Mathematics and Modern Science

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Mathematics is the tool-kit of science and technology, the implements by which we technocrats build and repair our models of nature. By mathematics I do not mean the abstractions of theory that exist for their own sake, the definitions and theorems used to prove the proofs, but the applications of analysis and calculation, that is, the results of those proofs that serve as the very foundations of what we call reality. For natural phenomena to be "true" in our world, they must submit to the symbols of logic and allow themselves to appear as formulae and equations. Only then can we know that what we call real is real.

Unless we are, ourselves, mathematicians, our faith in the equations we use rests on what has gone before and what is most likely outside our realm of expertise. We rely entirely on the work of others, and with that faith goes the implied infallibility of mathematical science and its wonder-workers. We, without question, believe in the tenets of mathematics and the word of its priesthood.

If, in our technical and scientific eyes, there is a high priest, it is the statistician, the one who verifies or nullifies our calculations according to the incomprehensible logic of his sacred law. It is the statistician who discovers the measure of truth in our measurements, who, in the end, justifies our work. And once he has passed judgment, we are obliged to acquiesce or be ostracized by our peers and the world.

Yet, in the course of our education, are we not ourselves initiated into some small part of the priestly code? Are we not able to calculate the standard deviations and confidence intervals with which we "properly" express our findings? We know, even if we often forget or neglect, the meaning of "significant figures" and the precision implied by their use. How is it, then, that we continue to accept and use numbers that even the simplest minded among us know to be little better than embellishments?

I speak of precision, that preciseness of experiment and analysis that is so happily and shamelessly shared with the world and offered up for judgment with the full knowledge that only the most extreme exceptions will be rejected. It is precision that stands over against our persistent absolutist view of the world and it is our (conscious?) neglect of the meaning of precision that tilts the Statistician's judgment in our favor.

If precision is the probability of repeatability, do we even know what we are repeating? We ignore the basic tenet of probability theory, that a normal distribution requires a proper collective (to use von Mises' term[1]), when we redefine "collective" to include members that are not logically inclusive. Consider this. If a group of analytical chemists develops a new method and "proves" its reliability through a confidence interval, what does this confidence interval say about the application of the method to my sample? If by collective we mean either many repetitive observations of one sample or the observation of a particular characteristic of many samples, to which definition does my sample belong? Obviously, it is not the former since my sample is from a different source than that from which the confidence interval was calculated. If it is the latter, then, by simply applying their method, my result becomes part of their collective, which makes no sense at all. If, for example, their expression of precision brackets a mean of 25 with a confidence interval of ± 10 and my sample measures 40, my results will skew their data or, more likely, be rejected as an "outlier." Consequently, I cannot use their statistics to prove the precision and accuracy of my result. The only recourse I have, then, is to create my own collective, determine the efficiency of recovery for my application of the method, and calculate a proper confidence interval for my own data. But, can calculating precision from two or three measurements be trusted, since the probability that all of the measurements lie on one side or the other of the true mean is really quite high? If not, then, the time required for the creation of useful statistics from ten or twenty analyses of a sample is substantial as this statistical scheme will become necessary for each analytical subject in each bath, which is precisely why we have come to apply one set of statistics to all "similar" collectives. Nevertheless, practice tells us that, if we remain honest with the significant figures we report and are able to recognize (and live with) the occasional (and inevitable) anomalous result, we can be reasonably confident that our analysis results reflect reality within some "acceptable" limits. Our only requisite condition is repeatable sampling.

Are we, therefore, justified in sacrificing reason for convenience? Every plating chemist knows that analytical variance is much more a function of sampling than of method, that repeating results is difficult enough from a single sample, how much more so for multiple samples. Yet, for the purpose of bath control, experience tells us that understanding is best achieved through a combination of analysis and intimate knowledge of the bath's behavioral tendencies. In the limited world of process control, then, the answer is "yes", so long as we remember the nature of our scientific dishonesty.

But, what may be acceptable for industrial process control is anathema to government regulations that demand detection limits and precision beyond our everyday methods and beyond what most of us are able to provide under any circumstances. Perhaps the most common source of analytical methods in the metal finishing industry, Electroplating Engineering Handbook,[2] lists decades-old analysis procedures with no reference to statistics. Can it be that the authors understood that confidence intervals can only be properly generated from the tested sample? It is ironic that once technology catches up with the demands of regulation, the regulators raise the bar. Where industrial analysts report best guesses, regulators demand accuracy and precision, with no credit given for impossibility. Plating chemists may know the limits of their sampling ability. Regulators apparently do not.

We have seen this phenomenon with lead, cadmium ("heavy metals" in general), and with cyanide and we see it today with hexavalent chromium. Based on data from a time when analysis of parts per million was uncertain at best, so-called "safe" permissible exposure limits are extrapolated in parts per billion. Somehow, this runs counter to what I would call logic. But, as so often happens in our technological world, the numbers are their own proof, they are the essence of truth, and the mathematicians who undoubtedly know better remain mute to it all, saying only that "mathematics is fundamental truth within itself. How it is abused is neither our fault nor our concern".

