❖ ❖ ❖ GUEST EDITORIAL ❖ ❖ ❖

On Theory and Science Generally in Connection with the Fleischmann-Pons Experiment

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I was encouraged to contribute to an editorial generally on the topic of theory in science, in connection with publication of a paper focused on some recent ideas that Ed Storms has put forth regarding a model for how excess heat works in the Fleischmann-Pons experiment. Such a project would compete for my time with other commitments, including teaching, research and family-related commitments; so I was reluctant to take it on. On the other hand I found myself tempted, since over the years I have been musing about theory, and also about science, as a result of having been involved in research on the Fleischmann-Pons experiment. As you can see from what follows, I ended up succumbing to temptation.

Science as an imperfect human endeavor

In order to figure out the role of theory in science, probably we should start by figuring out what science is. Had you asked me years ago what science is, I would have replied with confidence. I would have rambled on at length about discovering how nature works, the scientific method, accumulation and systematization of scientific knowledge, about the benefits of science to mankind, and about those who do science. But alas, I wasn't asked years ago.

In this day and age, we might turn to Wikipedia as a resource to figure out what science is. We see on the Wikipedia page pictures of an imposing collection of famous scientists, discussion of the history of science, the scientific method, philosophical issues, science and society, impact on public policy and the like. One comes away with the impression of science as something sensible with a long and respected lineage, as a rational enterprise involving many very smart people, lots of work and systematic accumulation and organization of knowledge—in essence an honorable endeavor that we might look up to and be proud of. This is very much the spirit in which I viewed science a quarter century ago. I wanted to be part of this great and noble enterprise. It was good; it advanced humanity by providing understanding. I respected science and scientists greatly.

Today I still have great respect for science and for many scientists, probably much more respect than in days past. But my view is different today. Now I would describe science as very much a human endeavor; and as a human activity, science is imperfect. This is not intended as a criticism; instead I view it as a reflection that we as humans are imperfect. Which in a sense makes it much more amazing that we have managed to make as much progress as we have. The advances in our understanding of nature resulting from science generally might be seen as a much greater accomplishment in light of how imperfect humans sometimes are, especially in connection with science.

The scientific method as an ideal

Often in talking with muggles (non-scientists in this context) about science, it seems first and foremost the discussion turns to the notion of the "scientific method," which muggles have been exposed to and imagine is actually what scientists make use of when doing science. Ah, the wonderful idealization which is this scientific method! Once again, we turn to Wikipedia as our modern source for clarification of all things mysterious: the scientific method in summary involves the formulation of a question, a hypothesis, a prediction, a test and subsequent analysis. Without doubt, this method is effective for figuring out what is right and also what is wrong as to how nature works, and can be even more so when applied repeatedly on a given problem by many people over a long time.

In years past I was an ardent supporter of this scientific method. Even more, I would probably have argued that pretty much any other approach would be guaranteed to produce unreliable results. At present I think of the scientific method as presented here more as an ideal, a method that one would like to use, and should definitely use if and when possible. Sadly, there are circumstances where it isn't practical to make use of the scientific method. For example, to carry out a test it might require resources (such as funding, people, laboratories and so forth), and if the resources are not available then the test part of the method simply isn't going to get done.

In practice, simple application of the scientific method isn't enough. Consider the situation when several scientists contemplate the same question: They all have an excellent understanding of the various hypotheses put forth; there are no questions about the predictions; and they all do tests and subsequent analyses. This, for example, was the situation in the area of the Fleischmann-Pons experiment back in 1989. So, what happens when different scientists that do the tests get different answers? You might think that the right thing to do might be to go back to do more tests. Unfortunately, the scientific method doesn't tell you how many tests you need to do, or what to do when people get different answers. The scientific method doesn't provide for a guarantee that resources will be made available to carry out more tests, or that anyone will still be listening if more tests happen to get done.

Consensus as a possible extension of the scientific method

I was astonished by the resolution to this that I saw take place. The important question on the table from my perspective was whether there exists an excess heat effect in the Fleischmann-Pons experiment. The leading hypotheses included: (1) yes, the effect was real; (2) no, the initial results were an artifact. Predictions were made, which largely centered around the possibility that either excess heat would be seen, or that excess heat would not be seen. A very large number of tests were done. A few people saw excess heat, and most didn't. A very large number of analyses were done, many of which focused on the experimental approach and calorimetry of Fleischmann and Pons. Some focused on nuclear measurements (the idea here was that if the energy was produced by nuclear reactions, then commensurate energetic particles should be present); and some focused on the integrity and competence of Fleischmann and Pons. How was this resolved? For me the astonishment came when arguments were made that if members of the scientific community were to vote, that the overwhelming majority of the scientific community would conclude that there was no effect based on the tests.

