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OVERALL MODELS OF THE INTERSTELLAR MEDIUM

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Abstract

Theoretical modeling of the interstellar medium is a fascinating activity that has led to a number of potentially useful concepts, but in my opinion is far from complete. In this paper I have chosen to discuss four aspects of this situation: categorization of models by scope, recent developments that have upset some of our favorite pictures, some recent thoughts I have been playing with, and, for those who may wish to hear even more of my opinions, some references in which I have discussed other aspects more fully. In addition, in response to a challenge by Colin Norman during my presentation, I discuss a list of observational and theoretical studies that could help resolve some of the major uncertainties.

1 Categorization of Models

A way to discuss models is to divide them according to two criteria, one being their key elements, the other the scope of their intended investigations. I have recently provided a discussion of key elements [12], and will here concentrate on the question of scope.

There are actually two forms of scope, limited and general. Limited scope models are those which address specific questions, often in a very broad way. A recent example is the Miller and Cox [20] study of whether the known O star population could provide the observed diffuse ionization, for an assumed distribution of interstellar material. Another is the Wolfire et al. [32] study of the heating cooling balance in diffuse clouds, examining a modern FGH-style [15] phase diagram and determining the thermal pressures which ought to be found in the diffuse clouds.

General models, however, attempt to understand the reasons for the general interstellar characteristics. They adopt results of various of the limited models as basic elements, and in return derive values of parameters needed in constructing the limited models. The McKee and Ostriker [18] model is an excellent example, though it does not follow this pattern exactly. (Within the paper proposing the general model, several limited models are developed as well, notably the evolution of an evaporative supernova remnant.) Their picture also included evaporating cloud models, approximate cloud-intercloud thermal pressure balance, an effective phase diagram setting the temperatures of the various components, diffuse H alpha production by B star radiation acting on the warm envelopes of clouds, etc. The model then predicted or interpreted a variety of interstellar properties: the pressure, the characteristic properties of the hot gas component, the mass transfer rate between components, the filling factors of the various components, the mean interstellar concentration of O VI, and so on. It is this aspect to which I refer as general.

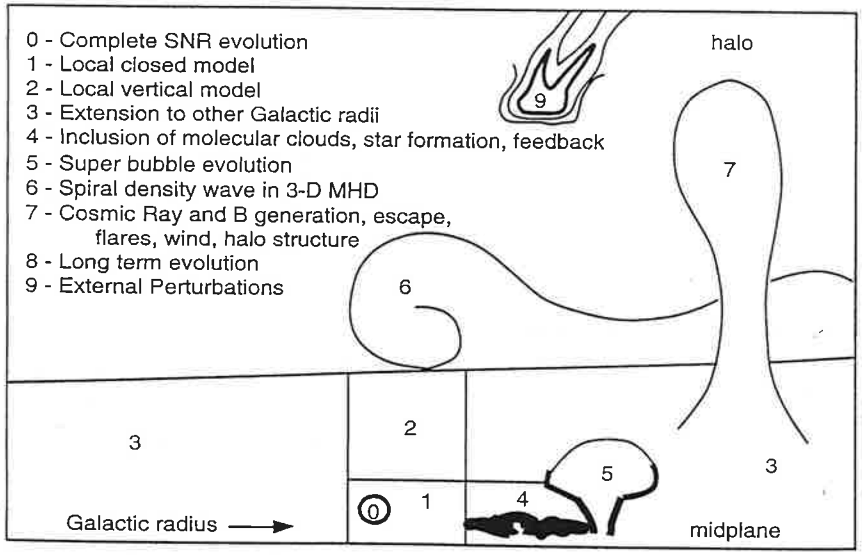

Figure 1 Scope Categories of General ISM Models

But general is a relative term. Figure 1 is an attempt to categorize the various regimes which need to be considered in addressing the nearby Galactic disk. I count nine, of which even the ambitious McKee and Ostriker model addressed only the first.

