

Physical Interpretation of Very Small Concentrations

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Abstract—Reported observations of allergy-like responses by basophils at extreme dilutions were treated as unbelievable and compared with homeopathy. However, in several "mainstream" fields of science accepted formulations also imply action by substances present at the level of much less than a single molecule or ion.

Reported observations of basophil degranulation at extreme dilution (Davenas et al., 1988) have been disbelieved, just as have homeopathic claims, on the grounds that some of the dilutions describe circumstances under which not even a single molecule of the active substance is present. (For more on that controversy, see Benveniste, 1988; Maddox, Randi, & Stewart, 1988; Pool, 1988; Truzzi, 1989.) Yet in some parts of science, similar paradoxes are countenanced: concerning solubility products or electrode potentials, for example.

According to standard, accepted notions, the maximum amount of an ionic substance, $M_a^{b+}N_b^{a-}$, that can dissolve in some liquid or solution is described by the solubility product $K_{sp} = [M^{b+}]^a[N^{a-}]^b$, where square brackets denote concentration, typically expressed in moles per liter. K_{sp} for mercuric sulfide, HgS, at 4×10^{-53} is by no means the smallest solubility product listed (Meites, 1963), yet it may describe physical circumstances whose comprehension poses difficulties. (How then, one might ask, can such quantities be measured? Most directly from the shift of standard electrode potentials as N is added to solutions of M, see later. Since electrode potentials are direct functions of free energies, diverse thermodynamic data offer alternate ways of calculating solubility or complexation constants.) Shake solid mercuric sulfide with a solution that is 1-molar in sulfide ion (for instance, molar sodium sulfide): at equilibrium, the solution is then supposed to contain 4×10^{-53} moles per liter of mercuric ions—or, 2.4×10^{-29} ions per liter (since one mole of ions comprises about 6×10^{23} ions). In most liters of such a solution, there cannot then be even a single mercuric ion; nevertheless, the solubility product predicts satisfactorily what happens in solutions that have both mercuric and sulfide ions—down to as low dilutions as one has so far been able to measure.

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The same paradox applies to complexes. The formation constant of copper cyanide (CuCN) is 10^{24} , so that the amounts of free (uncomplexed) copper or cyanide ions in small volumes of solution turn out to be fractions of ions; and, again, this is by no means the largest such constant cited in the literature (Meites, 1963).

Two resolutions of this paradox can be suggested. First, that calculations are meaningless for concentrations that are beyond experimental access: any "signal" from the purported equilibrium would be drowned in the "noise" of competing reactions from finite traces of other substances. Or, second, that the calculations ought not to be interpreted as time-average concentrations but as probabilities that, at any given moment, an ion of a given sort is present in a given space: thus, one would not confront the conundrum of fractional ions but the circumstance that for only some of the time is an ion present in a given volume of solution.

The first of these suggestions, which amounts to strict philosophic instrumentalism, is not intellectually satisfying. So long as there is no instance in which calculations have failed to describe experimental results, so long as there is nothing in the theory that prescribes a lower bound of concentration beyond which it is invalid, one wants to be able to use the same interpretation at all concentrations; "thought experiments" are, after all, a respectable aspect of science, and they do present us with this paradox. We should like to be able to think in the same way about a solubility product of 10^{-97} (for Bi_2S_3) as about one of almost unity (for As_2O_3 or V_2O_5 , see Meites, 1963).

The second suggestion amounts to interpreting a physical probability as an inherent propensity, rather than as a statistical frequency—something that is also commonly done with respect to radioactive decay. Thus, in one milligram of ^{48}Ca , whose half-life is about 2×10^{16} years (Chemical Rubber Co., 1966), there will occur about one disintegration per day, but each of the (approximately) 10^{19} atoms is equally likely to decay, and we cannot predict which one will do so at a given time.

However, it is not clear that either of the above-suggested resolutions—even were one of them in itself satisfying—can accommodate the shifting of electrode potentials in the presence of complexing agents.

Under accepted theory, equilibrium electrode potentials (E) are related logarithmically to the concentrations of the oxidized and reduced species:

$$E = E^\circ + 0.06/n \log_{10}([\text{Ox}]/[\text{Red}])$$

where E° is a standard potential and n is the number of moles of electrons transferred between one mole of Red and one of Ox. There is no magnitude of $(E - E^\circ)$ for which this relation is supposed not to apply. At $E = E^\circ$, the concentrations of Ox and Red at the electrode surface are equal. As the potential moves further away, for every $60/n$ millivolts the ratio of [Ox] to [Red] changes by one order of magnitude. So a couple of volts away from the standard potential, in small volumes of dilute solution, one has electrode

potentials supposedly determined by the effects of species, one of which is present in amounts of less than a single ion or atom or molecule.

