Computational Star Formation
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Historical Perspective on Computational Star Formation

Richard B. Larson

Department of Astronomy, Yale University,
New Haven, CT 06520-8101, USA
email: richard.larson@yale.edu

The idea that stars are formed by gravity goes back more than 300 years to Newton, and the idea that gravitational instability plays a role goes back more than 100 years to Jeans, but the idea that stars are forming at the present time in the interstellar medium is more recent and did not emerge until the energy source of stars had been identified and it was realized that the most luminous stars have short lifetimes and therefore must have formed recently. The first suggestion that stars may be forming now in the interstellar medium was credited by contemporary authors to a paper by Spitzer in 1941 in which he talks about the formation of interstellar condensations by radiation pressure, but then oddly says nothing about star formation. That may be because, as Spitzer later told me, when he first suggested very tentatively in a paper submitted to The Astrophysical Journal that stars might be forming now from interstellar matter, this was considered a radical idea and the referee said it was much too speculative and should be taken out of the paper. So Spitzer removed the speculation about star formation from the published version of his paper.

But the idea apparently got around anyway, and it was soon developed further by Whipple in a paper that credited Spitzer for the original suggestion. Whipple says in a footnote that although his work was first presented in 1942, its publication was delayed by "various circumstances" until 1946. By that time, the idea that stars are forming now in the interstellar medium had evidently become respectable enough to be published in The Astrophysical Journal, and Whipple's paper may be the first published presentation of it. In 1947, Bok & Reilly called attention to the compact dark clouds in the Milky Way that later became known as Bok globules, and they suggested that these dark globules might be prestellar objects and might form stars, referencing the papers by Spitzer and Whipple. This suggestion was controversial at the time, and it remained so for many years. But in 1948 Spitzer, in an article in Physics Today, laid out what are essentially modern ideas about star formation in dark clouds, and he pointed specifically to the dark globule Barnard 68 as a possible prestellar object, or 'protostar' as he called it.

By the 1950s, the theory of star formation had become a popular subject and many papers were written on it. The most influential one was probably a 1953 paper by Hoyle that introduced the concept of hierarchical fragmentation, whereby a cloud is assumed to collapse nearly uniformly until at some point separating or fragmenting into smaller clouds, which then individually collapse nearly uniformly and repeat the process. The idea of hierarchical fragmentation remained influential for a long time in theoretical work, even though the assumption of uniform collapse was later disproven by numerical calculations.

Numerical work on star formation began in a serious way in the 1960s, and I came into the picture in 1965 when the problem of protostellar collapse was suggested to me by my thesis advisor Guido Münch at Caltech. Originally I had grandiose ideas about calculating galaxy formation, but Guido was skeptical and said "before you try to understand how a galaxy forms, why don't you try to understand how one star forms?" He also suggested that I talk to Robert Christy, who had recently used numerical techniques to study stellar pulsation, and see if I could use similar techniques to calculate the collapse of an interstellar cloud to form a star. I thought that this sounded like an interesting and challenging project, and I went to talk to Christy, a nuclear physicist who had worked in the nuclear weapons program at Los Alamos. He thought that my calculation might be feasible, and he handed me some reprints and preprints, among which was a recently declassified report from the Livermore National Laboratory presenting a numerical method for doing gas dynamics with radiation and shocks that had originally been developed to calculate powerful explosions in the Earth's atmosphere. I realized that I could use some of the same techniques for the star formation problem, and I also recognized in this report the origin of what became the most widely used method for calculating stellar evolution, the 'Henyey method', which had been derived from the same Livermore bomb code by taking out the hydrodynamics. Many of the numerical techniques later used in astrophysics thus had their origins in nuclear weapons research, perhaps not surprisingly given that a nuclear explosion may be the closest terrestrial counterpart to astrophysics, involving similar physical processes.