Even when one has thorough, well tested models of the basic elements (level 0) and has combined them to provide an impressive degree of interpretation of the nearby interstellar state (level 1), there is still a huge amount of work left to be done to extend that interpretation to high z (level 2), generalize it to other galactic radii (level 3), incorporate into it the star formation and mass flow associated with the densest gas, and therefore the feedback mechanisms affecting the assumed rates of activity at level 1 (level 4), add to that the incredibly disruptive and organizational effects of OB associations (level 5) and the potentially even larger scale activity associated with spiral arm passage (level 6), and then top it off with a thorough understanding of the generation, interaction, and dispersal of the nonthermal components (level 7). With all that done, one has a truly general disk model which can be explored for its time evolution (level 8), and even for the effects of external perturbations (level 9) such as infalling high velocity clouds.

I have, of course, misrepresented the activity of ISM modeling by implying that we explore these levels only in the sequence described, or that it is ever done by one intrepid group of explorers. There are clever people crawling all over this territory, operating in parallel, making assumptions about what will eventually be learned at the other levels. I think that this is the necessary approach. I also do not think that we have a very good idea of what the truth is at any of the levels.

2 Recent Developments

One may now reasonably expect that I will provide an overview of the modeling activity at the various levels described above. It would make a fascinating review. But just deciding on reasonable categories has taxed my talent sufficiently. For the present, we will have to make do with the reader's general knowledge of what has been or is being done in the various areas, as background for the comments I next wish to make on recent developments. (Even this list is very parochial. It is really the list of theoretical and observational work that is shaping my present thinking. The thinking itself appears in section 3.)

The porosity imperative is dead. Jon Slavin's results [29, 30] on the long term evolution of SNRs in a hypothetical warm intercloud medium have shown that the implied porosity is much smaller than previously estimated. It is not so low, or the calculations without sufficient uncertainty, that we can know that there is not a pervasive hot phase in the ISM; but the once powerful porosity argument has lost its conclusiveness. One can reasonably doubt the existence of a general hot phase.

The low O VI concentration within the galactic plane is a powerful constraint on models. Substantial volume filling by hot gas with temperature somewhat below 10^6 K is excluded, unless the ions hide in very broad line profiles. Significant amount of mass heating/evaporation by supernovae is forbidden unless it occurs at sufficiently high density. Significant amounts of recombination of material once heated by SNRs is forbidden unless it also occurs at sufficiently high density, or out of the plane where we have not yet had a good look at it. The SNRs of the Slavin and Cox [30] limited scope model just barely satisfy these constraints, supplying all of the O VI in the disk with SNRs alone, most of it from very old ones. Any model in which SNRs evolve in a lower density than that assumed for the warm intercloud component is very likely to have serious difficulties.

The reanalysis of the Copernicus data by Robin Shelton [28] has shown that the difficulties are even more serious: the "features" containing O VI have rather large column densities, and are few in number along a sightline. The statistics agree with the Slavin and Cox results for SNRs, but there is a significant impression that superbubble boundaries may actually make a major contribution. There is no suggestion in the data that the average boundary of interstellar clouds evaporating into a pervasive hot phase has anything to do with the observed O VI features.

The fantastic pictures of hot gas in the disks of other galaxies are fantastic, but they do not put diffuse hot gas in the disk of our galaxy. In the galaxies with which I am familiar, very little of the SNR power appears in x-ray emission, less even than would be expected from the unresolved SNR population, let alone diffuse gas. When Bregman and Glassgold [1] went looking for the hot galactic fountain, we all thought it would be an obvious feature of disk galaxies. But it wasn't there, plain and simple. Subsequent observations have not created it, they have merely become sensitive enough to see what is there at a much lower level. Cui and Miller [13] are quantifying ROSAT results for several galaxies, comparing them with lifetime integrated spectra of SNR models [19, 21]. My understanding is that background subtraction has proven to be more complicated than previously imagined, and there may be more x-ray emission than once thought. It's possible that a hot phase similar to the local bubble conditions exists over much of the area of some galaxies, but there is still considerably less than required to dissipate the SNR power. (I gather there is a similar mystery involving the weakness of EUV emission by galaxies [23].) The bottom line is that the x-ray emission by other disk galaxies has further discouraged proponents of a hot interstellar phase powered by supernovae. There may be ways around some of this, with careful tuning, as I discussed in the Galactic context in 1981 [3], but it must be regarded as difficult.

