**10)**

**10) finally, the author’s opinion:**

The first thought is the approach to science. No theory, law or hypothesis should be thought of a settled. This psychological problem is a trap for advancement of science. Personally, I have observed this trap in fields such are corrosion, electrochemistry especially plating and now physisorption. When this happens, the field 1) shuts down to new ideas and 2) becomes an art rather than what should be a science with advanced theories that simplify calculations to obtain better results more quickly.[[1]](#footnote-1)

In electroplating it takes and experienced crafts person to get a plating with a profile that is smooth and reproducible. I have seen how that is done with masking and distance adjustments. Such a method takes time and is usually costly. Attempts have been made to computerize this, but the problem is still a trial-and-error input.

In corrosion, the field has been ruled by the Wagner theory from which nothing can be predicted. The literature is very large for the very reason that the only way to approach corrosion is to do multiple testing. Even then the testing may be insufficient. One only needs to look around the USA and the world to find buildings and bridges that are collapsing to be convinced that something better is needed. For parts that are supposed to last 25 – 50 years, the testing is too costly and slow to do ahead of time, thus, the public becomes essentially guineapigs for the engineers.

I have heard the arguments that the basic research is too costly and takes time. Yet, when you consider the magnitude and expense of all the bridges that are unsafe, of all the parts of airplanes that need to be replaced early or are replace too late, and many more examples, the research expenditures are very small. Engineering studies usually require large expenditure of time and money. They are almost always more expensive than basic research[[2]](#footnote-2) and more restrictive in application. What is worse is often engineers do not want to consult the basic researcher nor to believe what the basic research says.[[3]](#footnote-3)

I hear people now expressing the hope that AI will solve these problems, but what do you suppose AI would do for physical adsorption? Obviously, it would start with the BET and go downward from there. Use of QM or ESW seem very unlikely, since the literature on these subjects is too small in comparison. It is even less likely to invent a theory that is thought out and tested, after all, how would it test it, by comparison to BET, Monti Carlo and XXDFT? How would AI construct a critical experiment?

The BET is one of those theories that has served science in a negative way by stifling innovation and squelching presentation of new hypothesis for adsorption. Critical analyses by other investigators has not, at least in recent years, been performed. As stated in this series, the analysis which indicated a theoretical problem were ignored because the BET was the only theory with a value of surface area explicitly present. However, it was well known that it was a poor fit to the overall isotherm and that the value obtained was incorrect. The measurement of surface area is also in doubt, as the recent “effective surface area” handle (whatever that means) attests to. (At the Oak Ridge labs, measurement by electron transmission microscopy modified with a computer to act as a tomography always disagreed with the BET. It always yielded an answer that was too low in comparison to the BET. So, the technique was abandoned. That shows how blind the belief was that the BET was correct.) It must also be pointed out that the Langmuir isotherm for chemisorption has also been a stumbling block for several scientific areas and has not been tested adequately.

What about the future? Of course, no one can tell how things will fair for physisorption. It is my belief that the QM/ESW approach has promise as a step forward. However, in the rush to “improve” QM/ESW, the discipline may be again steered in the wrong direction by propping up a hypothesis whose validity is in question. It should be encouraged to attempt to disprove this hypothesis, and indeed this is a noble thing to do even if there is no replacement, at least it will be known that a replacement is needed.

As noted in the various sections, the testing of the QM/ESW hypothesis and the collection of data related to such test is very meager. Test of adsorptives on well-known adsorbents is still in order, especially if the HV or, if needed, UHV had been lacking. Data is required down to the threshold pressure in order to get the energies correctly, upon which the rest of the analysis hinges. For example, the data by Goldmann and Polanyi has been analyzed in the forementioned book, but the low-pressure data is missing. (This is no comment on their technique. The ability may not have been possible for them.) This would be an obvious group of isotherms to redo with modern equipment for which the “Big Errors” have been eliminated.

Finally, the statement by Bertrum Russel should be repeated as a mantra, as Polanyi stated:

1. This psychological barrier is just not a science “thing.” It is also important for engineering and for practical production. Here are two stories from my experience one had a bad outcome, if you wish to call a bad outcome the waste of $8,000,000,000 bad, but the other had a good outcome at literally the last day. This other project that cost about the same as the first, and had very large implications for the safety of USA. The first one could have been avoided, had they taken my advice. But the engineering concept was already set, but they tried for another 10 years to make it work with failure after failure. Eventually, the politician got involved and pulled the plug. The second one was solved on the deadline. The DoD was about to scrap an entire missile system that was already built except for this last part. They finally called upon the scientists, one day before the deadline. We provided a simple fix that solved the problem literally the next day. [↑](#footnote-ref-1)
2. I have several first-hand experiences where engineering studies spent (literally) billions ($Y x 109) to determine systems. In contrasted a ½ person yr of basic research study that predicted the results and gave a promising alternative. I have experienced this several times, sometimes I could put them out of a mess, but often it was too late, as the two example above illustrate. [↑](#footnote-ref-2)
3. In several instances, the engineers would refuse to believe the thermodynamic realities and insist upon having me do experiments that essentially violated thermodynamics. [↑](#footnote-ref-3)