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SHORT COMMUNICATION

A COMMENT ON THE PAPER 'FINITE DIFFERENCE METHODS FOR THE STOKES AND NAVIER–STOKES EQUATIONS' BY J. C. STRIKWERDA

PATRICK J. ROACHE

Ecodynamics Research Associates, Inc., PO Box 8172, Albuquerque, NM 87198, U.S.A.

SUMMARY

This work comments on a recent paper by J. C. Strikwerda in *SIAM Journal on Scientific and Statistical Computing*, in an attempt to clear up the evident confusion regarding the use of a Poisson equation for pressure in incompressible Navier–Stokes solutions.

KEY WORDS Finite differences Navier–Stokes equations Poisson equation Pressure equation

INTRODUCTION

Strikwerda[1] claims that the use of a Poisson equation for pressure in incompressible flow problems using primitive variables is incorrect. This would be bad news indeed for the many researchers who use this most common and efficient approach. The purpose of this comment is to point out what the author believes to be an error in Strikwerda's discussion.

Strikwerda contrasts the original Navier–Stokes equations, his system (1.2) consisting of the momentum and continuity equations, with the derived system (2.2) consisting of the same momentum equations and a Poisson pressure equation (PPE). The following quote[1] is from page 58.

'Roache (1972, p. 194) suggests that the additional boundary conditions be given by the normal derivative of pressure as determined by the first equation of (1.2) or (2.2) [i. e. the momentum equations] evaluated on the boundary. This, however, is not satisfactory as a boundary condition since it is not independent of the system of differential equations. Roache's suggestion leaves the system (2.2) [momentum equations and PPE] underdetermined.'

'Another boundary condition which is commonly used along boundaries corresponding to physical surfaces is to set the normal derivative of the pressure to zero, which is valid in the limit for high Reynolds number flow. With this boundary condition and (1.3) [Dirichlet conditions on velocity] the system (2.2) has the proper number of boundary conditions, however, its solutions are not solutions of (1.2) [the momentum and continuity equations].'

DISCUSSION OF DERIVED BOUNDARY CONDITIONS

As Strikwerda correctly notes in the discussion on the number of required boundary conditions for the original system (1.2), 'these boundary conditions will usually be Dirichlet or Neumann

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conditions on the velocity'. The implication is clear and is correct, that for the original system using the continuity equation, boundary conditions on velocity are all that is necessary to solve the system. Any more conditions would overspecify the solution; any less would underspecify the solution.

However, once we apply the divergence operator (i.e., in Cartesian co-ordinates, cross-differentiate[2]) to the momentum equations to obtain a Poisson equation for pressure, we have raised the order of the system being solved. In order to proceed with a solution, we need boundary conditions on the pressure, only because the derived system is of higher order. The trick is to avoid inventing some arbitrary boundary condition on pressure which is not part of the original system. This is accomplished by obtaining the additional boundary condition for the derived system by requiring consistency with the original system, evaluating the boundary pressure gradient from the momentum equations. No new restraints on the overall solution are introduced. The only boundary conditions on the final solution of the system are those on velocity, as would be the case if we solved the system (1.2) (momentum and continuity) directly. But the solution is arrived at via a higher-order derived system which utilizes higher-order derived boundary conditions on the Poisson pressure equation, specifying nothing more than consistency with the original lower-order system. (Indeed, if one concocted a system involving an equation for $\nabla^4 P$, yet another derived boundary condition on P of higher order would be required, and consistency with the original equations would require that it too be derived from the momentum equations.)

Strikwerda[1] has perhaps confused this procedure with the erroneous one of attempting to derive final boundary conditions from examination of the interior governing equations. For example, boundary conditions on temperature for a heat conduction problem cannot be obtained by considering a heat balance at the wall, since this conservation law is the basis of the interior heat conduction equation. But this is not what is being done in the suggested approach.

