Opinion

BUILDING A CULTURE OF SCIENTIFIC RESEARCH AND PUBLICATION IN MALAYSIA

In 1991, the Malaysian government launched the *Wawasan 2020* (2020 Insight) strategic plan that set a target for the nation to morph into an industrialized country by the year 2020. Science and technology, backed by an invigorated programme of research and development in the country's universities and research institutions would lay the foundation for the establishment of a scientific and progressive society. Eight years on, it was not obvious to me that Malaysian science, as a whole, was making as much headway as might be expected.

Tangible output in science can be tracked through research publication, even if it is hardly a fool-proof indicator of progress in science. It would have provided researchers and institutions with a useful reference by which to gauge their own progress and standing in the scientific community.

For various reasons, however, Malaysian scientists were simply not publishing. The fact that research publication was not closely linked to career advancement in most universities and research institutions at that time was perhaps one explanation for this. There has apparently been change now that universities have made ISI journal publication a 'key performance indicator' (KPI). I hear there has been a "30% increase in publications".

Back in 1999 when I headed Biotechnology at the Rubber Research Institute of Malaysia, I wrote a series of articles that I emailed to researchers in the Biotechnology Unit to encourage them to write and publish. One article was sent each week over a period of 12 weeks. Each article, in a *Q&A* format, was two pages in length so that it would not be too onerous to read at one sitting.

Although these articles were written more than a decade ago, many of the messages they contained are as relevant today as they were then. I have made no attempt to update the articles that are reproduced unchanged in the following pages. They were written with the RRI scenario in mind, but many of the challenges the RRI faced in building a culture of research and publication then apply to Malaysian universities and research institutes even today.

H.Y. Yeang

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1. Benchmarking Malaysian science: Knowing how well (or how badly) we are doing

Malaysia aims to be a developed country by 2020, by which time, it is envisaged that the country's progress will be led by advancements in science and technology. The Multimedia Supercorridor is one of the early building blocks the government has put into place. On another front, the government is actively encouraging and promoting R&D in the country, and millions of ringgit have been poured mainly into universities and research institutions for this purpose through the *IRPA* and other schemes. Developed countries dedicate a substantial amount of their resources towards R&D and the government sees the need for Malaysia to follow suit. Despite generously allocating millions towards supporting R&D, what is being spent represents less than 0.4% of the country's GDP. (In the US, about 2.8% of its GDP goes towards financing R&D. And since the American GDP is so much larger than Malaysia's, the actual financial US outlay is comparatively enormous.) While seeking to emulate others, we need to make the best of the opportunities that come our way. The IRPA funding and *Pembangunan* funding for the development of science are such opportunities to involve ourselves in quality R&D. Even before IRPA came about, research institutes like RRIM have of course already been embarking on research using their own funds.

Why is there a need to know how Malaysian R&D is doing and where we are placed on the stage of international science?

We should step back and take stock of the present status of our Malaysian R&D. We need to be conscious of how well, or how badly, we have been doing so far to enable us to be aware of our strengths and weaknesses. This will assist us in strategising for the future.

Malaysia is already manufacturing advanced computer chips, automobiles and the like for export. Surely that is proof enough that we are already advanced in science and technology?

Here, I am referring to a solid scientific base for the country. A lot of high tech industrialisation in Malaysia today is borrowed technology. Many multinationals come to Malaysia because we have an educated skilled populace that is prepared to accept moderate salaries. As the cost of production increases with the workers' expectations for higher wages, these companies can pull out of Malaysia as fast as they came in. In the meantime, we can of course learn from the multinationals. There is nothing intrinsically wrong with borrowed technology; Japan started a lot of their industries after the war just like this. But we can't always be relying on borrowed research in the long run. We need to build up our indigenous scientific and technological capability, and this cannot be achieved overnight. Along the way, we borrow some, buy some and make some. But let us be careful not to rely too much on the 'borrowing' and the 'buying'. While it is important to decide on the types of industries that would benefit the country's economy, we have to ensure that Malaysian scientists and technologists acquire the scientific base and capability to nurture, develop and exploit them. Otherwise, we would forever be followers hanging on to the coattails of scientists from the West. We ourselves would not be true scientists. We would be a country of technicians providing cheap technical support for innovators from the West.

Does this mean that we need better benchmarks and productivity indicators to assess Malaysian scientific competence?
We need to get objective feedback, preferably by international experts in the various fields of science.

Local universities regularly invite overseas scientists to visit. Aren't we already getting useful and objective feedback from these experts?
To some extent, perhaps. But I wonder if we end up hearing what we want to hear - rather than what we ought to hear - more often than we think. It is true that local universities are frequently hosts to academics from all parts of the world. These include visiting professors from world-renowned universities who may have been invited to assess or validate academic standards in the university's examinations or its appointments of professors. But when the visiting academic, at the conclusion to his evaluation, offers the opinion that the local professorial candidate is 'good enough', do we press further to clarify if the academic meant that the candidate is 'good enough' by Malaysian standards, by regional standards or by first world standards? Do we ask if the performance by the candidate might be similarly deemed acceptable in the visitor's own home university? Would a lecturer of the candidate's standing be as readily appointed professor in the visitor's own department? As a guest, the visitor could well be constrained by courtesy to be less candid than he might otherwise be. Unless very specific questions are posed to the visiting assessor, a forthright answer might not be offered.

Before LGM came about, RRIM had expert consultants in the* CAC *advising us. We paid big bucks inviting them over and we've received such glowing reports from them in the past. Isn't that vindication of RRIM's world class research capability?
The *CAC* served a very important role in advising the direction of RRIM research. As for their glowing reports of our scientific achievements, I think we should not let ourselves be overwhelmed by their praise. Much depends on what exactly is being endorsed by the august body of consultants and how the endorsement has been worded. Ironically, it is *because* of the big bucks we spent that makes it that much harder to decipher the consultants' true assessments. The point should not be lost that the consultants had been flown in First Class at the institute's expense to perform their task, nor the fact that they had been accorded five-star VIP treatment all the way by their host during their stay. Would we then be greatly surprised if we find the guests on these occasions to be rather more charitable in their evaluation and less scathing in their criticisms than usual? As in the case of the visiting university professor, it is crucially important that the consultants were asked direct, searching questions if objective answers were to be expected of them. For example, if the consultants were only asked if the on-going research was proceeding in the right direction, they might not hesitate to agree. If they had not been specifically asked whether the ongoing research matched the standards set by similar laboratories of world standing, it might not be surprising either if this unsolicited verdict were not volunteered. Feedback from the experts can be telling, but only when the right questions are asked.

If we need to get hold of other international experts (who are not our guests) to asses our scientific output objectively, won't their services incur further expense?
In fact, you can get their services for free.

2. Reviewing research the effective and affordable way

In the last segment, we discussed the need to evaluate our standing in scientific research at the international level objectively. We talked about why assessments by non-independent evaluators may not present the true picture. At the same time, evaluation of our research by independent expert evaluators need not be difficult or expensive to attain.

How do we get our research evaluated by international experts without incurring great expense?
If we write up our research findings and submit the manuscript for publication in a reputable international scientific journal, we would effectively be obtaining a free and objective evaluation of our work by experts. There is, of course, also the other major benefit from doing this. If the manuscript gets published, our work is disseminated and listed in international databases, the most important of which are the Science Citation Index and Current Contents. Our work receives global recognition among our peers in the same field of research.

Publications may be well and good to gauge basic research. But shouldn't we be channeling our main efforts towards applied science that can be commercialised?
I have written an article on the relationship between research and commercialisation that appeared in the *Malaysian Society of Plant Physiology Newsletter* recently. Faridah has been kind enough to email the article to all of you and I do not wish to repeat myself here. I'm all for commericalisation where the opportunity presents itself. I am even prepared to create opportunities for commercialisation where such opportunities do not yet exist. But we just can't always equate scientific excellence with commercial success.