When I began work on the protostellar collapse problem in late 1965, I had no idea what I would find or how far I would get, but I thought that even a start on the problem would be worthwhile. Along the way I wrote and tested two completely independent codes, Lagrangian and Eulerian, each with its advantages and disadvantages, and I tried as far as possible to replicate my results with both codes to increase my confidence in them. About a year later in late 1966, I completed my first calculation that had started with something like a Bok globule and ended with a pre-main sequence star. The basic result was that the collapsing cloud became so centrally condensed that only a tiny fraction of its mass at the center first attained stellar density, becoming a 'stellar core' that continued to grow in mass by accretion until eventually acquiring most of the initial cloud mass. The essential implication of this was that star formation is largely an accretion process. This was clearly an important result, and I realized that I still had a lot of work to do to demonstrate its correctness and robustness, so I spent another year running more cases and varying the assumptions and approximations involved. Eventually, after much testing, I acquired considerable confidence in my results, and I presented them in my thesis in 1968. In my thesis defense I was careful to note that my calculation was still an idealized case assuming spherical symmetry and neglecting rotation and magnetic fields, which seemed unlikely to be realistic. But one of my examiners, I think it was Peter Goldreich, said "don't be so apologetic, this is a good calculation and you should publish it."

Thus encouraged, I published my results in 1969 and presented them at meetings. They attracted considerable interest, but also received a lot of flak and criticism. There followed about a decade of debate and controversy over whether my results were correct, with some studies yielding conflicting results and with observers producing apparently conflicting observations showing outflows rather than inflows around newly formed stars. But Bok was delighted that I had shown how one of his globules could form a star, and he decided to spend his retirement years as a kind of evangelist for Bok globules. He was vindicated in 1978, when he proudly sent me a photograph he had taken of a dark globule with a Herbig-Haro jet emerging from it, showing that a star had recently formed in this globule. My vindication came in 1980 when two groups, Winkler & Newman and Stahler, Shu, & Taam, published results very similar to mine. More recently, Masunaga & Inutsuka in 2000 considerably refined the spherical collapse calculation and again obtained similar results.

What was learned from all this work that could be credited specifically to the use of numerical methods? Looking back, I think that the most important result of my work might have been the very first one that I found when I got my first collapse code running at the end of 1965. I had written a simple Lagrangian code to calculate isothermal collapse, and the first successful run with this code showed the runaway growth of a sharp central peak in density. I plotted the density distribution logarithmically and noticed that it was approaching a power-law form with $\rho \propto r^{-2}$, a form similar to that of a singular isothermal sphere, even though the cloud was collapsing almost in free fall. This power-law behavior extended to smaller and smaller radii as the collapse continued. Although this result was unexpected, I realized that it could be understood qualitatively in terms of the inward propagation of a pressure gradient from the boundary, and I later found an asymptotic similarity solution showing this behavior and was able to show that the numerical solution was evolving toward it, giving me increased confidence in the result. I also later learned that at about the same time Michael Penston had been doing similar work and finding similar results, and he independently derived the same similarity solution. This 'Larson-Penston solution', as it has been called, has been perhaps the most enduring result of that early work, and similar asymptotic similarity solutions have been found for a variety of other more realistic collapse problems, including non-isothermal and non-spherical collapse and even collapse with rotation and magnetic fields.

Concerning collapse with rotation, I tried in 1972 to calculate the collapse of a rotating cloud with axial symmetry, but this time I got it wrong. My numerical resolution in 2 dimensions, limited by the computers then available, turned out to be inadequate to follow the development of a sharp central density peak, and my calculation showed instead the formation of a ring. Later when we got a bigger computer, I repeated the calculation with a finer grid and got a smaller ring, causing me to wonder whether the ring might go away completely with infinite resolution. The first person to get it right was Michael Norman, and in 1980 Norman, Wilson, & Barton showed that when sufficient care is taken to ensure adequate resolution at the center, the result is not a ring but a centrally condensed disk that evolves in a quasi-oscillatory fashion toward a central singularity. This result was later confirmed in more detail in 1995 by Nakamura, Hanawa, & Nakano, who also derived an asymptotic similarity solution similar in form to the Larson-Penston solution describing the evolution of the disk toward a central singularity. Finally in 1997, Basu showed that a similar asymptotic similarity solution describing evolution toward a central singularity can be derived even when a magnetic field is included in addition to rotation and when ambipolar diffusion is properly included in the calculation.