The following thought experiment may be helpful. Suppose one were presented with a complete and accurate solution of the Navier–Stokes equations for velocity components only. (How the solution is obtained is not relevant, but if it helps to clear up the confusion, we may think of it being obtained numerically from a single fourth-order system involving the biharmonic operator for the stream function, for which the boundary conditions are (usually) unambiguous. Or, we may think of it coming from physical experiments using a laser–Doppler velocimetry system.) One is then asked to process the solution so as to solve for the normal pressure gradient at the walls. How would this information be obtained? The obvious answer is to evaluate the normal momentum equation numerically at (or near) the walls, using the supplied velocity data to evaluate the velocity derivatives, and to simply solve for the pressure gradient. This is indeed the same pressure gradient which is solved using the suggested approach. This approach does not leave the system underdetermined at all; the required final boundary conditions are precisely those of the original system. (Further, one could be asked to solve for the entire pressure field, up to the arbitrary constant, from the prescribed velocity data. In that case the non-homogeneous term[2] of the Poisson pressure equation is also determined from the prescribed velocity field; along with the boundary conditions, this allows complete solution. This method is used by Gresho and Sani[3] and produces unambiguous results, as expected.)

THE HOMOGENEOUS GRADIENT BOUNDARY CONDITION

It is strange that Strikwerda[1] says that the evaluation at physical boundaries of the normal pressure gradient $\mathrm{d}p/\mathrm{d}n = 0$ is valid in the limit of high-Reynolds-number flows. How is this homogeneous gradient boundary condition known to be valid, except by taking the high-*Re* limit of the normal momentum equation near a no-slip ($v = 0$) wall? He says that 'with this boundary condition, and (1.3) [Dirichlet conditions on velocity] the system (2.2) has the proper number of boundary conditions, however, its solutions are not solutions of (1.2)'. In fact, for finite *Re* this approach is somewhat inconsistent. How can we say that the Dirichlet conditions on velocity (1.3) are sufficient to determine the solution, and then add another and different final boundary condition on the pressure?

The use of $dp/dn = 0$ in fact may generate a 'jump' in the solution near the wall, since it will be incompatible with the momentum equations for finite *Re*. As a practical matter, it actually has advantages for high-*Re* flows when the boundary layer approximations are valid, since it can be an accurate approximation and aids in iterative convergence of the Poisson equation. The actual evaluation of the pressure gradient from the momentum equations, which I suggest,[2] is more accurate, but may require additional attention to stability in the intra-time step iteration procedure, requiring under-relaxation in both pointwise and global segregated solution procedures. (Alternately, and preferably, a pointwise iteration would not use a segregated solution procedure, but would proceed by solving the local velocities and pressure by the solution of a 3×3 system at the near-boundary node, with the pressure boundary condition implicit, rather than lagged in the iteration.)

The inaccuracy claimed by Strikwerda[1] for the use of the Poisson equation is in fact due to the use of $dp/dn = 0$ instead of its evaluation from the momentum equations, as I suggested.[2] As examples of such inaccuracy, he refers to four works in a compendium[4] of project papers from a graduate course in computational fluid dynamics. But an examination shows that all four works used $dp/dn = 0$ in a driven cavity problem, which is not a boundary layer problem. The accuracy of the correct procedure has been well established by Ghia *et al.*[5] (Note that the optimal efficiency of the segregated solution procedure with a Poisson equation for pressure depends on the availability of fast solvers for the Poisson equation, but even using crude iterative techniques, the approach is typically faster than that used by Strikwerda.[1] No information on the computer times is given therein.[1])

EQUIVALENCE OF THE POISSON PRESSURE APPROACH AND CONTINUITY

Although Strikwerda has cited the 1972 edition of my book[2] as the source for the suggestion to use the PPE, it is of course more widely used; see e.g. Harlow and Welch[6] and Williams.[7] These authors, and Piacsek and Williams,[8] are also fairly clear about the essential related point, that continuity and momentum equations taken together imply the PPE. However, the literature is a bit vague about the opposite inference. Strictly speaking, for the continuum equations, they use continuity and momentum equations to derive the PPE, but do not show that PPE and momentum implies continuity. (Nor is this step totally obvious, since it does not hold for the analogous situation in the vorticity–velocity variable system; see Fasel[9] and page 207 of Roache.[2] There, we obtained a Poisson equation for each velocity component. However, those Poisson equations do not imply that divergence $D = 0$ everywhere, but only that $D_x = 0$ and $D_y = 0$; thus boundary errors in continuity will be propagated to the interior.) However, Harlow and Welch[6] and Williams[7] are very clear that exact satisfaction of the PPE does give exact continuity satisfaction for centred differences on a staggered grid. Fortunately, Gresho and Sani[3] have recently made the point quite clear both in the continuum case, with a theorem showing that the inference goes both ways, and in discrete cases using both non-staggered and staggered grids. They also thoroughly discuss the pros and cons of alternate approaches to the PPE, as well as the intricacies of using Dirichlet conditions on the PPE.