Why not? Why isn't commercialisation the best productivity indicator for R&D?
We need to be practical about using successful commercialisation as the prime yardstick for research success. A lot of useful basic research findings do not find immediate application. Even for those that eventually do, it could take something like 10 years or longer from start to finish. The malaria vaccine, for example, has been a subject of intense research by various illustrious groups for more than 30 years with no commercial product in sight. That does not mean that the scientists have nothing to show for their years of hard work. Indeed, many landmark discoveries have been made along the way and these are given due recognition when they are documented in scientific journals. I'm sure someone will develop a successful vaccine eventually. When that happens, his feat would have been built upon the foundation of previous work by scientists before him. In the words of Newton, he would have been "standing on the shoulders of giants". But we cannot be assessing research only at the end of 10 years, 30 years, or longer. There has to be some basis by which to keep track of progress and to ensure quality research is being undertaken during this period. Publications serve as research milestones that receive expert vetting along the way.

Therefore, we would essentially be getting an unbiased expert evaluation of our work each time we submit our manuscript for publication?
No two people think exactly alike. In the ideal world, referees should be liberal enough to accommodate findings and reasoning that are contrary to their own, so

long as the data and arguments presented are sound. In the real world, however, some referees can be quite dogmatic about their own viewpoints and fail to see or accept those of others. Such bias is quite universal. Malaysian scientists may face yet another obstacle. There have also been complaints – some of which is undoubtedly justifiable - that a certain amount of discrimination exists against work coming from the third world. Sometimes, it is because the manuscript from a developing country concerns a topic (e.g. tropical medicine) that is of limited interest to readers in the West. In other instances, it is just the pre-conceived idea that Third World Science is just not up to scratch. Jerome Kassirer, editor-in-chief of the respected *New England Journal of Medicine* is on record as saying, "Very poor countries have much more to worry about than doing high quality research. There is no science there."

If we are aware bias exists, why do we submit ourselves to such discrimination?
Let's be pragmatic. Discrimination exists everywhere. People are discriminated against because of their race, skin colour, sex, religion or the company they keep. At a job or promotion interview, people are discriminated against because they are too fat, too short, too ugly or simply because they have crooked teeth. Undesirable as it is, we can't eliminate discrimination and we can't avoid it completely. We'll just have to live with it. There are thankfully also many editors and referees who do a good job without prejudice and I would like to think they are the majority. (Some journals don't reveal the names and affiliation of the authors to the referees.) Besides, there is yet another purpose that is no less important for having our research refereed.

What other good reason is there for us to seek peer review of our research output?
As a productivity indicator, journal publication has won international acceptance in the scientific community. Even as we assess our own selves by our publication record, that is also how others in the scientific community – especially those who don't know us well - will assess us. They look up our work output in the Science Citation Index or other publication databases, and they judge us accordingly.

Why do we always have to dance to the tune from the West? If we are not completely happy with the way papers are vetted, why don't we simply ignore them as indicators of scientific output? Why even refer to the Science Citation Index?
So long as we are a part of the world scientific community, it is not for us to choose how others will evaluate us. We might draw an analogy with the international economic indicators released by rating agencies such as Standard & Poor's or Moody's Investor Service. During the recent economic downturn, Malaysia's ratings by these agencies took a beating. The government reacted by strongly criticising the ratings as being biased, unfair, prejudicial, and what not. But this did not of course stop our international trading partners from continuing to judge us by these indices. Hence, when Malaysia first attempted to launch a sovereign bonds issue after the economic crash, we had to beat a hasty retreat because of the near 'junk grade' status following the downgrading of our credit rating. As long as we have to interact with the international community, it's not just how we see ourselves that matters. It's how others see us that's also important. Only three days ago, there was reason to cheer because Moody's upgraded the rating of a number of Malaysian banks. The KLSE shot up in response. Here again, we can see how influential these accepted performance indicators are when the parties we interact with refer to them and judge our performance by them. RRIM can afford to ignore the Science Citation Index as much as Bank Negara can afford to ignore Standard & Poor's or Moody's.

3. Re-thinking RRIM's publication policy

Like other research institutes and universities in Malaysia, RRIM scientists today do not have the habit of publishing in the leading scientific journals. In the last segment, we discussed the importance of a creating a good impression of our research output through scientific publications. It might be asked why we should feel the need to impress others. There are those who assert that RRIM is mature enough an institution to be confident of its own standing without having to seek the approval of others.

If RRIM has managed without a culture of publication for so long, why do we need to change now?

The world around us is changing. By clinging to the past, we could end up as research dinosaurs. We resist change at our peril. What was right for us and what made us great in the past might no longer be right for the future, or even the present.

RRIM built up a solid reputation of research excellence from its achievements in the past. What was the research climate like back then?

I refer specifically to upstream (biological) research which formed the gist of RRIM research until the 1960s. RRIM's upstream research revolved principally around nursery and field trials. Research on agronomy and rubber production took centre stage (as it still does today), with plant biochemistry and physiology playing marginal supporting roles. We were self-funding from the cess collection, and therefore self-sufficient financially. Over the years, methodologies pertaining to plant breeding and selection trials, exploitation trials and fertilizer trials were laid down and standardised. A lot of the research undertaken at the time was based on tried and tested classical approaches. That is not to say that innovation was lacking then. We had good scientists. Steady progress in upstream R&D was made through good observation and imaginative experimental manipulation. We were unique. Most crops are food or fibre, yet rubber is neither. Because we had a unique tree and a unique crop, there were not many people outside of the system who were knowledgeable enough to contribute substantially to our research. But then, of course, we didn't need anyone else. RRIM was the Mecca of natural rubber research for the world.

And now?

Rubber is no longer the mainstay of the Malaysian economy it used to be. There is now a need, in fact, to find new uses for the rubber tree to justify its continued cultivation. New experimental approaches are called for. But after 74 years of research, it is fair to say that the obvious experiments would already have been thought of and attempted. There are not any more fruits left on low branches waiting to be plucked and much greater effort is therefore required to advance research further. It is against this backdrop that three significant changes took place in the RRIM over the last ten years that has directly affected the way research is conducted now and in the future. *Firstly*, RRIM, within the framework of LGM, is no longer self-sufficient in its research funding. *Secondly*, the rubber tree has lost much of its uniqueness as a crop plant, especially where biotechnology research is concerned. And *thirdly*, information technology (IT) has become all-pervasive today. These changes alter how RRIM's research output is being looked at and evaluated by others, and make it important for the institute to be backed up by a credible record of international publications.

What relevance has publications to the RRIM being no longer self-financing?
As we are aware, the rubber cess fund is no longer sufficient to support the institute's research activities and we now depend heavily on the government *IRPA* funds. I foresee that in the near future, we shall have to look for additional sources of supplementary funding as government support for the institute winds down further. It is not unlikely that we may have to turn to international sources such as the Commodities Common Fund, the World Bank, the European Union, and various scientific foundations for this. These agencies would be concerned that their awards do not go to waste and that fund recipients have the proven capability to use the awards purposefully and competently. If they have to select between several applicants, it is very probable that the funding agencies would refer to publication databases to check on the grant applicants' research credentials. When that happens, RRIM must be prepared with a solid record of internationally recognised scientific publications.

Why has* Hevea *biology lost its uniqueness and how has this affected our research?
With the advent of DNA technology, we see more and more similarities between related organisms and even between largely unrelated organisms at the molecular level. The fundamental DNA laboratory techniques are not defined by species or even genera. Hence, working with *Hevea* is not all that different from working with any other plant species. Progress in these areas is very rapid and both knowledge and techniques are outdated quickly. Whereas in the past, foreign scientists unfamiliar with *Hevea* have only limited roles to play in Malaysian rubber research, this is no longer true. Today, we can no longer work in isolation. We seek to incorporate outside expertise and input into our research. Collaboration between laboratories is hence becoming increasingly common, desirable and strategically important.

