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Peer Review

Review of: "The Milky Way Radial Metallicity Gradient as an Equilibrium Phenomenon: Why Old Stars are Metal-Rich"

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Overall, I think this is a nice paper that clearly demonstrates, and then provides a plausible explanation for, the somewhat surprising absence of a difference in the metallicity gradients inferred for mono-age Milky Way stellar populations. The models are by intent fairly schematic, but in this case, that is a feature in that it helps bring out the key physical points. I do, however, have two suggested improvements in the paper, along with some more minor comments I will provide at the end.

First, it seems to me that there are some fairly basic budget questions that should be addressed with regard to the proposed equilibrium model. By budget, what I mean is the following: we do not precisely know the properties of the Milky Way in the past, but observations of similar galaxies in the local Universe and their progenitors at higher redshift give us some basic constraints on how Milky Way-like galaxies must be assembled, and it would be good to verify that the proposed equilibrium scenario is compatible with those constraints. The main ones that I think are worth checking are:

– The best-fitting model requires a fairly high-mass loading factor over the Milky Way's full formation history, including earlier on in that history when presumably the galaxy was a lot more gas-rich and much of the stellar population formed. Is this a problem when it comes to the available baryon budget? That is, in this model, the fraction of mass accreting onto the Galactic disk that winds up in stars is relatively small, $\epsilon_{\mathrm{disk}\to *} \sim 1/(1+\eta)$, and at the same time, simulations seem to suggest that the efficiency of getting baryons through the halo and into the disk, $\epsilon_{\mathrm{halo}\to\mathrm{disk}}$ is not all that high either, particularly at low redshift when stable hot halos form – just to pick one possible example

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paper, [1] gets efficiencies of tens of percent. But ultimately, these two factors determine the stellar mass-halo mass relation, i.e., $M_*/(f_b M_h) \sim \epsilon_{\mathrm{disk}\to *}\epsilon_{\mathrm{halo}\to\mathrm{disk}}$, where $f_b M_h$ is the universal baryon fraction times the halo mass. So my question is: given the small efficiency of converting disk gas to stellar mass, does this model require a halo-disk accretion efficiency that is uncomfortably high? In practice, my suggestion for answering this question would be simply to add up the total amount of material accreted onto the disk as a function of time and compare this to what might be expected from cosmological simulations. Is there any tension there?

– A second budget question is with regard to the gas fraction. The authors don't discuss it, but it seems like their model must at least implicitly make some predictions for the cosmological evolution of the Milky Way gas fraction. We know from observations that this was substantially higher in the past, e.g., see Figure 5 of [2]. Is the proposed model at least qualitatively consistent with this trend?

– Finally, what does this model predict about where metals ultimately wind up? We know from COS-HALOS and related studies that more than half the oxygen ever produced in a Milky Way-like galaxy ultimately ends up in the CGM rather than the disk[3]. Do the models presented in this paper give something consistent with that? (Obviously, the $\eta = 0$ model does not, but do the others?)

My second major comment is that the paper would be improved by doing a bit more to put it into the context of galaxy formation models. The idea that gas properties, including metallicity, rapidly reach equilibrium during galaxy formation because gas cycling times are short is not at all new; it goes back to the "bathtub"-type models of [4], if not earlier. The paper cites (at the very end) the proposal by [5] that galactic metallicity gradients are in equilibrium, but it would be good to give a bit more context to this idea, since it really is central to modern understandings of galaxy formation. In addition to the two papers just mentioned, other important ones that have contributed to the idea that galactic metal content as a function of radius is mostly an equilibrium phenomenon include [6] and [7]. There may be others too, but this is what I can think of off the top of my head. Similarly, there is significant observational evidence that galaxy metallicity gradients have evolved only relatively weakly since z ~ 2, for example, see Figure 8 of [8]. Thus, the result presented here is probably less surprising to the extragalactic metallicity community than it might be to Milky Way people. It would be good to discuss this literature a bit.

Finally, my more minor comments, in no particular order:

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– [9] appears to be an effort that is very similar to this one (reconstructing the history of radial metallicity gradients in the MW using APOGEE data), but is not cited. It would be good to discuss how the present paper differs from this one and why, on at least some points, the conclusions differ. Most notably, a major finding of the paper I just cited is that reproducing all the constraints requires some degree of preferential metal loading (i.e., the metallicity of winds is higher than that of the ISM, indicating some direct loss of SN ejecta into winds), whereas this paper seems to be able to reproduce the data without including this, and in fact comments that preferential metal loading makes the problem of explaining the non-evolution of gradients harder. What is the source of the difference?

– The idea that η could be ~1 for Solar neighborhood conditions, and that it is lower closer to the Galactic center, is presented as somewhat surprising, but it is almost exactly what simulations these days predict. Both TIGRESS/SMAUG[10] and QED[11] get $\eta \sim 1$ for their Solar neighborhood models, and the former finds lower mass loading at higher surface density. SILCC[12] also gets $\eta \sim 1$ for some of their models, though that result is more ambiguous since the value strongly depends on their treatment of some physical processes – but in any event, the normalization and radial dependence of η assumed in this model will not be at all surprising to people who work on galactic winds. It may be true, as the authors say, that many people in the Galactic chemical evolution community assume $\eta = 0$ based on the simulation papers cited (references 164 - 168), but these are all low-resolution simulations that rely on subgrid models and are not really reliable compared to TIGRESS, SILCC, or QED.

– I am a bit confused by the paragraph in the conclusion about how equilibrium models explain the uniformity of galaxy metallicity gradients. This paragraph at least implies that the existence of significant variation in the metallicity gradient with stellar mass would be problematic for an equilibrium model, but that seems wrong to me – galaxies with different present-day stellar masses presumably should have different values of the key parameter $\dot{\Sigma}_*/\dot{\Sigma}_{\rm in}$ that this paper identifies, so it seems like we quite naturally should expect variations in the metallicity gradient for galaxies of different stellar masses. Indeed, this is the whole point of [13], which is an equilibrium model that explains the mass dependence of metallicity gradients in exactly this way.

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Declarations

Potential competing interests: No potential competing interests to declare.