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Presidential Address

A Personal Philosophy for Research in Plant Pathology

After 7 years in plant pathology administration it would be difficult to deliver a highly technical paper. In such an administrative position however one must develop a philosophy on the approach plant pathologists should take in their work, if for no other reason than the staff for which you are responsible demand it. It may be of general interest then to hear something of the philosophy that I have developed. My approach must of course fit in with the overall policy and responsibilities of the organization that employs me. I am sure the problems I face are the same or very similar to those of other administrators in Plant Pathology in Australia today. What I will say will certainly be controversial, and some views perhaps exaggerated to some extent for emphasis. The theme I shall develop is that perhaps today too much of our very best effort is being devoted to acquiring new knowledge and developing principles and not enough to the practice of our profession.

There was a time when it could be legitimately argued that not enough effort was being put into developing and defining principles, that our efforts were too empirical. Professor N.H. White developed a theme along these lines in an article he wrote for the September issue of the 1971 University of Sydney Gazette. He applauded the emergence of the philosophical approach in the 1950's. We certainly still need more facts, more understanding particularly on the complex interactions associated with plant pathogens, their hosts, other micro-organisms and the environment.



Gordon S. Purss

**A.P.P.S. President
1973**

However what I think needs to be avoided is the headlong rush to acquire new knowledge without due regard to putting into practice that already established. We need to be constantly reminding ourselves what our main function is, what most of us are being paid for — to determine which plant diseases are important, to develop effective control measures and see that these are practised. To achieve these ends there will always have to be a balance between work aimed at breaking new barriers of knowledge and work aimed at seeing good practice in plant disease control.

Last year Mr. Bob Taylor reminded us all of the changes in research emphasis being forced on all agricultural scientists because of the stringent attitudes of funding authorities — there is a need for our research to be relevant. Despite the logic of this argument the profession as a whole continues to reserve its highest regard for those making contributions to work of a fundamental or specialist nature. In my view we should have equal regard for those making significant contributions to the practice of plant pathology.

My own attitude, for good or bad, is conditioned by the years I spent in the field working directly on practical problems with day to day contact with farmers and extension officers. I certainly knew what my role was and with minimal facilities had to find answers to pressing disease problems. The experience I had then is not advocated as a recipe for success — one had to work on too many problems and act too much as a guide to others. Even in this type of work one must restrict himself to one or two fields of investigation if effort is not to be dissipated. This I think is perhaps the first lesson any young practitioner must learn. However I quickly realised that you need to understand the agronomy of a crop and you can find out a tremendous amount by doing field experiments and making extensive field observations. As a result of my background I can lay claim to being nothing more than a practical plant pathologist who has made no significant contribution to defining new principles of breaking new barriers of knowledge. In this regard I probably represent the great majority of Australian plant pathologists many of whom were not able to come here today.

I attended my first plant pathology conference at Hawkesbury College in 1955. I was strangely dissatisfied with that conference as I have been with every one I have attended since. It seemed that most of what was said at formal sessions was already available in the literature. Also there was little time for discussion, no real opportunity for the average worker to question how a particular finding might be related to his every day job. There seemed then, as there seems today a reluctance to discuss the "gut" problems of practising plant pathologists. I found that most people with whom I discussed conferences felt the same way even with big international meetings they had attended. The great merit in conferences seems to be that you meet people and make contacts there, that you can discuss your particular problems with people outside the formal sessions. I do not deny this is so. It does seem strange however that we have to organise conferences along the lines we do to achieve this end. It has been suggested that perhaps as a profession we feel the need to put on a good scientific face at conferences but I think that is exaggerating the situation.

The second point on which I would take issue is publication. Let me say from the outset that I agree with the need for publication and would press as hard as anybody else to get good completed work published. It avoids repetition of work and assists a man to assess his own progress. However there is an over-emphasis on publication now, in my opinion, and I think this more than anything else is pushing people towards work on principles rather than practice. It is hard to get anything published unless it is original and breaks new ground — one can understand this with the great mass of material being printed. To

proceed in an organisation you are judged pretty much on your publications so why shouldn't one concentrate on new work rather than consolidating and putting into practice the old. High level administrators on one hand stress the need for relevant applied research and on the other hand demand that a man be a world authority in his field if he is to proceed to the higher positions in our profession. I hope it never reaches the stage in plant pathology that a friend of mine reached in plant breeding. The cultivars of an important field crop he bred dominated the acreage but he was denied advancement beyond a relatively low level because he had no publications showing more than basic plant breeding. A ludicrous situation it seems to me because other persons with far less practical output but with publications on genetics received the promotions. I think it would be interesting to compare the time plant pathologists spend in the field with crop or plot problems with that spent pouring over publications at their desks often trying to accommodate an unyielding referee.

My own criterion of the final success of a plant pathologist in our organisation is how much his work has contributed to cultural or management practices which have reduced yield losses. Such progress may have to be judged over a long period of time. In many cases this is merely a better spray schedule, a better prediction of the optimum planting time, better harvest procedures, improved hygiene or the selection of better cultivars. Obviously to achieve this result it could well be necessary to work closely with related disciplines such as agronomy or plant physiology. It may often be necessary to do work of a fundamental nature because the answers cannot be found with established principles. The man himself is in the best position to decide this and in my opinion this is the best reason I can find for people doing work of a fundamental nature. However no matter what the nature of the work and the publications arising from it, it matters little if the end result has not been of benefit to the grower. I would judge that the work has been either badly directed, badly executed or both.