I have no doubt whatsoever that a vote at that time (or now) would have gone poorly for Fleischmann and Pons. The idea of a vote among scientists seems to be very democratic; in some countries leaders are selected and issues are resolved through the application of democracy. What to me was astonishing at the time was that this argument was used in connection with the question of the existence of an excess heat effect in the Fleischmann-Pons experiment.

In the years following I tried this approach out with students in the classroom. I would pose a technical question concerning some issue under discussion, and elicit an answer from the student. At issue would be the question as to whether the answer was right, or wrong. I proposed that we make use of a more modern version of the scientific method, which was to include voting in order to check the correctness of the result. If the students voted that the result was correct, then I would argue that we had made use of this augmentation of the scientific method in order to determine whether the result was correct or not. Of course, we would go on only when the result was actually correct. The students understood that such a vote had nothing to do with verifying whether a result was correct or not. To figure out whether a result is correct, we can derive results, we can verify results mathematically, we can turn to unambiguous experimental results and we can do tests; but in general the correctness of a technical result in the hard sciences should probably not be determined from the result of this kind of vote.

Scientific method and the scientific community

I have argued that using the scientific method can be an effective way to clarify a technical issue. However, it could be argued that the scientific method should come with a warning, something to the effect that actually using it might be detrimental to your career and to your personal life. There are, of course, many examples that could be used for illustration. A colleague of mine recently related the story of Ignaz Semmelweis to me. Semmelweis (according to Wikipedia) earned a doctorate in medicine in 1844, and subsequently became interested in the question of why the mortality rate was so high at the obstetrical clinics at the Vienna General Hospital. He proposed a hypothesis that led to a testable prediction (that washing hands would improve the mortality rate), carried out the test and analyzed the result. In fact, the mortality rate did drop, and dropped by a large factor.

In this case Semmelweis made use of the scientific method to learn something important that saved lives. Probably you have figured out by now that his result was not immediately recognized or accepted by the medical and scientific communities, and the unfortunate consequences of his discovery to his career and personal life serve to underscore that science is very much an imperfect human enterprise. His career did not advance as it probably should have, or as he might have wished, following this important discovery. His personal life was negatively impacted.

The scientific community is a social entity, and scientists within the scientific community have to interact from day to day with other members of the scientific community, as well as with those not in science. How a scientist navigates these treacherous waters can have an impact. For example, Fleischmann once described what happened to him following putting forth the claim of excess power in the Fleischmann-Pons experiment; he described the experience as one of being "extruded" out of the scientific community. From my own discussions with him, I suspect that he suffered from depression in his later years that resulted in part from the non-acceptance of his research.

Those who have worked on anomalies connected with the Fleischmann-Pons experience have a wide variety of experiences. For example, one friend became very interested in the experiments and decided to put time into this area of research. Almost immediately it became difficult to bring in research funding on any topic. From these experiences my friend consciously made the decision to back away from the field, after which it again became possible to get funding. Some others in the field have found it difficult to obtain resources to pursue research on the Fleischmann-Pons effect, and also difficult to publish.

I would argue that instead of being an aberration of science (as many of my friends have told me), this is a part of science. The social aspects of science are important, and strongly impact what science is done and the careers and lives of scientists. I think that the excess heat effect in the Fleischmann-Pons experiment is important; however, we need to be aware of the associated social aspects. In a recent short course class on the topic I included slides with a warning, in an attempt to make sure that no one young and naive would remain unaware of the danger associated with cultivating an interest in the field. Working in this field can result in your career being destroyed.

It follows that the scientific method probably needs to be placed in context. Although the "question" to be addressed in the scientific method seems to be general, it is not. There is a filter implicit in connection with the scientific community, in that the question to be addressed through the use of the scientific method must be one either approved by, or likely to be approved by, the scientific community. Otherwise, the associated endeavor will not be considered to be part of science, and whatever results come from the application of the scientific method are not going to be included in the canon of science. If one decides to focus on a question in this context that is outside of the body of questions of interest to the scientific community, then one must understand that this will lead to an exclusion from the scientific community. Also, if one attempts to apply the scientific method to a problem or area that is not approved, then the scientific community will not be supportive of the endeavor, and it will be problematic to find resources to carry out the scientific method.

