

INTERSTELLAR MEDIUM

Crumbling canons

Don Cox

It is a little difficult to believe. A measurement of the thermal pressure in nearby interstellar space, averaged over a path of 100 light years or so, is said to shatter our grand conceptions of the interstellar medium. How can the critical connection be made?

The observation and an interpretation of its significance are reported by Bowyer, Lieu, Sidher, Lampton and Knude on page 212 of this issue. Using the Extreme Ultraviolet Explorer satellite, the Infrared Astronomy Satellite and ground-based photometry of stars, they set a lower limit to the thermal pressure in hot gas towards a small nearby cloud of gas, $p = 2nkT \geq 3 \times 10^{-12}$ dyn cm⁻². (This hot gas, at roughly a million kelvin, is thought to occupy a very large region which totally surrounds the Solar System.) The authors note that this pressure is at least a factor of 20 larger than the thermal pressure measured inside another small interstellar cloud, one that at the moment is blowing over the Solar System and is almost certainly immersed in the hotter, higher-pressure environment.

The authors then make their grand leap: the best known and most quoted model for the workings of the interstellar medium, that of McKee and Ostriker in 1977, has as an underlying premise that all components of the medium are in intimate thermal contact and local thermal pressure balance; yet, in the one place that we can measure the thermal pressures of a cloud and its environment, the balance fails miserably. In the authors' view, the model's being untenable at its core is sufficient reason to abandon it in its entirety.

I tried to tell Stu Bowyer, when he first told me of this paper, that the pressure discrepancy between the hot gas and the local wisp of denser gas had been known for a long time, that Ed Jenkins had written about it in 1984, and that Chris McKee himself had been aware of it for a decade. I also tried to tell him that no one believed McKee and Ostriker's model any more; the expectations one develops from the theory simply have too little correspondence to reality. But Bowyer persisted, and brought me round to his point of view. It is that people cannot carry the absence of a reasonable model as their world view. As a result, when pressed for a description about how the interstellar medium works, they will fall back on the McKee and Ostriker orthodoxy. Furthermore, there are those who continue to assert that the model is correct, although perhaps in a somewhat modified form. It is Bowyer's hope that if we are hit on the head with the fact that the underlying

tenets are false, liberation may be possible.

At the time of its publication, I thought McKee and Ostriker's model was one of the most interesting pieces of work I had ever seen. I was very surprised when the subsequent data caused me to reject it as a reasonable synthesis of reality. Several more years passed before I was able to understand how it could be wrong, and I still find some of its concepts useful in looking for a better picture.

There are essentially three tenets of the model. If the supernova rate in the Galaxy is sufficiently high, the explosions should disrupt the interstellar medium into a hot frothy mixture. There is insufficient power to keep all the gas hot, but too much violence to distribute the energy smoothly through the medium. Cold material would find itself in bits and shells or tunnel walls, pervaded by hotter gas of much lower density. This possibility was known before, but McKee and Ostriker proved that the local region of our Galaxy has a sufficient supernova rate to guarantee such disruption. It was not until about two years ago that a way was found around their reasoning, dissolving the certainty but not the possibility. A pervasive phase of hot gas is theoretically possible.

The second tenet was that there is, on average, a local thermal pressure balance between the different components of the medium. This assumption had been around for quite some time, and was loosely based on observations, denser gas usually being colder by about the right amount. But it does not hold in detail, not even between the local wisp and the local hot gas, as Bowyer *et al.* aver. One sometimes assumes that there are important dynamical effects to disrupt the balance temporarily, essentially the point of view suggested by Jenkins, but recent observations of the gas towards the star Capella with the Hubble Space Telescope have found the local wisp to be amazingly free of disturbance. Furthermore, observations of time-varying hydrogen absorption towards pulsars appear to require a significant amount of interstellar gas at extremely high densities. The pressures are probably also very high and with such a large amount of material in this condition, extraordinary dynamics begins to lose its appeal.