Yet calculations based on these notions are used routinely and without apparent qualm—and they work. Thus, when an ionic species takes part simultaneously in an electrode process and a complexation, the shift of the electrode potential is calculated by substituting, for the ionic concentration in the above equation, the concentration of free ion as calculated from the complexation constant:

$$E_{\text{complex}}^{\circ} = E^{\circ} + 0.06/n \log_{10} K_{\text{complex}},$$

as though it does not matter that the ionic concentrations so represented (or substituted for in the formulation) may correspond to fractions of ions in the actual volumes of solution manipulated. Nor have instances been reported in which such calculations failed to correspond to measured shifts of potential.

Indeed, it is precisely the measurement of electrode potentials that offers the readiest means for experimental determination of solubility products or complexation constants whose values are so extremely small or large. But other methods too are available, so that these numbers are not artefacts produced by fallacious measurement or interpretation of electrode potentials. For example, other thermodynamic data (heats of reaction, heats of solution, and so forth) can serve as checks of electrode potentials because free energy (G) is related to electrode potential:

$$G = -nFE,$$

where F is the charge corresponding to 1 mole of electrons. Where solubility products or complexation constants are of less extreme magnitude, direct analysis of the ionic or molecular concentrations affords an independent method of measurement—and analytical techniques are available down to concentrations significantly below the nanomolar (10^{-9} molar) level.

Compilations of values for complexation constants and solubility products obtained by a variety of techniques (chiefly the three already mentioned—potentiometry, thermodynamic data, and direct measurements of concentration) show (Bjerrum, Schwarzenbach, & Sillen, 1964) no systematic differences or anomalies arising from the use of any one given experimental approach. In other words, observational data and quantitative theoretical calculation remain in agreement for complexing constants whose magnitudes correspond to fractions, even minuscule fractions of ions or molecules. How can fractions of ions exert a physical effect? What alternative physical interpretation is available?

Returning for a moment to electrode potentials: for potentials sufficiently far from the standard potential, at the same time as the (apparent? envisaged? purported?) concentration of the one electroactive species becomes so small as to challenge physical interpretation, that of the other becomes enor-

mous; and such magnitudes have been ascribed physical significance in justifying the possibility that "cold nuclear fusion occurs in palladium electrodes: an overvoltage ($E - E^\circ$) of 0.8 volts corresponds to an effective pressure of hydrogen (deuterium) gas in the electrode of 10^{27} atmospheres (Fleischmann, 1989), which (if it represents physical reality) means that the reactant species are brought as closely together as under the conditions for which workers on "hot" fusion have been striving.

The point of this communication, then, is that for many phenomena, we (that is, scientists in the mainstream of science) can and do make calculations that work even though we have no intuitive feel for what mechanism might be involved, indeed even when the "mechanism" runs counter to everyday intuition (Dingle, 1972). Our physical understanding remains incomplete, in particular of circumstances involving very large or very small concentrations of materials.

It would seem unreasonable, then, to dismiss on a priori theoretical grounds such results as those reported by Davenas et al. (1988). The urge to so dismiss seems to have several roots, among them a concern that a failure to discredit would lend credence to homeopathic claims. But there are several fallacies here.

A failure to immediately discredit some claim made in science is far from any endorsement of that claim. All competent practising scientists understand that research papers are simply reports of work in which editors and referees have found no glaring error. Until independent workers confirm a claim, it remains nothing more than a claim; and if it remains unconfirmed long enough, it just drops out of sight. When the media or the public do not understand that—as typically they do not—they can legitimately be reminded of it, and forcefully if need be. That would impart an authentic picture of the workings of science, which is not presented when rushed attempts are made to discredit reports because they are "unbelievable" and therefore, it is inferred, somehow based on error even where no error can be demonstrated.

The concern that new claims might be taken too seriously stems from the common failure (Bauer, 1986) to distinguish frontier science—new claims that are inherently unreliable—from textbook science—long-established descriptions of phenomena that are extremely reliable; and from the related and widespread belief that whatever is called "science" is thereby to be relied upon and will be believed by the media and by the general public. To prevent that, we are tempted to dismiss as quite impossible, or to laugh at as absurd, a variety of unusual claims about parapsychology, cryptozoology, UFOs, and so on. When that is done in the name of science, it can appear to the general public as though science is being authoritarian and dogmatic even as it claims to be openminded and empirical. That undesirable state of affairs might not arise if the temptation could be resisted, to seek immediate, absolute disproof of implausible new claims. Such claims would better be allowed to sink into oblivion through lack of replication or use by others, which will

happen if the claims are incorrect—after all, it happens all the time to many claims within the mainstream of science. In any event, as illustrated above, our current theories are by no means always robust enough to permit us to entirely exclude from the realm of possibility, certain envisageable or claimed, albeit quite implausible, phenomena.

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