Communications

How Legitimate Are Names on Scientific Papers?

Are names on technical papers legitimate? Or are names on technical papers a perversion of the ideal of selfish devotion to science?

Is it not true that much of the competition for better jobs, the social climbing, the commercializing, the getting ahead in one way or another in the scientific world, has come to be channeled into the competition to get names on published papers? Is it possible that science now tends less to involve the high-minded search for knowledge than a mad scramble to get one's name in print?

> Getting promoted depends on the number of papers you have published.
>
> At my university promotions are based on the number of papers. Either we get papers out or get out!
>
> We hire our people on the strength of papers published.
>
> He's a comer, and he doesn't care who knows it. He puts down what everybody knows pretty well and sends it in. Since nobody else has written it up, he gets his name on it.
>
> That material had no business being printed. A man'll do anything to get his name in print these days.
>
> Everybody in the chain of command gets his name on every project that goes through his hands, whether he actually did any of the work or not. After all, wasn't he responsible, and hasn't he got to get his name in print just like everybody else?

So a man scrambles to get his name on papers. He has a living to make. He did not make this system, but he does have to live by it. If you do not like the system, what are you going to do about it? Like the displaced sharecropper in the *Grapes of Wrath,* who are you going to shoot? There is no person, organization, journal, or particular school of scientists that one can blame. It is all a part of the present competitive system. Whether scientists like it or not, the competitive spirit is now being applied to science no less than to business and industry.

In the sense that men are working for recognition, either socially or economically (which is also socially of course), these names are more defensible on papers than in the sense of credit due for work done. No scientist todays works in a vacuum. By and large, neither his basic ideas nor what he does with these basic ideas originate with him. This is true in two senses:

1) Most research today is team research. A problem is broken down into its various phases; each phase is assigned to one man or several men. Periodically, once a day, week, or month, men on the project will get together to discuss the work, exchange ideas, decide what tack to take.

2) The material worked with is picked up from the literature. Published reports and journal articles spread scientific ideas and theories, facts and figures, broadcast over the entire world. Science as practiced today would be impossible without this wholesale exchange. Often the wilder the ideas, the better. Ideas may be "wild" only because brains, manpower, and money have never been applied to bring them to fruition.

Any one man really owes no more than an exceedingly small amount to his own efforts. One can trace for oneself how much Einstein, Bohr, Dirac, Heisenberg, Born, and Planck originated for themselves and how much they owe to the work going on around them. The contribution of these men lies in their giving a new twist, or in integrating into a unified system, a heterogeneous mass of scientific material. To overemphasize the importance of their work is to belittle the achievemnts of all competent scientists.

The accent must be on the desire to pass along what has been discovered and worked out, not on getting one's name in print. This is the accent that the true scientific spirit must propagate. There is nothing inherently discreditable in men's names appearing on their papers. The mad scramble for credit, however, as demonstrated by the haste to get names on papers, cannot by any stretch of the imagination be said to be in the scientific spirit. Whoever gets the credit for the work done, this perversion of the scientific spirit cannot be to the credit of science.

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12 July 1954.

Pressor and Central Stimulant Properties of a Serotonin "Antagonist"

The pharmacology of 2-methyl-3-ethyl-5 aminoindole (MEAIN) is of interest because the compound was prepared (*1*) as an analog of serotonin (5-hydroxytryptamine). This aminoindole and a congener have been used to analyze the effects of serotonin, but certain reservations concerning the specificity of the effect should be suggested, since the compound appears to have sympathomimetic activity.

In dogs anesthetized with sodium pentobarbital, the aminoindole itself has a marked pressor effect, as Page and McCubbin (*2*) have shown. The effect varies unpredictably with the rate of administration and the concentration of the injected solution, possibly as a function of its limited solubility. If a dose of 1 mg/kg is injected as a 0.1 percent solution in saline over a period of 50 sec the rise in mean blood pressure is 30 to 60 mm of mercury. More important, the pressor effect is abolished but not reversed by previous administration of an adrenergic blocking agent (piperoxan, 4 mg/kg). Respiratory stimulation and greater pulse pressures appear irregularly in normal dogs after the small doses used but are marked in animals in the shocked state induced by acute reduction of cardiac output. Robson and others (*3*) report that the closely related 2,3-dimethyl-5-aminoindole inhibits carbachol stimulation of the isolated, diestrus uterus, another fact that suggests a sympathomimetic action.

