In response to Göran Ericsson and Stephan Pomp's comments on the 3rd party report,

" Comments on the report "Indications of anomalous heat energy production in a reactor device containing hydrogen loaded nickel powder." by G.Levi, E.Foschi, T.Hartman, B.Höistad, R.Pettersson, L.Tegnér, H.Essén."

Intro

The criticism in the report has to the most part been vented by Göran Ericsson and others in miscellaneous fora since the 3rd report was first released to arXiv.

I would like to list all points of contention and argue them one by one, but first let me interpret the purpose of the 3rd party report as described in the text:

*"**The scope** of the present work is to make an independent test of the E-Cat HT reactor under controlled conditions and with high precision instrumentation. It should be emphasized that the measurement must be performed with high accuracy and reliability, so that any possible excess heat production can be established beyond any doubt, as no known processes exist which can explain any abundant heat production in the E-Cat reactor."*

In other words, this is a black box test. Purpose is to determine the energy going in and the energy going out as accurately as the situation allows. Hence any probing into the black box, is out of scope of the paper. Looking closer at the scope, there is electrical energy going in, and thermal energy going out. Looking at the paper, and understanding the contention in the past, the thermal output energy is what need to be addressed most deeply, as it will be an area of contention. Hence the paper deals with that very closely. When it comes to the input, it is AC current from the mains, which is closely checked.

I **will apply the above scope definitions in my commentary.** To me there is a lot of criticism towards the report, that does not really apply for the simple reason that it is comments on things really outside the scope.

General comments

** A description, motivation and discussion on the choice of measurement methods and procedures.*

What would such a section contain? The black box setup is predefined and allow not for any liquid calorimeter setup. The most obvious tool is an IR camera, a technique which is in no way less relevant in this case than any other method. It is applicable to the scope though. Hence no explicit description on the choice of instrumentation is required as such a section would be out of scope and irrelevant.

** Complete electrical wiring diagrams including the resistive heating coils, the control box and the measurement points, extending from input mains, and including ground and all three phases.*

What would it contain, except the most trivial picture with a squared box and a number of lines attached to it. No great loss, but sure, why not. As this has been added to the v3 of the 3rd party report, this is a moot argument.

** Mechanical diagrams of the device, including geometry and material composition.*

It is vividly apparent that it is a cylinder, that it has a certain length and a certain radius. In terms of the scope, any info about the geometry is redundant, and not really needed. But you get it anyway in good enough detail for both devices, including good enough description of the materials.

** Any data whatsoever from the "dummy" test mentioned on page 17 of R*ef [1].

? What does it mean? There are data for the dummy test presented in page 18-19.

** We would also have expected a presentation of comprehensive tables giving all the validated, fundamental measurements done in the course of the tests – or possibly a reference to a web page where such information can be found and retrieved.*

Why? Is this in any way norm for a paper sent to arXive? I guess, if you were to ask for it, you may get it.

Found the need to comment the below statement as well :

“Since no access to the core of the device was granted, and thereby no real scientific investigation of the core processes were allowed, it is unclear why a group of scientists (including chemists, nuclear and theoretical physicists) should be assigned to perform this measurement. The task seems rather to require expertise in IR and electrical measurements.“

As the scope was not including the core, the core was not part of the analysis. As ECAT (and the underlying phenomenon) is an area of heavy contention by main stream science, to be able to present the thermal analysis in reasonable way, it would require the support of established scientists. Also, there is reason to believe that this is only a first step on a ladder that will involve scientists and ECAT and that there will be subsequent reports/papers published. So, yes. It required the scientific hand, albeit the scope is limited to a black box test at this point in time.

Level of independence etc.

First of all a general comment about independence. As I have stated before there are a couple of problems related to finding a good team to do the testing.

