Sugarcane R,D&E: over managed and underperforming?

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Abstract In general, few productivity improvements in sugarcane have occurred during the past three decades. At the same time, production costs have increased and production statistics reflect decreased yields globally. In comparison to the ‘golden years’ of new technology and improved germplasm in the second half of the previous century, little more than optimisation of existing practises has emerged from the past two decades. Given the slowdown in new technology delivery, it is not surprising that many industries have placed more scrutiny on how they manage their Research & Development institutions and investments. The result of this ‘slowdown’ has created a perception that poor management of research projects and programs by scientists is at the core of the problem. This has led to the introduction of ‘real’ managers with the subsequent management of R&D as if it is a ‘normal’ production and sales business through well established ‘business models’. Strong emphasis has been placed on project selection, project management and minimising risk. Research, especially in the discovery phase, is a very high-risk endeavour and a high proportion of all projects fail. Institutions that have a low appetite for risk quickly run out of new technology innovation. Because of the inability to predict, a discovery project cannot easily accommodate management issues such as budgeting, milestone definition and timeframes. Managers generally prefer D and Extension over R because of the higher predictability and lower perceived failure rate. The key to proper management of R&D is a recognition that researchers and managers operate under very different codes of conduct. If this is not properly managed, then conflict between researchers and the rest of the business follows. It has become customary to view RD&E as a unit following a ‘systems’ approach. Despite obvious advantages of this approach, it often fails to recognise the most significant shortfall(s) in the value chain. This practice can unnecessarily inflate the cost, slow project progress and is dependent on consensus that tends to favour the lowest common denominator or more vocal team members. Consensus and innovation tend to be opposing objectives, as innovation requires thinking with an ‘outside the box’ mindset. Consequently, innovation can be stifled using this approach. Peer review is a great tool to measure progress in projects and selecting projects for development. It is not suited for selection of new innovative ideas. With no obvious improvement in technology delivery and adoption, it is timely to ask whether the current approaches are achieving their objectives. In addressing this question it is important to look at the global evolution of R&D models and modern trends in highly innovative businesses. Instead of trying to ensure that every research project entering the technology funnel delivers a product, a greater emphasis is needed to create an innovative environment where all role players are focussed on key strategic objectives, and all research results are seen as key learnings for future deployment.

Key words Innovation, technology funnel, project portfolio, code of conduct

INTRODUCTION

Productivity improvements in sugarcane have been negligible in the past three decades, and production costs and production statistics are reflecting decreased yields globally (FAO 2014). In all cases, increased production is linked to expansion of production area rather than increases in yield. In addition, the fact that the gap between top producers and the rest has remained the same for decades suggests that technology transfer from D and E to the end users is also failing, and that new models/approaches are urgently needed.

The period between 1950 and 1990 saw many significant new developments impacting on sugarcane productivity. This included overcoming production constraints, development of traits such as low lodging, self-trashing, high early sugar, mechanical harvesting, control of flowering, better rotational cropping, irrigation scheduling, modelling of crop performance, and crop forecasting along with many other developments.

However, technology development in the past two decades seem to be locked in just tweaking the current practises through D, with little new and innovative technologies emerging. For many key players this was viewed as a shortfall, or failing of how they managed their internal research and development (R&D) programs, and the way that investment targets were set. Consequently, many institutions have in the past two decades shifted to restructuring their RD&E efforts to include...
more structured research programs and projects, and a more formalised approach to project management associated with long-term milestone definition.

There is no doubt that managerial processes and tight control over people and resources in the R&D environment has increased exponentially during the past four decades. It is important to recognise that a significant portion of this additional control is associated with regulations and requirements from external funding agencies. Despite this, there is no evidence that these steps have led to better technology development or transfer to the end-users.

Many industries and institutions continue to place scientific inputs and research programs under scrutiny and regular review. However, now, might be the appropriate time to question whether the industry’s approach to R&D, and the structures built to control and manage it, are failing! In those commodities where productivity did increase, innovative approaches and development of improved technologies drove the change, rather than more control and better ‘business’ management.

In this paper, I explore the evolution of R&D institutions during the past century, and the challenges faced managing R&D to strike a balance between scientific excellence, innovation, and technology transfer.