Neither is science blameless. What scientist doesn't subscribe to the ideal of unlimited progress in spite of its "inherent admission that the good and the true are unattainable"[3], as he rushes headlong toward the ever-receding end of all knowledge? If the paradox is lost on us, the effects are nevertheless felt; doing never quite catches up with thinking. (Or is it the other way around?). Yet, this is the main attraction to the world of science, the opportunity to pursue a theory of everything or, more modestly, to become an "authority" in some narrow field of study. Who will discover our lies?

Or, what scientist or technician doesn't, somewhere in the course of his career, fall prey to the logical flaw of hypothesis as proof, of the rapid metamorphosis of an idea (good or bad) into scientific fact to be used to "prove" further hypotheses "with the result that the purely speculative character of the whole enterprise is forgotten".[4] If this description of twentieth century pseudo-science does not give us pause to consider our own thinking processes, then it can only be assumed that we are fostering the continuation of such anti-logic. In an industry where troubleshooting of chemical imbalances is an everyday occurrence, hypotheses are commonly offered that, since they are always difficult, if not impossible, to disprove, are handled precisely as Arendt describes. The "experts" are, all too often, those who can spin the best tales.

It is the scientific method that has fallen victim to this modern corruption of technology. Where the order of observation, hypothesis, experiment, and theory was once the undisputed law of science, hypothesis or even theory has, of late, come to be the impetus and the starting point of modern research. And, with the continued and increasing acceptance of this reordering, observation has become skewed, biased toward what we want to see, especially if the hypothesis or theory is already present. Since mathematics cares nothing about facts per se and only about how well the data fit together, that is, since mathematics presumes numbers to be inherently honest, then our statistics, having no superior to which they must answer, are free to be used as we see fit. Hence, with attention focused on our hypotheses and theories, which are easily proved by logically precise though factually flawed premises, the flaws go quite unnoticed except, perhaps, by the most astute of observers.

The data we generate and use for statistical analysis are too often drawn from single samples. Given what we have already said, this stands to reason if we are to show useful precision and accuracy with our methods. We cannot allow ourselves to be embarrassed by large relative standard deviations and wide confidence intervals. But reliance on data from one sample also masks the so often overlooked problem of repeatable sampling, especially in multiphase matrices. With data from a single sample, we can provide very tall and narrow Gaussian curves to describe probability distributions that cannot possibly represent the precision of the method if multiple real world samples are analyzed. It takes neither scientific nor mathematical genius to imagine the differences in samples where the matrix and the specie of interest are different states of matter. In fact, without the assurance of equal distribution, sample results are dependent on an endless number of variables and, while we may obtain some semblance of qualitative reality, the kind of quantitative precision that we tout with our methods is but a pipe dream.

That we know all this, that nothing I have said here is new, that is what I find so disappointing. We, with our supposed higher intellects, allow ourselves to be hood-winked by our own analytical laissez-faire into trusting our knowledge of the limits we work within only to be punished over and over again for trying to apply those same practices to the exacting and impossible demands of government regulation. The regulators' methods have become so complex that we are forced to hire "outside" laboratories not only to analyze our samples but also to collect them, and we have no way to ensure the truthfulness of either action. And, if we are not awakened to our shortcomings by the "progress" in chemistry, itself, we are most certainly overwhelmed by the mathematics that we haven't seen or used since our school days, if at all. Rather than going back to the books and relearning the calculations used by the method developers to prove their procedures, we are happy to rely on their superior experience and training as proof enough; that is, we rely on the infallibility of mathematics, however (and by whomever) it is used.

Implementing or tightening a regulation has the same effect as making a new law: someone must sacrifice a freedom. For the plating shop owner who finds it too difficult to comply, it is the freedom to do business. For his employees, it is the freedom to earn a living. For his customers, it is the freedom to use his services. If we are to allow the government to arbitrarily curtail our freedoms, then it is our duty to call the government to account. The burden of proof, both for necessity and for feasibility, of all regulations must fall on the government, not on the regulated, as is common practice.

But, before we can debate the regulators' claims, we must be fully secure in our counter-claims; that is, we must possess the deepest possible understanding of the scientific and mathematical intricacies with which we work. We must think about what we do. If the tools of our trade are contained in mathematics, then, as challenges grow, so too must the complexity of our tools. It is our responsibility to know how to use them. But, more than that, we must be able to first identify them and then to understand how they really work.

References

1. Von Mises, Richard, *Probability, Statistics, and Truth,* 1981, Dover Publications, Inc,, Mineola, NY, p. 11 ff. This rather short volume was originally published (in German) in 1928 and is, in my opinion, a "must read" for anyone truly interested in the meaning of probability and statistics.
2. Durney, L.J., ed., *Electroplating Engineering Handbook*, 4th ed, 1984 (Reprinted 1996, 1998), Chapman & Hall, London, pp 289-308. While some of the analytical methods may be antiquated, this book contains more information about plating in general than any other I have seen.
3. Arendt, Hannah, *The Life of the Mind*, 1978, Harcourt, Inc., San Diego, etc., p. 55.
4. Arendt, Hannah, *On Violence,* 1970, Harcourt Brace & Co., San Diego, etc., p. 6-7.

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