letters

"Back-to-nature" movement a threat?

The articles by Marvin Goldberger and others in December seem to indicate a misunderstanding of the character of the current "ecology" fad. This concern with the environment, if studied in the publications of its proponents, is something much more serious and ominous.

Last April the "Earth Day" teach-in published a paperback *Environmental Handbook* putting forward the concerns and goals of this movement. This handbook urges a return to tribal economies (page 6), rejects solutions based on technology whether through a capitalist or a socialist system (page 32) and advocates a diminished standard of living through a reduction of energy consumption (page 323).

The level of scientific competence possessed by members of this movement is indicated by an article that appeared in the *Los Angeles Free Press* of 10 April 1970. This article, by H. Bert Frank, claimed that the increased use of electricity is rubbing charges off electrons. These neutralized electrons are allegedly causing an imbalance of positive charge, producing a disease called "Locatelli's Syndrome." Frank further charges that the federal government is ruthlessly suppressing all evidence for and warnings about this catastrophe.

In short, the "ecology" movement is a contemporary version of the unrest and unhappiness about the scientific revolution, which can be traced back at least as far as John Ruskin's opposition to the 19th-century industrialization of Britain. Lewis Mumford's attack, mentioned in your editorial (December, page 80), is of a piece with this attitude. It is presumed that science and technology have "dehumanized" life, and that science must be curbed if certain human values are to be retained. Forty years ago, at the time of the "monkey-law" troubles, this was urged in the name of religion; now it is urged in the name of ecology.

What this can lead to may be seen in the advocates of "ecology," and the things they have already accomplished. Kenneth Watt has put it this way: "My feeling is we simply go back to the kind of culture we lived with handily in 1800 when everything was horsedrawn." (*New York Post*, 3 March 1970.) Such a course of action would sentence over three fourths of the human race to death by starvation or disease.

The ecology movement's practical effect on the problem of air pollution so far has been the prevention of the building of a plant to remove sulfur from fuel oils. (*New York Times*, 1 April 1970.) They are also lobbying a bill before the New York City Council to prevent the construction of nuclear reactors within the city limits. They seem to imagine that electrical power can be generated with no waste whatsoever, and that if it can't, we should do without it. In contradiction to the evidence, they are claiming that the oxygen content of the atmosphere is falling. And, as one might expect in this violent era, the slogan "Environment Control Grows Out of the Barrel of a Gun" has already been heard. The *Los Angeles Free Press* (31 July 1970) is urging a national day of sabotaging automobiles next 4 July.

"The 'ecology' movement is a contemporary version of the . . . opposition to the 19th-century industrialization of Britain." (Sketch of the destruction of a spinning jenny from The Bettmann Archive.)

The current concern over the environment does present an urgent problem for physicists, but not of the sort discussed in the December issue. We need more and better science education at all levels for nonscientists, so that pseudoscientific views of this sort do not get generally accepted by laymen. The present educational system does not make people aware of the profound beneficial changes produced by the scientific revolution. The current drop in the number of undergraduate science majors, and in public funding for scientific research, is a reflection of this failure.