1. This is an invention containing trade secrets.
2. This is an area which is heavily contended. Main stream science does not regard the underlying phenomenon as real science therein and the ECAT device to be fraudulent. In this kind of environment, you need to find a group of ranking scientists willing to risk their status, future funding etc.

i. *The first author, G. Levi, has been closely involved in numerous tests and promotions of the E-Cat together with the inventor, A. Rossi, over the last 2 1⁄2 years. His independence is not as clear as one would wish,*

Levi is a highly regarded member of the department where he work at the University of Bologna. He has a large amount of publications to his name. When this line of argumentation is coming up, you really put in question the honesty of the person Levi. Is he bought? Is he a fraud? Is he incompetent? If you think not, then this must be put aside. He is as ready as any other in the team doing the work to uncover the truth. He has thrown his reputation into the fray.

ii. *Several of the other authors, at least R. Pettersson and H. Essén, have also participated in previous demonstrations arranged by Rossi and have then to some degree committed to a positive appreciation of the device,*

Again, this is a low way of building a case. The attended a couple of demos. They have made some positive indications. But they are scientist or rank, they would not want to be caught dead in a fraud. If you do not think they are fraudulent, or incompetent, then this type of argument is not relevant.

iii. *The measurements were done in Rossi's premises in Italy with a corresponding loss of control over important factors in the measurement process,*

Obviously, this is not good. However, as I point out above, this is an invention containing trade secrets. We are not talking about a new recipe of CocaCola, we are talking of a potential game changer, a disruptive technology that will cause a lot of change - and that is worth a considerable amount of money if that will come to pass.

As such this is one criteria that is part of the setup, and the definition of the scope.

iv. *The reactor and its control circuits were operated by personnel assigned by Rossi (in the December test even started before the investigators were given access),*

Not sure if that is fully correct at all times during the tests, but again it does not violate the scope of the investigation. They were free to measure whatever they wanted outside the black box. Also, whether or not the machine is started before hand is not relevant to the scope either. Note that this is prototypes, and there may be many reasons why they chose to have it started before hand in the December test run, none of them fraudulent or in any way deceptive.

v. *Measurements were done on (at least) two different types of devices,*
So? Completely irrelevant with regard to the scope.

vi. *Estimating the heat output by a combination of IR camera measurements and convection calculations represents a new situation compared to previous tests, seemingly imposed by the circumstances (i.e. Rossi) rather than by choice.*

IR thermography as a calorimeter is a well established technique, and does in no way imply fraudulence or deception by the team doing the measurements. The line or argumentation is based on innuendo rather than objectiveness by the authors. As such it is a completely irrelevant comment with respect to the scope.

Input

Regarding the INPUT electrical power we note:

** Considering the fundamental and crucial importance of the measurement of the input electrical power, it is rather surprising that the report is quite brief on the details of the electrical circuits and measurements. The lack of a clear circuit diagram has already been mentioned. Other concerns not discussed in the report are the possibility of DC power, the waveforms of voltage and current at various points in the system, the possibility of power through ground leads or other ways that undisclosed electrical power can be supplied to the device.*

Again calling upon the scope of the work, the only relevant thing to measure is the input electrical energy. Whatever takes place in sub blocks of the black box, may be intellectually stimulating to query about, but is not relevant to the scope. It is by now a well established fact that the instrument PCE-830 is unable to detect DC components and as such that leaves the door open for contention as it could provide a channel for extra energy to be fed to the system, however unlikely.

As for a diagram, please check version 3 of the report, where a trivial electric diagram is shown.

** Previous tests have reported important discrepancies between the electrical input power as claimed by Rossi and those actually measured by specialists with proper electrical measurement equipment, to the extent where no excess heat production could be inferred [2]. With the knowledge of such critical observations a much more thorough reporting on the electrical measurements should have been provided.*

** To be more specific still, since the results of the expert measurements referred to in the previous paragraph seem to have deviated from what was claimed by Rossi by a factor of about 3, which happens to coincide with the excess heat observed also in the March test, we would have expected a clear description of how the risk of such inconsistencies was avoided, and even an involvement of the specialists from the SP institute.*

True, there is the story with the tester that was asked to leave the premises of the test before his work was done. There was some argument on how the testing should be conducted and of the performance of the instruments brought in by the tester.

