Physics 224: Paper Discussion 2
Winter 2020

Stromgren 1939:

- Stromgren briefly discusses how the ionization volumes change for clusters, namely that the volume is proportional to the number of stars. How do typical stellar separations in clusters compare to length scales discussed here? When will this approximation break down (i.e. for cooler stars/open clusters)? Finally, can you see some reflection of the cluster distribution in how sharp the transition from the ionized to neutral region is?

- The sharpness of the ionization drop-off at the Stromgren radius is characterized by the parameter a, shown in table 1, and approximations of $a << 1$ are made to provide an analytic solution. Stromgren explains this by noting that "in the cases of actual interest, a is a small quantity". This isn't obvious to me from it's definition, since it depends on the star's temperature, the electron temperature, the hydrogen number density, and a few other factors. In particular, it seems like in cases of very low hydrogen density a may not be << 1. Is there a more detailed reason why we only care about the $a << 1$ case?

- The author states on page 9, "If a region of interstellar space is ionized by a cluster of n similar stars, close together, then s0 has to be calculated with an equivalent R equal to n1/2R. This follows immediately from equation (2) for the dilution factor w. Consequently, So for such a cluster is equal to wl/3 times s0, calculated for the individual star in the cluster. This may also be expressed by saying that the volume of interstellar space ionized by a cluster of stars close together is equal to the sum of the individual volumes that would be ionized by the stars if placed so far apart in interstellar space that the volumes did not overlap." I am having trouble seeing why this would be true. I would guess that if a stars region ¡ s0 overlapped then the total volume of this region would decrease because of some other physical processes. This in my mind has a correlary statement that the total volume in a galaxy in a region ¡ s0 of some star is constant and independent of the density. Is there any justification for this claim?

- While the answer to this question may just be "its complicated," I think it would be interesting to look at this in terms of a flux of particles coming into the star and the resulting flow off of the star. Depending on the temperature of the star and speed of the flux the resulting gas coming out of the star could even have a layered structure.

- In this paper, the analysis assumes spherical symmetry, suggesting that the ionization fraction only varies with the radial distance from the star. However, the images of real HII regions (e.g. the Orion Nebula, M42) appear highly asymmetric. What physical mechanisms lead to this incredibly common symmetry breaking and how do they affect the results of the paper?
- The derivation in this paper doesn't take into account the effects of radiation pressure on determining the size of the photoionized region around a star. Could radiation pressure have any significant effect on the size/density profile of HII regions, or is it negligible?
- I'm not 100% sure if I understand the abrupt decrease of ionization correctly. Does it mean that it should be deceasing gradually if the absorption of H atoms are not considered? And once the absorption is considered, the decrease would be more rapidly than the inverse-squared dilution effect? This paper also makes me think of the reionization in high-redshift universe, where an abrupt change of ionized and neutral Hydrogen were observed.
- The assumptions of sphericity and uniform density break down for real HII regions, but modelers (e.g CLOUDY) are now able to reliably reproduce spectra from better physical assumptions. What are some of the remaining open questions in modeling HII regions?
- Do Stromgren spheres form around other hot, massive objects (not just O and B type stars), and if so, what can they tell us about the environment they formed in?
- When the paper discusses excitation of higher energy levels in hydrogen in the ISM, it dismisses the case of excitation by electron collisions as subdominant to the other 4 cases because of the low cross-section and low energy of the electron population in the ISM. He also mentioned that even if there was a substantial population of ISM electrons with high enough energy, the other mechanisms would dominate because of the elevated temperature of the region. He also notes that those electrons would have to be so energetic that it would likely just ionize the hydrogen. Are there any other cases where this assumption doesn?t hold? Could you have high-energy charged or neutral particles, electrons or otherwise, coming from other sources that could excite the hydrogen in the ISM to higher energy states, or is this mechanism always suppressed because of the explanation provided by Stromgren?
- In this paper, it states that in the region of O-type stars we will find a sharp boundary in the ionized hydrogen. Is this phenomena true for any other type of stars? Other elements in the interstellar medium are discussed in section V, such as helium, they claim that depending on the density of helium, the ionized

region of helium will either be much smaller than or equal ton the region of ionized hydrogen in radius. how does the mass and charge respectively alter as a function of the density how these ionized regions behave? Can one have regions of phase separated ions, where there are regions of exclusively ionized element A and separately element B?