**EVOLUTION OF RESEARCH INSTITUTIONS**

R&D systems have gradually, and continuously, evolved over time. There is no consensus on how many generations of R&D systems there are, but they are at least in part linked to the nature of their particular environment. It is not my purpose to discuss the differences between the different R&D generations (for reviews see Ransley and Rogers 1994; Nobelius 2004; Park and Kim 2006), but only to point out a few important stages.

The first generation, starting from the beginning of the 20th century lasted until the 1960s. In this era, little attention was given to the economic management of R&D. Science-based R&D institutions and laboratories virtually had free reign and often operated as independent cost centres. There is often reference to this era as the ‘Ivory tower R&D’ where the attention was entirely on scientific breakthroughs. The R&D process was viewed as linear and focused on pushing technology downstream towards the marketplace, which was characterised by a demand exceeding the supply (Fig. 1).

**Fig. 1.** Diagrammatic representation of a technology push of a new invention by developers (A) or a market driven pull of new technology by the end user or customer that is needs based (B).

History shows that this period delivered astonishing scientific breakthroughs and the basic technologies that fuelled many of the modern day agricultural applications, communication, and electronic technologies were developed. In fairness, one would have to agree that the first generation R&D environment delivered handsomely on its goals.

During the mid-1960s and through the 1970s more emphasis was placed on marketing efforts to increase market shares and competitiveness (Rothwell 1994). In this second generation, R&D focus was placed on the short-term demand side, neglecting long-term research in favour of ideas from the market and direct clients. In contrast to the ‘push model’ from the first generation, this generation was driven by a market-pull (Fig. 1), where ideas originated from the market, to be refined and developed by R&D (von Hippel 1993).

Instead of having a free reign, R&D was reigned in, in the second generation, with the introduction of better management, which included project management to direct and monitor the R&D efforts. The business side of R&D institutions became the internal customer of R&D.
Strong economic downturn, a saturation of the demand and high inflation became the main drivers of third generation R&D, where cost control, cost reduction and downsizing drove the development of R&D. Management processes started to move strongly towards removing 'wasteful efforts' and naively ignoring the nature of the technology development funnel (Fig. 2). We see the emergence of a strong movement away from individual projects to project portfolios or programs and attempts to balance the risk-reward continuum of probability of technical and market success (Nobelius 2004 and references therein).

Fig. 2. The technology funnel comprises early research and development with significant overlap between development, extension, and adoption. Because of high ‘failure’ of ideas in the early stages of discovery a linear model as depicted in Figure 1A is not a reality.

In the fourth and fifth generation R&D, a much stronger emphasis developed on fully integrated activities, a bigger role and involvement of customers in setting the research agenda, system-based approaches, diversification, multidisciplinary teams, and an emphasis on intellectual property development and protection. Towards what probably is the end of the fifth generation R&D, much more emphasis is placed on network development and collaboration within a wider system, even involving competitors. Speed has now become imperative with product and technology development and there is a strong realisation of a need to separate R from D.

There is a more radical shift occurring as we move into the next generation of R&D evolution (Nobelius 2004). There is a strong return to the roots of the first generation of R&D as it becomes more evident that innovations that are more radical can only follow on to re-focus activities towards the research part of research and development. No matter how well the process is managed, the top end of the technology funnel must be maintained.

This is not suggesting that there will be a return to the old corporate research laboratory and research institutions. Instead the re-focus is taking on other approaches, including technology-sourcing strategies, internal corporate venturing, technology acquisitions, intellectual property acquisitions, joint ventures, independent research groups or networks, and strong internally driven R&D (Nobelius 2004 and references therein).

A major goal of a R&D business is not just to apply limited resources to selected projects with the highest expected payoff, but to create a portfolio of projects that will meet the business objectives of the firm while enhancing the firm's strategic ability to carry out future projects.

As discussed later technology development is hardly ever a financially-constrained – it is mostly limited by innovative ideas and creative thinking.

THE TECHNOLOGY PIPELINE

Despite many attempts to make research linear through management intervention there is not a linear relationship between the components of R,D&E (Dunphy et al. 1996).
It is customary for institutions and businesses to use the term ‘technology pipeline’ to describe the full set of potential innovations/concepts that are being worked on. Some of the terminology used to describe the technology development funnel (Fig. 2) includes discovery, proof of concept, early development, late development, regulatory approval, market launch, and adoption.