One can wonder whether interphase thermal pressure balance is as fundamental to the McKee and Ostriker picture as Bowyer *et al.* hope. My opinion is that it should not be. It was probably assumed as a matter of tradition and simplicity. But it is related to the third tenet, which is fundamental. This is that

mass exchange takes place between the hot gas of expanding supernova disturbances and the colder clouds that it engulfs. Initially clouds are evaporated to raise the density and radiation rate of the hot gas; later the cooling hot gas condenses and is redeposited on the clouds farther out. This mass exchange was invoked in order to have overall mass and energy conservation.

In its purest form, it was imagined that the cloud evaporation took place through thermal conduction to the cloud surfaces. Later, when it became clear that magnetic fields threading the clouds would be swept back over their surfaces to a cometary form by the passing flow of hot gas, proponents began to speak of other ablation mechanisms to avoid the fact that it is difficult for thermal conduction in a plasma to cross a magnetic field.

Why is this related to pressure balance? Without magnetic fields tangential to cloud surfaces, thermal pressure balance should eventually come about. But with a tangential magnetic sheath, it is not required. The measured field strengths in the interstellar medium are in fact of just the right size for such fields to play an important role in equilibrating the interphase pressure. Pressure balance is as useful a concept as ever, but the balance is often dominated by magnetic field rather than thermal pressure. In that case, thermal evaporation of clouds is probably impossible. McKee and Ostriker's pure and original model just cannot work because the field shuts down the first part of the mass exchange. So thermal pressure imbalance, such as that now found between the local hot gas and at least one of the wisps of denser material within it, becomes an important observational indicator of the field strength and surface geometry.

One can inquire whether other ablation mechanisms might save a modified theory; whether the local region might be atypical; how a magnetic field might hold together the extremely high density regions to which I referred earlier (when it is usually used instead to support regions of unusually low thermal pressure); whether real supernova remnants actually show signs of the accumulation of mass from clouds into the hot gas; whether anyone has ever seen a cloud that was being evaporated or ablated by surrounding hot gas; why, if such evaporation occurs, it does not show up in data for the O³⁺ absorption line to the degree expected; or why, looking at other galaxies, we do not see the extensive quantities of hot gas expected in the standard picture to be pervading the disk. These points are all under discussion. So far, in my opinion, there is no evidence that a model like that of McKee and Ostriker can be constructed to resemble the real interstellar medium. The original model certainly does not. ▶

So Bowyer was right. With a host of difficulties but no alternative, what do we tell our children when they ask? We lie, I guess. Yes, Virginia, there is a viable model of the interstellar medium, published back in '77.

But let us not end on a depressing note. The interstellar medium is an exciting subject of study, with a great deal known about it. Although we do not know the fundamental reasons why everything is the way it is, we have a partial understanding of many aspects. And to put McKee and Ostriker's model in perspective, it was written in a simpler time when much less was known about how material is distributed, about superbubbles and superdense gas, about the distribution of X-ray-emitting gas, about the magnetic field strength and the almost total insensitivity of its magnitude to gas density (except at very high density), about the vertical structure of the disk and its great scale

height in all but the most quiescent components, and many other aspects. If one wishes to hang on to that theory as somehow relevant, the minimum admission required is not only that it knew nothing of all these things, but that it said nothing about them either. I don't mean that as a hostile criticism; it was a great model. But it is not a synthesis of the current state of knowledge of the interstellar medium and is irrelevant to further useful discussion. To pretend otherwise is to stand in the way of enlightenment. Besides, isn't it more fun for a while to talk about the great mysteries and our feeble attempts to peek behind the tapestry, rather than to describe a whitewash we have thrown over it? □

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BOTANY

Opening time by degrees

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SCIENTIFIC ignorance, especially of matters requiring routine but painstaking screening and recording, is a never-ending source of wonder. Take, for instance, flowering time, about which we know little more than the Victorians. This is one of the main events in the life of most plants, and long-term, environmentally induced changes could well have considerable effects on floral communities and their distribution.