Be that as it may, disturbing on its own, it does not relate to the 3rd party report, nor is it relevant to the scope therein. Again I find the authors being less objective and more following a line of argument that is based on discredit the report based on innuendo.

** In view of these severe inconsistencies, the fact that the control unit providing the electrical power was "not available for inspection, inasmuch as they are part of the industrial trade secret" (pg 15) is even more disturbing.*

In no way can this be disturbing. The authors is using a highly negatively charged wording to attribute something that is in fact not within the scope.

Fuel

The authors claim "the reaction is fueled by a mixture of nickel, hydrogen, and a catalyst, which is kept an industrial trade secret" (pg 1). In view of the secrets surrounding the reactor fuel powder we wonder:

First a general comment. As much as the innards of the black box is intellectually challenging and interesting, it is outside the scope of the report. Any line of argument demeaning the value of the report based on the internal working of the black box is moot.

** How can the authors know there is nickel inside the reactor?*

It does not matter. It is outside the scope.

** How can the authors know there is hydrogen inside the reactor?*

It does not matter. It is outside the scope.

** In addition, the reference to "industrial trade secrets" with regard to the composition of the "fuel" makes all speculation about what is powering the alleged reactions meaningless.*

True. It is meaningless. It is outside the scope.

Output

Energy OUTPUT is purely by heat – radiation and convection – since no radioactivity above background has been observed in any of the tests reported here.

** The report describes in some detail the IR camera measurements and convection calculations performed to estimate the energy OUT. At least for the March test, a number of additional validations of the IR measurements seem to have been performed: a contact measurement on the reactor surface by a thermocouple and the application of emissivity calibration pads. The absence of such important information, together with the factor 2 difference of the COP in the two measurements, makes the December test of less value in our opinion.*
As it has been pointed out, there were two different prototype devices being tested. It is therefore not surprising that there are different results. It is infact outside the scope to speculate as to why it is so. It is merely a property of the difference in device and configuration that make it so. Granted that the March test was more rigorous, it does not render the December data invalid in no way. Regardless of the apprehensions of the authors of the commentary, data does show excess heat in a black box capacity.

** To our understanding, the sensor of the IR camera actually provides an electrical signal proportional to the emitted power in its region of sensitivity. It would seem to us that this signal, in combination with the wavelength response of the sensor, should have been reported and used for the derivation of the total emitted power. It would seem that going via an inferred surface temperature of the emitting object is an unnecessary detour.*

This is why so much effort was put into describing how the thermal energy was computed, as there may be other opinions how it could be computed. It does not in any way invalidate the measurements in the report, and as such the comment is not relevant to the scope.

** Since the final energy density levels reported are much beyond any known chemical fuel and approaching or surpassing those of conventional nuclear fuel, we find the absence of any detailed reporting and discussions on (nuclear) radiation measurements a disturbing omission.*

There is such a report, and it showed no measurable radiation. It is available per request.

Regarding the need to provide an external electrical heat source we wonder:

** Why is such a heat source necessary? How is it controlled?*
** Is there a threshold temperature for the internal "anomalous" process to start?*
** If so, what is the observed value for this temperature and how is it recognized?*
** In this context, data from the start-up phase of the device would be highly relevant. Why are such studies missing and in particular why are the observed surface temperatures for the start-up phase of the device not reported?*

As much as it interesting to really get into those question, it is not relevant to the scope.
As far as startup goes, it is a part of the data for the March test run, maybe not in the format you like. But again, it is irrelevant to the scope.

Remarks on the November 2012 test

In the November test, the reactor was destroyed but the authors still claim that "although the run was not successful [...] it demonstrated a huge production of excess heat, which could not be quantified".

This test is really not relevant to the scope, but is most likely to add the anecdotal evidence for completeness. I do not understand really why the conclusion is such a problem? The device was put into operations and steel core melted. That would encompass temperatures of about 1450 C (2650 F). Assuming a similar setup and power as in the other tests, that would amount to some serious energy surge. But again, anecdotal and not relevant to the scope.

** No data are presented to support the claim for "huge excess heat" in this test. How do the authors know there was any power output beyond what was supplied by the resistive heating?*

The authors claim to know that the "fuel" powder in the November test was not evenly distributed laterally in the reactor, but rather concentrated at the two ends of the cylinder.