The third point in which I think we need to really look at ourselves is the question of putting into practice, or at least testing, principles already established. This is very clearly evident in the field of soil borne diseases in which I am most familiar. Despite the great mass of literature on cereal root rots, probably more than for any other single group of crops, there has been little work in Australia based on field plots and little effective advice based on field experimentation. Principles have been established on research results obtained under the most artificial of conditions. I would have no quarrel with this if those who established principles under these conditions set about doing or getting done some field experimentation to test out their findings. This unfortunately has not generally been the case but I have a feeling that the conference we are now attending has shown that the need for greater emphasis on field work has finally been accepted. There is of course another side of this. Those funding research must realise how hard, laborious and frustrating field studies and experimentation can be and be more prepared to finance such work.

For the benefit of young practitioners I would like to recount an experience we had with peanuts which shows how the application of principles already established elsewhere helped to overcome a serious problem in the industry. Crown rot caused by *Aspergillus niger* van Teigh had been a serious problem in the industry since its inception. Gibson in Africa did some elegant work on this problem showing that organo-mercurials commonly used as seed dressings actually increased the incidence of this disease. With minor modifications to Gibson's recommendations we have succeeded in reducing this disease to one of

very low importance. We can expect little professional kudos for this work but an industry is forever grateful for our efforts and continues to give support to our work with other diseases. Our role as plant pathologists is easily identified by the rural community by this achievement even if it is not worthy of recording in the history of plant pathology in Australia. To my mind it demonstrates our main function as plant pathologists yet the ambition of most young and even older plant pathologists is rather to discover new things, new approaches.

The fourth point I would like to make concerns the things we as plant pathologists are proud of and our unusual attitude towards status. I have no quarrel with claiming great advances in principle as important in our standing in the scientific community. The great names in plant pathology will always be those who have made significant breakthroughs in knowledge. We do not however depend on the scientific community for our ultimate support whether it be for fundamental or applied work. I think it is time we promulgated with vigour how important some of the things we do are for the well being of many primary industries. There are many examples of this around Australia. The banana industry and bunchy top control in N.S.W. and S.E. Queensland, brown rot control of stone fruit in eastern Australia, root rot control in citrus, clean stocks in the nursery and potato industry and so it goes on. We make little capital out of all this. Some examples in Queensland with which I am most familiar I think highlight this fact. Dr. J.H. Simmonds is acknowledged in many places of the world for the pioneering work he did in establishing latent infection principles. As his colleagues, we are naturally very proud of this. However the contributions he made in establishing and putting into practice control measures for a myriad of diseases such as brown spot of passion fruit, squinter disease of banana and water blister of pineapple mean much to the economy of Queensland and Australia yet we seldom speak of them. Mr. Pont in north Queensland has achieved a reputation as a tropical plant pathologist for contributing to the understanding of many diseases such as speckle of banana, blue mould of tobacco and *Marasmius* stalk rot of maize. Yet in terms of Queensland's economy the control programme using fungicides he worked out for leaf spot and speckle enabled the banana industry to become established and to continue to survive in north Queensland. This latter type of achievement is not always rated as highly as it should be in our professional circles. I wonder what capital the medical or veterinary profession would make in terms of general public relations out of similar achievements.

I had an experience some years ago which I think illustrates the curious sense of values that prevails in our profession. In my early years as a plant pathologist I was associated with others in developing a control for fusarium wilt of passion vine caused by *Fusarium oxysporum* Schl. f. sp. *passiflorae* Gordon apud Purss. This disease had literally wiped out the industry in S.E. Queensland in the late 1940's and early 1950's. As a result of this work a resistant rootstock, *Passiflora edulis* Sims f. *flavicarpa* Degener, became the basis of the industry and the disease was overcome. It was pretty straight forward work applying principles most of us learnt in undergraduate studies. The control is still so effective that the market now suffers consistently from gluts. I was relating the story to a very eminent plant pathologist from overseas and he seemed little interested until I told him that during the course of the work we looked closely for resistance in the common passion vine, *P. edulis*, before looking elsewhere for resistance. We found that in occasional plants the disease was slow to develop, that there was a form of resistance present, which, while interesting enough to be reported in the literature, was not good enough to be of value commer-

cially. My visitor's reaction was that he would have put two men to work on this resistance. Well we have no intention of doing that while our other resistance, which has stood up for 20 years, remains effective. I have no doubt however that had somebody worked on this resistance his work may well have been acknowledged the world over. As it is an example of an industry depending on disease control for its very existence goes virtually unnoticed.

At the last three plant pathology conferences the need for work to be done urgently on disease assessment has been stressed. Yet we still seem to have made little progress. One might well ask why the subject is being continually ignored or hedged. There are of course innate difficulties in this type of work. I would like to suggest however that one of the reasons could well be that a lot of field data has got to be collected and that as a profession we are tardy on collecting this. As an area of work it has not got the smell of quick success, of tidy little experiments which can quickly be prepared for publication. It possesses little professional status.

A contemporary of mine as a working pathologist in Queensland, the late J.C. Johnson published very little of his work. I think this a great pity if only because much of what he did is not readily available to others. As a result of course he is little known outside the vegetable industries in which he worked. Yet he was highly respected by the industry, received much financial support for his work and the disease resistant bean cultivars he helped develop are still the basis of the fresh green bean industry in S.E. Queensland. It is this sort of recognition which helps to establish our profession outside our own narrow limits. We should be proud of it and hold it up as an example for young plant pathologists starting off on a career.

Perhaps I could summarise what I have said in asking that we all stand up and be counted for our contribution to agriculture. That we be proud of straight practical achievements to the same extent as we are proud of fundamental breakthroughs. Then I will say that our profession has grown up and we will not need to be constantly reminded by our masters of the need to keep our research relevant.

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