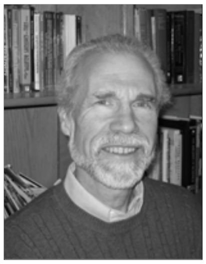
# BREAKING THROUGH EDITORIAL



## Celebrating 100 Issues

**Bill Zebuhr**

Publishing 100 issues of a very specialized, difficult to read magazine that is well-respected worldwide is an accomplishment to be proud of. We have tried to advance the state of science and technology by publishing articles that we believe have a value in provoking thought and adding to the total knowledge of the fields we have chosen to emphasize. *IE* was started by Gene Mallove in 1995 and the early emphasis was on cold fusion since it was a young subject with what looked like virtually unlimited potential. It was also very controversial, which appealed to Gene since he was a fighter for truth and he felt that the field was being unjustly maligned. Years have gone by and the number of issues published after his tragic death is approaching the number published before it. Much of that credit goes to the solid momentum and reputation the magazine had established and the great foresight he had in hiring Christy Frazier, our managing editor, who was knowledgeable, well-organized and determined to continue the good work Gene had begun.

Times have changed and the subject of cold fusion is now usually referred to as low-energy nuclear reactions (LENR) and other names to try to more accurately describe the technology and avoid the stigma that some still attach to "cold fusion" because of its early great claims and lack of substantial results. There is no doubt now of the validity of much of the science and there have been many peer-reviewed demonstrations made, but we are still far from a commercial machine based on the science. Some recent work has stimulated a lot of hope and discussion but remains controversial with no credible timeline to commercialization.

The emphasis of the magazine changed with the progression of developments in the general field. When LENR had well over ten years of history with no clear view of commercialization or even a consensus regarding the mechanisms at work to get the unreliable results that were being obtained, we started to emphasize the science that might lead to a better understanding of the subject rather than specific experiments and demonstrations of devices. We took a broader view of "new science"—by which we mean non-mainstream ideas that are credible and may explain some of the phenomenon that conventional science does not properly address. We have published many excellent articles in the past 99 issues. We believe some of these are among the best ever published in science. Some of these have explained the failures of various aspects of conventional theories of relativity, cosmology, nuclear theory, quantum reality and biological phenomenon. Others have offered theories behind various devices such as LENR reactors, magnetic machines and various hypothetical machines that, for example, may or may not violate the second law of thermodynamics.

We have also published a number of questionable articles. We try to give every well thought out idea a chance if it has no easily seen fallacy. Certainly many ideas presented turn out to be invalid later, but even these can stimulate thought that could lead to progress in a related issue. We have to be aware that many of the ideas presented in well-respected peer-reviewed journals are also proven wrong later. In fact, many have substantial holes in them that do not stand up under intelligent outside scrutiny but are offered time and time again often patched to "fix" the most recent discrepancy with facts. Even though we consciously err on the side of letting "wild" theories through, our record is probably as good as or better than some magazines that repeatedly report on fundamentally flawed theories as though they were facts. We think our method is much more conducive to creative scientific progress.

Creative scientific progress is what is required to explain various observed phenomenon that do not fit conventional theories and is required to allow repeatable results to be obtained from which commercially viable devices might be made. In the early years of cold fusion many expected quick results that might even "solve the world's energy problem." But after 22 years from the original Fleischmann/Pons demonstration there is no cold fusion device on the market and the science behind it is still in theoretical flux in spite of years of work from great minds. It is very difficult to make a fundamental change in technology when the theoretical underpinnings are not known. Man wanted to fly for centuries before he achieved it at the expense of a lot of effort and lives. The science of aerodynamics, thermodynamics and structures, among other things, simply could not support it.

One of the difficult lessons to be learned from being exposed to so many theories, ideas and opinions regarding the universe and technology is that we are still missing the answers to fundamental questions as well as the explanation of many specific processes and operation of devices. In some cases these devices have been working for extended periods of time. In fact, there is not a single device that we understand at a fundamental level because the nature of fundamental particles and their interactions is not known except by the results that they exhibit to the instruments that we apply. We have lived with that more apparent knowledge for a long time, but to make the next great leaps in technology we will need to know more about the fundamental nature of matter and energy. Some of the articles we have published have helped in that pursuit. Some of these intuitive breakthroughs can be done by individuals on their own and most have been throughout history.

