

LUGANO AND THEORY

Lugano report subtle details, not identified in the text of the report, have been extracted from the report's SEMs and EDS the large Lugano fuel particle 100 microns to be a sintered Ni-Li-Al conglomerate that has come from heating Vale T-255 Ni powder with LiAlH_4 in a prior process

The large Fe_2O_3 particle in the fuel will dissolve in the molten Li-Al-H above about 800C. Rossi appears to have been adding the Fe_2O_3 to his catalyst mix since his early eCat days. I believe his early eCats used nano-metric Fe_2O_3 , but it also appears that Li was present in his original eCat fuel. The original eCat did not have a solute to distribute the Fe, so it had to be nano-metric. However, in the Lugano HotCat, the hot molten Li-Al-h will dissolve the Fe_2O_3 , so it does not appear to be a requirement to be in a physically small form.

When the LAH evolves some hydrogen at about 300C, the oxide will quickly be reduced/removed from the Ni and the Ni particles will sinter where ever they touch. At the same time, the LAH is decomposing. By 700C, the LAH has decomposed into liquid lithium hydride and liquid aluminum hydride. These will readily wet as a film to the clean Ni surface. This is valuable because the lithium hydride is an ionic hydride. That means that in the liquid lithium solution, the hydride exists as hydrogen anions (H^- ions). These are what Piantelli implicates in Ni-H LENR, specifically the hydrogen anions.

Also, the more Fe_2O_3 that is present, the greater will be the thermite exchange reaction between the aluminum and Fe_2O_3 . This thermite reaction is a brief exothermic reaction and will result in pure Fe and Al_2O_3 . So, at the temperatures of the LENR, the Fe_2O_3 has long since been fully reduced to Fe and is dissolved in the molten Li-H and Al-H. The Al_2O_3 will precipitate.

The MFMP has discussed increasing the Li/Al ratio. Handling pure lithium is difficult. I suggested that we use LiH instead of the pure metal to adjust the Li/Al ratio. LiH is still pretty volatile, but not nearly as difficult to handle as pure lithium metal

In Rossi's early devices, Li was present and Fe was present. One of the compounds that could provide this is lithium iron phosphate, a common battery material. Rossi may have ball milled ordinary LIP to small particle size and mixed it with the Vale T255 powder he uses. I am guessing that the LIP is mixed onto the surface of the Ni (as I reported in the paper I published about putting the Fe_2O_3 powder onto the Ni). Adding Li to the Ni is hard unless it is done with nanopowder in a non-metallic form. Once mixed onto the Ni particles, and heated in the presence of H_2 , the oxide will be stripped from the Ni surface and the Li that is there will wet to it, producing a film of Ni on the surface of the Ni powder.

The Li must be added to the Ni after the oxide is stripped from the Ni so that the Li can wet as a film on the Ni particles. This can be done in a pre-processing process in a furnace in a crucible, but must be done after the Ni powder is heated in H_2 to >250C and the Li (or LiH) applied in liquid form before the Ni is exposed to oxygen in the atmosphere.

As Piantelli teaches, the metal grains are the cluster size that is important. The fine powder particles each have metallic grains - they are not each single crystal. According to Piantelli, if the grains are too small or too big, LENR does not occur. The powder exposes the greatest possible number of grains to the Li film and to the H₂. When Piantelli creates LENR with a rod, only the surface area of the rod is active. He gets watts of excess heat while Rossi gets kilowatts of excess heat.

The reaction with liquid LiH (from decomposition of LAH) makes sense to me. The earlier lower temperature eCat fuel is harder for me to understand. The Li and Fe on the Ni must be at nano-scale size. This reduces the melting points of these particles by a factor of 2. Even still, it is hard to understand how LENR occurs at such low temperature with Rossi's fuel. Piantelli also operates at low temperature and his fuel is only Ni and hydrogen (when he adds Li, it is in a tube around the Ni rod and is a heat enhancer).

Stainless steel is a well known proton conductor above 600C. The conduction at 600C is more than 1000x what it is at 300C.

The Li that forms a skin on the Ni surface is really lithium hydride, not pure Li. I believe the lithium hydride presents H⁻ (anions) to the surface of the Ni. These H⁻ anions are what Piantelli implicates in the reaction. The Ni does not become "loaded" with H, but each crystallite of the Ni surface captures an H⁻ anion into the crystallite. See Piantelli's patents and papers. Piantelli says that this H⁻ anion enters a Ni atom and approaches the nucleus as a composite fermion. I believe he describes 2 branches of the reaction - transmutation of the Ni and ejection of high energy proton. The high energy protons are absorbed in the Li causing an additional exothermic nuclear reaction. However, Piantelli sees excess heat with just Ni and H₂. He only adds some lithium in a tube around his Ni rod to increase the heat output.

