

## Charge Clusters: The Work of Ken Shoulders

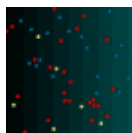
### William Zebuhr

Our cover article in this issue is about charge clusters, which are compact groups of electrons. They defy the common belief that such things cannot occur because of the great repelling forces these electrons should exert on each other. Yet they do appear to exist and are not even that difficult to create, once you know how. Ken Shoulders knows how and has been doing it for about 25 years. Ken and his son Steve experiment with charge clusters (also called exotic vacuum objects or EVOs) in a well-equipped, privately funded lab in California. Ken has a history of working at the edge of science and engineering. He worked at SRI International for ten years as a staff scientist and started a field now called vacuum nanoelectronics, which is based on field emission active devices. He also worked at MIT (during this time he fabricated the world's first microcircuits) and later did some exotic and confidential work for the CIA and others.

Ken's very qualified background has helped convince many people that the EVO phenomenon is real, in spite of the difficulty in believing that electrons can form such clusters. Ken notes: "Nobody believed anything ever said. They only believed the gadgets that were run by EVs. I actually had to almost beat the patent office into submission by a series of brutal assaults with my devices!"

Not only are the clusters themselves exotic to mainstream science but they seem to offer a way of connecting to the most fundamental and universal element of existence, sometimes called the aether. Ken refers to this as the Potentum, a name given by Joe Firmage. Whatever the name, it is a little understood part of the universe that many agree is loaded with energy if we only knew how to tap into it. The EVO may be the fundamental connecting link. Ken considers it a "kind of Universal Clutch as it clutches the basic fabric of the universe by biasing the medium with a dense, asymmetrical charge distribution." This is a very powerful concept and may well be the foundation of any device that operates with an "efficiency" over-unity.

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This clutch apparently can operate in many environments. EVOs are likely the source of the extremely high temperatures recently measured in collapsing bubbles formed by cavitation. Temperatures as high as 20,000 degrees K have been measured (see p. 37). Temperatures in this range need an exotic explanation and EVOs exhibit extremely high temperatures. Sonofusion is likely triggered by the same mechanism and may not really qualify as "cold" fusion. The destructive effects of cavitation on marine propellers has been known for over 150 years but never explained satisfactorily. Likewise the cavitation that can occur in pumps. There have been many claims of over-unity performance in pumps designed to produce a lot of cavitation but never enough to be useful as an energy source because the input is electrical and the output is heat at moderate temperatures.

Tesla is known to have been able to create what appeared to be ball lightning and even was able to let it touch him without harm. Ball lightning is probably a large charge cluster. It is not well understood but can appear in lightning storms, coming and going in mysterious ways, sometimes quietly and sometimes with a great deal of commotion. Understanding charged clusters better may clarify some of Tesla's work as well as give a better understanding of a strange natural phenomenon.

I have mentioned before that it may take decades from now to market a useful over-unity or "free energy" device. I based that on the work that Ken and others are doing in the cutting edge of this science. Ken has been working on this problem for 25 years and cold fusion/LENR work has been going on for over 15 years. This is difficult work that requires a rare combination of creativity, persistence, energy, and attention to fine detail. It also often involves the ability to improvise because of lack of funding and all too often a thick skin to put up with the unimaginative naysayers. On top of all this the work can also be dangerous. EVOs contain incredible potential energy for their size and it is not difficult to make them big enough to blow up a lab or more.

The work to date is much more science than engineering though doing the science often involves considerable engineering. This is quite different from the engineering that goes into an energy machine that could be sold to the public as an appliance or to a utility as a generator. We are still a long way from that. Fundamental understanding needs to progress to the point

where results are consistent, reliable, safe, and can be had at reasonable expense. Only then can a product be engineered around the concept. This will take many years and many dollars.

One of the most difficult problems in the new energy field is funding for good ideas. This is because of the very long time to market by conventional investment standards and because of the great difficulty of evaluation of the potential of the technology. A significant part of the evaluation problem is caused by the fact that most new energy concepts are either pure bunk or just on the wrong path. The very encouraging part of what Ken and Steve are doing is that, 1) they have great credentials for understanding this work, 2) the experimental techniques are very good and the associated theory is sound, and 3) the potential payoff of this approach is huge. This technology has the potential of being engineered into a very powerful, compact, and economical machine with endless variations and applications. There is still the problem of time to market.

Investors who are inclined to take this kind of risk like to see the potential of making 30 or 40% annual returns. This is not only because they want to make a killing but because so many ventures fail that each winner must support several complete losses. This means that even if an idea looks like it will be worth \$1 billion when developed it may not be worth enough to an investor to put the required money in during the early phases of development. It is very realistic that a given idea from concept to product could take 20 years. If it is worth \$1 billion in 20 years it is now worth only \$5.2 million discounted at 30% and only \$1.2 million discounted at 40%. Since it will often take more than this to even get traction with the idea, it is clear that even if the inventor sells his whole idea to the investor he will not have enough money to do the work. Actual cases are probably much worse than this. The project will take longer, cost more, and may even have competition problems when it is finally on the market.

Ken has mostly self-funded his work to date and the potential is so big that there ought to be a good investment opportunity in the work, but the time to market is still unknown and the real cost of development is unknown. This means the investor would still have to have a very long-range vision and deep pockets. These are hard to find. More conventional technology gets

funding in the many established ways, including private investors, venture capital, corporations, and governments. These avenues are rarely open to risky and long-range projects. Many historic inventors, such as the Wright brothers, funded their own work for the same reasons but technology has gotten orders of magnitude more complex in the last 100 years and this method is only open to the truly wealthy today.

The new energy field needs more people willing to give grants to promising ideas with minimal strings attached. Many foundations exist for endless causes, few of which come close to offering the result that some of the good ideas in the field offer.

Foundations don't invest because they hardly even know the new energy field exists and they have no way of evaluating ideas even if they learn about them. There are very few people qualified to evaluate this technology. Even if they are very knowledgeable in the general field it is very difficult to make judgments of work done by others without spending a lot of time with the innovator. This is expensive and often the innovator is reluctant to expose critical parts of the technology. Furthermore, a hired consultant is usually correct if he declares that there will be a lot of problems and recommends against investment.

A new energy peer review process involving a credible group of scientist and engineers would be very helpful in selecting the best technology for funding but this won't happen soon. The whole nature of the field is against it. Many are innovative in their own project but not interested in others. There are theories and there are experimental results that may or may not be facts and the two often don't agree. These problems are not necessarily anyone's fault. It is a logical result of a few people trying to solve huge problems using a science they don't understand yet. A lot of planes crashed before (and after) the Wright brothers for the same reasons. The net result is that at this time investors in this field must be willing to take large risks. Even a non-profit foundation or private donor cannot take excessive risks or their money and credibility will soon disappear. There are a few generous and thoughtful donors in the field today but much more is needed to generate some traction and get a useful device out in the market.

Infinite Energy's founding editor, the late Gene Mallove, was an advocate of taking even a very simple device into the market—even if it was just a toy—to generate interest and publicity.

This is still a good idea but even that modest goal is very elusive. I have lost track of the number of "magnetic" motors that people have gotten excited about but have never heard a credible story about one that actually worked. A concentrated effort is needed on a chosen technology to achieve Gene's goal. The EVO technology has promise of yielding very exciting results with a fairly modest amount of funding.



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