** In the December and March tests, the authors seem to have little or no control over the core of the reactor where the fuel is kept. So, how do they know this? Why was the test arranged in this way?*

** Could the observed effects be explained by pure electrical heating?*

In this test, the authors observed lighter and darker horizontal bands on the surface of the reactor during operation (Fig. 1, pg 2). They claim that five dark bands in the image "nicely match the areas overlying the resistor coils."

** How was the exact projection on the surface of the internal resistor coils determined, in order to make the association of dark bands with the resistor coils?*

** Since there are 16 heating resistor coils it is not clear why there should be only five horizontal "footprints" seen on the surface.*

** The authors make no attempt to investigate if instead the six (possibly seven) bright horizontal bands could in fact be the surface "footprint" of the resistor coils seen in this view. In any case, since on a cylindrical surface there must be an equal number of dark and bright bands, and unless the exact spatial location of the resistor coils is known in the plane projection view of the camera, there seems no compelling reason to assume that either dark or bright bands are caused by the coils.*

** Since there are no thermal calculations reported, how do the authors know that the 1 kW (constant, presumably, since it is mentioned that the "pulsed" self-sustained mode was only active in the March test) was not sufficient to cause the observed effects of (light or dark) bands and melting? It would seem that the authors jump to conclusions that fit their preconceived idea about the origin of the energy production, rather than approach the problem in a scientific manner.*

The remarks above, however to the point or not, is irrelevant to the scope. To jump to conclusions that fit preconceived ideas, is a matter of opinion only, and I conclude that the authors of this commentary does the same. But none of it is relevant to the scope.

Remarks on the December 2012 test

The emissivity of the surfaces measured by the IR camera was not known. The authors claim that the most conservative assumption is then to set an emissivity of 1, and offer an example to show so.

* *To us, the fact that the emissivity of the studied surface was not established seems to be a fundamental lack of experimental procedure. It was shown in the March test that quite straight-forward measures can be taken in order to estimate the emissivity. The emissivity could easily have been obtained after the measurements, in a manner shown by the March tests. We can only wonder why such a fundamental, complementary measurement was not performed before the report was published.*

The highly negative charged comment "*fundamental lack of experimental procedure"* has no other intention than to add negative innuendo, not adding to the objectiveness of the commentary. Setting a value of 1 does not invalidate the outcome of the report.

* *We can only speculate that one factor contributing to the different results obtained in the December and March tests might be the lack of knowledge of the emissivity in the December test.*

It may, but so may many other things. It does not alter the fact that there was excessive energy produced.

Remarks on the March 2013 test

The authors present a long discussion on the deviation of the time-dependence of the observed temperature/emitted power from that of a generic resistor (pgs 25-26). They further argue that this time-evolution is supporting evidence for the presence of an anomalous heat source in the E-Cat device. We do not find this discussion convincing.

Making a MatLab model using COMSOL was a creative initiative, but one should be weary of such tools. Do note that your model is no match for the real thing. It is like saying that theory is superior to empirical data.

* *We have performed a thermal analysis using the physics simulation tool "COMSOL" for MatLab, using values for geometry, materials and electrical heating coils extracted from the report (March test, cylindrical part only†). In Fig. 1 we show results from a calculation including resistive heating only (810W for 2min, then 0W for 4 min). The shape of the temperature curve at the surface is quite similar to what is presented in Ref. 1, Plot 3. Since we have not included any anomalous heat source in our model, it seems clear that the observed shape of the temperature at the surface can simply be attributed to heat diffusion from the internal heat source(s) through the steel cylinder to the surface. As a matter of fact,*

there seems to be no reason whatsoever why the surface temperature curves should look anything like those of a generic resistor (as, e.g., claimed in the caption of Plot 3 on page 25 in Ref. [1]), when this resistor is buried deep within the device.