MEAIN in large enough doses (100 or more times the challenge dose of serotonin) does prevent the pressor response to serotonin but not the reflex effects (*1*). However, the same is true of ephedrine for the duration of its pressor effect. Moreover, the direct vascular effect of serotonin is enhanced after the blood pressure has returned to normal following a single injection of ephedrine or during a tachyphylactic state following repeated doses of ephedrine, further indicating the effect of altered vascular reactivity.

These observations, which suggest an ephedrine-like activity of MEAIN, appear to be applicable also to a 2-methyl-3-ethyl-5-dimethyl aminoindole (Medmain). The toxicity of this compound, given intraperitoneally to mice in doses that approximate 150 to 300 mg/kg (5 to 10 mg per mouse), impresses Woolley and Shaw (*4*) as being "remarkably similar to the seizures of human epilepsy," but the description also parallels closely the description of ephedrine toxicity (*5, 6*), if one substitutes respiratory stimulation for hyperventilation and tonic convulsion for opisthotonus. It is doubtful whether the pharmacologic properties of this substance are specific enough to permit inferences about the genesis of disease entities (*7*).

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References and Notes

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24 May 1954.

Peculiar Physiological Behavior in Rice (*Oryza Sativa*)

A wide disparity in the flowering time of the parents is one of the chief handicaps of a plant breeder for hybridization in rice or, for that matter, in any crop. The usual methods adopted by breeders to synchronize flowering time are (i) periodical sowings, (ii) photoperiod treatment, and (iii) seed vernalization. Although the first method is simple, the second and third are cumbrous, time-consuming, and expensive. In a search for a simpler method of synchronizing flowering, an interesting physiological phenomenon was encountered.

Table 1. Mean flowering duration in days.

Pot size (in.)	Number of seedlings per pot			Mean
	One	Two	Three	
		1950–51 results		
6	150.4	148.2	159.0	152.5
9	141.8	145.2	146.0	144.3
12	131.6	137.6	139.2	136.1
Mean	141.3	143.6	148.1	
Critical difference for comparing: means within a treatment, 2.39; marginal means, 1.38.				
		1951–52 results		
6	136.4	147.8	148.2	141.1
9	121.2	144.0	151.8	139.0
12	125.4	137.6	136.2	133.1
Mean	127.7	143.1	145.4	
Critical difference for comparing: means within a treatment, 6.64; marginal means, 3.83.				

In the (1950–51) second crop season (Nov.–Apr.), 30-day-old seedlings of a rice variety from Madras (G.E.B.24) were transplanted in different-sized pots with varying numbers of seedlings. Pots with face diameter and height of 6, 9, and 12 in. were used; and one, two, or three seedlings were transplanted in each pot size. There were nine treatments with five replications. The time of first flowering for each treatment was noted. The experiment was repeated in the 1951–52 second crop (Nov.–Apr.) season. The results obtained are given in Table 1.

The 2-yr experiment results show that (i) the flowering duration is delayed by reducing the size of the pots from 12 to 6 in., (ii) the flowering duration is delayed by increasing the number of plants per pot from one to three, and (iii) the maximum difference in flowering duration is obtained between treatments growing only one plant in the largest pot size and three plants in the smallest pot size, the difference observed being 23 to 27 days.

To ascertain whether the flowering-time difference in different pot sizes was due to the volume of soil contained in each, the following experiment was conducted. The ratio of the volume of soil held by pots of face diameter 9, 12, and 15 in. is 1:4:7. Hence in 9-in. pots, one rice plant was transplanted; in 12-in. pots, four plants; and in 15-in. pots, seven plants. There were six pots of each size. The time of first flowering was noted in each case.

That the nutrients contained in the soil do determine the flowering duration is clear from Table 2, wherein for a unit volume of soil per plant, the flowering occurs almost simultaneously. Horticulturists (*1*) hold that the higher the level of nutrition available, the better will be the vegetative growth and the longer the commencement of the reproductive phase is postponed. With a lower level of nutrition, plants flower earlier. Nitrogen manuring on some of the cereals of the temperate region has a similar effect (*2*). But nothing