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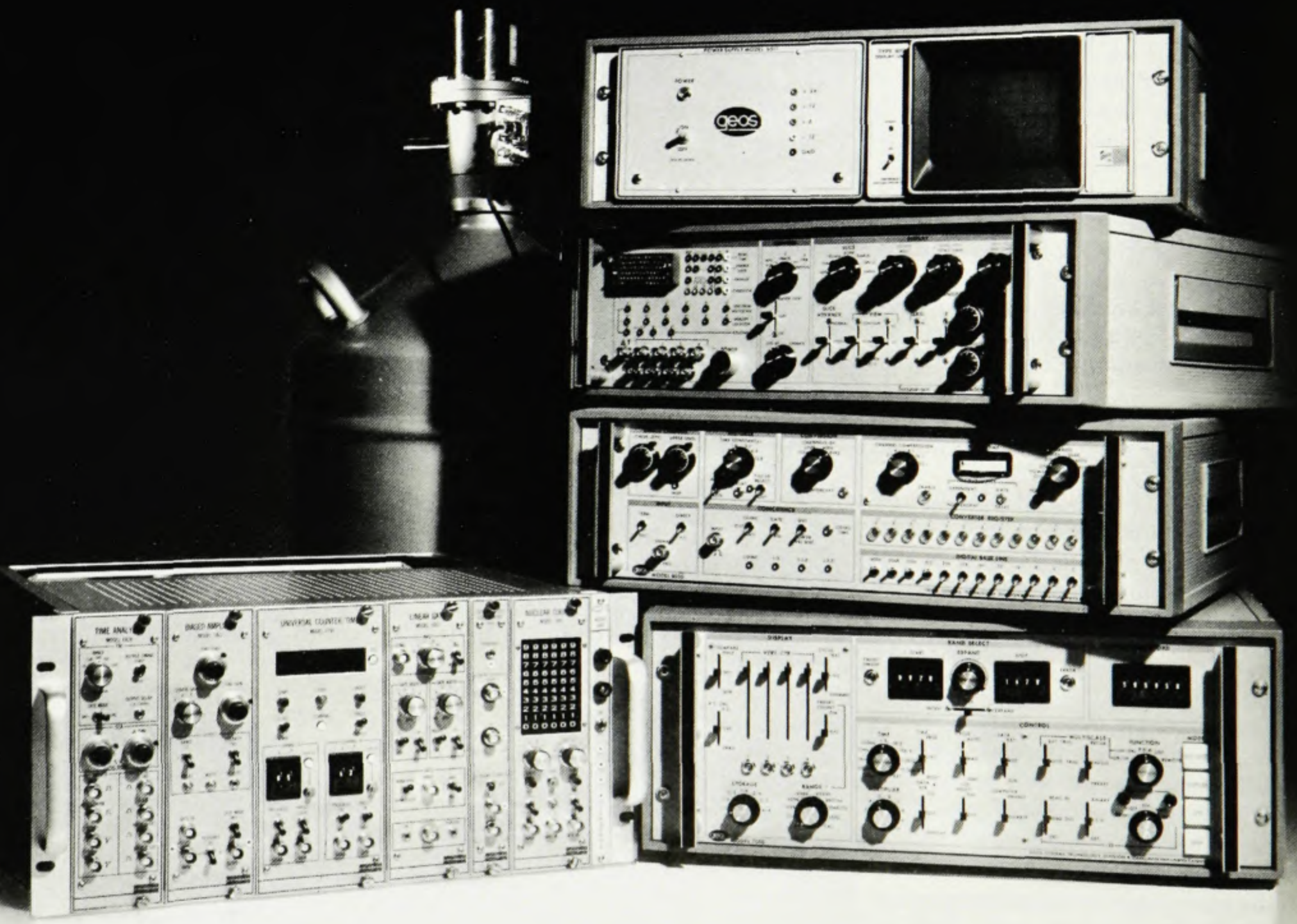
Discussion on the chemical bond

In his article "The Chemical Bond and Solid-State Physics," February 1970, page 23), J. C. Phillips discusses my development of a treatment of ionicity of chemical bonds[1,2] in 1932, and reaches the conclusion that it "is simply not accurate" and is far inferior to a treatment that he has formulated. He states that "the scatter associated with Pauling's scale is 10 or 20 times greater than that of the dielectric scale (labelled "Phillips" in the table)," and that "From a statistical point of view the dielectric definition is at least 20 times more accurate than Pauling's."

These conclusions by Phillips result from errors in his paper, which are themselves the result of a basic failure by him to understand the principles of structural chemistry that he is discussing, as they are presented in my papers from 1927 on and in my book *The Nature of the Chemical Bond.*

One misunderstanding is essentially the same as the one that he made in another paper.[3] In this earlier paper Phillips discussed crystals such as beryllium oxide, and stated that "Pauling's resonating bond theory is reformulated in terms of an itinerant dielectric model. In extreme cases discrepancies of more than 200 kcal/mole between the observed cohesive energy and Pauling's value are reduced to 1 kcal/mole." I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Calculated Values of Ionicity

Ligancy 4				Ligancy 6			
	i	i'	Phillips		i	i'	Phillips
CdTe	0.039	0.519	0.675	MgS	0.345	0.782	0.786
CdSe	0.115	0.558	0.699	MgSe	0.302	0.767	0.790
CuBr	0.183	0.796	0.735	MgO	0.733	0.911	0.841
CuCl	0.261	0.815	0.746	AgBr	0.183	0.864	0.850
AgI	0.086	0.772	0.770	AgCl	0.261	0.877	0.956
Average	0.14	0.69	0.72		0.36	0.84	0.83

pointed out[4] that these great discrepancies do not, in fact, exist; instead, Phillips made mistakes in calculating the values, because of his failure to understand structural chemistry theory.