** Our modeling indicates that the observed time-dependence is perfectly consistent with a situation where the internal heat source is supplied purely by resistive heating. Thus, the argument that the shape of the temperature/power curves is an indication of an anomalous internal heat source is not correct. And even if this has no direct bearing on the existence (or not) of any anomalous heat source within the reactor, the lack of understanding of such a fundamental physical process does not inspire confidence in the presented results.*

This is seriously only intended to add negative innuendo. The authors of the commentary say though, that this has nothing to do with the claim of excessive heat being generated. Possibly it is so that the self-sustaining mode period is not very long, but it say nothing about if there is a substantial effect while the resistive heating is ongoing. Maybe they should add a component for self sustaining excessive heat, and move the thermal heating into a coil further out, in the model, and try to actually get the temperatures and input energy to match the real experiment.

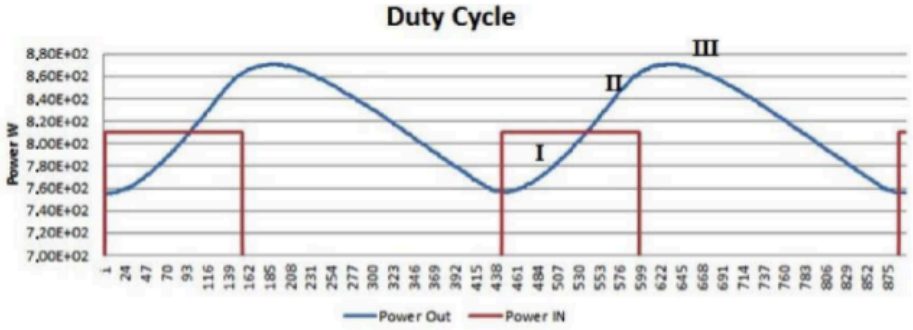
** It would seem to us that a careful thermal modeling and analysis should have been performed before far-reaching claims about the shape of the temperature/power curves were made. Furthermore, with sufficiently detailed thermal modeling, it could be possible to shed some light on the possible internal workings of the reactor, possibly serving as an independent corroboration of the energy balance calculations.*

Maybe it would have been a fine exercise, but it would not be relevant to the scope.

** From our thermal analysis it is also clear that if any form of pulsed, internal (electrical plus possibly "anomalous") power is supplied, the temperature (and hence the emitted power) at the surface of the reactor by necessity has to be out of phase with the power contributed by the internal sources. And obviously, the displayed pattern of surface heating and cooling clearly indicates that the internal heat source is not constant. However, the curves presented in [1] (especially Plot 8 on pg 27) show the electrical power and the surface emission power to be in phase. This can have two explanations. The simplest one is that the two curves have been forced "synchronized" for the sake of illustration by the authors. This should then have been clearly pointed out in the report, but, and this is more serious, it would then also constitute a deviation from reporting the actual experimental data as well as withholding important information that could contribute to an understanding of the device and its internal processes. The second possibility could be that the phase and amplitude of the additional anomalous heat source is such as to precisely synch the two curves. We leave it open if such a scenario can even be conceived, but it would of course be a rather remarkable coincidence, and could actually offer an independent possibility to assess some of the properties of the alleged anomalous source. At present we have not had the resources to take our modeling to that level.*

The authors again indulge in innuendo to paint their negative picture. It could also be so that the curves are in fact so synced because they are, and their model - may be due to its simplicity - has several problems in mimic the real world.

But take plot 8, as they mention in this comment. It is evident that the integrated output is about three times the value of the integrated input (simply integrate the areas under the blue and red curves during one period in your mind and take the difference). This is regardless of any details in the shape of the curves, or the synchronization of ON/OFF state and min/max.



Plot 8. Detail taken from Plot 7, reproducing the first two periods of the cycle. The three time intervals in which each period may be divided are labeled by Roman numerals.