How does RRIM's publication record affect our research collaboration with others?
When we seek collaboration to strengthen our research position, we would obviously be looking for a competent partner who can help us. The last thing we would want in a collaboration is a partner who becomes more a hindrance and a liability than a help. Just as we would want select our collaborator carefully, our prospective partners would be similarly on the lookout for a partner who would be an asset. As the research grant awarding panels might do, our publication record is likely to be scrutinised by the prospective collaborator. A few years ago when Vienna University wanted to team up with scientists having expertise in latex biochemistry and molecular biology to help in their allergy research, their first contact was not RRIM, but Hong Kong University's Dr. Chye (formerly of IMCB, Singapore). Dr. Chye is an excellent scientist, but she only had a handful of *Hevea*-related publications. What tipped the balance was that her publications appeared on the *Science Citation Index* whereas most of RRIM's did not.

How has IT made it even more important for RRIM researchers to publish?
Scientific databases such as *Biological Abstracts* have been around for a long time. In the past, users of such databases had to seek out weighty tomes on dusty library shelves. But with the advent of IT, anyone - researcher, research manager, and research-funding panel alike – has such information literally at his fingertips. Checking up on RRIM's research record today takes no more than a few clicks on the computer keyboard. With our publication record so transparent and open to scrutiny, it had better look good.

H.Y. Yeang

4. Applying international standards to Malaysian scientific research

In the last segment, I said that the international scientific community places a lot of emphasis on publications in assessing research output. If we wish to be a member of this international community, we cannot afford to turn our backs on this critical benchmark. If Malaysian industries plan to compete at international level, then the Malaysian R&D that supports it has also to be set at international standards. There are no two ways about it

Are publications universally accepted as a major benchmark for research excellence?
Every single science-oriented university department or research institute of international repute boasts of an excellent publication record. I know of no exception.

But it's not a foolproof indicator, is it?
If you were a boss planning to hire a clerk and you need to short-list from forty applications that you receive, you might, for example, use *SPM* grades as an indicator of the candidates' ability. If you call for interview only those with *SPM* Grade One, don't be surprised to find major disappointments among those short-listed. You could well be asking yourself, "How on earth did this candidate get Grade I in his *SPM*!" *SPM* grades are a very useful indicator, but we have to accept that no screening system can be completely foolproof. From time to time, we come across a research paper (sometimes published in a respected journal) that we consider to be 'a load of rubbish', and we wonder how that kind of paper managed to pass through peer-review in the first place. Indeed, the system of paper refereeing is not foolproof and some journals are more stringent than others in accepting papers. It doesn't work all the time, but it does work a lot of the time, and arguably, most of the time. Until something better comes along, this is about the best indicator we've got.

Using international journal publication as a criterion, how is Malaysian scientific research faring?
Unfortunately, there are few kind words I can think of to describe the performance of Malaysian universities and research institutions. By one estimate, Malaysian scientists account for only a miniscule 0.064% of the total world output of scientific papers. Compared with our South East Asian neighbours, we rank 56 in the world, falling behind Thailand (Rank 52). In terms of the number of citations per published paper, we rank 61, well behind the Philippines (32), Thailand (39) and Indonesia (48). If this is the science that will lead Malaysia to developed country status by 2020, it is as worrying as it is embarrassing.
Source: *Science Citation Index* 1994; *Scientific American*, August 1995; *Science* 6 March 1998

Aren't we being unnecessarily critical of ourselves? Aren't there hordes of countries that are even less productive in scientific output than Malaysia?
Sure there are: countries like Outer Mongolia and Burkina Faso to name just a couple (with apologies to these countries), and I'm sure there are others - including many exotic sounding ones - that come to mind. But while these countries might not have much to boast about regarding their scientific research, neither do they need to be ashamed of their non-achievement because they haven't really been investing in

research. It's basically got to do with *accountability* for money spent. You expect to get what you pay for. The Malaysian government has handed out millions to support scientific research through the IRPA and other schemes and can justifiably ask to see some tangible results.

Malaysia is still a developing country. Is it fair to compare ourselves with the developed countries that are better funded for science and that have better facilities? Aren't we just being too ambitious?

I am not talking about earth-shaking, revolutionary cutting-edge research here. I am talking about a modest amount of quality research - not necessarily the most advanced - that should be within our reach, resources and capability. Our laboratories are not the best equipped in the world, but we have many that are adequately equipped. Certainly those at the RRIM Biotech Unit are no worse off than many others that I've seen overseas. In any case, even among the developed countries, the best research need not always come from the most modern laboratories. Commercial sector research laboratories are arguably the best equipped because they have practically inexhaustible funding. Unilever's new high-tech research facility at Colworth, England, that I visited comes to mind. Nevertheless, top-class research continues to come from universities despite frequent financial and other constraints. For example, Cambridge University consistently tops the list for research excellence in Britain. I visited Keng See when she was completing her Ph.D. at Cambridge and I would say her laboratory set-up was closer to RRIM's than to Unilever's. In fact, Keng See says she is better funded now for her molecular kits at RRIM than at Cambridge. That said, it's not without some truth that researchers do face fewer constraints in the West.

Shouldn't an allowance be made for the expectations of scientific output from Malaysian scientists, considering the additional constraints they have to face?

It is true, for example, that the nucleus of Malaysian researchers in a given field of specialisation may be small, thereby limiting opportunities for the type of productive discussion and interaction among peers that can lead to problem solving. There are other niggling problems as well. For example, research consumables on order that take forever to arrive add to the small irritations. Because these constraints exist, we can make a certain allowance and accept lower research productivity as compared with the West. But there has to be a realistic limit to this so that it does not become an excuse for non-performance. A liberal allowance, for example, might see Malaysian scientists taking 20 to 40% more time to complete a piece of research as compared with the same work carried out overseas. We look forward to this discrepancy decreasing as Malaysian science progresses.

If we target our research for international standards, how would we know when we get there? How do we tell if we're nearing our goal or perhaps already there?

If we accept peer review by journal referees as objective (even if imperfect) validation of our scientific research competence, we can assess our research output and research standing internationally from established databases such as those of the Institute of Scientific Information (ISI) that includes the *Science Citation Index (SCI)*. If our research papers appear regularly in reputable international standard journals, we can reasonably claim our research is nearing or has reached an international standard. It's one way to determine if a basic competence in R&D has been attained. It's not the only way, but it's a pretty good one. While this yardstick is not strictly quantitative, it does indicate that our performance has reached a minimal standard acceptable to the international scientific community.

H.Y. Yeang

5. Bringing out the 'right stuff' in Malaysian scientists

In the last segment, I contended that international standards of research were in fact realistic and within the grasp of Malaysian scientists. But would this apply only to a small select sector of the Malaysian scientific community? Or does Malaysia already have sufficient scientists of calibre who are capable of putting the country on the international research map? My view is that we do have vast untapped potential. The reason why such potential remains under-utilised is discussed in this segment.

If Malaysian science is not as good as it ought to be, could the problem be that Malaysian scientists are unaware what standards they ought to be shooting for to achieve an internationally acknowledged level of research?

Many of our scientists have been trained overseas and have therefore been exposed to the standards being maintained in overseas laboratories. They should know what to expect as they themselves have been in that working environment completing their post-graduate degrees. Nevertheless, they seem to be content with lower expectations for themselves when they return to work in Malaysia.

Do Malaysian scientists have 'the right stuff' to attain international standards? Do we have it in ourselves to compete with the West?

Let us leave scientific research for a while and turn our attention to the business world. Malaysia is among the top trading nations in the world. Many Malaysian firms compete in international commerce with enviable records of success and Malaysians can take pride in the laudable achievements of companies like Petronas and others. It is not only in large Government-backed conglomerates that we have done well. A lot of private start-ups that began small have similarly made their mark internationally. Early entrepreneurs in the rubberwood furniture industry are good examples that we are familiar with. The standards of excellence being set and maintained by these successful Malaysian companies are no lower than those in the West. Indeed, we do have 'the right stuff' in ourselves to compete with the rest of the world!

