have to believe firmly in the project he wanted to have supported. If he could not get the others to agree with him, he could then use his proportional share of the total crackpot pot to support the project. The researcher, of course, would never learn from which funds his support came.

After a period of a decade, agencies using this method might review its results to see whether the plan had been a total loss or whether there had been some brilliant and significant exceptions. A friend of mine, a professor in a major university, told me of sitting on a fund-granting committee that had a few thousand dollars left over after they had made all the grants that they could agree on for the year. At his request, part of the remaining sum was given to an unknown young woman at an institution of no renown for research. This woman seemed to have a good idea but her qualifications were highly doubtful. At the end of three years her investigations were completed, and there was unanimous agreement that her work had been outstanding—superior to any of the others supported. Such an event might be repeated many times over if the crackpot pot were institutionalized.

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Identification of the Auxin Present in Apple Endosperm[1]

A RECENT paper by Luckwill (*1*) provides evidence for the rather wide distribution of a natural plant growth substance that is not identical with indoleacetic acid (IA). This unknown auxin has been characterized by paper chromatography and has an Rf value of 0.83 as compared to 0.35 for IA (*1*). Luckwill reports that apple endosperm is a particularly rich source of this unidentified growth substance. Identification of the fruit setting factor of corn endosperm as the ethyl ester of indoleacetic acid (EtIA) (*2*), suggested the possibility that the substance isolated from apple endosperm might also be EtIA.

An ether extract of endosperm tissue from 55-day-old apple seeds was prepared according to the methods employed by Luckwill (*1*). Paper chromatograms were run using Whatman No. 1 strips and *n*-butyl alcohol saturated with 3% ammonium hydroxide as the solvent. Standard solutions of IA and EtIA were prepared in ethyl ether at a concentration of 10 mg/ml. After removal of the papers and drying, the spots were developed using the ferric chloride-sulfuric acid reagent of Tang and Bonner (*3*). Preliminary observations had indicated that EtIA as well as IA gave a bright reddish-violet color on filter paper when this reagent was applied. All three chromatograms gave spots of the same color and approximately the same intensity. The following Rf values were obtained. First trial (total solvent migration 12 cm): IA, 0.35; EtIA, 0.82; and endosperm extract, 0.81. Second trial (total solvent migration 20 cm): IA, 0.35; EtIA, 0.84; and endosperm extract, 0.83. The fact that only the one spot was found with the endosperm extract would seem to preclude the presence of indoleacetic acid. The agreement of the Rf values that were obtained in this study with those found by Luckwill is very good. This strongly suggests that the native auxin of apple endosperm is the ethyl ester of indoleacetic acid, and that this substance may be of rather widespread occurrence in other plants.

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True Scientists

IT is likely that discussion of Dr. Hammett's letter (SCIENCE, 117, 64 [1953]) on true scientists may go on for a long time, or at least as long as editorial patience will permit, if for no other reason than that true is a relative term, but true science is an umbrella under which all scientists would like to be covered.

There is however one point raised by Dr. Kahn (SCIENCE, 117, 697) in his comment on Dr. Hammett's letter with which I wish to take issue. Dr. Kahn suggests that the conception of a scientist as a "man who sits in an ivory tower" is not only untrue but "prejudicial to the interests of science, since in these days to be different is to be suspect." Is a scientist today who dares to be different doing something thereby which is prejudicial to the interests of science?

Is it not of supreme importance to be different when circumstances seem to demand it, whether it is suspect or not? Is not the very fact that all too many scientists do, in effect, sit in an ivory tower, devoting their energies and even their reading almost exclusively to their teaching and research, that has led to demands by some that there be even a moratorium on scientific investigation for a time, until enough is known of motivation and control of human behavior to make safe use of scientific discovery?

Whatever else the true scientist may be, he must be a dedicated person—dedicated not simply to his field of study but to human welfare in its broadest sense. He cannot afford to detach himself from concerns of economics, government, politics, or any other of the many human activities without which democracy cannot long function. If he does so, it will be at his own peril and ultimately that of science itself. For real science can only progress in an atmosphere of freedom, and freedom will last only as long as intellectual leaders, of whom scientists make a large proportion, dare to be nonconformists when the prevailing political climate demands conformity. The recent history of Germany and Russia shows how quickly science is fettered, once totalitarianism gains control.