For decades now we have had before us the question of what fills interstellar space, with only two major contenders, hot gas and a warm intercloud medium. But there are many days when I haven't much confidence in either, as will be apparent in section 5. In section 3, I will discuss an idea prompted by time varying 21 cm absorption column density measurements toward pulsars. Perhaps it is time to let in a little heresy.

Meanwhile, a new type of general scope interstellar modeling has appeared on the scene. It uses hydrocodes and a lot of approximation to local level processes to get a general, but dynamically evolving picture of conditions in the ISM. Input parameters include the surface density of interstellar material, the galactic gravity, heating and cooling rate coefficients, and assumed rules about the star formation resulting from the condensation of material beyond a critical level. The results of that star formation, the evolution of OB associations, photoevaporative destruction of dense clouds, growth of superbubbles, and so on are not fully resolved, but can be introduced by means of rules for the introduction of heat in the star formation environment. The results are a fascinating example of category 1, 2, and 3 (in the sense of Figure 1) models, with a rough attempt at categories 4 and 5 as well. I will leave a more complete description to Joel Bregman's paper in this volume, where it rightfully belongs. But I can't resist making four comments about these attempts to do it nearly all:

1) They are probably the most effective "theoretical" tool we will have in the long run, and the work so far has been great frontier stuff.

2) Because the smaller scale processes are below the resolution of the calculations, it may be rather difficult to get predictions of some of the key observables, such as the O VI content, from this sort of model. During the talk which this paper purports to represent, I was very bold, betting 200 FF that if Bregman were to calculate the O VI content of his favorite Rosen and Bregman model, he would find it enormously greater than the observed mean density of this ion. Perhaps the challenge will be answered in his written contribution, but the probable truth is that it will be difficult to make a reliable calculation.

3) The difficulty with not resolving the smaller scale processes where hot gas activity is initiated also means that the results for the quantity of hot gas found in the model are probably very dependent on the rules made for the diffuse input of heat in star formation regions. The critical parameter, very roughly, is how soon a supernova remnant heats its first thousand solar masses. At a particle density of 1 per cc, that mass occupies a radius of only 20 pc. If the models do not have a significantly higher resolution than that, they will be unable to follow the histories of the hot, long lived bubbles that SNRs probably generate. Actually, I am not yet well acquainted with the existing models and skepticism on my part has largely to do with the arguments still raging over what happens in the simpler models of old. Creation of computer models can help a great deal, even now, but we need better knowledge of the micro- and mesoscale physics to put into them before we can embrace their conclusions.

4) Having met Enrique Vázquez in Mexico, I know that this kind of modeling is not the exclusive province of Alex Rosen, Bregman, and Mike Norman.

Vázquez-Semadeni, Thierry Passot, and Annick Pouquet have worked on similar models, though without the z distribution, I believe [22, 31]. But a big difference is that this group has made both hydrodynamic and magnetohydrodynamic models of otherwise identical situations. Enrique tells me that the results in the two cases are completely different. My tentative conclusion is that we have a wonderful new tool that can provide great insights, but which has quite some developmental history yet ahead. Until models embrace level 7, they probably haven't done levels 0 through 6 reliably either.

I was going to say more about level 6, but space is short, and I have already bragged about the pioneering work of Marco Martos in several other conference papers. If level 6 is of interest to you, consider references [17 and 5-12]. Think MHD hydraulic jumps. On the same note, if superbubble activity is of special interest (level 5), you won't want to overlook Katia Ferrière's work [e.g. 14].