In 1932 I formulated an equation for the extra stability of a bond between unlike atoms that could be ascribed to the effect of the partial ionic character of the bond. In the beryllium-oxide crystal the beryllium atom has covalence 2 and the oxygen atom has covalence 2; there are two Be–O single bonds per BeO, resonating among four positions in the crystal. Phillips calculated the extra ionic energy for the BeO crystal on the assumption that the beryllium atom and the oxygen atom are quadrivalent, rather than bivalent, and he accordingly obtained a value twice as large as the correct one. It was this error in his calculation[3] that caused him to state that discrepancies of more than 200 kcal/mole were found between the observed cohesive energy and "Pauling's value."

In his PHYSICS TODAY article Phillips refers to his *Physical Review Letters* communication (his reference 8), but does not refer to my correction[4] of the errors in this paper, published on 1 September 1969.

The principal error in the PHYSICS TODAY article is closely related to the one described above. Phillips discusses several crystals in which the atoms have ligancy 4, and several others in which they have ligancy 6. Ten of these crystals are listed in the accompanying table, which corresponds to Phillips's Table 1. Phillips stated that for the five crystals with ligancy 4 the ionicity calculated from my structural principles (i) ranges from 0.04 to 0.07, and for five crystals with ligancy 6 from 0.18 to 0.73, whereas the ionicity calculated by him ranges from 0.675 to 0.770 for ligancy 4 and from 0.786 to 0.856 for ligancy 6. I have recalculated the values of i and corrected the numerical errors in Phillips's Table 1. There was one large error: Phillips gave the value 0.67 for CuCl, whereas the correct value is 0.261. The range of values of i should accordingly have been given by him as 0.04 to 0.26, rather than 0.04 to 0.67.

The calculation of ionicity that he attributes to my structural system is, however, incorrect, in that he has assumed that the covalence of the atoms is equal to the ligancy, 4 or 6. Instead, the elements copper, silver, fluorine, bromine, and iodine are univalent in these crystals, and cadmium, magnesium, oxygen, sulfur, and selenium are bivalent. For comparison with Phillips's values of the ionicity in these crystals, the ionicity should be calculated on the basis of my structural system by determining the covalent character of one bond or of two bonds, for uniunivalent and bibivalent crystals, respectively, and dividing this covalent character among the four or six bond positions in the crystal. When this is done the values i' listed in the table are obtained, differing greatly from the values i given by Phillips.

The calculation of i' (not given this symbol) was first presented in 1939, on page 72 of the first edition of *The Nature of the Chemical Bond*.[1] It was reproduced in the second edition, but removed from the third to make room for the discussion of later developments.

Lithium iodide, with ligancy 6 for the ions, was used as an example. It was pointed out that in the lithium-iodide molecule, LiI, the electronegativity scale leads to 43% ionic character and 57% covalent character of the bond. In the crystal, however, each lithium atom is surrounded by six iodine atoms, equivalently related to it. We may describe the crystal by saying that the one bond with 57% covalent character resonates equally among the six positions, and that each of the bonds in the crystal thus has about 10% covalent character; that is, ionicity 0.90.

The silver halides were also discussed, and the bonds in the silver-iodide crystal were described as having 23% covalent character, and hence 0.77 ionicity; compare $i' = 0.772$ in the table. (Another possible structure was also discussed.)

The values i' in the table are seen not to differ greatly from Phillips's values of the ionicity. The mean deviation of i' and Phillips's ionicities is 0.06. This approximate agreement provides, I think, some support for the greatly simplified theoretical interpretation of the observed values of the dielectric constant of the crystals that has been presented by Phillips.

The so-called statistical test of the two definitions of ionicity presented for 70 crystals with 4-fold or 6-fold coordinated structure in figure 6 of Phillips's PHYSICS TODAY paper is, of course, also invalidated by the same error in his calculation of the ionicity on the basis of my equations.

An extensive semi-empirical theory of the chemical bond, based upon sound quantum-mechanical principles, was formulated during the decade following the development of quantum mechanics. In recent years it has become possible to carry out rather accurate quantum-mechanical calculations for molecules containing several atoms. The results that have been obtained have in general supported the semi-empirical theory. This theory is by no means so unreliable as was suggested by Phillips in his PHYSICS TODAY paper.