Werk et al. 2014:

- Figure 8 and Figure 9 show how N_H and N_{Si} drop off with radius, and the best fit for their slopes are consistent with each other. Naively, I would expect Si to drop off more steeply with radius (as it is produced by stars which are much more present at small radii). Does this imply galactic outflows are substantially enriching the CGM or something else?

- It seems like understanding the O VI regions are important in order to understand the total baryon fraction in galaxies. The paper says that they weren't able to fit for these parameters, what other pieces of information would be necessary in order to properly constrain a separate ionization region comprised of the O VI? Can the data presented here be used to concert with other observations to help fit the model better, or would that be infeasible given the regions necessary to observe?

- What could be a plausible explanation for the absence of O VI around non-star-forming galaxies mentioned in section 5.4? I suppose since 2014, there must have been some kind of progress

- In this paper, they report calculating 60% of the missing baryons, and claim that with some altered calculations they could raise their result an additional 20%. This seems to almost completely explain away this missing baryon in the halo problem. There are some other theories out there that I read about for this missing matter as well. do these results completely nullify other theories? If there is such a discrepancy in their results due to the saturation of the Hi column densities, how accurate are these results not just for an order of magnitude which the paper claims is above 50%.

- How could direct detection of the CGM improve our measurements of total mass? Could this be done by imaging fluorescently illuminated regions around QSOs at low-redshift??

- The authors note that "group environements significantly complicate any interpretation on the origin of the CGM..." and to that point "Galaxy environment and interactions surely play some role in the observed properties of the CGM." If a similar research effort performed on galaxies which are not so isolated would the results of that effort likely be dramatically different or provide only a small correction to the results found in this work?

- In section 3, the authors list "four key assumptions" that are used in their calculations. This study focuses on the CGM surrounding low-redshift galaxies. Would these assumptions hold up for high-redshift galaxies?

- 75% of the 44 sightlines show low/intermediate ionization, and the authors mention that they perform at K-S test that indicates no statistically significant difference between the full sample and this 75% subsample in terms of any galaxy property. So why wasn't ionization detected in the remaining 25%?

- In the discussion for this paper they begin piecing together different contribution to the total contribution to Baryonic mass from different measurements of the CGM to reach 35% of the baryonic mass suggesting that correcting for HI column density may increase this by 20%, but this doesn't seem to take into account uncertainties for their measurements as they are adding these together? Which would seem to have pretty significant effects on the the final total especially with the large uncertainties in some of the power laws they got earlier in the paper.

- Suppose that an inverse analysis is carried out: i.e., the unaccounted cosmological baryon fraction is assumed to lie entirely in the CGM. Is this a valid approach to constrain the phase abundances and density profiles of galactic halos and, if yes, how significant would the inferred values differ from those that are currently present in the paper based on its more conservative assumptions?

- Before reading this paper, I didn't know anything about CGM. It is surprising that such a diffuse and multiphase component throughout the halo could contribute to a galaxy's baryonic mass as much as the total contribution from stars and gas! What could be the mechanism that forms and supports the existence of CGM? Also, are there important relations between CGM and the dark matter halo?

- A quasar/galaxy sight line pair samples a column at a distinct point in the CGM. The authors calculate the column densities of the HI using this analysis and try to establish the baryonic percentage in the CGM compared to the stellar mas or gas mass in the galaxy (referring to the missing baryonic problem). However, on page 9, first paragraph on left they mention "...we can now characterize the physical nature of CGM without any additional model based assumptions regarding for example, its origin or underlying density profile." I find this quite strange. What I was expecting from the analysis was to use the column densities to set upper and lower bounds on the possible column densities of HI in the CGM. However, since it is not possible to determine the density profile of a CGM from sightlines, how can these be used to assert that "there is a 92% chance that log NH declines with impact parameter" (page 9 last paragraph on right). Just from an intuitive perspective, this doesn't take into account the fact that gas ejected from galaxy and that being accreted onto galaxy may result in complicated density profiles as a result of inflows and outflows. I don't understand how a sightline analysis can tell us about the baryonic percentage in a CGM without knowledge of these density profiles.