How are high standards maintained in the business environment?

Excellence in business is self-regulating. Well-run businesses prosper while poorly run ones perish. The Darwinian 'survival of the fittest' is manifested in the business environment as vividly as it does in nature. In international business, no quarter is given or asked for. If you are not up to the mark, you get buried and you disappear from the scene. If Petronas and others have fared well, it is not owing to the charity of any of the other international players. To maintain a high standard of competitiveness, companies have various forms of 'reward and penalty' incentive schemes. Employees are rewarded for good performance (e.g. promotions, bonuses) and penalised for poor performance, the ultimate penalty being dismissal. No business can tolerate sub-standard performance for long. If the non-performing employee does not go, the entire company might end up going under. The Malaysian business community understands this as much as their counterparts elsewhere in the world.

If Malaysians excel in international business, why do we do so badly in science?

Basically, the high standards of international business are not being applied to R&D. The *Survival of the fittest* adage does not apply to Malaysian science as it does to Malaysian business. You still get by even if you aren't terribly 'fit'.

Why is that?
The link between performance and reward/penalty in Malaysian research institutes and in Malaysian universities is at best not well delineated or not rigidly enforced. At worse, it is absent altogether. Career advancements are not always based on performance, and scientists are often promoted for reasons other than their research performance. Whereas a below-par performance threatens the survival of a business, a university or research institute can carry on regardless, so long as it is not called upon to account for and defend the quality of its research output. In the Malaysian research community, the lack of performance rarely constitutes grounds for invoking a penalty. I have not heard of a researcher in a research institute or a lecturer in the university having been demoted or dismissed because his or her research output has been found wanting. The incentive for good performance is therefore absent.

Is it any different in scientific research overseas?
The element of reward and penalty is normally in place, in one form or another in the West. Scientists are continually evaluated by their scientific output. In universities in the United States, tenure is not awarded to academic staff as a matter of course. The academician concerned must have a good record of research output and a record of securing research funding for his or her own research programme. Such funds are usually very competitive. For the US National Institute of Health (NIH) funding (a prestigious award), the success rate is only about 10%. This does not mean that the unsuccessful applicants are incompetent. It's just that others are adjudged to be better. Such competition pushes scientists to perform better and to maintain high standards. At the Johns Hopkins University Medical School, associate professors are given *one* chance to apply for full professorship. The candidate who fails on this one occasion would be invited to resign.

Should similar systems of accountability be implemented in Malaysia?
There are good points and bad points about any system of administrating science. There will be drawbacks too in the US system or any other systems practiced overseas. For example, a lack of job security might engender undue anxiety on the part of the researcher. It is for the country's social scientists and top scientific managers to come up with the best service structure for Malaysian scientists. But one element that cannot be ignored if Malaysian science is to progress is the element of incentive and accountability tied to the scientist's performance and output.

What can be done to bring out the hidden potential in Malaysian scientists?
Many researchers ignore the fact that a Ph.D. is only a basic certification of competence that allows the holder to *begin* serious research work. Instead, they consider acquiring their Ph.D. the pinnacle of their working careers. They are no longer prepared to put in the hard work and long hours of their university days. Malaysian scientists look around themselves and they see most of their colleagues and counterparts in other universities and institutes achieving no more than they. We have the potential to do well, but lack the mindset – the culture of research – that is already well entrenched in the scientific community in the West. Malaysian science is lagging because the system allows it to. Basically, the reason why we're not doing better is that *Malaysian science tolerates mediocrity*. Managers of research need therefore to emulate their successful counterparts in business by setting international standards of excellence and in insisting on better accountability in terms of scientific performance and output.

6. Training that makes the researcher and judgement that makes (or breaks) the research

The preceding segments discuss how we can assess our own research output and how others are going to assess us. The next few segments cover the work commitment expected on the part of researchers and what research output might be reasonably expected of them. But before that, the groundwork for good research has to be laid.

Does LGM send Biotech Unit Research Officers for their Ph.D.s overseas in order to acquire the very latest biotech laboratory techniques?
Laboratory skills are of course highly desirable and a definite advantage in biotech research. But if we are largely interested in acquiring laboratory techniques, we could just send a technician overseas for training. That might take, say, all of three weeks. Why should LGM send an officer overseas for three years for his Ph.D. to achieve the same ends? An institute that hires a Ph.D. for his technical skills alone would have acquired for itself a very expensively trained technician.

So, what is actually the main objective for a researcher to acquire a Ph.D.?
What the researcher gains from his Ph.D. is in terms of responsibility, resourcefulness and independence that will stand him on good ground in the research that he undertakes after his degree. The Ph.D. course trains the researcher in the *discipline of research*. This, then, is the most important goal in attaining a research-based Ph.D. The graduate would be deemed to be equipped to carry out any research related to the general subject area in which he has been trained. He might still require supplementary instruction in specific techniques, but he is otherwise mentally prepared to shoulder the responsibility of independent research from its concept to its execution and analysis.

Despite their Ph.D. training, many researchers fail to impress when they are back in Malaysia, even if they have seemingly performed well overseas. Why is that?
We'll leave aside researchers who are poor candidates to begin with and those who simply don't try hard enough. Otherwise, I believe such a situation commonly arises where the researcher has not been adequately challenged in his degree project. For instance, he might have been assigned a project that was a part of an established programme where the direction of research was already pretty much entrenched. The student carried out set-piece research that presented little scope to develop initiative and originality. When this new Ph.D. is placed in a different working environment on his return to Malaysia, he is unable to adapt and he is happy only to continue doing the same kind of work he has been doing overseas. In such a case, I would say his Ph.D. training has not fulfilled its intended objective. The university where he did his degree benefited from his labour, but he himself did not benefit. He got his degree, but the training failed.

Besides the right training, what else would a researcher need to get his research off the ground?
Hard work and good judgement.

When a research project falls short of expectation, is it usually because insufficient hard work has been put in?

There's no escape from hard work of course, but long hours in running experiments alone aren't sufficient to guarantee a successful outcome. Success in science demands careful and close attention to each of the essential stages: 1. Research concept; 2. Experimental design and planning; 3. Research execution; 4. Data analysis; 5. Result interpretation. The pitfalls in research can be in any of these phases. But assuming sufficient work commitment has been put into a project and the researcher is technically competent, I consider research failure to stem more from bad judgement than from anything else. Many research foul-ups can be rescued and the work resurrected. For example, data can be re-analysed, inferences can be re-cast and some experiments can be redone. But the consequences of bad decisions that arise from poor research judgement can sometimes be hard to undo.

When is such critical judgement demanded of the researcher?
Right from the beginning. When we set out to do our research, we have to decide what we want to do. That might seem patently obvious, but the wrong choice of research project could easily lead the researcher into a dead end with little fighting chance to come up with something tangible. The researcher (or research manager) has to decide from the outset what is worthwhile and useful doing and what is not, bearing in mind the research priorities of the institute. Critical decisions are taken at this point on the basis of what is thought to be workable and 'do-able' and what is not, also taking into consideration the availability of resources, including key personnel with the competence to run core aspects of the research. Bad decisions give rise to a lot of activity in the lab, but with little to show in the end.

Are there yet more critical decisions to be made after the project is selected?
The experimental approaches have to be selected. At any given time, most of us have more ideas and hypotheses that we want test out than we have the time and resources to actually do. We can fit in only so many treatments into our experiments at a time. Here again, sound decision and judgement is called for. It does not mean, however, that once the course is set, it can never be altered because mid-way changes will almost certainly happen. As the results of the study emerge, course correction would be necessary to re-tune the project and keep on-track or perhaps to change track.

Decisions can make or break a research project. How do researchers learn to make the right decisions?
Making the right decisions require insight, experience and a conscious effort to look at the 'big picture'. Good judgement is called for, and a good portion of this is acquired on the job. What some people call 'intuition' is to a large extent just experience. Newer researchers should not hesitate to consult with their more senior colleagues should they feel they need assistance. Some researchers fare better than others in research aptitude but with experience, everyone can acquire a measure of this ability.