3 Heretical Notions

At Toulouse, in 1986, I was agonizing over the relative volume occupations of warm and hot gas in the ISM when Priscilla Frisch suggested that the problems would be solved if much of the space was simply empty. Empty? How? What about, "Nature abhors a vacuum," and all that? What about interphase pressure balance?

Well, I have long since abandoned the idea that interphase thermal pressure balance has much to do with the ISM. Still I have been uncomfortable with the notion that there could be huge discrepancies. But finally, observations may have forced me to even that extreme. Time varying H I column densities have been interpreted [16] to imply that much of the interstellar H I is in extremely dense, small scale features. They are so dense that their thermal pressures are probably orders of magnitude higher that the average interstellar value. Something holds them together. Whatever it is, it might be similarly capable of giving cohesion to much larger structures of interstellar material, leaving essentially vacuum between them. The only mechanism I have thought of that could potentially do that is "flux ropes," twisted tubes of magnetic flux. I have noticed that these have already gained popularity among some Japanese astronomers, for explaining specific interstellar structures at fairly large scale. We may see many more flux rope explanations in coming years. Among other uses, they offer a simple mechanism for moving material from low density to high, against the opposing magnetic pressure. Twist on the end of a flux tube, and the material inside is compressed by magnetic tension. At sufficiently high densities, the neutrals may begin to diffuse out of the twisted rope, reducing the field pressure with which they have to cope.

A difficulty that has long bothered me is that when lower and lower densities are assumed for the "intercloud' component, supernovae become better and better heating agents. If thermal evaporation is suppressed by the field geometry, the temperature should rise until the SN input power is balanced by the mechanical luminosity of a galactic wind. But I long ago convinced myself that the cosmic ray escape power of the Galaxy is much smaller than it would be if the SN power left as a thermal wind. In the disk, the cosmic rays have a comparable energy density to other forms. If a wind carried one of those forms away, it should take cosmic rays at at least the same rate. Perhaps there is a flaw in this argument, such as that SNe in superbubbles should perhaps not be counted. But it is my current opinion.

But there is a way out of this difficulty if the intercloud density gets small enough, with magnetic cohesion of the denser structures. When supernovae occur in very low density, they find nothing to heat. The ejecta expands over whatever distance required, until the energy is eventually deposited on the boundary of a mass filled region. It there produces a dilute splat. The distance being great, the rate of arrival of ejecta is low, the ram pressure is low, and a slow radiative shockwave proceeds into the higher density region. If the magnetic field were ignored, one might suppose that there would be some material that would be heated enough to leave the cloud, raising the intercloud density, but with the field, even that cannot occur except as a transient.

Maybe there is something to this, a dichotomous pair of interstellar conditions, fibers of interstellar material with effectively a vacuum between. The magnetic field, though is still present between the fibers; the circumferential component of a fiber drops only as 1/r, like that of a current carrying wire. And parallel currents attract, antiparallel currents repel. There's bound to be some interesting physics in there. Time to read Parker? Or shall we question the observations for a respectable period before leaping into unknown waters?

4 Further Discussions

I have the chance to write one or two of these soul searching papers each year [e.g. 4-10]. Two very recent ones which have treated the subject somewhat differently are references [11] and [12]. One that I mentioned in my talk, dealing with "the great pressure mystery," is in the second Tetons conference volume [6].

This ends the presentation of material I discussed at the conference. What remains now is a response to Colin Norman's complaint that we need better information about what to do to lessen our uncertainties.

5 Potentially Critical Observational (and Theoretical) Needs

5.1 DO EVAPORATING CLOUDS EXIST?

Modelers of the interstellar medium are repeatedly confronted by our ignorance of whether thermal conduction operates at a rate close to that given by our formulae, in the presence of the complicating and poorly characterized magnetic field. Whether or not to include it in any given calculation has become a subject for tedious debate. The question separates into two parts, whether conduction is rapid in very low density hot regions whose field has been seriously disturbed by the dynamical event being studied, and whether conduction can cross boundaries between hot gas and embedded cooler regions, causing thermal evaporation. I address a limited form of the first question in section 5.10 below.