What these results show is that in all of these cases, star formation begins with the runaway development of a central singularity in the density distribution. This conclusion now seems to be universal, and even in more realistic 3-dimensional simulations of the formation of systems of stars, the formation of each simulated star or 'sink particle' always begins with the sudden appearance of a near-singularity in the density distribution in a place where local collapse is occurring. This might now seem an unsurprising result because stars are essentially mass points or singularities on the scale of interstellar clouds, so that the formation of a star must involve the development of a near-singularity in the density distribution. But this result was not anticipated before the numerical calculations were done by Penston and me, and also by Bodenheimer & Sweigart at about the same time. Even though earlier studies, notably the work of Hayashi & Nakano in 1965, had shown a tendency for collapsing clouds to become increasingly centrally condensed, no one had anticipated the runaway development of a density singularity, and it took computers to discover this result (computers which at the time had far less computing power than your cell phone.) So this seemingly universal feature of star formation can be regarded as a true discovery of numerical work, and as an example of how computation can discover qualitatively new phenomena.

A second apparently universal feature of star formation that has become clear from much computational work over the years is that, when no artificial symmetries are imposed and fully 3-dimensional behavior is allowed to occur, we are immediately in the realm of chaotic dynamics, because only the very simplest physical systems show regular and predictable behavior. Newton famously solved the 2-body problem but failed to solve the 3-body problem because it exhibits chaotic behavior, a phenomenon that is now understood largely on the basis of computational work. Even the restricted 3-body problem, where the third body is massless, is chaotic and can show exceedingly complex and unpredictable behavior. Three-body interactions are almost certainly very common in star formation, and in my 1972 paper on collapse with rotation I had speculated that in reality the result might often be the formation of a triple system that decays into a binary and a single star, yielding binaries and single stars in roughly the right proportions. Such unstable and chaotic behavior is in fact often seen in 3D simulations of the formation of systems of stars, even the first crude ones that I made in 1978, and it is not surprising because as more mass accumulates into the near-singularities or 'sink particles', the system becomes increasingly like a gravitational n-body system whose dynamics is well known to be chaotic. In addition to chaotic gravitational dynamics, another source of chaotic behavior that can be important in star formation is the development of fluid-dynamical turbulence in star-forming clouds.

Because of these effects, even the simplest extension of star formation modeling from one star forming in isolation to two stars forming in a binary system involves chaotic dynamics. Not only is the gravitational dynamics of the gas circulating around the forming stars intrinsically chaotic, but the gas flow can become turbulent, in which case there are two sources of chaotic behavior in the system. Gravitational and MHD instabilities in the gas orbiting around the forming stars might introduce yet additional sources of chaotic behavior. As a result, the formation of a binary system is not a deterministic or predictable process in its details – every calculation will produce a different result. Therefore we can only hope to predict the statistical properties of binary systems. Large 3D simulations are beginning to be able to do this, and they have already yielded some realistic-looking results for the distributions of binary properties, including a very wide spread in separations resulting from the chaotic dynamics. Similar considerations also apply to predicting stellar masses – we can't predict the mass of an individual star, whose accretion history may be very chaotic and irregular, but we might be able to predict the IMF of a large ensemble of stars if we can include enough of the relevant physics. Again, large numerical simulations are beginning to be able to address this problem. Of course, extensive computations are needed to do these things, and powerful computers are required; computers with the power of cell-phone processors are no longer adequate.