Do experienced researchers always get it right, then?
Some businessmen are more successful than others because they make more right judgements than wrong ones when it comes to the critical decisions. They call this 'business acumen'. The champion archer wins a tournament not because he never misses, but because he is on target more often than his fellow competitors. There will always be instances when researchers, new or experienced, are off target. Good researchers don't get it right all of the time of course. But they make the right calls more of the time, and make them when it counts.

H.Y. Yeang

7. Input from the researcher: Doing more when you can get away with doing less

Reasonable research output can only come from reasonable input. This segment discusses what 'reasonable' level of commitment is expected of a researcher in the Biotech Unit and how to maximise the productivity from such commitment.

In the private sector, there is incentive to work hard because of the reward and penalty mechanisms that are in place. Isn't it true that LGM (and generally universities and other research organisations in Malaysia) have limited opportunities to reward good work performance?

I believe the LGM management tries to promote good researchers and to hold back promotion for those who have been unproductive. Nevertheless, the management is constrained as to how far it can reward good research staff while keeping within the bounds of the government rules and regulations.

Isn't it true that LGM researchers can actually get away with performing the minimum? Has any LGM RO ever been penalised for poor research output?

I don't know of any researcher who has been dismissed or demoted for poor research performance. Neither do I recall anyone having had his yearly increment stopped for poor performance so long as he clocks in regularly for work.

Why would Biotech Unit researchers be expected to work hard without expectation of reward?

The basic reward we receive should not be taken for granted: we do receive a salary for the duty that is expected of us. It is true, nonetheless, that the reward received is not commensurate with performance. We can view the situation thus. Not everything can be measured in terms of material reward; we can take pride and satisfaction in our work. Anything worth doing is worth doing well. A stage performer hones his skill to a level well beyond what is expected or can even be appreciated by the paying audience. He takes pride in his performance. A master craftsman toils on his masterpiece, putting in effort beyond what the ordinary art connoisseur would prize and be willing to pay for. He takes pride in his handiwork. You can't always put a monetary value on the satisfaction of a job well done. Like these artistes, we can take pride in our research work. When we travel overseas to attend conferences, our counterparts often recognise us through our published research papers. We take pride in our publications. Therefore, even though it is possible to get away with doing the minimum, we can choose not to. It takes character to make that conscious decision.

Therefore, we should all throw ourselves selflessly into a regime of hard work even in the absence of incentives?

Selflessness on the part of the researcher is not something that can be taken for granted either. There is a realistic limit as to how far job satisfaction and duty can take RRIM towards research excellence in the long run if recognition and incentive for good work is not forthcoming from the management. As we have made this point to LGM management repeatedly, I imagine it would not have escaped their attention.

Just how hard are Biotech Unit Research Officers expected to work?

When ROs were completing their Ph.Ds, they think nothing about staying back in the

lab into the night. We know that researchers at the Korea Kumho take dinner at their institute and continue their laboratory work after that. Nevertheless, we have to be realistic about expectations from Biotech Unit staff. They are no longer Ph.D. students and LGM is not run along commercial lines. Moreover, many ROs in the unit are women who have a family to take care of, and long hours in the laboratory can be inconvenient to working mothers with a growing family. It is my view that so long as the RO gives his or her full effort from 8 a.m. to 4.15 p.m. every working day, a good measure of productivity can still be achieved, provided that the work is well planned and the time productively spent. ROs, including the ladies, should be prepared to stay behind after work or come in at the weekends from time to time when exceptional workload demands it. This would not be something routine, but ROs should accept small sacrifices on their part on the infrequent occasions that their time is called upon after office hours. Devoting more hours than the minimum 8 to 4.15 in the laboratory on a more regular basis would of course be much appreciated. But under our circumstances, it would not be realistic for me to deem this obligatory. I should re-emphasise that careful planning and good time management maximises the amount of useful work that can be completed in the time spent.

Wouldn't it be a good idea to add more working hours per RO by employing additional supporting staff (e.g. contract staff using IRPA funds)?

This is not something I can agree with. Additional support staff will certainly result in increased activity, but this does not necessarily translate into meaningful activity or productive work. I have worked in the RRIM for 26 years and in only 9 of those years in the 1980s when I was assigned field research in additional to laboratory research did I have two assistants. (It would not have been practical to ask Fatimah to supervise the field labour; Choo therefore did this.) The rest of the time, I had one full time assistant, and for various short periods, none. I had never felt handicapped in my research because I had been assigned only one assistant. For laboratory work (which is what Biotech ROs are mainly doing), it is absolutely essential that the researcher be at the bench to observe the experimental results himself. A lot of my own work that I am pleased with has roots in serendipitous observations that I made in the course of experimentation. With an increased number of assistants, ROs would be more tempted to do research by 'remote control' and in doing so, miss out on critical personal observations. I feel that an increased number of assistants is justified only when large-scale repetitive work (e.g. tissue culture) or field work is involved. At CSIRO, Australia, a researcher is deemed to have 'arrived' if he is assigned a *shared* assistant. If increased research output is the goal, it would make more sense to recruit additional ROs or contract graduate staff who can work independently.

Other than their duties in the laboratory or in the field, how else should Biotech researchers occupy themselves to improve their work performance?

Researchers need to learn from others who are active in the same fields of study. One way to do this is to attend scientific meetings, seminars and conferences and Biotech ROs should make it a point to look out for relevant meetings to participate. However, they need not wait to attend infrequent conferences to be exposed to useful ideas because the library (even if somewhat depleted these days) is an invaluable source of information. I am amazed by how little library work Biotech ROs do nowadays and I can't understand why this important and essential source of information being under-utilised. I hope to see all ROs, especially the more junior ones, spending more time in the library to learn how others approach research problems similar to their own.

H.Y. Yeang

8. Output from the researcher: Between tangible results and hard luck stories

Having delivered the expected input as discussed in the last segment, Biotech Unit ROs can expect tangible and measurable output from their effort. This segment describes the commitment of results expected of the researcher and how the results obtained would be perceived.

Given the vagaries of scientific research, is it fair to expect researchers to commit specific research targets beforehand?

No employer would pay an employee - whether a researcher, a bank clerk or a toothbrush salesman - a salary without having some idea of what the payback might be. It is not sufficient for the employee to play safe by saying that he 'cannot promise', or to declare that he 'will do his best' and to leave it as that. By making a commitment to a research target, the researcher of course runs the risk of not being able to deliver on what has been promised. But he must be prepared and willing to stand up and be counted. With commitment comes responsibility. That is why research target setting is not trivial. The set targets should be achievable within reason, but should not be too unchallenging or ridiculously easy to achieve.

If research outcomes are so unpredictable, wouldn't it be fairer for a researcher's performance to be judged on the effort put into his work rather than by the results that he can come up with?

Diligence is of course something to be admired and appreciated of the researcher. However, hard work in itself is not necessarily useful or productive work. For example, if the researcher spends a lot of time on a poorly designed experiment, then he is only wasting his time and the institute's money because no meaningful results will be obtained in the end. This is again true if his seeming hard work consists essentially of repeating what others have already done previously ('re-inventing the wheel') because he has not taken the trouble to familiarise himself with the literature. In either example, the researcher would be seen to be busy and time and money would certainly be spent. Yet it is all merely futile effort that the institute would not benefit from. Hence, it is not always easy to judge from the time spent or the amount of activity going on in the laboratory whether meaningful work is really being done.

What's the solution to this predicament?

We won't have to make such a judgement if we assess performance essentially by tangible results rather than by the level of activity.

But consider the case where a researcher is competent and industrious, yet his experiment fails because of bad luck. If his work is to be judged mainly by successful results, won't he be unfairly penalised?