** We note that the authors' reasoning in the case of the thermal signature is unfortunately typical of a kind of thinking that otherwise is ubiquitous in pseudo science: the tendency to quickly jump to interpretations and conclusions that support the extra-ordinary claim, rather than to try to find more mundane explanations based on already known, standard physics. In this case, it seems that normal heat diffusion and transport is sufficient to explain the observation (except for the perfect synchronization of electrical power with surface emitted power). The observed thermal time-evolution gives no reason to resort to anomalous explanations. It is also remarkable that the authors seem not to reflect upon the need of a pulsed heat source. If a heat source is needed to start a process in a very small amount of "fuel" in the E-Cat, it seems relevant to reflect about how the process can be controlled and why it can be*
stopped by switching off the external heat source.

Linking the work of Levi et.al. to pseudo-science is so revealing for the purpose of their commentary report.

The authors would be well advised then to offer their own "mundane" explanation, if they now take a superior stance. If you see excessive heat on the levels that are shown, how can one draw any other conclusion?

But fair enough, let us not call it by name, but in fairness, let us exclude what it is not. Then we

end up with “we do not know what it is”. But we do know it is very potent.

The heat diffusion and transport is not enough to explain anything from their model. They can model anything they like and present graphs on that, but it does not remove the fact that excess heat is produced. Any such modelling is irrelevant to the scope of the work they comment on. Lets continue.

We note that there are (at least) three wires going into the E-Cat in Fig. 10 of Ref. [1]. What is the
function of each of them and how were they connected externally to the control box and internally in the device?

Data on electrical input power were provided from the continuous video recordings of the PCE-830 instrument. However, only an average value for the measurements during the resistive heating period is given. No data from the period with heating off are given.

Well, check version 3 and there will be that trivial graph. There are 4 connections, 1 GND, and 3 phases. Any details about how it goes into the control box is not relevant to the scope.

* *It is unclear to us why the operation of the device in this test is labeled “self-sustained” when it obviously needs a supply of external heating throughout the test. From our thermal analysis there seems to be no reason to assume that power is supplied outside of the 2 min electrical heating phase – although it does seem that more power than 810 W is required to obtain the observed surface temperature of about 300 degrees Celcius.*

Now this is the holy graal of the commentary report. They admit to the fact that their model 810 W input does not actually yield the 300 C. The ECAT get to 300 C with 810 W when running continuously with resistivity heating only (dummy run). In the real experiment 300 C is reached when pumping 810W one third of the time, and 0W the remaining time. Hmm... I would like to know what kind of temperature they do reach in their simulations.

** We would have expected a diagram or table showing the power drawn from the grid over (at least) one full 6 minute cycle, including both the 2 minute phase of resistive heating and the 4 minute phase without any external power to the device.*

Granted. It would improve on clarity.

** It would seem to us that the power consumed by the control box could be obtained from the power measurements during the phase when no external heating was supplied to the reactor device. Instead, the power to the control box was obtained from the dummy measurement. Why?*

That would be because it was in an OFF state. Estimating the power consumption of the control

box in that state would likely not be representative as compared to when there is resistive heating going on.
** We would have expected to see a discussion and data on power consumption all the way from the start-up phase of the device to the steady-state phase, in order to show how and when the "anomalous" heating source kicks in. After all, that source allegedly provides about twice as much power as the heating coils and we would have expected the signature of its onset to be presented and discussed.*

There is in a way some of that in Plot 9. As such a discussion would be illuminating, but it does not alter the outcome of the report.

The authors give their best estimate for the amount of fuel as 0.3 g. However, in most of their calculations they select not to use this value but rather an ad hoc value of 1 g. The reason for this seems to be to stay on the conservative side in their estimated energy density. However, we find this effort somewhat misguided as it is important to provide a best estimate of the effect with actual measured values; only after that one should proceed to discuss the validity of the measured values and assign appropriate uncertainties in order to estimate the range of possible values.

It is true that a stringent error propagation regime would have been more scientific in many ways. However, as there are many limiting factors and the scope is limited basically to validate whether or not the device is capable of producing excess heat, the slight handwaving handling of the errors - as long as the handwaving actually work in the conservative direction - would be just fine.

