

Reply to the 3rd

Review of Gamborino et al., Mercury's Sodium Exosphere
by Rosemary Killen

Dear Editor,

The following is the reply to the third review. Our answers are written in blue.

We thank the reviewer for her comments and have carefully elaborated on each answer.

Best regards,
Diana Gamborino and Peter Wurz

Section 3.3 line 12:

"These particles are thermally accommodated to the local surface temperature..."

From Yakshinskiy and Madey, Surface Science 593, 202-209, 2005:

"Because the mass of Na is smaller than Si and larger than O in the SiO₂ film, a quasi-elastic binary collision can lead to backscattering of incoming Na from Si, but not from O; in comparison, more massive K would not backscatter from either O or Si. Potassium has a higher probability than Na of losing energy to substrate phonons, and being trapped."

I read this to mean that Na can backscatter without losing energy to the substrate, thus not accommodating to the surface temperature.

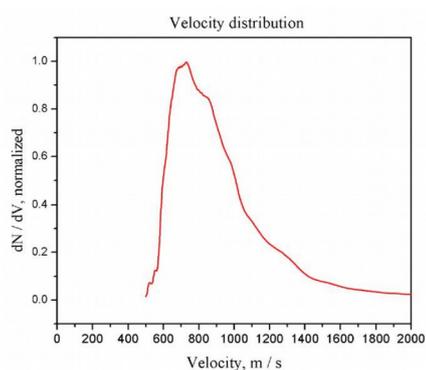


Figure 1

Backscatter from surfaces without energy loss is physically impossible, the simplest approximation used in surface is the binary collision approximation (BCA) which assumes two hard spheres interact with each other. Based on BCA, having a Na atom of mass 23 interaction with a Si atom of the surface of mass 28, one would expect a significant energy exchange given the similarity of mass. The situation is not that different for Na hitting an O atom. More sophisticated models of the particle-surface interaction invoke a more realistic potential of the surface atoms, which modifies the energy exchange between the impaction atom and the surface atoms. There is no way to do a completely elastic backscattering, and this has not been observed experimentally. Yakshinskiy and Madey say "quasi-elastic".

Why are you using a temperature of 594 K for PSD. Again I show the measured velocity distribution of Na from PSD.

"The desorbing Na is suprathermal (~900 K) with respect to a 100 K substrate (Yakshinskiy and Madey, Icarus 168, 53-59, 2004).

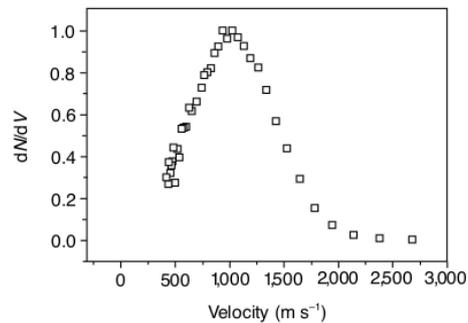


Figure 2 Velocity distribution for ESD of neutral Na from SiO₂ (0.22ML); electron energy $E_e = 200$ eV. The electron source is pulsed, and Na atoms that desorb from SiO₂ have flight times of $\sim 50 \mu\text{s}$ to the detector Ir ribbons (see Fig. 1 legend). The time-of-flight technique is highly sensitive for detection of Na atoms, with low background signal. dN is the number of desorbing atoms having velocities within the range $V + dV$.

Figure 2.

As explained in Gamborino and Wurz, 2018, we use a non-thermal distribution for PSD that has the actual surface temperature as one of the defining parameters.

Velocity distribution for substrate temperature of 250 K (Yakshinskiy and Madey, Nature Lett, 1999.)

There is clearly a surface temperature dependence but the PSD temperature is about 800 K higher than the surface temperature.

We fully agree with that, and the dependence on the surface temperature is fully accounted for in the used velocity distribution of PSD, see Gamborino & Wurz (2018).

In our previous work (Gamborino & Wurz, 2018) we presented a comprehensive statistical study of exactly the velocity distributions functions (VDFs) shown by the reviewer in Fig. 1 and Fig 2., which are reported by Yakshinskiy and Madey (1999, 2004). In that work we demonstrated that: “[..] *the Maxwell-Boltzmann distribution is neither statistically nor physically adequate to describe non-thermal processes such as ESD and PSD*”, if one wants to model a thermal distribution, one would need to arbitrarily choose a very high temperature to fit the observed VDFs, like Yakshinskiy and Madey later did, but with no physical interpretation.

Also a temperature of 4000 K for MIV is higher than usually inferred but not completely out of line.

We show in Figure 4 a range of temperatures for modelling MIV and find that the agreement with a temperature of 4000 K is satisfactory, perhaps a temperature somewhere between 3000 K and 4000 K would be possible.

Section 5.

lines 13 - 14: "Using the Chamberlain theory implies that the only way to increase the particles' characteristic temperature (and thus able to reach high altitudes) is by increasing the surface temperature."

I disagree with this statement. Simply a source temperature. The Chamberlain exosphere is only going to give you an estimate of what is going on with a surface-bounded exosphere but you can, (assume any source temperature you want. The source can be IV or PSD or whatever. Also if you are using MC as well as Chamberlain you can assume any value of sticking and any value of thermal accommodation 0 and 1. Thus the MC model does not have to rely on a thermal source at the surface temperature or any given temperature or any velocity distribution. According to your paper you have a MC model.