The Li is good for neutron capture, but you need large bulk to capture high energy (MeV) neutrons (as they are typically created). This is part of the hot fusion system. There is no evidence of high energy neutrons coming from LENR. If LENR creates low energy (thermal or less, 0.025eV neutrons), then they will react with any nearby metal.

Al₂O₃ is one of the most stable oxides in existence. What everyone is using is "alumina ceramics". There are many different grades of alumina ceramics. Typically they are described in terms of % theoretical density if the ceramic were entirely Al₂O₃. These ceramics are generally made from pure Al₂O₃ particles mixed with various silicates that allow the ceramic to form at lower temperature. The ceramic Parkhomov used was probably what is known as "mullite", an alumina ceramic with about 40% silicates. These silicates are glasses and melt at a much lower temperature than the Al₂O₃ particles. The high grade alumina goes from 96% to >99% Al₂O₃. They must be fired at a higher temperature and are harder to make and have them straight and meet specified dimensions.

The Li does NOT attack the Al₂O₃ - it attacks the silicates. Also, Parkhomov proved that it was not the alumina that was the center of the reaction by running experiments where the fuel was in contact with the ceramic and other where the fuel was in a stainless steel can inside the ceramic. Both worked, so it is unlikely that it is a reaction with the ceramic.

The big heat issue is getting the heat OUT of the Ni so that it doesn't melt the nuclear active spots (Storms' NAE). How this heat gets conveyed away from the reaction site is a key discovery to be made in LENR. If the reaction occurs in nano-scale spots, then the carrier of the heat cannot be phonons because the phonon wavelength is so short the heat would be absorbed too close to the spot and the NAE would evaporate before much heat is conducted out of the reaction spot. I believe the carrier of the bulk of the heat is low energy gamma in the 0.5-5 keV energy range or in thermal neutrons (not high energy neutrons). The range of absorption of each of these carriers would be such that little would leak out of a reactor, and the heat would not be absorbed very close to the NAE.

It could be that ^{62}Ni does optimize output. If I were Rossi and I was trying to optimize my reactions, I would certainly test with isotopically pure samples - I am sure he did. Perhaps he found that ^{62}Ni is best. Still, it is so expensive, it would be better to just put on more of the natural Ni (3.6% ^{62}Ni) to get the power needed.

Hydrides are odd chemical systems. When the hydride gives off H_2 , it doesn't give up all of its hydrogen. So, over temperature, the amount of the H that is in the Li will go up and down, but it will never go to pure Li. Whenever Li and H are present, there will be some LiH in the Li.

We have no real idea of the value of ^{62}Ni in the reaction. It could act as a catalyst and perhaps only a small amount is needed to enhance the reaction rate of a much greater content of natural Ni. You can speculate any purpose for it. Rossi seems to think ^{62}Ni is valuable but it is not clear that he could ever base a product on ^{62}Ni . So, it would seem that his reactors don't need to be filled with ONLY ^{62}Ni or that product direction would be not valuable.

I once had colleagues working on a field emission flat screen display technology. They were experimenting with diamond films as the cold electron emitter and experiments seemed to show strong field emission only from one crystallographic facet of the diamond film, but they did not know why. Was it something special about that crystal face of the diamond that emitted electrons? When they got higher resolution SEM images, they found that the electrons were being emitted by carbon nanotubes that preferentially grew on that crystal face of the diamond. Both the diamond and the nanotubes were carbon. Carbon can take many forms and it is not clear whether the role of carbon is: none, carbon as an insulator in the grain boundaries, carbon nanotubes that form on the surface, or what? We have no evidence that carbon plays any role at all.

Also, for the Ni particles being used (AH50, or T255), these are produced from nickel tetracarbonyl - an extremely poison volatile liquid with a Ni atom surrounded by 4 CO molecules. When the pure Ni is precipitated in the Monde process, the Ni is left somewhat contaminated with a small amount of residual carbon. Who knows whether this important?

One of the reasons that these catalysts are of interest is that Rossi comes from a petrochemical processing plant where the catalytic breaking down of hydrocarbons would have been familiar to him. In analysis of his early eCat ash, the components are found of something like a Shell-105 catalyst. So, it is not a big leap of faith to presume Rossi would

have tried this type of catalyst in his low temperature eCats. This Shell-105 catalyst is mostly Fe_2O_3 with about 8% of potassium and maybe a few percent of Cr_2O_3 . This is also known as a Fischer-Tropsch (F-T) catalyst. Alan Goldwater has ordered 1 kg of an F-T catalyst from China and will be distributing samples to experimenters once it arrives. I believe this would be easy to make beginning with an Fe_2O_3 nanopowder, mixing the other components, and then sintering in air at about 650C.