** Using the 0.3 g at face value, and assuming that the energy balance is correctly determined in the report, we find that the energy density of the fuel powder is about 250 kWh/g thermal. This can be compared with the thermal density of ordinary fission reactor fuel of about 750 kWh/g thermal. Thus, the fuel powder would under these assumptions give about 1/3 of the energy density of fission reactor fuel. Even with the ad hoc assumption of 1 g of fuel powder, the energy density is about 1/10 of nuclear reactor fuel, far beyond any known chemical compound.*

Well.... Is that not just what this is all about. The authors of this sceptical commentary do infact say that this is far beyond any known chemical compound. That would mean that the reactor must contain something capable of generating energy in excess of what any known chemical compound would.

** The authors half acknowledge the extremely high energy densities implied by their measurements, but do not carry the discussion to the logical end. The only processes we know of today that can give such energy densities are nuclear. This would normally be associated with strong emission of radiation, in particular gamma, but also neutrons, beta and even some heavier charges particles (depending on the exact nuclear transformations*

involved). It would also cause nuclear transmutations of the fuel. In view of this background, which must have been very clear to the authors as they include several experts in nuclear physics and measurements, it is surprising that the investigations of radiation emitted during the operation of the device are not presented as part of the report, but only referred to indirectly by a quote from a Dr Bianchini. In a truly scientific investigation, the analysis of the fuel would of course be the top priority – but unfortunately the restrictions imposed by Rossi on the present tests did not allow for this. It is unfortunate that the group of investigators agreed to such conditions during these tests. This is even more surprising as investigations of the fuel powder have been conducted and reported in the past, then with no indications of radioactivity or nuclear transmutations of the Nickel isotopes [3].

Naturally, these are the most poignant contentive points from scientists in the nuclear and high energy fields.

As there is no radiation, the report from Dr. Bianchini was considered redundant as an appendix. The authors of the commentary may request the report from the authors of the 3rd party report. Also, the matter of the ash, is something part of the trade secret, and not within the scope.

Possibly there is some new theory that need to be developed.

** Even more extreme seem to be the claimed power densities. If the fuel weighs 0.3 g and is supposed to produce on the order of 1667 W during the 96 hours of the December test (to get 160 net kWh, see pg 28), or 534 W during 116 hours of the March test (to get 62 net kWh, see pg 28), then power densities in the range of 1.78 to 5.56 MW/kg would have been achieved (the authors give a value of 7 · 103 W/kg on pg 28). That is far more than the power density obtained from fuel in today's light-water reactors (there is about 100 tons of fuel in a 3 GW (thermal) fission reactor, i.e. 0.03 MW/kg), and far beyond the y-axis scale in the power density plot shown as Fig 9 in Ref. [1]. The authors seem not to reflect on this remarkable claim.*

The delving too deep into those issues are irrelevant to the scope.

Remarks on the "dummy" test

A "dummy" test was conducted in conjunction with the March test, after the measurements of the loaded reactor. This involved a reactor without powder charge and end caps. The test was performed with a continuous electrical input power of about 810 W.

** No data or figures associated with the "dummy" test are reported. This makes it very hard to assess the validity and usefulness of this measurement*.

Yes, there are data presented in pages 18-19 of the report. Perhaps not what they were looking for.

* *The dummy test was not performed in the same way as the test of the "loaded" reactor and can therefore hardly be seen as a conclusive no-charge test. For example, in a proper dummy test, care should have been taken to supply input power in the same pulsed manner as in the loaded test. (It would then also have been clear to what extent the shape (time dependence) of the surface temperature curves as reported from the March test require any anomalous source, or can be explained as a simple consequence of heat diffusion.)*

No, as they did run it continuously. The authors of the commentary tried a simulation with 810 W for 2 min then 4 min with 0W. They did not reach 300 C. How very strange, as the ECAT needed continuously 810 W to get to get to 300 C (in the dummy mode, with resistive heating only).

Conclusions

The commentary report by Ericsson and Pomp does not really amount to much. Infact, if anything, it actually strengthen the position of the 3rd party report and the ECAT.

There is nothing in their comments that are new, or if it is new, does not contribute to invalidate the result of the 3rd party report.

The report seem more serve the purpose of propagating innuendo and falsely discrediting the authors of the 3rd party report.