The scientist may have "wages, fame, (and) fortune" but if these become his major goals, it may not be long before society begins to ask whether he is entitled to any of them, and even his scientific achievement may suffer. It is a noteworthy fact that the majority of scientists listed in *American Men of Science* had rather humble origins and received their training in a selected group of the smaller liberal arts colleges. They were apparently motivated there by teachers of ability and idealism—men and women who were dedicated less to science than to students, and to human welfare broadly conceived. Let us have more of them, and more scientists who dare to be different, since acceptance of present conditions and values may mean the rather speedy extinction of civilization itself, and science too may disappear as learning did for so many centuries after the fall of the Roman Empire.

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On the Nuclear Envelope

IN N. G. ANDERSON'S recent discussion of the nuclear envelope (*1*), some investigations conducted in our laboratory were referred to so briefly and in such a manner that our views concerning the permeability of the nuclear membrane may possibly be misconstrued. For this reason, and because of the fundamental importance of establishing definitively the physical nature of the nuclear membrane, we feel that a comment on Dr. Anderson's paper is in order.

The hypothesis (*1*) that "The nuclear envelope is a porous structure, generally permeable to macromolecules, yet containing within itself a mechanism for markedly altering its own permeability" is attractive in that it is capable of reconciling many conflicting, observations. Yet from the standpoint of cytochemistry, the crucial question is whether or not the membrane of the nucleus of a resting cell (e.g., the mammalian liver cell) is sufficiently permeable to permit the escape of enzymes and other proteins when the nuclei are isolated under conditions that leave the membrane unaltered.

The evidence mustered by Dr. Anderson to indicate that the nuclear membrane is a porous structure does not withstand critical examination. Many of the experiments mentioned were, for example, carried out on isolated nuclei and under conditions that may well have led to alteration of the membranes. Whether the apparent migration of antigens into nuclei has a bearing on the problem is also questionable. In one of the investigations (*2*) cited by Dr. Anderson, most of the antigen was, in fact recovered in mitochondria, which almost certainly have a protein-impermeable membrane. Furthermore, although it is generally accepted that the nucleus plays a role in the synthesis of ribose-nucleic acid, which is then transferred to the cytoplasm, we are not aware of data indicating that the RNA is in a "macromolecular" state during its transfer. The statement by Jeener and Szafarz (*3*) that the RNA is in the form of molecules (not macromolecules) that remain unsedimented at $60{,}000 \times g$ is not based on published data and, in any case, cannot be accepted as positive evidence for Dr. Anderson's conclusions.

With respect to our own work, we should like to point out that we are well aware of the possibility that the nuclear membrane may be a permeable structure and have so stated on several occasions, most recently in a discussion of the $CaCl_2$-sucrose method of isolation of nuclei (*4*). In general, this and other studies of the distribution of enzymes among fractions isolated from mammalian liver have indicated the absence, rather than the presence of enzymes in the nucleus (*4–7*), a situation that in itself might be interpreted as resulting from a porous nuclear membrane. Evidence that the membrane may be impermeable to proteins has arisen, however, from the more recent finding that the water-soluble enzyme catalyzing the synthesis of diphosphopyridine nucleotide was recovered almost in its entirety in isolated liver cell nuclei (*8*). This enzyme was released into solution when the nuclei were disrupted by exposure to sonic (not ultrasonic) oscillations for a short time at low temperature (*8*). The fact that about 50% of the DPN-synthesizing activity of a 1 *M* NaCl extract of nuclei was precipitated on dilution of the extract to a NaCl concentration of 0.17 *M* indicated, however, that this enzyme, like many others, is capable of combining with nucleic acid (*8*). Although the latter finding may not have any bearing on the state of the enzyme in the living cell, the implication, namely, that the enzyme may be combined with nucleic acid within the nucleus, was so obvious that further comment was considered unnecessary. As far as we are concerned, therefore, the degree of permeability of the nuclear membrane is still an open question. The situation with respect to DPN synthesis, however, can hardly be ignored as evidence in favor of a membrane impermeable to proteins.

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