The R&D components are normally divided into three separate elements: basic research, applied research, and development (Wingate 2015 and references therein). In its purest form, basic research has the focus to acquire new knowledge without a defined goal or expected application of the knowledge. Applied research is really the art phase where this basic knowledge is used to create something. Project proposals in this space will usually contain words like novel, understanding, and identification. The nature of these projects can be explanatory (why certain responses or reactions occur) or exploratory (identification of mechanisms, compounds, or organisms).

Development can be thought of as the craft phase - evolving a current state of something by either modification to, or the creation of a product, process, system, or service.

Developing a new product or technology is a very high-risk endeavour and a high proportion of all projects entering the funnel fail to deliver the originally anticipated result/outcome. Some would argue that the technology or innovation funnel (Fig. 2) does not work and that it should rather be converted into a technology rocket (Nichols 2007). In such a model, the emphasis is on picking winners early on and then devoting all energy and resources on these to get them through the technology pipeline.

For obvious reasons the picking of only winners at the early discovery phase is not possible. However, with strict discipline and a well-defined strategic plan, businesses can ensure that the early basic research projects that enter the funnel are well aligned with the business model (Hobcraft 2011). This process of properly capturing the correct market drivers from gathering knowledge and identifying gaps is a funnel by itself and should be a strong focus of the business. Successful innovative businesses have mastered the art of recognising these two distinctive parts of the innovation funnel (Hobcraft 2011).

Fig. 3. Levels of certainty, predictability, creativity, and expected change at different stages in the technology pipeline. The nature of projects in (A) requires completely different management strategies than for those in (B). Adopted from Wingate (2015).

If not executed properly the funnel process brings with it a huge paper load and with modern management strategies a process that is getting bigger, not smaller. When companies put more and more emphasis on measuring efficiencies, accountability on the top-end of the funnel increases and an overload in measurement and reporting is created.

If the outcome is predictable then it is not R but rather late stage development. Projects that represent D rather than R will have an emphasis on optimisation or comparisons between options. It is easy to differentiate projects in this category, as they will always contain phrases such as a comparison between, optimisation of, and the like.
Managers generally prefer D and E to R because of the higher predictability and lower perceived failure rate (Figs 2 and 3). In the case of a basic research project, the outcomes are unknown, and the number of trials/experiments/iterations are almost endless. If an institution has a low appetite for risk then the whole system quickly moves to a D and E only enterprise.

The concept that we can accurately predict and manipulate the outcomes of research is fundamentally wrong. Behaving as if the research outcomes are predictable is a recipe for disappointment and even disaster. The over emphasis on outcomes defined at the start of the project (in the past two decades) has seriously compromised innovation and R delivery, and ironically, both D and E become seriously compromised and even redundant as a result.

Technology developers or institutions that do not play an active role in the early discovery phase must develop and find ways to get access to new technologies. This can be achieved through partnerships with other leading institutions, or licencing agreements with technology owners.

It is also important to note that technology adoption is not a linear process and significant failure to get a high level of market penetration is likely. This is seldom linked to a lack of information flow but rather to factors such as socio-economic drivers, personal preference, lack of risk taking, etc. It is well established that in difficult economic circumstances most new technologies that require significant capital investment become stuck in the technology pipeline.

**MANAGEMENT OF RESEARCH AND DEVELOPMENT**

What constitutes proper and efficient management of R&D processes has long been a matter of debate and considered a troublesome area with no simple answers. It is evident that for those companies, institutions, and industries that get it right it is a winning recipe for product development, increase in lead-time precision, increased production efficiency, cost reduction, reduced environmental footprints and overall global competitiveness. It is the Achilles heel for those that get it wrong.

The perceived poor management by scientists of research projects and programs, led to the introduction of ‘real’ managers. Subsequently, RD&E is being managed like a ‘normal’ business using well established ‘business models’. There is nothing wrong with the concept of introduction of professional managers. In fact, when done for the purpose of better delivery of support to the core business and setting business objectives, it can lead to significant enhancement of R&D delivery. However, when it is done to ensure better control and enhanced project success rates, the results will be disappointing.