These examples illustrate that, in my view, the most valuable contributions that computing can make to science are not numbers but new discoveries and insights. So I hope that the participants in this meeting who are doing computational work on star formation keep this in mind, and I look forward to learning about many new discoveries made by computational work.

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Historical perspective on astrophysical MHD simulations

Michael L. Norman[1,2]

[1]Center for Astrophysics and Space Sciences
[2]San Diego Supercomputer Center
La Jolla, CA 92093, USA
email: mlnorman@ucsd.edu

Abstract. This contribution contains the introductory remarks that I presented at IAU Symposium 270 on "Computational Star Formation" held in Barcelona, Spain, May 31 – June 4, 2010. I discuss the historical development of numerical MHD methods in astrophysics from a personal perspective. The recent advent of robust, higher-order accurate MHD algorithms and adaptive mesh refinement numerical simulations promises to greatly improve our understanding of the role of magnetic fields in star formation.

Keywords. ISM: star formation, ISM: magnetic fields, methods: numerical

1. Introduction

It is a distinct pleasure to be invited to speak to you today about numerical MHD simulations of star formation. Moreover it is a great honor to speak second following Richard Larson, whom I consider the founder of computational star formation. As I will relate, his research influenced me in ways he is probably unaware of, and it is nice to have the opportunity to tell that story. I must admit this is the first historical perspectives talk I have been asked to give which means I must be getting old. On the other hand I cannot deny that I have been meddling in computational star formation on and off for 35 years now and have a few reminiscences and battle scars to relate. In this short contribution I do not attempt to be comprehensive about the given topic, but rather describe my personal experiences developing and applying numerical MHD methods to problems of interest, including star formation.

2. Caltech coincidences

Before I do that I must relate a couple of strange coincidences that occurred to me when I was an undergraduate at Caltech which in hindsight foreshadowed my graduate research at Livermore. First, as a new freshman I wandered into Millikan Library–a Caltech landmark–to browse the astronomy and physics library. I saw a shelf filled with beautifully bound red volumes, and picked one off the shelf at random to see what they were. I picked Richard Larson's PhD thesis which I would later, as a graduate student, study in great detail. At the time though I didn't understand anything and could barely comprehend how a PhD thesis came into existence. I flipped through it, impressed with the graphs and equations, and put it back on the shelf. The second foreshadowing occurred when I was a sophomore or junior. I did a term paper on supernova explosions for Peter Goldreich's class on the interstellar medium. In the process I ran across a paper in the Astrophysical Journal written by Jim Wilson on numerical simulations of neutrino-driven iron core collapse supernova explosions. Jim would later become my PhD thesis advisor and suggest a topic in star formation that would eventually bring me into contact with Richard Larson's early research.

3. Livermore Years

I did my PhD thesis on numerical star formation under the supervision of Jim Wilson at the Lawrence Livermore National Laboratory from 1975 to 1980. Jim was one of the true pioneers of numerical astrophysics (Centrella *et al.* 1985), and I was fortunate to have him as my supervisor. He was absolutely fearless when it came to tackling a new problem numerically. This was due to the fact that in the 1960s he had developed 2D multiphysics codes to simulate the internal operations of nuclear weapons, which gave him an encyclopedic knowledge of hydrodynamics and MHD, neutronics and radiative transfer, plasma physics, nuclear reactions, etc. In the late 60s Jim became interested in astrophysics and started to work on core collapse supernovae, relativistic stars, magneto-rotationally driven jets, and, somewhat later, numerical general relativity. In the 1970s Jim had assisted David Black and Peter Bodenheimer at UC Santa Cruz to develop a 2D hydro code which they applied to axisymmetric, rotating, protostellar cloud collapse simulations (Black & Bodenheimer 1975, Black & Bodenheimer 1976). They found the collapse produced a gravitationally bound ring, confirming a result published by Richard Larson in 1972. Jim suggested I look at the stability of this ring to nonaxisymmetric perturbations using a 3D self-gravitating hydro code he had written. I said OK. He gave me two boxes of IBM punch cards and said get to work. I did, and two years later I had my first publication (Norman & Wilson 1978).