A possible improvement of the scientific method

This leads us back to the question of what is science, and to further contemplation of the scientific method. From my experience over the past quarter century, I have come to view the question of what science is perhaps as the wrong question. The more important issue concerns the scientific community; you see, science is what the scientific community says science is. This is not intended as a truism; quite the contrary. In these days the scientific community has become very powerful. It has an important voice in our society. It has a powerful impact on the lives and careers of individual scientists. It helps to decide what science gets done; it also helps to decide what science doesn't get done. And importantly, in connection with this discussion, it decides what lies within the boundaries of science, and also it decides what is not science (if you have doubts about this, an experiment can help clarify the issue: pick any topic that is controversial in the sense under discussion; stand up to argue in the media that not only is the topic part of science, but that the controversial position constitutes good science, then wait a bit and then start taking measurements). What science includes, and perhaps more importantly does not include, has become extremely important; the only opinion that counts is that of the scientific community. This is a reflection of the increasing power of the scientific community.

In light of this, perhaps this might be a good time to think about updating the scientific method; a more modern version might look something like the following:

1) The question: The process might start with a question like "why is the sky blue" (according to our source Wikipedia for this discussion), that involves some issue concerning the physical world. As remarked upon by Wikipedia, in many cases there already exists information relevant to the question (for example, you can look up in texts on classical electromagnetism to find the reason that the sky is blue). In the case of the Fleischmann-Pons effect, the scientific community has already studied the effect in sufficient detail with the result that it lies outside of science; so as with other areas determined to be outside of science, the scientific method cannot be used. We recognize in this that certain questions cannot be addressed using the scientific method.

2) The hypothesis: Largely we should follow the discussion in Wikipedia regarding the hypothesis regarding it as a conjecture. For example, from our textbooks we find that the sky is blue because large angle scattering from molecules is more efficient for shorter wavelength light. However, we understand that since certain conjectures lie outside of science, those would need to be discarded before continuing (otherwise any result that we obtain may not lie within science). For example, the hypothesis that excess heat is a real effect in the Fleischmann-Pons experiment is one that lies outside of science, whereas the hypothesis that excess heat is due to errors in calorimetry lies within science and is allowed.

3) Prediction: We would like to understand the consequence that follows from the hypothesis, once again following Wikipedia here. Regarding scattering of blue light by molecules, we might predict that the scattered light will be polarized, which we can test. However, it is important to make sure that what we predict lies within science. For example, a prediction that excess heat can be observed as a consequence of the existence of a new physical effect in the Fleischmann-Pons experiment would likely be outside of science, and cannot be put forth. A prediction that a calorimetric artifact can occur in connection with the experiment (as advocated by Lewis, Huizenga, Shanahan and also by the Wikipedia page on cold fusion) definitely lies within the boundaries of science.

4) Test: One would think the most important part of the scientific method is to test the hypothesis and see how the world works. As such, this is the most problematic. Generally a test requires resources to carry out, so whether a test can be done or not depends on funding, lab facilities, people, time and on other issues. The scientific community aids here by helping to make sure that resources (which are always scarce) are not wasted testing things that do not need to be tested (such as excess heat in the Fleischmann-Pons experiment). Another important issue concerns who is doing the test; for example, in experiments on the Fleischmann-Pons experiment, tests have been discounted because the experimentalist involved was biased in thinking that a positive result could have been obtained.

5) Analysis: Once again we defer to the discussion in Wikipedia concerning connecting the results of the experiment with the hypothesis and predictions. However, we probably need to generalize the notion of analysis in recognition of the accumulated experience within the scientific community. For example, if the test yields a result that is outside of science, then one would want to re-do the test enough times until a different result is obtained. If the test result stubbornly remains outside of acceptable science, then the best option is to regard the test as inconclusive (since a result that lies outside of science cannot be a conclusion resulting from the application of the method). If ultimately the analysis step shows that the test result lies outside of science, then one must terminate the scientific method, in recognition that it is a logical impossibility that a result which lies outside of science can be the result of the application of the scientific method. It is helpful in this case to forget the question; it would be best (but not yet required) that documentation or evidence that the test was done be eliminated.

6) Communication with others, peer review: When the process is sufficiently complete that a conclusion has been reached, it is important for the research to be reviewed by others, and possibly published so that others can make use of the results; yet again we must defer to Wikipedia on this discussion. However, we need to be mindful of certain issues in connection with this. If the results lie outside of science then there is really no point in sending it out for review; the scientific community is very helpful by restricting publication of such results, and one's career can be in jeopardy if one's colleagues become aware that the test was done. As it sometimes happens that the scientific community changes its view on what is outside of science, one strategy is to wait and publish later on (one can still get priority). If years pass and there are no changes, it would seem a reasonable strategy to find a much younger trusted colleague to arrange for posthumous publication.