Ask any businessman. When times are good, every business – even the badly managed ones - prospers. It is in bad times that we can sort the good managers from the mediocre by seeing how well they cope with and respond to setbacks. Experiment failure is commonplace. I would be greatly surprised (and more than a tad suspicious) if *all* of a researcher's experiments were to turn out successful. Investigative research involves hypothesis testing, and obviously, we can't always get our hypotheses right. There has to be a certain amount of trial and error and we expect some failure to crop

up unless we are doing something very routine and unchallenging. To make allowance for such failure, researchers should not put all their eggs in one basket. They should be looking at a number of sub-studies (comprising those that are short and long term, high and low risk) at the same time to spread out the risks. In that way, even if one work area falters due to 'bad luck', there are always others to fall back on.

What if the researcher has 'really rotten luck', and* all *his experiments fail?

Even an expert card player will lose a round from time to time if he is dealt a bad hand. He loses because of 'bad luck'. But if he loses consistently, we might hesitate to attribute all this to plain 'bad luck'. There is a far simpler explanation, and it is that he is not the expert card player he claims to be. A tennis champion can lose a match due to 'bad luck' if the shot he plays falls just outside the line at a crucial moment. If this tends to become a common occurrence, his problem may not be so much 'bad luck' as 'bad tennis'. Similarly in research, it is common and expected that a researcher fails in his experiments from time to time. But if the researcher consistently fails in most of his experiments, we would have to consider seriously if his repeated failures might really be due to 'bad planning' or 'bad techniques' rather than to 'bad luck'. In other words, is it simply a case of 'bad science'? Be thankful for any lucky breaks that you get in your work. But don't depend on good luck to bring you success and don't blame bad luck for your failure. *The fault, dear Brutus, is not in our stars, but in ourselves, that we are underlings.*

So, excuses are not acceptable for the lack of results?

As stated, a certain amount of failure is expected in research; here, excuses are neither necessary nor relevant. Research managers would generally be satisfied if most - even if not all - of the main research targets are reached. It is another matter, of course, if there are hardly any tangible results to show. If an employer (or research manager) were to let it be known that he would be happy to accept *either* results *or* excuses (in lieu of results) from his staff, he shouldn't be surprised to be inundated with the latter. Instead of handing up results, workers would be coming up with excuses by the armful. It's just so much easier to generate excuses than to generate results! A researcher invariably faces numerous hurdles in the course of his work; it's all a part and parcel of the job. It is then up to him to find or devise ways to get around these obstacles. How well he succeeds in doing this marks how competent a researcher he is. If excuses were as acceptable as results, there would be temptation to back away from even the very first obstacle encountered, and to supplant results with excuses. That is not to say that whatever excuses dished out are invariably without merit. Individual cases may be considered, but hard luck stories are rarely good substitutes for tangible results.

Supposing a researcher has extraordinary good luck and achieves excellent results without even trying. Won't he be undeservedly recognised and rewarded for his results?

Why begrudge someone's good luck, it that's really what it is? But just as it is unlikely that a researcher encounters repeated failure due to bad luck, it is improbable that a researcher can count on good luck to do well consistently. It was Louis Pasteur who said, "Chance favours the prepared mind". It is easier to explain and believe your colleague's commendable experimental results as being the outcome of his experience, perception, planning and technique. Don't envy his 'good luck'. Instead, appreciate his good work

H.Y. Yeang

9. Writing for scientific journals

Good research results form the basis of research publications. This segment discusses what research results are publishable and what it takes to get the paper written up properly. The importance of systematic preparation from the outset in anticipation of publication is emphasised.

Why is research output always linked to publication? Isn't there also good research output that is by its nature unsuitable for publication, but yet should be recognised?
Some ROs in the LGM produce excellent work that does not generate publishable data. It is true also that a certain amount of 'house-keeping' research needs to be done and the information generated is not always presentable in research papers. But on the whole, most substantial pieces of new information coming from the Biotech Unit are publishable and - with preparation and effort - should be published.

Some projects have long-term targets. Won't there be a rather lengthy wait before publications on these projects can be expected?
In a previous segment, I have referred to malarial vaccine development and the fact that many important papers have emerged from this research even though the final goal, the commercial product, is still elusive. If we have similar project objectives that take time to realise, various papers on related aspects of the research could come out along the way. For example, it would probably take years before we see recombinant pharmaceuticals from the transgenic rubber tree ready for commercialisation. This should not stop papers on GUS expression, super-virulence, etc. being published in the meantime. As another example, a commercial immunoassay is the target of the latex allergy project. While work on this is progressing, several papers characterising the allergenic proteins have already emerged.

Why do some researchers find it hard to get their results written up for publication?
A common reason why some good results remain unpublished (other than because it is institute policy) is that the results are disjointed and do not make up a coherent sequence of results (a 'story') that is suitable for publication. To avoid such disjointed results, it is necessary to plan research carefully from the outset with the intention to publish. It is not usually a good strategy to accumulate a lot of data without much planning over a period of time, and then to put all the files on the table some day and see how the data might be collated into a publishable paper. If you do this, you will frequently find you have a mish-mash of data, much of which are superfluous and unusable, whereas other essential pieces of data (e.g. a crucial control) might be missing or incomplete.

So, good planning is essential if there is intention to publish?
Very much so. If you aim to write research papers, they have to be planned for at the outset of the research. This point is so important that I shall highlight it in a box.

> Papers do not just happen by themselves.
>
> They have to be planned for from the outset of research.

Before you even begin your work on the bench, you should have already some idea of the type of data you would get, how you would analyse the data and what conclusions you are likely to obtain. At any time, a Biotech Unit RO should be able to say what papers are expected from the project he is currently working on. Of course, even if you plan your research, things will probably not work out exactly as you originally envisage it. Sometimes when unexpected trends emerge, the paper that is finally completed becomes quite different from what has been planned initially. Be prepared to modify and adapt. Even after careful planning, the vagaries of research are such that there is *still* no guarantee anything publishable will emerge. But just imagine what happens if you *don't* plan. You don't really stand a chance at all.

What goes into a publishable manuscript?
Reputable journals expect high standards in the design, execution and analysis of the research. Good scientific standards are universal and regular readers of scientific publications would be familiar with them. Nevertheless, the stringency exercised in the specifics may vary with individual journals. For example, a journal specialising in allergy may require the author who uses serum from allergic patients to specify the patient profile, the patient's allergic symptoms, the diagnostics used to verify allergy, etc., whereas a biochemistry journal might simply accept that the patients are allergic without requiring further supporting data. To appreciate what a particular journal requires of the author, he should familiarise himself with papers appearing in that journal. Stringency in scientific standards sometimes also depends on what is commonly accepted by practitioners in the particular scientific discipline. For example, if an author reports that treatment with chemical X increases the girthing rate of the tree, appropriate statistics (t-test, analysis of variance, etc.) would probably be required to support such a statement. However, if an author reports that chemical X increases gene transcription, all he needs to show is a Northern blot with the 'treatment' showing a distinctly bigger blob than the 'control'. There is no real reason why better quantitation of mRNA supported by statistics is not insisted upon, but the journals just don't require it.

With the relevant data is at hand and properly analysed, what else does it take to get a paper accepted for publication?
It takes good presentation. That means a lot of hard work in writing up the paper, especially for the better journals. English is the language of science. A good command of the language and the ability to write with good flow and clarity is essential. Several drafts are normally required before the manuscript is deemed suitable for submission. After the manuscript is submitted, practically all require revision to accommodate the referees' comments before acceptance (if it has not been rejected outright). From my own experience, it is not uncommon for more than 10 substantial revisions of a manuscript to be prepared before it gets accepted in a good journal. So be prepared to write and re-write. And then re-write some more.

What else can we do to increase the chances of getting our paper accepted?
Put your institutional affiliation as *The Rubber Research Institute of Malaysia* rather than as *The Malaysian Rubber Board.* The past reputation of the RRIM notwithstanding, we take advantage of any bias referees may have for research undertaken at a research institution as compared with a 'Board' which sounds rather like some government administrative or regulatory body.