We strongly disagree with this point, and a lot of research and experiments (see references in Introduction, see Gamborino & Wurz, 2018) support that different release mechanisms produce different energy/velocity distributions of the particles released from a surface/exobase.

On the other hand, the core of the MC model is to repeatedly and randomly sample the different energy distributions associated with the different release mechanisms to obtain a numerical solution (in our case we want to determine the density vs. altitude). In fact, any Monte Carlo model relies solely on the given probability distributions.

Line 1 page 13: "The Chamberlain model works fine only for an exospheric population in thermal equilibrium with the surface temperature."

This is incorrect. Actually it works fine as long as the atoms do not exchange energy with the surface. According to Yakshinskiy and Madey they do not thermalize to the surface temperature, so keeping the different sources separate works fine.

We have to disagree with the reviewer, quoting Madey et al. JGR 1998: *A low-energy component characteristic of the surface temperature may arise from scattering of "hot" atoms in the surface regions of the porous regolith, or by thermal desorption.*

Chamberlain's theory is clear about the source of exospheric particles, stating that "[...] the controlling factors [of exospheric particles] are gravitational attraction and **thermal energy conducted from below**" (Chamberlain, PSS, 1963). In that same work, they also clearly state that below the exobase, "[...] collisions maintain a complete Maxwellian distribution of velocities".

Furthermore, PSD is not a thermal process but a photon of sufficient energy that breaks the binding of an atom or molecule with the surface, i.e., an electronic process. Therefore PSD should not be treated like a thermal

process. Chamberlain theory assumes a Maxwell-Boltzmann distribution at the exobase, i.e., it works only for thermally desorbed particles, not for PSD. Therefore, one should not use this theory to describe atoms released via PSD.

Page 13 line 8: "The PSD TCD profile does not fit quite as well to the observations and it has to be scaled with a factor of 4×10^{-4} to match part of the tail." Given that you used the same temperature for PSD as thermal desorption I don't see that you have two different populations.

We use the same surface temperature for PSD and for TD but we **do not** use the same velocity distribution for both mechanisms, therefore they **do** produce different populations. You can see that in Figure 4: TD population is the dashed-black curve, and PSD population is the vertical-dashed-grey curve, both for the same temperature.

line 9: delete Uzcanga.

This typo appears only in the first manuscript version submitted on 20.09.2018. We kindly ask the reviewer to read the latest manuscript version with corrections (submitted 29.04.2019). This typo is not there anymore.

Line 14: "Moreover the TCD from SP falls off much less with altitude than the observations."

Perhaps you should change the binding energy in the equation.

Given that observations correspond to the subsolar point, we expect Sputtering to be inactive and not an important source. Moreover, the surface binding energy is not a free parameter to be used as a parameter to fit the data, it is a material constant and thus given.

Equation 6: I think that each source needs to be scaled by the source flux times the time per ballistic hop times the number of bounces per unit time. In other words, the ambient source rate times the lifetime per bounce times the number of bounces equals the sum of the source terms times their respective bounce time times the number of bounces in the same time...

It is not clear whether you have done this.

As described in section 3, our Monte Carlo model considers that once a particle falls back to the surface, it will be thermalized and no longer considered in the numerical integration of that particular trajectory. In case of Na, this particle becomes part of the ambient population, which subsequently will be released by thermal desorption. That is explained in detail in the manuscript.

line 27: losses assumed are ionization and gravitational escape. What about loss to high energy activation sites on the surface?

It is not clear to us what the reviewer means by "high energy activation sites", and a literature research did not provide a suitable publication. What is clear

is that the thermal desorption from the surface is driven by thermodynamics, and thus the energies available to a desorbed atom or molecule are defined by a Maxwell-Boltzmann velocity distribution.

6. Conclusions:

The conclusions of this paper are that TD dominates governed by a surface temperature of 594 K. It needs to be made clear that the velocity distribution assumed for PSD was the same as for thermal vaporization and is much less than the measured temperature for PSD. Therefore no PSD was used in this model. That needs to be explicitly stated. No conclusions can be reached about whether there is PSD or not if PSD was not included in the model. Similarly sputtering is probably not an important source at the equator where these data were taken. This needs to be clearly stated that this is not a global model. The temperature assumed for IV is probably too high to be realistic and a lower temperature IV plus a small amount of sputter would probably match just as well.

Firstly, we do not use the same velocity distribution for TD and for PSD, and this is clearly stated in the manuscript. Please see Eq. (3) in the manuscript, this is a Weibull distribution, which is a non-thermal, long-tailed distribution adapted to the PSD process (for details see Gamborino & Wurz, 2018). For TD we used the thermal Maxwell-Boltzmann distribution, as stated in section 3.1.1. These distributions are not the same.

Secondly, we do include PSD in the model. Equation (3) is the velocity distribution used in our MC model and the results for TCD are shown as the vertical-dashed-grey curve in Figure 4.

Thirdly, we do state that SP is not an important source at the equator of Mercury. Perhaps this was not clear in the first version of the manuscript, but it is clear in the last manuscript version (please see version submitted 29.04.2019). In this version, you can see that we have stated clearly several times that SP is not important source near the equator, see the following places: page 7, line 7; page 12, lines 2-5; page 13, lines 9-13.

Finally, Figure 4 shows how well MIV modelled by 4000 K fits the observations, and also it shows a range of temperatures to understand if there is a better temperature for modelling MIV. If there is, it would be not far below the 4000 K we used in our calculations.