It is interesting to see Holmlid's claims that this same type of catalyst is useful in forming hydrogen Rydberg Matter - BUT - there is not evidence at this point that Rydberg Matter is involved in Ni-H LENR. Note that Piantelli does not use any of these catalysts. Piantelli uses just natural hydrogen gas, and a nickel rod prepared to have small surface grains.

There are plenty of forms of Ni, but we have seen a jar of Vale T255 on the shelf in one of Rossi's videos. Vale T255 is a carbonyl Ni that has connected carbonyl grains that are optimized for batteries. If you look closely at the SEM images of particle 3 in the Lugano report and look at the small loose particles around the big Fe-rich particle, you will see that they are Vale T255 particles matching the Vale advertisement SEMs exactly.

TECHNIQUE ECROUS BOULONS

I am using a heater wrapped around an alumina tube, and then the separate reactor tube is inserted inside the heater tube. This is a lot like the inside of the Rossi Lugano HotCat, and a lot like Parkhomov's latest AP2 reactor. My reactor tube is a "closed one end" alumina tube. The other end is outside of the heater by 150mm and is terminated in a Swagelok fitting. From this Swagelok fitting I have a 1.5mm OD x 0.2mm ID stainless capillary tube that goes to a "zero dead volume" header and to a pressure sensor, a storage cylinder, and a valve. I can vent my gas to atmosphere slowly or vent it into a pre-evacuated sample cylinder for later gas analysis. The whole assembly has only a tiny dead volume (few mm^3) and so it could be subject to very high pressure. I may have to vent the evolved gas just to prevent explosion. Of course, as I also said, I may want to vent to low pressure just to get the LENR to happen.

It would be quite dangerous to use a completely hermetic sealed tube with no dead volume. The pressure has the capability of reaching 400 bar, but we haven't seen more than about 150 bar. I will be able to record the pressure to 300 bar. Parkhomov's leaky system never saw more than about 5 bar

There is an important dissociation stage of LiAlH_4 at 180-200C. During heat-up, the system should be paused at this temperature for 1-2 hours. The next important stage is at 680-700C where both the LiH and Al are becoming molten. Beyond that, I am not sure.

You cannot put the pressure sensor in temperature that high. I have a Swagelok connector on one end of my alumina tube (the other end has been molded closed when the tube was made). From the Swagelok connector, I have a small 1.5mm OD stainless tube 8" long that goes to a low temperature section where the pressure sensor is attached. The pressure sensor I use came from DigiKey - it is **DigiKey part number 480-2550-ND** and is an electronic pressure sensor good to 5000PSI with output of 0.5-4.5V. This goes into my DAQ and is recorded at every sample along with the temperatures, voltage and current input, and

radiation counts. I have no real concern over the temperature - by the time the gas gets to the sensor it will be cool enough to not cause a problem.

Are you milling with steel balls? Usually I mill with alumina balls in a ceramic or rubber jar.

I need your help to know how you avoid liquid Li to plug H2 duct?

I use a closed-one-end stainless steel tube that is 6.35mm OD (1/4") x 200mm long. First goes in the fuel near the closed end for about 50mm. Then I put a ceramic felt disk that is about 2mm thick x 6mm diameter - cut from a sheet in a circle using a hand punch that is designed for punching holes in notebook paper. This is followed with ~25mm of alumina wool, that is not packed - just lightly tamped in against the ceramic felt disk to hold it in place.

Second point, if you used stainless steel tube as powder box , did you succeed to use it for another run?

I specifically chose the stainless steel tube for my experiments for a single use only. It will be cut open to extract the ash after completion. Each tube only costs \$3.60. If I have a successful experiment for which I want to have isotopic analysis, I will send the whole tube to the lab for them to cut open - this will help guarantee that the sample has not been altered.

I am having to re-build my dry glove box because it was not staying at low enough humidity. I am building a dessication system with semi-automated regeneration. I hope this will do the job.

The open side of your stainless steel tube is connected by a swagelock, i think ?

Yes. I have a Swagelok adapter to go from 1/4" tube (the reactor tube end) to 1/16" plumbing tube.

Between them, could you put inside something as a alumina plug drilled (H2 path) to avoid that too much temperature reach you H2 manometer ?

This is not needed. I put alumina fiber wool (loose) as a barrier and support for the ceramic felt disk. The ceramic felt is zirconia fiber and is permeable to gas but will block the liquid. This felt comes in thin sheets and I punch out small disks to fit in the ID of the tube.

you buy directly some one closed stainless steel tube ? do you have references ?

I use these inexpensive thermocouple shields: <http://www.brewershardware.com/8-Stainless-Steel-Temperature-Probe-End-316SS-PE6-3161.html>

About your ceramic disk , doesn't he blocks to much fast H2 pressure variation, if you do ?

It is a ceramic fiber felt (zirconia fiber) that is permeable to gas.

For a glove box you haven't choice to inject Argon gaz during powder preparation ?