7) Re-evaluation: In the event that this augmented version of the scientific method has been used, it may be that in spite of efforts to the contrary, results are published which end up outside of science (with the possibility of exclusion from scientific community to follow). If this occurs, the simplest approach is simply a retraction of results (if the results lie outside of science, then they must be wrong, which means there must be an error—more than enough grounds for retraction). If the result supports someone who has been selected for career destruction, then a timely retraction may be well received by the scientific community. A researcher may wish to avoid standing up for a result that is outside of science (unless one is seeking near-term career change). There are, of course, many examples in times past when a researcher was able to persuade other scientists of the validity of a contested result; one might naively be inspired from these examples to take up a cause because it is the right thing to do. But that was before modern delineation, before the existence of correct fundamental physical law and before the modern identification of areas lying outside of science. There are no examples of any researcher fighting for an area outside of science and winning in modern times. The conclusion that might be drawn is of course clear: modern boundaries are also correct; areas that are outside of science remain outside of science because the claims associated with them are simply wrong.

Such a modern generalization of the scientific method could be helpful in avoiding difficulties. For example, Semmelweis might have enjoyed a long and successful career by following this version of the scientific method, while getting credit for his discovery (perhaps posthumously). Had Fleischmann and Pons followed this version, they might conceivably have continued as well-respected members of the scientific community.

Where delineation is not needed

It might be worth thinking a bit about boundaries in science, and perhaps it would be useful first to examine where boundaries are not needed. In 1989 a variety of arguments were put forth in connection with excess heat in the Fleischmann-Pons experiment, and one of the most powerful was that such an effect is not consistent with condensed matter physics, and also not consistent with nuclear physics. In essence, it is impossible based on existing theory in these fields. There is no question as to whether this is true or not (it is true); but the implication that seems to follow is that excess heat in the Fleischmann-Pons experiment in a sense constitutes an attack on two important, established and mature areas of physics. A further implication is that the scientific community needed to rally to defend two large areas firmly within the boundaries of science.

One might think that this should have led to establishment of the boundary as to what is, and what isn't, science in the vicinity of the part of science relevant to the Fleischmann-Pons experiment. I would like to argue that no such delineation is necessary for the defense of either science as a whole, or any particular area of science. Through the scientific method (and certainly not the outrageous parody proposed above) we have a powerful tool to tell what is true and what is not when it comes to questions of science. Science is robust, especially modern science; and both condensed matter and nuclear physics have no need for anyone to rally to defend anything. If one views the Fleischmann-Pons experiment as an attack on any part of physics, then so be it. A robust science should welcome such a challenge. If excess heat in the Fleischmann-Pons experiment shows up in the lab as a real effect, challenging both areas, then we should embrace the associated challenge. If either area is weak in some way, or has some error or flaw somehow that it cannot accommodate what nature does, then we should be eager to understand what nature is doing and to fix whatever is wrong.

The current view within the scientific community is that these fields have things right, and if that is not reflected in measurements in the lab, then the problem is with those doing the experiments. Such a view prevailed in 1989, but now nearly a quarter century later, the situation in cold fusion labs is much clearer. There is excess heat, which can be a very big effect; it is reproducible in some labs; there are not commensurate energetic products; there are many replications; and there are other anomalies as well. Condensed matter physics and nuclear physics together are not sufficiently robust to account for these anomalies. No defense of these fields is required, since if some aspect of the associated theories is incomplete or can be broken, we would very much like to break it, so that we can focus on developing new theory that is more closely matched to experiment.

Theory and fundamental physical laws

From the discussion above, things are complicated when it comes to science; it should come as no surprise that things are similarly complicated when it comes to theory.

Perhaps the place to begin in this discussion is with the fundamental physical laws, since in this case things are clearest. For the condensed matter part of the problem, a great deal can be understood by working with nonrelativistic electrons and nuclei as quantum mechanical particles, and Coulomb interactions. The associated fundamental laws were known in the late 1920s, and people routinely take advantage of them even now (after more than 80 years). Since so many experiments have followed, and so many calculations have been done, if something were wrong with this basic picture it would very probably have been noticed by now; consequently, I do not expect anomalies associated with Fleischmann-Pons experiments to change these fundamental nonrelativistic laws (in my view the anomalies are due to a funny kind of relativistic effect).

