AIAA Journal Review of "New Theory of Flight"

The paper essentially consists of two related parts:

(a) An attempt to discredit the authors' version of "the existing circulation theory by Kutta-Zhukovsky-Prandtl formed 100 years ago".
(b) The presentation of their new theory to calculate and explain the physics of the flow past a lifting wing

First of all, there is no "circulation theory by Kutta-*Joukowski*-Prandtl" that has remained untouched for a century. In fact, the authors appear to misunderstand boundary-layer theory (and the context it provides for potential flow theory) and seem unaware of decades of research of modern modifications to it. **The authors provide no documented scientific evidence to discredit the current state of the art.**

They claim that a "circulation" theory of lift (potential flow) and a boundary-layer theory (viscous flow) are unrelated. First of all, the concept of circulation is not necessary for the physical explanation of lift-the physical Kutta condition leads to the correct (as verified by experiment) solution to the potential flow problem. Circulation enters the **mathematical** problem for the incompressible potential flow past an airfoil since the problem is non-unique without its specification.

More importantly, Prandtl's boundary-layer theory is not a viscous theory for drag but an asymptotic theory for the solution to the Navier-Stokes equations at large Reynolds number. Potential flow is not presented as the solution for lift but as the first term in an asymptotic expansion-the potential flow and boundary-layer theories are connected through the matching process. The versions of potential theory and boundary-layer theory the authors present are only the first terms in the expansion. Their claim that the theories of Kutta-Joukowski and Prandtl are both incorrect at "separation" (undefined by the authors but apparently only considered at the trailing edge) does not take into account the extensive research into the potential flow-boundary-layer coupling. In fact, the inclusion of the effect of the displacement thickness in the second-order potential flow solution renders arguments associated with a trailing-edge stagnation point moot. (The trailing-edge stagnation point does not appear for a cusped trailing edge). In addition, problems arising with the calculation of the boundary layer past the trailing edge or a separation point are addressed with a *strong-interaction* version of the boundary-layer equations (see the discussion in Chapter 14 of **Katz and Plotkin** which also includes a detailed discussion of the matching process referred to above).

An example of an approach which blends the potential and viscous flows to provide well-accepted solutions for the lifting flow past an airfoil is given in the XFOIL code of Drela (also described in Chapter 14 of **Katz and Plotkin**).

The authors' new approach is to solve the Navier-Stokes equations numerically with an unphysical slip boundary condition. They state that "We will discover that solutions of the Navier-Stokes equations with small viscosity and skin friction can be viewed as modified potential solutions, which are partly turbulent and which arise from the instability of potential flow at separation. The slip boundary condition does not give rise to boundary layers and the real flow may thus stay close to potential flow before separation."

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. They therefore create a mathematical problem which isn't based on the physics and use it to claim the absence of boundary layers. They claim that potential flow is a model of the complete flow and that an instability at "separation" leads to the correct resulting flow. How do the authors even define separation in a potential flow?

The authors create their model to explain lift but it must also describe the complete flowfield. The failure of their model to reproduce the flowfield for a well-documented example is demonstrated in their solution for the zero angle-of-attack flow (no lift) past a NACA 0012 airfoil. They state that "the drag results from 3d rotational separation at the trailing edge". There is no boundary layer. How would their model behave in the limit of a symmetric airfoil of vanishing thickness at zero angle of attack? Where would the drag come from? The drag is not calculated by an integration of wall shear stress (can the shear stress be found with the authors' slip boundary condition?). In addition, they even introduce leading edge suction into the drag discussion when it is only defined with respect to linearized potential flow aerodynamics.

In summary, the authors have not presented us with an aerodynamic theory alternative to the modern boundary-layer theory in the literature which addresses lift and drag. 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.