If it was just for one session of powder prep, I could evacuate the glove box, add Ar and repeat several times to get to 5% humidity or less. This would probably take about 30 ft³ of Ar for each session. This would empty a big bottle of Ar in about 3 sessions. I would have to rent the bottle and drive for 40 minutes each way to have it refilled. I don't care too much about the air, but I want the humidity at 5% or less. The system I am working on will continuously dry the air, and that will keep it low humidity for storage at the same time. I won't incur any rental charges (\$25/month) for the gas bottle.

I think your Swagelok adapter must remain at a certain distance from the part heated ?

The tube is 200mm long and the fuel is in the 50mm at the end. When the fuel is centered in my heater tube, only the last 75mm is directly heated. This leaves the Swagelok connector about 125mm from the heated area.

SUPPLIERS

I do not have LiH yet. I have a quote to get 1kg from China for \$400. That is a lot of powder and I only need about 20-50g maximum. If you can get LiH from Sigma, I would go that route.

I just got a budgetary quote for 62Ni from Trace Sciences for 1 gram of 62Ni with purity of 96% - \$11300.00 USD. I think I would like to be able to make this - 250x more val

CHINA AND OTHERS REPLICATIONS

In Jiang's case, he is baking water out of the system. However, neither Rossi nor Parkhomov did this. BUT, one thing Rossi did was to heat the reactor to very high temperature before adding the fuel (the Lugano dummy test). So, the water would have been driven from the reactor even though it had not been evacuated. The fuel powder Rossi added would not have been dried or degassed. There is no evidence that Parkhomov did anything to evacuate or dry his powders. He even mixed them in the open air.

Jiang's reactor dead volume looks pretty large - I would say in the 100's of cm³. However, I don't have a number from him.

From my meeting with Piantelli, it appears his primary surface treatment of his Ni for optimizing his Ni-H LENR is to get the surface metal grain size that he wants. He believes that the each grain should have a particular range of atoms for his proposed condensate behavior of the metal grain. As I understand it, he is not trying to grow nano-scale features, but rather, to grow a particular metal grain size. Piantelli's surface treatment is not electrolytic as far as I can tell - I saw no signs of electrochemistry in his labs. I believe he is using E-beam evaporation onto a cold Ni surface to produce the grain size he wants. But, all

of his original rods were just plain Ni rods, essentially straight from the vendor. He did not add a surface coating or any treatment in his original LENR.

As I understand it, he is not trying to grow nano-scale features, but rather, to grow a particular metal grain size. Piantelli's surface treatment is not electrolytic as far as I can tell - I saw no signs of electrochemistry in his labs. I believe he is using E-beam evaporation onto a cold Ni surface to produce the grain size he wants. But, all of his original rods were just plain Ni rods, essentially straight from the vendor. He did not add a surface coating or any treatment in his original LENR.

I am communicating with Jiang with a couple of questions.

I thought it was interesting that in the second round that he was able to treat his fuel in-situ, and restore it to be capable of showing the reaction. I found it surprising that his treatment did not involve pulling a vacuum. **I would recommend cycles of heating in H₂, and then vacuum, then H₂, then vacuum.** He also was treating his materials at very high temperature. Perhaps the H₂ reacted with the oxides present in the system, made them into H₂O vapor, which allowed them to be transported to the liquid aluminum, where the Al oxidized and became Al₂O₃ and getterted out the oxygen.

In his first round, the H₂ pressure falls through apparent absorption. This is probably absorption into the Li, not the Ni, but how can we say for sure. We know Li forms a reversible hydride, and we know the Ni lattice does not want to absorb any H.

The dead volume is large in his experiment. All of the SS tube that has the fuel vessel inside (which is not sealed) is dead volume.

In the second round experiment, the pulse of pressure around 14:10 is a manual addition of pressure to the system from a gas bottle. When it suddenly drops at about 14:30, it was a manual venting of H₂ back down to about 1 bar. When this is vented, the expansion of gas out of the fuel box caused the sudden drop in temperature of the fuel measured by T₂. When the power is turned off and T₂ crosses T₁, that looks like heat being generated in the fuel.

To fix the problem that his second round experiment did not seem to behave like his first round, he applied high pressure H₂ and heated to high temperature. He let it soak like this for a long time, then he pulled a vacuum. After that he re-introduced high pressure H₂ for a short while and then regulated it to low pressure (partial vacuum of H₂) and the reaction started.

I think we have seen that it is important for the pressure to be in the 0.3-0.5 bar (absolute) range during operation. Parkhomov was just lucky that his devices operated in that range by leaking.

I do not have information about what powder Songsheng and Zhanghang use in their experiments. It could be the carbonyl (Monde process) Ni powder. It is well known that Rossi used Vale T255 in his earlier experiments and this same powder can be seen in the SEM images in the Lugano report.