It is reasonable to think that the hot gas of the Local Bubble and the cool clouds embedded within it might offer a good laboratory for investigating the thermal evaporation question. There are two ways: study the outer boundary of the Local Cloud in which the Sun finds itself, via high resolution absorption line studies, or study the thermal emission structure around other embedded clouds. I will elaborate on the latter.

I expected, if thermal evaporation were active, that when ROSAT or EUVE looked at a cloud within the local hot gas, it would find an emission halo due to the higher density and slightly lower temperatures in the conduction region surrounding the cloud. Instead, most of what has been seen so far has been characterized as cloud shadows. We need more complete models of what the conduction region should look like (fortunately Jon Slavin is working on that), and direct comparisons between those and good soft x-ray/EUV measurements. I have been told that MINISAT will be very useful for the latter. We also need clearer evidence for whether the clouds studied are actually within hot environments.

5.2 DO SUPERNOVA REMNANTS ACQUIRE (I.E. HEAT TO > $10^{5.5}$ K) SIGNIFICANT AMOUNTS OF MASS FROM CLOUDS, VIA THERMAL EVAPORATION OR OTHERWISE?

ISM models deriving from the McKee and Ostriker prototype depend on mass acquisition from clouds to enhance the diffuse gas density within supernova remnants, leading to cooling of remnants prior to overlap. The original model was extremely robust in this regard, depending on the great temperature sensitivity of the classical evaporation rate to deliver the mass early. When questions arose about the efficacy of thermal evaporation, other modes of stripping matter off clouds became important. But the important question is whether real remnants evolve initially into an extremely low density environment and then subsequently raise their effective density by stripping cloud mass. A study of the detailed evolution of evaporative remnants was made by Cowie, McKee, and Ostriker [2]. It was my opinion at the time that the evolving structure did not resemble real supernova remnants, though I do not believe that quantitative comparison has been attempted. But that one model is not the limit of possibilities for its class, and mass acquisition by processes other than thermal evaporation could drive very different evolutionary behavior. Thus, a conclusive proof that the one model we have is inappropriate will not invalidate the whole class. We need a sharper tool.

It has, in fact, been suggested many times that this or that aspect of cloud evaporation models helps to understand certain troublesome characteristics of real supernova remnants. But these claims have, to my knowledge all proven groundless when investigated thoroughly. (If you want more details, consult Jeff Hester.) We have not been looking at the question correctly. What is needed are very good models for what we should expect to see, INCLUDING THE SCALE ON WHICH IT SHOULD BE FOUND, and then the identification of specific clouds which are being stripped or evaporated within remnants, showing the predicted (spatially resolved) characteristics. There is a framed $5 bill in Mexico City waiting for the discoverer of the first interstellar cloud shown conclusively to be evaporating. Most days I expect it never to be claimed. If one such cloud is eventually found, the next step will be to demonstrate that the process is sufficiently general and active to load individual remnants with several hundred solar masses of hot gas.

5.3 ARE MODELS OF THE ISM WHICH DEPEND ON MASS TRANSFER FROM CLOUDS IN SNRS CONSISTENT WITH THE HIGH STAGE ION CONTENT OF THE GALAXY?

My impression from making simple models of the high ion content of a Galaxy in which a large component of hot gas radiates the supernova power, is that any such model will contain much larger quantities of high stage ions, notably O VI, than are observed. The discrepancy is 1 to 2 orders of magnitude, and is much worse yet if clouds are evaporating or hot gas condensing (except possibly in fountain models). (Jon Slavin and I are preparing a paper to document this more carefully.) And yet, there are still those who like to think that much of the interstellar volume is filled with a hot component. I would like to suggest that these adherents make their own independent calculations of the high ion content, and show how it can be consistent with the observational data. Perhaps, with a strong incentive for success they will be able to find something Jon and I have overlooked.