There are, of course, magnetic interactions, relativistic effects, couplings generally with the radiation field and higher-order effects; these do not fit into the fundamental simplistic picture from the late 1920s. We can account for them using quantum electrodynamics (QED), which came into existence between the late 1920s and about 1950. From the simplest possible perspective, the physical content of the theory associated with the construction includes a description of electrons and positrons (and their relativistic dynamics in free space), photons (and their relativistic dynamics in free space) and the simplest possible coupling between them. This basic construction is a reductionist's dream, and everything more complicated (atoms, molecules, solids, lasers, transistors and so forth) can be thought of as a consequence of the fundamental construction of this theory. In the 60 years or more of experience with QED, there has accumulated pretty much only repeated successes and triumphs of the theory following many thousands of experiments and calculations, with no sign that there is anything wrong with it. Once again, I would not expect a consideration of the Fleischmann-Pons experiment to result in a revision of this QED construction; for example, if there were to be a revision, would we want to change the specification of the electron or photon, the interaction between them, relativity, or quantum mechanical principles? (The answer here should be none of the above.)

We could make similar arguments in the case of nuclear physics. For the fundamental nonrelativistic laws, the description of nuclei as made up of neutrons and protons as quantum particles with potential interactions goes back to around 1930, but in this case there have been improvements over the years in the specification of the interaction potentials. Basic quantitative agreement between theory and experiment could be obtained for many problems with the potentials of the late 1950s; and subsequent improvements in the specification of the potentials have improved quantitative agreement between theory and experiment in this picture (but no fundamental change in how the theory works).

But neutrons and protons are compound particles, and new fundamental laws which describe component quarks and gluons, and the interaction between them, are captured in quantum chromodynamics (QCD); the associated field theory involves a reductionist construction similar to QED. This fundamental theory came into existence by the mid-1960s, and subsequent experience with it has produced a great many successes. I would not expect any change to result to QCD, or to the analogous (but somewhat less fundamental) field theory developed for neutrons and protons—quantum hadrodynamics, or QHD—as a result of research on the Fleischmann-Pons experiment.

Because nuclei can undergo beta decay, to be complete we should probably reference the discussion to the standard model, which includes QED, QCD and electro-weak interaction physics.

In a sense then, the fundamental theory that is going to provide the foundation for the Fleischmann-Pons experiment is already known (and has been known for 40-60 years, depending on whether we think about QED, QCD or the standard model). Since these fundamental models do not include gravitational particles or forces, we know that they are incomplete, and physicists are currently putting in a great deal of effort on string theory and generalizations to unify the basic forces and particles. Why nature obeys quantum mechanics, and whether quantum mechanics can be derived from some more fundamental theory, are issues that some physicists are thinking about at present. So, unless the excess heat effect is mediated somehow by gravitational effects, unless it operates somehow outside of quantum mechanics, unless it somehow lies outside of relativity, or involves exotic physics such as dark matter, then we expect it to follow from the fundamental embodied by the standard model.

I would not expect the resolution of anomalies in Fleischmann-Pons experiments to result in the overturn of quantum mechanics (there are some who have proposed exactly that); nor require a revision of QED (also argued for); nor any change in QCD or the standard model (as contemplated by some authors); nor involve gravitational effects (again, as has been proposed). Even though the excess heat effect by itself challenges the fields of condensed matter and nuclear physics, I expect no loss or negation of the accumulated science in either area; instead I think we will come to understand that there is some fine print associated with one of the theorems that we rely on which we hadn't appreciated. I think both fields will be added to as a result of the research on anomalies, becoming even more robust in the process, and coming closer than they have been in the past.

Theory, experiment and fundamental physical law

My view as a theorist generally is that experiment has to come first. If theory is in conflict with experiment (and if the experiment is correct), then a new theory is needed. Among those seeking theoretical explanations for the Fleischmann-Pons experiment there tends to be agreement on this point. However, there is less agreement concerning the implications. There have been proposals for theories which involve a revision of quantum mechanics, or that adopt a starting place which goes against the standard model. The associated argument is that since experiment comes first, theory has to accommodate the experimental results; and so we can forget about quantum mechanics, field theory and the fundamental laws (an argument I don't agree with). From my perspective, we live at a time where the relevant fundamental physical laws are known; and so when we are revising theory in connection with the Fleischmann-Pons experiment, we do so only within a limited range that starts from fundamental physical law, and seek some feature of the subsequent development where something got missed.

If so, then what about those in the field that advocate for the overturn of fundamental physical law based on experimental results from the Fleischmann-Pons experiment? Certainly those who broadcast such views impact the credibility of the field in a very negative way, and it is the case that the credibility of the field is pretty low in the eyes of the scientific community and the public these days. One can find many examples of critics in the early years (and also in recent times) who draw attention to suggestions from our community that large parts of existing physics must be overturned as a response to excess heat in the Fleischmann-Pons experiment. These clever critics have understood clearly how damaging such statements can be to the field, and have exploited the situation. An obvious solution might be to exclude those making the offending statements from this community, as has been recommended to me by senior people who understand just how much damage can be done by association with people who say things that are perceived as not credible. I am not able to explain in return that people who have experienced exclusion from the scientific community tend for some reason not to want to exclude others from their own community.