For my PhD thesis I developed a new 2D, axisymmetric, Eulerian hydro code to study rotating protostellar cloud collapse. Years later this code would become the basis for the first ZEUS code. I showed that the self-gravitating ring seen by Larson (1972) and Black & Bodenheimer (1976) was a numerical artifact produced by spurious transport of angular momentum (Norman, Wilson & Barton 1980). I presented this result, and the truncation error analysis it was based on, at the 1979 Santa Cruz star formation summer school. Larson, Black, and Bodenheimer were in the audience. Here I was, an unknown graduate student, telling the big names in the field that their results were incorrect in front of the star formation community. Afterwards Richard was very gracious about it.

That work taught me an important lesson about numerical simulations which I have never forgotten and young researchers should not forget: that numerical errors masquerade as physics, and that one needs not to take numerical results at face value. A high level of skepticism needs to be applied to any new and interesting result, because it may simply be wrong. The code may simply be doing the best it can under difficult circumstances. Numerical star formation, with its vast range of scales, is a very difficult problem. This I learned reading Richard Larson's thesis.

4. Protostars and Planets, Tucson, 1978

I became aware of the importance of magnetic fields to star formation when I attended the first Protostars and Planets meeting in Tucson, Arizona in January 1978. That is where I met Richard Larson for the first time. All the big names were there, including George Field, Hannes Alfvén, and Joe Silk. Chaisson and Vrba talked about magnetic field structures in dark clouds. Field talked about conditions in collapsing clouds, and John Scalo talked about the stellar mass spectrum. A combative young astrophysicist by the name of Telemachos Mouschovias presented theoretical models of magnetically supported clouds, and of how ambipolar diffusion would lead to gravitational instability

GMCs → clouds → cores → disks → jets and outflows

Object	Process	B important?	Simulation
Jets and outflows	Hydro-magnetic wind	yes	2 ½ D 3D
Proto-stellar disk	magnetic viscosity MRI	yes	2 ½ D 3D
Cores	magnetic support magnetic braking	maybe	2D, 2 ½ D, 3D
Clouds	MHD turbulence	at some level	3D, 3D AMR
GMCs	Parker instability MRI	at some level	3D, 3D AMR

importance ↑

numerical difficulty ↓

Figure 1. Magnetic fields and star formation. We understand star formation as a sequence of related objects and phenomena involving self-gravity, magnetic fields, and turbulence. The importance of magnetic fields seems to increase with decreasing length scale, while the difficulty of numerical modeling the relevant systems increases with increasing length scale because of the lack of simplifying symmetries.

once a critical mass to magnetic flux ratio was exceeded. This work is exceedingly well known now, but in 1978 it was still rather new. One of my strongest recollections of the conference was the Q & A after Alfvén's talk. Mouschovias and Alfvén were in violent agreement about the fundamental importance of magnetic fields to star formation, but seemed to agree on nothing else. That evening I presented a 16mm movie of my 3D hydrodynamic ring fragmentation instability simulations to a receptive audience. But by then I was convinced I was solving the wrong equations, and that what was really required was 3D MHD simulations with ambipolar diffusion and self-gravity, a tall order. In fact, this was what Jim Wilson suggested I work on for my thesis, but I got side-tracked on the 2D axisymmetric work and then decided it was time to graduate. Nonetheless, the takeaway that astrophysical fluid dynamics is fundamentally MHD, not HD, was strongly impressed on me.

Fig. 1 summarizes the current view of the star formation process, and the role magnetic fields are thought to play. We tend to organize the subject around objects at different length scales, proceeding from the largest (giant molecular clouds or complexes) to the smallest (protostars). In between are clouds, cloud cores, protostellar accretion disks and jets. Magnetic fields appear to be important at all these scales, and at some scales fundamental. In the last column I list the minimum useful computational model to study these objects. The importance of magnetic fields seems to increase with decreasing length scale, while the difficulty of numerical modeling increases with increasing length scale because of the lack of simplifying symmetries.