Upon re-reading the relevant sections of my book,[2] it is clear that the equations and recommended procedures are correct, but there is a significant pedagogical flaw in the derivation of the PPE. The continuity equation appears to enter only as a device for simplifying the right-hand side of the PPE, rather than as an essential step. If continuity is not introduced, one could surmise that one had generated an equation for pressure, which completes the momentum equations for a total system, without ever making use of the physical law of incompressibility!

ADDITIONAL COMMENTS ON STRIKWERDA'S PAPER

In addition to the above comments on the use of the Poisson equation for pressure, several other comments on Strikwerda's paper[1] and his method may be of interest.

Strikwerda's discussion of the integrability condition is correct, but gives the appearance of being original. Even more complete discussions and detailed implementation of the same approach are to be found in the 20 year old paper by Miyakoda,[10] and especially in Briley[11] (page 20). These references are also cited on page 184 of the 1976 edition of my book.[2] Later works have elucidated the concept and extended the detailed implementation to tranformed co-ordinates; see especially Ghia *et al.* [5,12,13]

Strikwerda[1] also says 'Roache (1972) has a discussion of the difficulties of obtaining a zero divergence for the velocity field when using the above approach [Poisson equation for pressure] for time dependent flows (see also Harlow and Welch (1964))'. This may leave the reader with the incorrect impression that the MAC method has problems. A 'discussion of difficulties' does not mean that the difficulties have not been solved. In the MAC method the zero divergence for the velocity field can be obtained arbitrarily closely, depending on the iteration level. But a significant advantage of the MAC method is that incomplete iterative convergence can be prevented from introducing non-linear instability by the ingenious device of retaining a time derivative for the dilatation, which in the continuum is identically zero, and setting the dilatation to zero at the advanced time step. The technique is also recommended for non-staggered grids.[5] Hirt and Harlow[14] generalized the concept to any initial value problem.

Also, Strikwerda's reference[1] (page 59) to the difficulties with staggered mesh schemes when both velocity components are specified at a boundary is exaggerated. These are easy to code and have been well understood for 20 years (see Harlow and Welch[6] and detailed implementation in Gresho and Sani[3]).

Finally, we note that the above criticisms do not affect a contribution of Strikwerda's paper,[1] which is a method of 'regularization' of the primitive equations. This addresses a fundamental difficulty (which may be somewhat obscured in the paper), namely that, with centred differences for the pressure gradient (or indeed with any obvious symmetrical discretization), the value of the gradient of pressure in the momentum equation at a node is decoupled from the value for pressure itself at that node. As such, the proposed method joins other candidate techniques (such as the staggered MAC mesh, the penalty function method, the use of double meshes for pressure, the use of two Poisson equations for pressure gradients as dependent variables, the use of one-sided differences weighted with centred differences for pressure gradients in the momentum equations, and the addition of an artificial pressure diffusion term in the momentum equation). This difficulty becomes manifest only at moderately high Re; it will be of interest to evaluate Strikwerda's method once the author performs computations for $Re > 0$.

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Some explanation may be in order for the publication of this comment in other than the same journal as Reference 1, and for the long publication lag. An earlier draft of this comment was indeed submitted to *SIAM Journal on Scientific and Statistical Computing*. However, the associate editor of that journal rejected publication, having obtained two negative reviews, one from the author of Reference 1, who did not think this paper correct, and another who did not judge upon the correctness of either position but thought the subject too important to relegate to the obscurity of a comment. My protest to the chief editor of SIAM elicited the final rejection, based on the evaluation that Strikwerda and I were not even in disagreement.

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