Many of these fundamental discoveries have led to useful machines and many of these were first developed by one or a few individuals with little capital. Technology today is generally far too complex and expensive to experiment with to be done without significant outside investment to support the innovator. Investment in the best current ideas and most promising empirical evidence is now needed to fully demonstrate technology and then attract the much greater capital needed to bring it to production to realize significant benefit. There have been many ideas brought forth in the last 20 years and by far the majority had little or no merit and many of these were presented by people with very poor understanding of the core issues. These ideas, mostly based on wishful thinking, have seriously damaged the reputation of the new science and technology field and have drowned out some of the viable ideas that could have been brought to profitable fruition with a serious investment of capital and time.

It is time to separate the most viable ideas from the rest and start concentrating on generating collaborative efforts by the most able innovators backed by serious investment and management. Finding the right management is almost as hard as and sometimes harder than finding the technical innovators. Management needs to interface with investors and work with the technical team to efficiently utilize resources for development and then to produce and sell. Management has to have some technical knowledge and appreciation of the issues and has to generate confidence and trust in the investors.

The generation of trust is crucial to the success of complex and exotic technical projects because investors will not understand the technology and will base their decisions on how they trust the management to be presenting an honest picture of the risks and rewards of an investment. This is not a criticism of the investors, because they are not expected to be experts in the field being investigated and in many cases there are only a few people in the world capable of understanding the technology fully enough to have any degree of certainty. In some cases no one can accurately assess the risk until a lot of money has been spent, so the first investors have to expect great rewards if success is achieved. Technical innovators have to accept that they need a team to support their effort and that means sharing in the rewards and sometimes owning a small portion at the end of a long effort—if it is successful.

The economic, social and political climate in the U.S. today is not conducive to building a company based on an exotic and risky technology. Much of the venture capital today is put into a few fields where investors have had past success. They are fearful of new things that they do not understand no matter the promised reward and the world benefit. Many do not consider benefit except cash in their own pockets and in fact are willing to finance damaging technologies to make a profit. The government is a serious impediment to success. The many arbitrary rules and regulations often regarding taxes require resources that could be better spent on the business. When a technology looks promising they often invest in projects that are in direct competition with an entrepreneur. Capital will then follow that investment just because it seems there is already "free" money invested. These projects often fail and damage entire industries, sometimes for many years. Subsidies, tax credits and grants are other means by which the government undercuts private entrepreneurs.

These obstacles may seem overwhelming and often are, which is why so few new companies are formed around a complex new technology. To overcome them a very strong team including strong management is needed. In the future more investment will come from outside the U.S. where the technology is better understood and/or appreciated. There are a few visionary investors throughout the world, including the U.S. They are often self-made and often give a lot of money away to causes they believe in. They can easily afford to lose money on a given investment and may even give a grant to a cause that later could generate ideas that can lead to a profitable enterprise. Some of the theoretical projects that are ongoing that we are aware of could be moved much further ahead with a modest grant. Experimental projects are closer to producing a product and need more capital, but some of those could also be financed by grants or other nonprofit means until there is a reasonable assessment of risk. Grants can be repaid upon success of a project. Most grantors do it as a way of improving the world but giving back will encourage more cooperation in the future.

A prototype device that demonstrates the technical claims made for it makes raising investment capital much easier. If the supported claims are even close to being as good as many that we have heard rumors of over the years, investment would follow. The problem is that most projects in the new science and technology field are still in the early experimental stage where results are erratic, costs are high and the potential product that would result is still only vaguely designed. This is the way it is in the early stage of new technology, but there now are a few very promising demonstrations being made that could be good enough to attract enough financing to take them to production. The theories supporting the technologies are also advancing and in fact have sometimes been way ahead of the experimental evidence with no way of knowing if they are right. An operating device that can be explained by a new science theory would be the breakthrough many have been struggling to achieve for many years.

A successful product based on new science would advance the whole of physics and probably chemistry and maybe break the old paradigms. That would bring many more good thinkers to bear on the issues who have been stuck in the old theories and just needed a breakthrough to get them thinking in a new direction. It is very difficult to change fundamental thinking and even most very skilled scientists and engineers cannot do it. They need the rare outlier to pave the way. *IE* exists to help find and support those outliers so that many other competent minds can follow.

We thank all who have supported this effort over the last 16-plus years.

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