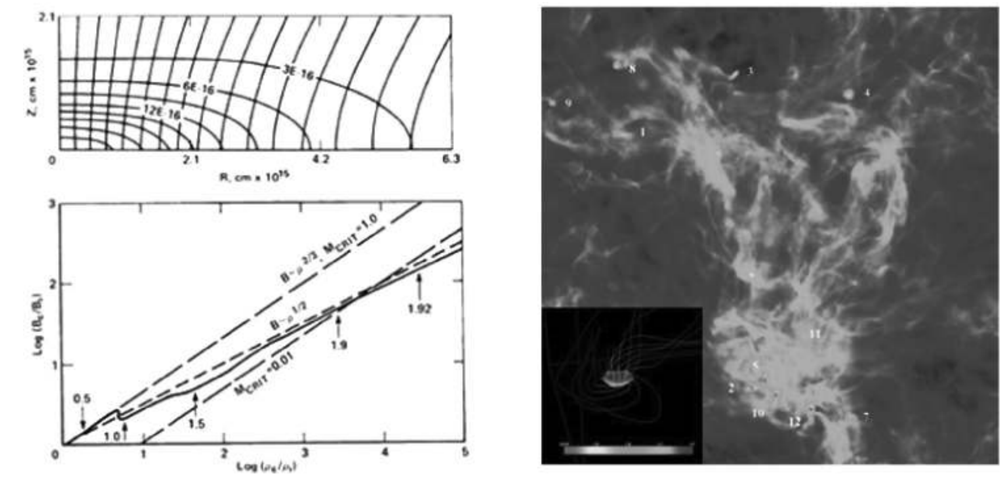

Figure 2. Progress with MHD simulations of star formation. Left: flattened cloud core and central B-rho relation in a 2D non-rotating magnetized collapse simulation (from Scott & Black 1980). Right: self-gravitating cores in a 3D simulation of super-Alfvénic turbulence. Inset: magnetic field topology in a core (from Li *et al.* 2004).

5. Astrophysical Jets

After graduation, my career took a decade-long detour into simulations of astrophysical jets. It was this application, not protostars that got me seriously and permanently involved in developing numerical MHD methods. The VLA had just come online and was producing spectacular radio maps of extragalactic radio jets like those of Cygnus A which were undeniably magnetized. Hydromagnetic launching mechanisms were being proposed by Blandford & Payne (1982) for radio jets, and by Pudritz & Norman (Colin) (1986) and Shibata & Uchida (1985) for protostellar jets. My first simulations of radio jets were purely hydrodynamic, carried out with an improved version of my thesis code. But by 1986 I had incorporated magnetic fields. Working with University of New Mexico radio astronomer Jack Burns and his graduate student David Clarke, I applied this code to magnetically-confined supersonic jet models of extragalactic radio sources (Clarke, Norman & Burns 1986).

6. Evolution of Numerical MHD

6.1. *Early Days*

The development and application of numerical MHD to problems in star formation lagged HD simulations by more than a decade because the simplest nontrivial problem is 2D axisymmetric, where as the early hydrodynamic work could be done in 1D spherical symmetry (e.g., Larson 1969, Westbrook & Tarter 1975). Although Mouschovias had already published by the mid 1970s 2D static models of magnetically supported clouds, it was not until 1980 that the first dynamic MHD simulation was published. Scott & Black (1980) simulated the gravitational collapse of a non-rotating cloud threaded by a uniform magnetic field. They used a first order upwind scheme (donor cell) to evolve the poloidal flux function, ensuring divergence-free poloidal fields. They showed that collapse produces flattened cores as expected, and that the central density and magnetic field scale as $B_c \propto \rho_c^{1/2}$ (Fig. 2a).

