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

Section 3.3 line 12:

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

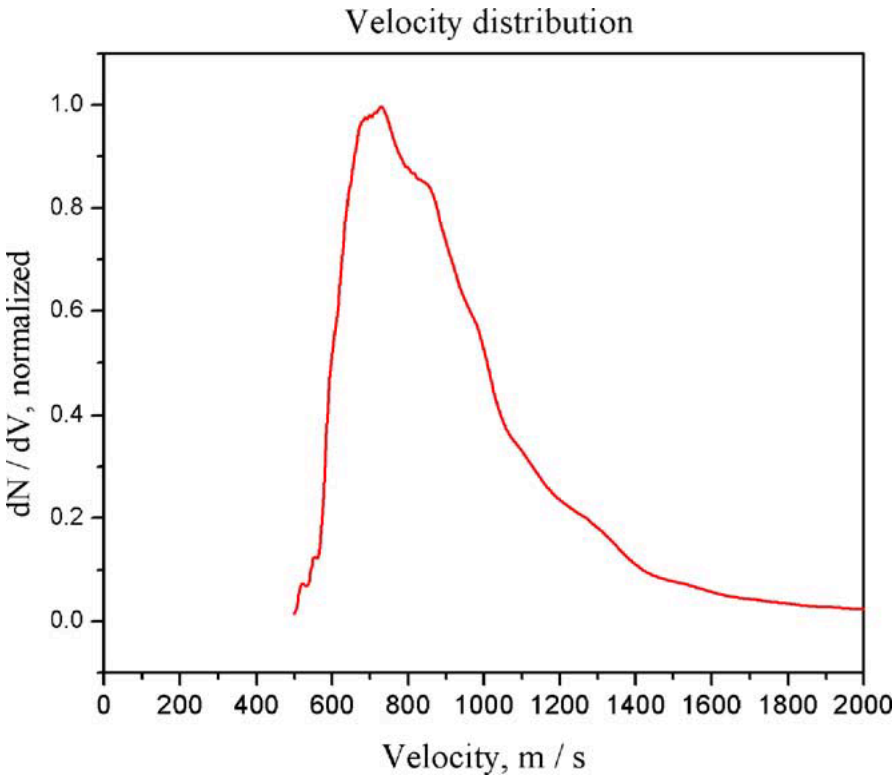
From Yakshinskiy and Madey, Surface Science 593, 202-209, 2005:

Because the mass of Na is smaller than
Si and larger than O in the SiO_2 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:

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).

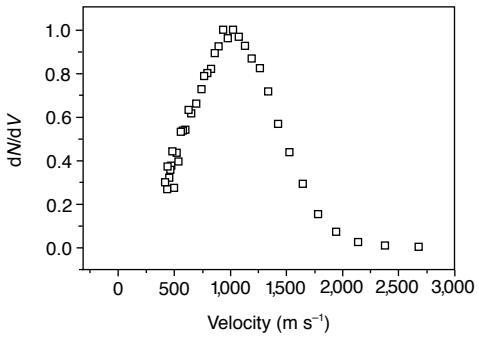


Figure 2 Velocity distribution for ESD of neutral Na from SiO_2 (0.22 ML); electron energy $E_e = 200$ eV. The electron source is pulsed, and Na atoms that desorb from SiO_2 have flight times of ~50 μs to the detector Ir ribbons (see Fig. 1 legend). The time-of-flight technique is highly sensitive for detection of Na atoms, with low background signal. dN is the number of desorbing atoms having velocities within the range $V + dV$.

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.

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

Section 5.
lines 13 - 14: "Using the Chamberlain theory implies that the only way to increase the praticle's characteristic temperature (and thus able to reach high altitudes) is by increasing the surface temperature."

I disagree with this statement. You simply assume 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 between 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.

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.

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 $4x10^{-4}$ 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.

line 9: delete Uzcanga.

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.

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.

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

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.