H.Y. Yeang

10. Getting those manuscripts published

The last segment touched on the hard work that goes into writing a paper. This segment contains suggestions on the journals to submit our manuscripts to and discusses how the manuscripts are likely to be processed and reviewed by the journal. While we hope our submissions will find ready acceptance, we should also be prepared for rejection.

Where should we publish our research findings?
LGM has a house journal which is the *Journal of Rubber Research (JRR)* and Biotech Unit researchers have an obligation to contribute to it. My proposal is that we send half of our manuscripts to our house journal. Papers pertaining specifically to *Hevea*, and especially the more specialised reports that attract a niche readership among scientists researching on rubber can be picked for *JRR*. We should send our other publications to the most prestigious relevant journal that would accept our paper. (Remember to obtain permission from Management to publish in outside journals.)

How do we determine which journals are prestigious and respected? How are journals rated internationally?
About 3,300 reputable scientific journals are listed in the database of the Institute of Scientific Information (ISI) and they are rated by their Impact Factor (number of citations divided by the number of papers appearing in a journal) in the ISI's Science Citation Index (SCI). You can obtain the 1994 SCI Impact Factor list through the Internet at www.pg.gda.pl/chem/Miscellany/docs/imp-fac.html. A partial 1997 list can also be obtained from www.mdc-berlin.de/biblio/impact.htm. The most current list has to be purchased but our library cannot afford it. Nevertheless, Impact Factors do not change drastically overnight and so even an old list would still be useful. You can also try writing to the publisher for this information. Values for impact factors are not absolute and generally, comparisons are valid only between journals of the same discipline. Among biological journals, I consider an Impact Factor above 1 to be quite good, but other journals on the SCI list are still acceptable, as they would have already satisfied various minimum merit criteria for inclusion on the database.

In submitting our manuscripts, is it essential to restrict ourselves to SCI-listed journals? Shouldn't we also consider journals that are not in the SCI?
Writer W.W. Gibbs offers his view in *Scientific American* (August 1995) that inclusion of a publication in the SCI or a similar top database guarantees that it would be seen globally when scientists search the literature and decide which work to cite in their own work. On the other hand, papers excluded from the database remain largely unread and uncited, and they are 'condemned to a ghostlike existence'. Since Biotech Unit ROs are already contributing to *JRR* which is not in the SCI, I feel we should reserve our other papers for SCI-listed journals, especially those that are highly rated.

After we put in all the required hard work in preparing the manuscript, can we reasonably expect our paper to be accepted for publication?
If we go for publication in the leading journals of a discipline, we have to accept that rather strong likelihood that it might get rejected. Acceptance rates in premier journals such as *Nature* and *Science* are about 1 in 30. Unless the work is outstanding and very original, the chances of getting our papers into these journals are slim. Less formidably ranked publications like the *Journal of Allergy and Clinical Immunology*

(the leading allergy journal) can have an 80% rejection rate: out of every10 manuscripts that the editor receives, eight get the thumbs down. So it should not come as a complete surprise to us if our manuscripts get rejected. But there is really no shame in having our manuscript returned. It need not necessarily mean that the paper is no good as there are different reasons for rejection. I have mentioned third-world bias before, but let's not dwell on that because a lot of grounds for rejection are totally without prejudice or malice on the part of the referee. The referee could just be holding another personal viewpoint on the subject of the paper and he cannot be persuaded from his conviction. At the news conference following the announcement of his winning the 1999 Nobel Prize for Medicine, Dr.Güenter Blobel spoke of his many disappointments in the 30 years of research, "such as when your grants and papers are rejected because some stupid reviewer rejected them for dogmatic adherence to old ideas." Even Nobel laureates are not exempt from rejection.

So everything lies in the hands of the journal referees?
Not quite everything. In most international journals, the editor has a big say in deciding what gets into the journal. The role of the editor or Editorial Committee is even more pervasive in the most prestigious journals like *Nature* or *Science*. They screen through the large numbers of submitted manuscripts and select only a very small proportion that actually gets sent to referees. When journals like these reject your manuscript, chances are they got rejected by the editors. The manuscript probably never even got to the referees. The editorial assistant to a leading international plant journal tells me that she routinely recommends rejections of quite competent papers simply because the journal receives many more good papers than it has space to publish.

Therefore, we've first got to convince the editor that our manuscript is worthy of publication?
That's the first thing we need to do. In this connection, the covering letter that accompanies the manuscript can be very important. It has to say a lot more than: "Please find enclosed herewith three copies of my manuscript…." In the covering letters that I send to the editor of foreign journals, I usually give a brief description of the paper and highlight the important points to show why the findings in the paper are important and noteworthy to the scientific community. Where possible, I would try to show how the research findings relate to other important areas of current interest. I consider the covering letter so important that I routinely go through four or five drafts.

What do we do when, despite our best effort, our paper gets rejected?
The good thing about submitting your paper for publication is that you get more than one chance. If you think you have a solid paper and it is just that the referee doesn't see eye to eye with you (a matter of subjective opinion), just send the manuscript to another journal. But do make the effort to incorporate whatever improvements you can glean from the previous referee who rejected your effort. If you are aware of weaknesses in the paper that cannot be remedied, you might want to consider submitting to a less than top-notch journal (but still one listed in the SCI).

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What if our paper* still *gets rejected after sending to several journals?
Perhaps, then, the writing is on the wall. As much as we might not want to accept it, repeated rejection could mean that our work is *really* not up to standard. It's time for a serious post-mortem and time to pull up our socks!

11. Publication report card for the RRIM Biotech Unit

The preceding segments have emphasised the importance of publishing and included suggestions as to how to go about it. Publishing of scientific findings is especially relevant to the type of work that is carried out in the Biotech Unit. The unit's performance is examined in this segment.

How frequently are Biotech Unit officers expected to publish?
I have set a target of a minimum of one paper per year, irrespective of whether it is a single author paper or a joint-author paper. This is something well within the reach of all officers in the unit. Some may feel this is too low a mark, but don't forget it's only a *minimum* target. If you feel you can better that, do set your own target.

How well has the Biotech Unit done in publications compared with other Malaysian research institutes and universities, especially in plant biotech and plant science?
I can't say I've made a detailed survey, but I have looked at the scientific publications emanating from Malaysian universities and research institutes from their Annual Reports, research grant applications, etc. While there is certainly room for improvement, I believe RRIM Biotechnology Unit has generally outperformed similar plant science departments/units in other institutes and universities, including those that have more researchers and a bigger research budget than ours. As the head of RRIM Biotechnology, I am proud of the performance of researchers in the unit. The table of selected data below summarises our performance as compared with the output of some Asia-Pacific universities.

Institution	***Number of papers published*** per teacher or researcher per year
Tokyo University	2.1*
Melbourne University	1.9*
Australian National University	1.5*
Kyoto University	1.4*
Hong Kong University	1.3*
Singapore University	0.83*
RRIM Biotech Unit, Q1 officers only, 1996-98	**1.1**
RRIM, A7 Research Officers in basic biological science, 1971-80	**0.60****
RRIM Biotech Unit, All officers, 1996-98	**0.54**
University of Malaya	0.11*
Universiti Sains Malaysia	0.09*
Vietnam National University	0.07*
Universiti Kebangsaan Malaysia	0.03*

* Number of papers published in international journals and in 'Asian Academic Journals'. Source: Asiaweek May 15 1998. These are averages for the university concerned. Some individual departments fare better.
** Output from five A7 officers. (A7 is a 1970s salary scale normally reserved for Heads of Divisions and a select few others.)

Should we be concerned only about the number of papers published? Isn't the quality of the papers more important?
Of course, the quality of the paper is important, and that's what I mean when I say we should get our paper published in the most prestigious journal that would accept it.