It should be mentioned also that Phillips's statements about the theory of the relative stability of crystals with different structures, such as those with ligancy 4 and ligancy 6, need some amplification. For essentially ionic crystals the stability is largely determined by the ratio of the radius of the cation to that of the anion. The importance of the radius ratio to the choice of structure was discussed by A. E. van Arkel[5] in 1924, by V. M. Goldschmidt[6] in 1926, and more thoroughly by me[7,8,9] in 1927, 1928 and 1929. There is no doubt about the importance of the radius ratio to the relative energy of alternative ionic structures; the theory is simple and straightforward. It is possible that the amount of ionic character of the bonds also has some significance; but the amount of significance can not be determined if the principal factor, the radius ratio, is ignored.

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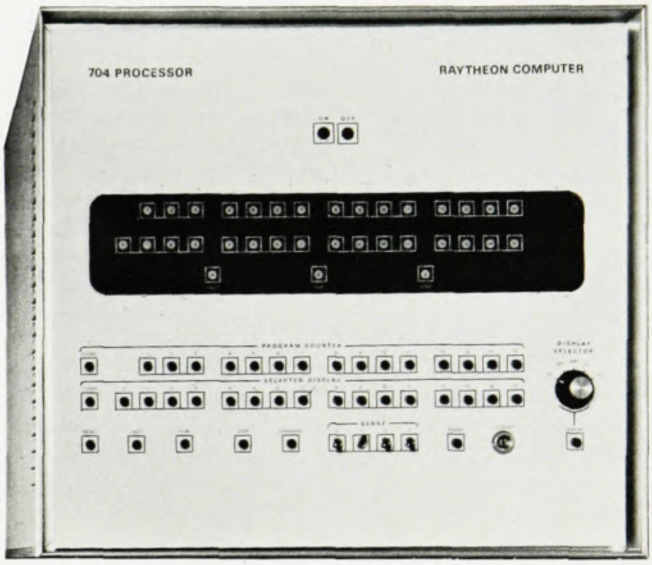

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LINUS PAULING
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THE AUTHOR COMMENTS: In reply to Professor Pauling's comment on my article, I would like to apologize for having relied on the definition of ionicity i given in the third edition (1960) of his book. I therefore overlooked the definition i' given on page 72 of the first edition (1939) of his work but omitted from the third edition, which is the one he applies to crystals.

As pointed out by Pauling, the definition i' generally yields numerical values in good agreement (median deviation 0.06 for ten crystals) with the spectroscopic definition. As for the accuracy of his definition, he now seems more confident about this than he was in 1939. In particular, on page 73 he said: "It is probable that these amounts of covalent character of the crystal bonds [of the silver halides] should be doubled . . . giving . . . 46% [covalent character] for AgI." This is quite different from the value of 23% given by him in the table.

Having conceded these points, I wish to reaffirm my confidence in two major points:

1. The spectroscopic definition is more nearly consistent with the requirements of quantum mechanics than the thermochemical one.

2. The ability of a definition to predict the crystal structures of A^NB^{8-N} compounds is an unbiased and rigorous test of the accuracy of any scale of ionicity. We have considered 70 compounds and tabulated detailed results for them.[1] When these are compared with Pauling's definition i', the following results emerge:

a. The median deviation is still only 0.06 between i' and f_i (spectroscopic). However, even discounting difficulties with three crystals that can be found in both structures, the definition i' makes four errors in predicting coordination numbers, while the spectroscopic definition makes none.

b. A large part of the improved agreement stems from a feature of the definition of i' that one could easily overlook. This is the postulate that the covalent character (but *not* the ionic character) of the bonds is to be divided among the four or six bond positions in the crystal. This automatically guarantees that the ionicity per bond will be greater (all other things being equal) for six-fold coordination, than for four-fold coordination. For the three crystals MgS, MgSe and HgS, which are found with both coordination configurations, the definition i' assigns a different ionicity to each, the two values differing by about 0.12. It is therefore difficult to understand *why* these particular crystals are found in both structures. The spectroscopic theory explains this because all three have f_i equal to the critical ionicity F_i within 0.01.

c. Although the deviations of i' from f_i are small in many cases, there are some glaring discrepancies. These are summarized in the accompanying table, which shows the range spanned by each definition for each group of A^NB^{8-N} compounds with $N = 1, 2, 3$. Note that for the definition i' the $N = 2$ and $N = 3$ ranges scarcely overlap, making valence almost a "good quantum number." But for the spectroscopic definition there is substantial overlap.

d. An individual pair of crystals that is particularly striking is BP and SiC. Pauling ionicities are $i'(\mathrm{BP}) = 0.25$, $i'(\mathrm{SiC}) = 0.11$, while spectroscopically $f_i(\mathrm{BP}) = 0$ and $f_i(\mathrm{SiC}) = 0.18$. The effective charge e^* measured by optic lattice vibrations of BP is $<0.1e$, and is too small to measure, whereas that of SiC is quite large (of order $0.5e$).