However, managing the technology funnel involves three very different tasks or challenges. The first is to widen the mouth of the funnel - the organisation must expand its knowledge base and access to information in order to increase the number of new products and new process ideas. The second challenge is to narrow the funnel neck - ideas generated must be screened and resources focused on the most attractive opportunities (Wingate 2015).

The core of a successful R&D business will always be dependent on the existence and creation of an innovative culture (see later). For most R&D organisations, there is much more than just execution of new project ideas and hence achieving the optimal balance to support the entire business and adequately grow innovation, is critical to sustained growth.

It is therefore crucial that senior managers realise and take into account the different environmental requirements set by operational excellence and innovation. When a business focuses on efficiency, scale and low-variance operational excellence, the culture and behaviour will be on processes, measurements, rewards and lack of tolerance for failure.

These drivers are fundamentally different from those needed to create innovation. Innovation requires an emphasis on exploration and invention and a reward for risk taking and failure. Innovative businesses celebrate the learning that comes from trying.

Many would argue that an organisation either has to segregate its innovation activities from its execution activities, or alternatively, change organisational design from execution dominance to innovation dominance when needed (Hess and Liedika 2012).

The key ingredients for success include innovation management, strategic planning, performance measurement, creativity, project portfolio management, performance appraisal, knowledge management and teams to offer an easily applied recipe for enterprise growth (O'Sullivan and Dooley 2009).
Innovation management

Perceived poor management by scientists of research projects and programs has led to the introduction of ‘real’ managers in the past few decades to R&D institutions. Subsequently, R&D&E is increasingly being managed like a ‘normal’ business using well established ‘business models’.

There is no doubt that this per se has introduced much needed discipline and financial control. However, at the same time brought with it the approach of ‘one size fits all’.

The huge difference in predictability and risk (Figs 2 and 3) at different stages in the technology pipeline points to the fact that projects cannot be managed in a simple rigid manner. An attempt to do so is one of the most efficient ways to completely stifle innovation in a R&D environment (see later).

The basic principle of project management is the execution of an activity with a defined scope or goal, to be accomplished within a specific time frame or schedule, and with a dedicated budget. These principles can be successfully applied to a late stage development project (Fig. 2) when proof of concept is available and potential impact of the technology on the end-user can be better estimated. At this stage, the risk associated with the project is also significantly reduced. Because timelines and deliverables can now be set, issues such as milestone definition for long periods (3 to 5 years) can be set and measured.

Trying to force researchers to define budgets, milestones, and specific deliverables in early stage research projects enforces wrong and sometimes fraudulent behaviour.

There are major concerns about the lack of reproducibility in science (for review Begley and Loannidis 2015). Some investigations suggest that less than 10% of research results may be reproducible. In technology development, where a current project builds on the outcomes of previous projects, this is extremely alarming.

The factors involved in this lack of reproducibility are complex and include lack of scientific rigour and publication/output pressures. However, this is not a failure of the scientific method. It is more a result of the consequence of a system or business willing to overlook and ignore the lack of scientific rigor. Even worse one that rewards flashy results that generate scientific/customer buzz or excitement and encourages early preliminary observations (Begley and Loannidis 2015).

If the objectives can be set out up front then the researchers either knew the outcomes before they start or even worse, this could lead to reporting of fraudulent data. In early stage research projects neither the time frame nor the activity of specific budget requirements can be defined with accuracy.

It is alarming that in some disciplines more than 90% of tested compounds or hypothesis turns out to be correct (Begley and Loannidis 2015). This number might even be higher in project milestone delivery aligned with the initial forecast or prediction. A system evolves and delivers according to what it incentivises.

For R&D activities the expectation and objective is to increase knowledge in a particular area with the expectation that this will eventually lead directly or indirectly to something new. A result other than what was predicted in the initial hypothesis, or lack of progress due to the unpredictability should not be seen or measured as failure.

Systems approach

It is customary in modern research management to view RD&E as a unit and project execution along a ‘systems’ approach. Despite obvious advantages of this approach, it often fails to recognise the most obvious specific shortfall(s) in the value chain. This practice can unnecessarily inflate the cost of R, more often than not, complicate, and slow project progress.

