonstrate that the slip boundary condition somehow matches the physics of viscous flow near a solid boundary. They do not do this.*

There is a vast literature (see e.g. [34]) of wall shear stress models, derived from physics arguments and validated in experiments, used to model the effect of a turbulent boundary layer on the exterior flow (e.g. the flow around an airfoil). The slip boundary condition takes the form of such a wall shear stress model with the skin friction of the wall being a parameter in the model. The skin friction parameter that corresponds to the high Re relevant for flight is very small (which is well understood from physics experiments). In the computational results we show in the paper with slip boundary conditions, we compare with careful experiments to find good agreement in Cp, lift and drag. The small skin friction is here approximated by zero skin friction, and drag results from an integral of the surface pressure over the wing.

- *The authors create their model to explain lift but it must also describe the complete flowfield.*

The computations provide the complete flow field (velocity and pressure fields) on the surface of the wing and in the air around it.

- *At most, perhaps they present a numerical model of the governing equations which avoids the need to discretize the boundary layer. They would however need to demonstrate how drag is calculated with such a model and that the results match with detailed experiments.*

As stated above, drag is computed from the surface pressure and the results are shown to compare well with detailed experiments presented in the paper.

Reviewer 2:

- *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.*

Yes, the computations can be regarded as implicit LES, as is stated in Section II. And no, the simulated flow is not potential, even if the flow near the upstream part of the wing is "near potential" in the sense that vorticity and dissipation is small.

- *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.*

The question here is if potential flow is a suitable model for real flow in 3d at high Re, which we show it cannot be since it is unstable. There can be no restriction on perturbations to have zero curl in the real case of 3d time dependent flow.