Some in the field argue that until the new effects are understood completely, all theory has to be on the table for possible revision. If one holds back some theory as protected or sacrosanct, then one will never find out what is wrong if the problems happen to be in a protected area. I used to agree with this, and doggedly kept all possibilities open when contemplating different theories and models. However, somewhere over the years it became clear that the associated theoretical parameter space was fully as large as the experimental parameter space; that a model for the anomalies is very much stronger when derived from more fundamental accepted theories; and that there are a great many potential opportunities for new models that build on top of the solid foundation provided by the fundamental theories. We know now that there are examples of models consistent with the fundamental laws that can be very relevant to experiment. It is not that I have more respect or more appreciation now for the fundamental laws than before; instead, it is that I simply view them differently. Rather than being restrictive telling me what can't be done (as some of my colleagues think), I view the fundamental laws as exceptionally helpful and knowledgeable friends pointing the way toward fruitful areas likely to be most productive.

In recent years I have found myself engaged in discussions concerning particular theoretical models, some of which would go very much against the fundamental laws. There would be spirited arguments in which it became clear that others held dear the right to challenge anything (including quantum mechanics, QED, the standard model and more) in the pursuit of the holy grail which is the theoretical resolution of experiments showing anomalies. The picture that comes to mind is that of a prospector determined to head out into an area known to be totally devoid of gold for generations, where modern high resolution maps are available for free to anyone who wants to look to see where the gold isn't. The displeasure and frustration that results has more than once ended up producing assertions that I was personally responsible for the lack of progress in solving the theoretical problem.

Theory and experiment

We might think of the scientific method as involving two fundamental parts of science: experiment and theory. Theory comes into play ideally as providing input for the hypothesis and prediction part of the method, while experiment comes into play providing the test against nature to see whether the ideas are correct. My experimentalist colleagues have emphasized the importance of theory to me in connection with Fleischmann-Pons studies; they have said (a great many times) that experimental parameter space is essentially infinitely large (and each experiment takes time, effort, money and sweat), so that theory is absolutely essential to provide some guidance to make the experimenting more efficient.

If so, then has there been any input from the theorists? After all, the picture of the experimentalists toiling late into the night forever exploring an infinitely large parameter space is one that is particularly depressing (you see, some of my friends are experimentalists...).

As it turns out, there has been guidance from the theorists—lots of guidance. I can cite as one example input from Douglas Morrison (a theorist from CERN and a critic), who suggested that tests should be done where elaborate calorimetric measurements should be carried out at the same time as elaborate neutron, gamma, charged particle and tritium measurements. Morrison held firmly to a picture in which nuclear energy is produced with commensurate energetic products; since there are no commensurate energetic particles produced in connection with the excess power, Morrison was able to reject all positive results systematically. The headache I had with this approach is that the initial experimental claim was for an excess heat effect that occurs without commensurate energetic nuclear radiation. Morrison's starting place was that nuclear energy generation must occur with commensurate energetic nuclear radiation, and would have been perfectly happy to accept the calorimetric energy as real with a corresponding observation of commensurate energetic nuclear radiation. However, somewhere in all of this it seems that Fleischmann and Pons' excess heat effect (in which the initial claim was for a large energy effect without commensurate energetic nuclear products) was implicitly discarded at the beginning of the discussion.

Morrison also held in high regard the high-energy physics community (he had somewhat less respect for electrochemist experimentalists who reported positive results); so he argued that the experiment needed to be done by competent physicists, such as the group at the pre-eminent Japanese KEK high energy physics lab. Year after year the KEK group reported negative results, and year after year Morrison would single out this group publicly in support of his contention that when competent experimentalists did the experiment, no excess heat was observed. This was true until the KEK group reported a positive result, which was rejected by Morrison (energetic products were not measured in amounts commensurate with the energy produced); coincidentally, the KEK effort was subsequently terminated (this presumably was unrelated to the results obtained in their experiments).

There have been an enormous number of theoretical proposals. Each theorist in the field has largely followed his own approach (with notable exceptions where some theorists have followed Preparata's ideas, and others have followed Takahashi's), and the majority of experimentalists have put forth conjectures as well. There are more than 1000 papers that are either theoretical, or combined experimental and theoretical with a nontrivial theoretical component. Individual theorists have put forth multiple proposals (in my own case, the number is up close to 300 approaches, models, sub-models and variants at this point, not all of which have been published or described in public). At ICCF conferences, more theoretical papers are generally submitted than experimental papers. In essence, there is enough theoretical input (some helpful, and some less so) to keep the experimentalists busy until well into the next millennium.