It is still possible that very high temperature gas ($T > 10^{6.3}$ K), with very little x-ray emissivity or high ion content, fills much of the galactic disk, but this is far from the regime of existing models and is not a territory that has received adequate theoretical attention. If it is present, its capacity to evaporate clouds must be severely restricted.

5.4 Is the FUV Sky Alight with the Boundaries of Slavin Bubbles?

Slavin's [29, 30] modeling of the complete evolution of SNRs in a warm intercloud component found that supernovae would not easily disrupt the medium into a froth of hot gas and embedded clouds. He further showed that such remnants might occupy 5 to 15% of interstellar space, and within their boundaries contain the quantity of high stage ions actually found in the interstellar medium. The subsequent reanalysis of the O VI data [28] showed that Slavin's results were also consistent with the frequency of encounter of O VI features. It is the only model ever constructed that is consistent with the data. (It would be nice to have similarly detailed models for superbubbles.)

The applicability of this model, however, depends on the existence of a pervasive warm intercloud phase which often seems to hide from our attempts to find it (see below). BUT THERE IS ONE CONCLUSIVE OBSERVATION THAT COULD SETTLE THE MATTER. Slavin has told us what the relic population of SNR bubbles would look like to a far ultraviolet (FUV) telescope. We really need to commit to looking. A large field of view telescope with good spatial and velocity resolution would be ideal.

5.5 Are Low Column Density Lines of Sight Really Empty?

It is my impression that when absorption is measured against background stars along unusually low total column density sightlines, there are unusually small numbers of clouds that are themselves relatively low in column density [27]. We should finally have had a good clean look at the pervasive warm intercloud medium commonly assumed to be the only alternative to hot gas filling most of the volume. But its presence isn't noticeable. So, the first thing I want to know is whether the intercloud gas could have been seen if it were there. My best guess is that it should have an average density of 0.15 cm^{-3}, a temperature of a few thousand degrees, and an electron density of roughly 0.01 cm^{-3}. It probably would be concentrated into velocity features just from the waves present within it (velocity crowding). Would it be possible to see this material on kpc sightlines within the disk?

My second question: if the warm gas could have been seen but is not present, are there sightlines with both no intercloud warm gas and extremely small quantities of very high ions such as N V and O VI? If it is possible to rule out significant volume occupation by both warm gas, and hot gas below some meaningful temperature (e.g. 10^6 K) and above some meaningful pressure (e.g. $p/k \sim 3000$ cm^{-3} K), then we can give more serious attention to possibilities with very high temperatures, having nothing to do with models existing so far, or those with nothing present at all. It could prove a major turning point in our quest for the volume filling component.

5.6 Are the Extremely High Density HI Features Real and, if so, Are They Magnetically Confined?

Time varying column densities of order 10^{20} cm^{-2} with size scales of roughly 10^{15} cm imply HI densities of roughly 10^5 cm^{-3}. With temperatures of, say, 40 K, the implied thermal pressure is roughly 3 orders of magnitude higher than "normal". And the students of these regions tell us they are common, containing roughly 10% of the total atomic hydrogen in the Galaxy [16]. Can the observations have somehow misled, perhaps by some characteristic of scintillation's path shifting? Or are these structures real and, thus, something we need to understand as a key feature of interstellar normalcy? If they exist, how are they stabilized against their enormous thermal pressures? My recollection is that gravity doesn't work, there being more gravitating objects needed than stars to associate with. Of course, if the dark matter consisted of macroscopic mass concentrations, perhaps it could be done. Alternatively, maybe the HI is in linear structures stabilized by the tension in a twisted magnetic field, flux ropes. We need to know. The required field strength is in excess of 100 microGauss.

5.7 Is the Filamentary Structure Seen in the Cirrus and Elsewhere a Pervasive Feature of the Interstellar Mass Distribution?