Happily, however, the field naturalist is not extinct, and Richard Fitter has compiled an extensive data set on first flowering dates going back almost four decades. The data are analysed and interpreted by Fitter and others in *Functional Ecology*¹, and the results allow a fairly precise relationship to be detected between the temperatures of preceding months and the first flowering date for a range of plant species. The authors also consider the possible influence of any climate warming on plant communities: their predictable conclusion is that the consequences will be largely unpredictable.

One problem that blights this kind of study is the range of factors that can be involved. There is an underlying genetic basis to flowering requirements, which can vary from one individual to another, as any botanist can observe when particular plants always seem to be the first to flower in a local population. Some plant species also require certain environmental stimuli, such as chilling, before they are able to initiate flower development; and many need a particular, and closely defined, day-length/night-length ratio,

which however can vary even within a particular species.

The cocklebur (*Xanthium strumarium*), for example, is a 'long-day' plant, needing a critically short night-length as a cue for flowering in the spring. But, as a widely-distributed species, it would be inappropriate to exhibit the same requirements wherever it is found, so geographically separated populations vary in their precise demands². In Florida, for example, cockleburs flower once the night-length decreases to about 10 hours, but in northern Michigan the night-length requirement is 8 hours. Selection for the appropriate flowering time has evidently determined the development of a latitudinal gradient of populations with distinct genetic constitutions.

Given the very basic physiological requirements, however, other environmental factors may then determine whether flower development can go ahead, and chief among these is temperature. Its effect upon the date of earliest flowering is apparent from microclimate observations, especially where the microclimates concerned are patchy and diverse, as in alpine situations³. Sheltered, south-facing locations often permit early flowering. But although autecological and physiological information is available for certain (yet surprisingly few) plant species, it would be impossible to predict the effect of weather conditions in a given season on the flowering patterns of whole plant communities. And despite the relative abundance of amateur botanists and taxonomists, there are very few good, long-term systematic

records of flowering dates for specific localities.

Richard Fitter has assembled one such data set by compiling recordings of the first flowering dates (FFDs) of 243 plant species within a few kilometres of his home in Chinnor, Oxfordshire, central England, over the 36-year period between 1954 and 1989. The localized source has the advantage that not only does it eliminate any altitudinal or latitudinal effects; it also lessens the confusion caused by variation between individuals of populations because, at least among perennials, it will be the early-flowering individuals (whether because of genetic or environmental causes) which are likely to be first recorded each year. Apart from the detail and taxonomic reliability of the data, there is the added bonus of a sound set of temperature records for the area over this time period. A team from the University of York has now digested all this information¹, using a range of techniques including stepwise multiple regressions of the mean monthly temperatures of the months preceding flowering in relation to FFD in any given season.

Some species proved much more variable than others in their FFDs. The most variable were annual and ephemeral species, which is what one would expect from plants with opportunistic ecological strategies. When taking advantage of chance disturbance or occasions of reduced competition, a plant that relies heavily on rapid and considerable seed production for its survival cannot afford to be fussy about flowering times. The least variable were those species that flowered later in the year (May to August). These may have been generally less limited by temperature conditions during flower development.

Of the 243 species, all but 24 produced significant results for multiple regressions of FFD on mean monthly temperatures of the months preceding flowering, showing that temperature is a major factor in determining the time of flowering for most species (90 per cent of those recorded). Of the very best fits, three species of *Prunus* fell in the first six plants (*P. cerasifera*, *P. spinosa* and *P. avium*, in that order). Other reliable species included *Sanicula europaea*, *Arum maculatum* and *Oxalis acetosella* (interestingly, all woodland species). The most influential month (not very surprisingly) was February, followed by March, but June and July temperatures were proportionately more important to late-flowering species than would have been expected. An increase in temperature of 1 °C above the norm for any of the four months preceding flowering generally produced an advance in flowering date of between 3 and 4 days. One unexpected outcome of the analysis, however, is the negative influence of the preceding autumn on a large number of species. For