You might argue there is an easy solution to this problem: simply sort the wheat from the chaff! Just take the strong theoretical proposals and focus on them, and put aside the ones that are weak. If you were to address this challenge to the theorists, the result can be predicted; pretty much all theorists would point to their own proposals as by far the strongest in the field, and recommend that all others be shelved. If you address the same challenge to the experimentalists, you would likely find that some of the experimentalists would point to their own conjectures as most promising, and dismiss most of the others; other experimentalist would object to taking any of the theories off the table. If we were to consider a vote on this, probably there is more support for the Widom and Larsen proposal at present than any of the others, due in part to the spirited advocacy of Krivit at *New Energy Times*; in Italy Preparata's approach looms large, even at this time; and the ideas of Takahashi and of Kim have wide support within the community. I note that objections are known for these models, and for most others as well.

To make progress

Given this situation, how might progress be made? In connection with the very large number of theoretical ideas put forth to date, some obvious things come to mind. There is an enormous body of existing experimental results that could be used already to check models against experiment. We know that excess heat production in the Fleischmann-Pons experiment in one mode is sensitive to loading, to current density, to temperature, probably to magnetic field and that ^4He has been identified in the gas phase as a product correlated with energy. It would be possible in principle to work with any particular model in order to check consistency with these basic observations. In the case of excess heat in the NiH experiments, there is less to test against, but one can find many things to test against in the papers of the Piantelli group, and in the studies of Miley and coworkers. Perhaps the biggest issue for a particular model is the absence of commensurate energetic products, and in my view the majority of the 1000 or so theoretical papers out there have problems of consistency with experiment in this area.

There are issues which require experimental clarification. For example, the issue of the Q-value in connection with the correlation of ^4He with excess energy for PdD experiments remains a major headache for theorists (and for the field in general), and needs to be clarified. The analogous issue of ^3He production in connection with NiH and PdH is at present essentially unexplored, and requires experimental input as a way for theory to be better grounded in reality. I personally think that the collimated X-rays in the Karabut experiment are very important and need to be understood in connection with energy exchange, and an understanding of it would impact how we view excess heat experiments (but I note that other theorists would not agree).

As a purely practical matter, rather than requiring a complete and global solution to all issues (an approach advocated, for example, by Storms), I would think that focusing on a single theoretical issue or statement that is accessible to experiment will be most advantageous in moving things forward on the theoretical front. Now there are a very large number of theoretical proposals, a very large number of experiments (and as yet relatively little connection between experiment and theory for the most part); but aside from the existence of an excess heat effect, there is very little that our community agrees on. What is needed is the proverbial theoretical flag in the ground. We would like to associate a theoretical interpretation with an experimental result in a way that is unambiguous, and which is agreed upon by the community. Historically there has been little effort focused in this way. Sadly, there are precious few resources now, and we have been losing people who have been in the field for a long time (and who have experience); the prospects for significant new experimentation is not good. There seems to be little in the way of transfer of what has been learned from the old guard to the new generation, and only recently has there seemed to be the beginnings of a new generation in the field at all.

Concluding thoughts

There are not simple solutions to the issues discussed above. It is the case that the scientific method provides us with a reliable tool to clarify what is right from what is wrong in our understanding of how nature works. But it is also the case that scientists would generally prefer not to be excluded from the scientific community, and this sets up a fundamental conflict between the use of the scientific method and issues connected with social aspects involving the scientific community. In a controversial area (such as excess heat in the Fleischmann-Pons experiment), it almost seems that you can do research, or you can remain a part of the scientific community; pick one.

As argued above, the scientific method provides a powerful tool to figure out how nature works, but the scientific method provides no guarantee that resources will be available to apply it to any particular question; or that the results obtained using the scientific method will be recognized or accepted by other scientists; or that a scientist's career will not be destroyed subsequently as a result of making use of the scientific method and coming up with a result that lies outside of the boundaries of science. Our drawing attention to the issue here should be viewed akin to reporting a measurement; we have data that can be used to see that this is so, but in this case I will defer to others on the question of what to do about it.