But for the moment, I am mainly concerned that the papers meet an acceptable standard (i.e. a standard acceptable to international referees), rather than that they have to be outstanding. We need to realise that RRIM does not have an entrenched culture of publication and just getting people to publish regularly is in itself a worthwhile goal. Over time, I'm sure researchers will become increasingly motivated not just to publish, but to publish truly excellent papers. For the time being, it is my hope that all Biotech Unit officers make the effort to publish, and that they would appreciate and emulate the achievements of their colleagues whose papers appear in journals.

All the discussion so far has centred on refereed journal papers. What about conference papers? Surely they must count for something too!

The presentation of papers in conferences and seminars, either orally or as posters, is encouraged. Conference papers serve various important purposes. They provide useful and relevant information to other researchers in the same field of research. In many meetings, such papers provide the latest information that is disclosed in advance of the formal journal publication. Hence, conferences are often good venues to learn about the latest discoveries by others and to let others know about our own latest findings. More than that, the personal interactions between researchers from different laboratories that occur at conferences are often more useful than otherwise faceless communications by letters and email.

A 'Last Three Years' list of refereed journal publications is prepared in the Biotech Unit annually. If conferences are also important, why are conference papers not included in the list?

As stated above, conference papers often provide advance information that will appear later as journal papers. Hence, when the paper appears in a journal later on, it will be duly included in the Biotech journal publication list. The question of it being left out does not therefore arise. Conference papers are often repeated wholly or partly in different meetings that are attended by different audiences. Essentially, there is nothing wrong with such repetition. A famous scientist on a world tour, for example, can hardly be expected to come up with an original paper at every one of his stops. He delivers the same paper, but to different people. If you are invited to present such a paper, you should feel honoured. But you should expect to receive credit for the substance of your paper only *once*, and that is when it gets published in a journal.

What about papers that are presented in conferences, but that don't make it to a journal? Shouldn't there be some recognition for such efforts as well?

Very few papers fall into this category and we should ask why those papers don't get published in a journal eventually. Here are some possible reasons. It could be that the new findings described in the paper are minor and unsubstantial (i.e. they are trivial). Or the paper repeats what has already been reported previously (unoriginal). Perhaps there are major failings in the experiments described (flawed). Such shortcomings might not be evident in a conference presentation or poster, but they would be picked up by the referees in a full paper. Finally, it could simply be that the researcher has not taken the trouble to expand the conference abstract into a full journal paper. We should ask this of ourselves: If a conference paper were to report on findings that are trivial, or are unoriginal, or are flawed, or if the researcher has not bothered to produce a journal paper, why should he expect credit or recognition?

12. Collaboration with local and overseas laboratories

It is a paradox of research that even as the boundaries of research disciplines blur, the scope of scientific research is expanding so rapidly that complete mastery within the discipline is becoming an unrealistic expectation. Resources have therefore to be outsourced sometimes and collaborations with local and overseas laboratories to pool expertise will become increasingly common. In a previous segment, I emphasised the importance of our publication record if we intend to team up with top-notch research institutes or universities. In this final segment, we look at what makes a collaboration work and how we should go about setting it up.

In a research project, when is the contribution from one party considered a 'collaboration' and when is it regarded as a 'service'.

A 'service' generally involves repetitive laboratory manipulations based on established procedures. There is usually very little scientific judgement expected of the scientist-in-charge whose main concern may more be towards quality control. Examples of services are DNA sequencing, antibody production, and such like, many of which are available as paid commercial services. Someone providing a service would not normally be named a co-author in a publication (although his contribution may be acknowledged), but discretion is sometimes called for. The Biotech Unit often provides services of routine electron microscopy, protein assays, etc. without expectation of recognition. Should the work involve developing a new laboratory procedure or a substantial modification of an existing method, or if it demands expert analysis and interpretation, it is then justifiably a part of a true research collaboration.

The merits of strategic inter-institutional collaboration have been widely touted. Are there instances where a proposed collaboration is in fact undesirable?

Research collaboration so invokes images of co-operation, sharing and synergy that the very suggestion of rejecting collaboration is almost tantamount to heresy! Yet as we go for the much-bandied 'smart partnership', we should be vigilant not only towards potential gains but also to the pitfalls that could arise out of research collaboration. Examples of pitfalls are unequal agreements, unequal profit sharing or loss of access and ownership of genetic resources. Lack of commitment and sincerity ('bad attitude') on the part of the collaborator is of course another obvious reason to decline or to re-think co-operation. In the past, we have declined collaboration with universities and commercial companies where we felt the deal to be inequitable.

There have been cases where MOUs are signed with much fanfare, only to have the alliance fizzle out with little to show. What makes a collaboration work?

Some of us who attended the National Biotech Meeting last month will remember UPM's Dr. Harikrishna lament concerning erstwhile partners whose contributions to the Top-Down collaborative project had dwindled to naught (but not before partaking their slice of the research funds). From my own experience, two ingredients are necessary for a collaboration to succeed. Firstly, there must be *commitment* to the project based on trust and respect among the co-operating parties. While everyone is enthusiastic at the beginning, such interest is not always easy to sustain. Even if collaborators start off with the best of intentions, the pledged roles may not be fully played out when priorities change as new obligations are pressed upon the participants at their workplaces. The fruits of short-term collaboration are easier to come by, but only sustained commitment can see through a longer-term research collaboration.

What is the second essential ingredient for successful collaboration?
Research collaboration has the best chance of success where each participant stands to gain from the link-up. This might seem pretty much common sense, but can hardly be overstated. The research *need* must be identified first, usually by those close to the project. (What is worthwhile collaborating on? What aspects of work can suitably be divided up among the participants so that each has a tangible role to play? How would each participant gain?) It is only then that appropriate and qualified collaborating partners can be picked. If research managers take it upon themselves to set up a collaboration without first identifying at bench level the potential benefits to the co-operating parties, it would be difficult to make the union work. While such top-down declarations of collaboration make good PR vehicles, they rarely amount to much.

What do we hope to get out of a research collaboration with others?
The benefits of a collaboration vary between cases. We could stand to gain in terms of expertise (because our collaborators have know-how that we lack), materials (because they have something – e.g. a new DNA clone – that we lack), access to facilities (because they have equipment that we lack) or time (because they can share out some of the work in a large project).

What do our collaborators expect to get out of us?
Hopefully, much the same thing we expect from them. Yet there will also be those, especially from overseas, who seek collaboration mainly to get access to raw materials (e.g. germplasm, fresh latex) without really expecting us to have the competence to contribute further. While we may sometimes have to put up with a certain amount of such attitude, there has to be more to a collaboration than our being 'official material suppliers'. We would otherwise be merely providing a service for the convenience of others, and not participating in a true working partnership. Depending on the expertise and resources at their disposal, some partners in a joint project may take on larger roles than others. But, large or small, the contribution from each partner should embody a discernible scientific element. Otherwise, the partner is only a collaborator in name. I would especially like to see first authorships from RRIM in at least some of the publications written jointly with our collaborators (even though papers where we are not first authors are of course also greatly valued). First authorship denotes that the core work is done in the author's laboratory (unless he is on attachment elsewhere).

How do we respond to a proposal for a nominal 'collaboration' that actually amounts to no more than, for example, routine sample collection?
It depends on what the samples are and how much work is entailed. Even if we feel there is no merit in simple sample collection, we could still volunteer to do this purely as a favour - if not as a collaboration - to assist in the advancement of science provided that it is not too time-consuming. For example, I have sent small samples of ammoniated latex to people whom I have never met. It isn't a collaboration; it didn't take much of my time and I don't expect anything in return. On the other hand, if mRNA were asked of me, I would probably decline if it were not linked to tangible research collaboration. Notwithstanding the fact that RNA preparation is technically more demanding, there is the more important concern about 'giving away' germplasm. What happens to altruism in the interest of the 'advancement of science' in such cases? Where do we draw the line whether to give or to hold back? There are really no easy answers.

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