As conceded implicity by Pauling in his 1939 discussion of the Ag halides, the concept of resonating bonds can be ambiguous in certain cases. (Silver is commonly divalent, while the halides are strictly monovalent. So how many resonating bonds are there in the Ag halides?) Nevertheless I believe that the differences between his approach and mine are ones of style, not substance. On the other hand, a view that is very widespread at present, especially among theorists of a mathematical per-

Ranges spanned by definitions of ionicities $f_i(A^NB^{8-N})$ as a function of N

N	f_i (Phillips 1969)	f_i' (Pauling 1939)
1	0.69–0.96	0.78–0.99
2	0.17–0.91	0.55–0.97
3	0.00–0.58	0.25–0.56

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suasion, is that ionicity is not a useful concept at all, because it does not appear in the Schrödinger equation. My point is that for a certain family of homologous structures, a rigorous mathematical definition can be demonstrated with remarkable precision. In this sense I regard dispersion theory as a tool for demonstrating the generality of Pauling's ideas, and I hope he will welcome this demonstration as a refinement of his work.

Reference

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No new breed

I take issue with the theme of your editorial, "A new breed of PhD?" (November, page 80). Evidently you believe that PhD training in physics is likely to develop individuals who can formulate solutions to environmental and sociological problems in the most general way, and that this presumptive skill would create a demand for physicists on interdisciplinary teams directed at solving such problems. My own experience convinces me that, but for a few, physicists are specialists, as are most other highly trained persons, and when they are removed from their field of specialization they do not possess insights or faculties superior to those of other specialists. The exceptional few with broader gifts may also be found in other disciplines. Granting however, for the sake of argument, that one is more likely to find the exceptional talent in physics than in biology, such a case offers little justification for training the average, bright student as a physicist; in this realm brightness is not enough. Indeed, the most celebrated physicists have been in sharp disagreement on environmental and sociological problems, such as the health hazards of man-made radiation. This fact does not commend the physicist to the market for reliable answers to environmental and sociological problems. Present circumstances demonstrate most poignantly that physicists have been rather ineffectual in solving the sociological problem of their own economic survival in the industrial–political environment. They have not had the organizational intelligence to limit their members to ensure a favorable market for their skills, as have others with a heavy investment in specialized training. This fact does not encourage one to look for leaders of men among physicists. If any qualities may be said to be widespread among physicists, they might be the propensity for self-deception, mutual disregard and the willingness to be subservient.

Alvin D. Hofer
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The editorial in the November issue, if read and taken seriously by other prospective members of interdisciplinary teams, ought to close a few more doors for unemployed physicists. . .

Highly developed and skillfully analytical minds are encountered in all disciplines, including "unlikely" ones such as art, business, music and advertising. Physicists have no distinguishing characteristics as a group beyond a greater than average exposure to the field of physics.

. . . unless he is already irrevocably committed to being known as a physicist, my advice to the alert, intelligent, nascently analytical young man with an interest in physics is to identify his formal training directly with one of the developing areas that may more certainly in the near future provide employment among satisfyingly challenging problems. He is then more likely to be a leader than simply a minor member of an interdisciplinary team.

Let's face it—physics has advanced in recent years because of public support, not because of unique qualities of physicists. The honeymoon is over. The support is going elsewhere for the forseeable future.

Thomas Mariner
Mt. Joy, Pa.

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What is it that compels editors of PHYSICS TODAY to continue to pontificate about the unsurpassed training of persons currently holding a PhD in physics, and about the vast opportunities for intellectual conquest open to any alert young man or woman possessing such a degree? . . .

Perhaps my horizons are not what they should be, but it seems to me that a physicist is not "someone who has a knack for formulating the description of a system or theory of a process in the most fundamental and general terms," although that definition certainly makes fine public-relations copy. At the risk of displaying my naivete, I must confess that I had always thought that a physicist is someone who (at least initially) is more deeply involved in, committed to, and interested in the physical description of nature than anything else. I distinctly remember hearing words of this sort from senior faculty when it came time for me to apply to graduate schools. But let us suppose that I have been suffering delusions about the purpose of a graduate education in physics,

continued on page 58