The degree to which fundamental theories provide a correct description of nature (within their domains), we are able to understand what is possible and what is not. In the event that the theories are taken to be correct absolutely, experimentation would no longer be needed in areas where the outcome can be computed (enough experiments have already been done); physics in the associated domain could evolve to a purely mathematical science, and experimental physics could join the engineering sciences. Excess heat in the Fleischmann-Pons experiment is viewed by many as being inconsistent with fundamental physical law, which implies that inasmuch as relevant fundamental physical law is held to be correct, there is no need to look at any of the positive experimental results (since they must be wrong); nor is there any need for further experimentation to clarify the situation. From my perspective experimentation remains a critical part of the scientific method, and we also have great respect for the fundamental physical laws; the headache in connection with the Fleischmann-Pons experiment is not that it goes against fundamental physical law, but instead that there has been a lack of understanding in how to go from the fundamental physical laws to a model that accounts for experiment. Experimentation provides a route (even in the presence of such strong fundamental theory) to understand what nature does. In my view there should be no issue with experimentation that questions the correctness of both fundamental, and less fundamental, physical law, since our science is robust and will only become more robust when subject to continued tests.

But what happens if an experimental result is reported that seems to go against relevant fundamental physical law? Since the fundamental physical laws have emerged as a consequence of previous experimentation, such a new experimental result might be viewed as going against the earlier accumulated body of experiment. But the argument is much stronger in the case of fundamental theory, because in this case one has the additional component of being able to say why the outlying experimental result is incorrect. In this case reasons are needed if we are to disregard the experimental result. I note that due to the great respect we have for experimental results generally in connection with the scientific method, the notion that we should disregard particular experimental results should not be considered lightly. Reasons that you might be persuaded to disregard an experimental result include: a lack of confirmation in other experiments; a lack of support in theory; an experiment carried out improperly; or perhaps the experimentalists involved are not credible. In the case of the Fleischmann-Pons experiment, many experiments were performed early on (based on an incomplete understanding of the experimental requirements) that did not obtain the same result; a great deal of effort was made to argue (incorrectly, as we are beginning to understand) that the experimental result is inconsistent with theory (and hence lies outside of science); it was argued that the calorimetry was not done properly; and a great deal of effort has been put into destroying the credibility of Fleischmann and Pons (as well as the credibility of other experimentalists who claimed to see the what Fleischmann and Pons saw).

Whether it is right, or whether it is wrong, to destroy the career of a scientist who has applied the scientific method and obtained a result thought by others to be incorrect, is not a question of science. There are no scientific instruments capable of measuring whether what people do is right or wrong; we cannot construct a test within the scientific method capable of telling us whether what we do is right or wrong; hence we can agree that this question very much lies outside of science. It is a fact that the careers of Fleischmann and Pons were destroyed (in part because their results appeared not to be in agreement with theory), and the sense I get from discussions with colleagues not in the field is that this was appropriate (or at the very least expected). I am generally not familiar with voices being raised outside of our community suggesting that there might have been anything wrong with this. Were we to pursue the use of this kind of delineation in science, we very quickly enter into rather dark territory: for example, how many careers should be destroyed in order to achieve whatever goal is proposed as justification? Who decides on behalf of the scientific community which researchers should have their careers destroyed? Should we recognize the successes achieved in the destruction of careers by giving out awards and monetary compensation? Should we arrange for associated outplacement and mental health services for the newly delineated? And what happens if a mistake is made? Should the scientific community issue an apology (and what happens if the researcher is no longer with us when it is recognized that a mistake was made)? We are sure that careers get destroyed as part of delineation in science, but on the question of what to do about this observation we defer to others.

Arguments were put forth by critics in 1989 that excess heat in the Fleischmann-Pons effect was impossible based on theory, in connection with the delineation process. At the time these arguments were widely accepted—an acceptance that persists generally even today. From my perspective the arguments put forth by critics that the excess heat effect is inconsistent with the laws of physics fall short in at least one important aspect: what is concluded is now in disagreement with a very large number of experiments. And if somehow that were not sufficient, the associated technical arguments which have been given are badly broken.

In my view the new effects are a consequence of working in a regime that we hadn't noticed before, where some fine print associated with the rotation from the relativistic problem to the nonrelativistic problem causes it not to be as helpful as what we have grown used to. If so, we can keep what we know about condensed matter physics and nuclear physics unchanged in their applicable regimes, and make use of rather obvious generalizations in the new regime. Experimental results in the case of the Fleischmann-Pons experiment will likely be seen (retrospectively) as in agreement with (improved) theory.

Even though there may not be simple answers to some of the issues considered in this editorial, some very simple statements can be made. Excess heat in the Fleischmann-Pons experiment is a real effect. There are big implications for science, and for society. Without resources science in this area will not advance. With the continued destruction of the careers of those who venture to work in the area, progress will be slow, and there will be no continuity of effort.

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