Filamentary structure in the ISM is certainly not confined to the cirrus, as anyone looking at pictures of the Pleides or near the Horsehead Nebula is aware. Yet, for some reason, we do not seem to carry the impression that the bulk of the ISM is arranged like mats and strands of "Angel Hair", glass wool once used to decorate Christmas trees. Is that because it does not have such an arrangement, because we do not have the tools to characterize it, or because the filamentary portion is a low contrast structure in a much smoother distribution?

5.8 Is the Filamentary Structure of the ISM Due to Magnetic Guidance (as Perhaps in Auroras), or Even to Mass Concentration in Flux Ropes (as Perhaps on the Solar Surface)?

The filamentarity may have nothing whatever to do with a magnetic field structure, but I strongly doubt it (even though it presumably does not in the Earth's atmospheric cirrus); so I will bet at least on differential mass loading of flux tubes. But what I really wonder is to what extent those flux tubes are twisted, and to what extent such twist would complicate attempts to study the field's role via, for example, polarization of starlight studies. A Wisconsin pundit on polarization and the Pleides has been heard to say that he thinks the striation is not due to magnetic effects. Is he right?

5.9 ARE THERE EXAMPLES OF SUPERNOVA REMNANTS OUTSIDE OF SUPERBUBBLES WHICH APPEAR TO HAVE EXPLODED IN EMPTY REGIONS, AS PERHAPS THE CRAB, BUT FOR WHICH WE CAN SEE THE SUBSEQUENT INTERACTION WITH THE WALLS OF NEIGHBORING INTERSTELLAR MATERIAL, THE "SPLAT"?

It may be extremely difficult to know whether a given SNR is within some unidentified ancient superbubble, or a generic empty portion of the ISM. In addition, it is likely that the observability of the ejecta/wall interaction will probably be very sensitive to the initial distance to the wall. Perhaps it would be better to explore the SNR catalogs for the probable numbers of missing members in the "middle aged" category. There may be even better tracers of emptiness than SNRs, but what?

5.10 ARE THERE REGIONS OF THE SOFT X-RAY BACKGROUND WHICH SHOW THE SPECTRAL SIGNATURE OF CONDENSATION FLOW TO A COLD WALL AND CAN THE SCALE BE USED TO ESTIMATE THE EFFECTIVE THERMAL CONDUCTIVITY IN THE CONDENSING GAS?

To address this question, we need both improved spectral emissivity code and a diffuse X-ray spectrometer capable of telling us specifically what spectral lines are responsible for the flux. Given those, there appear to be some directions where we may be looking tangentially through the radial emission profile of the SXRB, and may be able to see the ionization and temperature structure. The details of that profile depend on the thermal conductivity, giving us a measure of it.

5.11 IS THE ASSOCIATION BETWEEN DIFFUSE H ALPHA AND HI WHAT IS EXPECTED FROM DILUTE O STAR RADIATION FILTERING THROUGH THE ISM?

This should be settled by WHAM, the Wisconsin H Alpha Mapper, coming soon on line. With luck, it will show us the ionized boundaries of clouds exposed to the direct radiation from the O stars. In some cases the geometry may be sufficiently clear that we can tell which star is responsible and hope to reconstruct some of the three dimensional configuration of material. Intensities of the much dimmer and more diffuse parts of the velocity- and spatially resolved H alpha may then tell us a great deal about the distribution of intercloud gas.

5.12 IS THERE A SATISFACTORY EXPLANATION OF THE APPARENTLY LOW RELATIVE IONIZATION OF HELIUM IN THE DIFFUSE ISM?

Address innovative solutions to this problem to cox@wisp.physics.wisc.edu. Robin Shelton tells me that I should say something more here, end less abruptly, at least tell you why it is important. But it's time to quit and you can find the discussion elsewhere [24, 10, 12]. It's just another of those things that plagues efforts to make sense of what we can measure in any simple and straightforward way. Sometimes one can get lost in the details, not know which new observational twist is worth ignoring for a while longer. In the whole of section 5, I've tried to identify ones that we can't ignore if we want to get it right someday.

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IV. GLOBAL STAR FORMATION PROCESSES