Motivated by the recently discovered jets from young stellar objects, Shibata & Uchida (1985) carried out 2-1/2D axisymmetric MHD simulations of hydromagnetically-driven disk wind models. The difference between a 2D and a 2-1/2D simulation is that in rotating axisymmetric systems, toroidal velocity and magnetic components are also evolved. Their so-named sweeping magnetic twist mechanism rediscovered much earlier work by LeBlanc & Wilson (1970) in which rotation efficiently coverts poloidal B-fields into toroidal B-fields, producing what is in effect a coiled magnetic spring that uncoils along the rotation axis due to magnetic pressure, launching a jet. They evolved all three components of B using the second order Lax-Wendroff method, stabilized with artificial viscosity. Such an approach is not guaranteed to maintain divergence-free B-fields.

Clarke, Burns & Norman (1989) performed 2-1/2D MHD simulations of extragalactic radio jets using the original code called ZEUS. The code evolved the poloidal flux function and the toroidal component of the magnetic field using 2nd-order upwind finite differences. This ensures divergence-free magnetic fields, as can easily be demonstrated. The poloidal flux function is defined $a_\phi = rA_\phi$, where r is the cylindrical radius and A_ϕ is the magnetic vector potential. We then have $B_r = -\frac{1}{r}\frac{\partial a_\phi}{\partial z}, B_z = \frac{1}{r}\frac{\partial a_\phi}{\partial r}$. By virtue of the axisymmetry of the toroidal field B_ϕ it is evident that

$$\nabla \cdot \vec{B} = \frac{\partial B_z}{\partial z} + \frac{1}{r}\frac{\partial r B_r}{\partial r} + \frac{1}{r}\frac{\partial B_\phi}{\partial \phi} = +\frac{1}{r}\frac{\partial^2 a_\phi}{\partial z \partial r} - \frac{1}{r}\frac{\partial^2 a_\phi}{\partial r \partial z} + 0 = 0.$$

Faraday's law for evolving the magnetic field becomes

$$\frac{\partial B_\phi}{\partial t} + \frac{\partial}{\partial r}(B_\phi v_r) + \frac{\partial}{\partial z}(B_\phi v_z) = r\vec{B} \cdot \nabla\Omega$$

$$\frac{\partial a_\phi}{\partial t} + v_r\frac{\partial a_\phi}{\partial r} + v_z\frac{\partial a_\phi}{\partial z} = 0,$$

where $\Omega = v_\phi/r$. These equations were evolved in ZEUS using a second-order monotonic upwind scheme alongside the hydrodynamic equations, with the Lorentz force term constructed from first and second difference of B_ϕ and A_ϕ. This was a very neat, stable, and reasonably accurate scheme for 2-1/2D MHD simulations. However it could not be generalized to 3D, and therefore a divergence-free method working directly with the components of B had to be found.

6.2. *Constrained Transport*

Fortunately, in 1988 Evans & Hawley solved half the problem when they introduced the Constrained Transport (CT) method. CT solves the magnetic induction equation in integral form and uses a particular centering of the magnetic and velocity field components in the unit cell so as to transport vector B through a 3D mesh in a divergence-free way. For a recent exposition of this see Hayes *et al.* (2006). I say they solved only half the problem because what they addressed was how to treat the kinematics of magnetic fields, not their dynamics. As we discuss below, an accurate and stable treatment of the dynamics of magnetic fields requires judicious choices for how the EMFs and Lorentz force terms are evaluated.

6.3. *ZEUS and Sons*

In 1987 University of Illinois grad student Jim Stone and I set out to build a version of the Clarke-Norman ZEUS code that evolved (B_z, B_r, B_ϕ) in a divergence-free way using CT (Evans visited NCSA in 1987 and told us about it). We figured if we could make this work in 2-1/2D, it could easily be generalized to 3D. The end result of this effort was a code called ZEUS-2D (Stone & Norman 1992a,b), developed by Jim, and a