Similarly, as teams operate under consensus the approach tends to favour the lowest common denominator. Consensus and innovation tend to be opposing objectives, as innovation requires thinking and an ‘outside the box’ mindset. Consequently, innovation can be stifled using this approach.

Consensus aims at pleasing everyone and mostly leads to the dilution of good ideas because of add-ons, addendums, and one-off extras. In principle, brainstorming can enhance but the reality is that too many cooks can lead to an ineffective middle ground. Well-intended efforts to reach consensus invariably please no-one. Somehow the group agrees, but they disconnect the intended goal and fail to produce meaningful innovation (Gandolf 2016).
Project selection

Significant amounts of energy and resources in R&D businesses are spent on developing systems and processes for the selection of R&D projects. Generally, the aim is to evaluate existing projects or project proposals to recommend support, and rejection or downscaling of the rest (Bordley 1998 and references therein).

This ‘analytical decision’ project-selection methodology usually is based on two criteria, neither of which is really well suited for early stage, highly innovative projects. Firstly, an attempt is made to quantify the probability of a project being technically and commercially successful (using subjective estimates from the technologists and the potential implementers). Secondly, R&D institutions try to quantify the benefits of the project if it was successful (based on estimates from panel members). In some extreme cases attempts might be made to calculate benefit-costs ratios, which is even more inexplicable for early and mid-stage stage projects in the pipeline.

At best, in a project selection system the business chooses the best of the project offering. Much more emphasis should be placed on first deciding what is needed and then figuring out how to get it (Keeney 1992). This approach is value-focused starting at the best and working to make it a reality. It is a project development, rather than project selection process.

People management

Often overlooked and ignored by all is that any RD& E environment is occupied by individuals from professions with a very different code of practice (Table 1).

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<th>Table 1. Code of practice and decision making processes followed by management and research.</th>
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<td>Behaviour</td>
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<td>Code of Practice</td>
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<td>Will it result in producing practical solutions</td>
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<tr>
<td>Solutions by consensus among participants</td>
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<tr>
<td>Decision making</td>
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<td>Rapid and often intuitive decision making</td>
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<td>Assumption simple and aversion to scientific knowledge pointing</td>
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Management is largely driven by a consideration of what is relevant or irrelevant for achieving set targets and delivering on clients’ expectations. In this dynamic space, decisions need to be made quickly and often without the desired level of scientific evidence. Decisions are mostly taken on a consensus view.

In contrast, scientists are driven by a search for the truth. The fundamental basis of the scientific method is that one can never arrive at the truth but, at best, find supporting evidence. Therefore, one’s findings/results unlock new questions and this leads to more experimentation. Managers dislike scientific observations pointing to the gaps in knowledge, as this generally paralyses decision making.

These two elements create a rigour-relevance gap (Kieser and Leiner, 2009). From a system theory perspective, social systems are self-referential or autopoetic, meaning that the communication elements of one system, such as science, cannot be authentically integrated into communication of another system, such as the system of a business organisation and management. Researchers and managers often find it difficult to work together and can at best irritate and provoke each other. However, sometimes irritations or provocations result in inspiration (for review Kieser and Leiner 2009).

In my opinion, these two different groupings are essential in a successful R&D business. However, it should be evident that these two different groupings will always create tension in the business and this needs to be appreciated, managed and exploited to be successful.
TECHNOLOGY LIFE CYCLES

Businesses, products and technologies usually follow an S-shaped life-cycle curve when outputs and efficiency are plotted over time (Fig. 4). Depending on the particular product or commodity the duration of a technology life cycle can vary significantly.

Each of these stages in the technology life cycle have indicative characteristics. During the initiation or introduction phase, there is new technology, significant product innovation and generally poor quality. During the growth phase, considerable standardisation around technology, design and product quality occurs. In the mature phase, technology and know-how become well diffused, the business is very cost efficient and attempts might be initiated to try and differentiate the product offering. The decline phase is characterised by little or no innovation, and an over-emphasis on cost reduction.

![Fig. 4. A typical product lifecycle. The initiation of a new product pipeline and breaking the production plateau are dependent on innovation.](image)

Many commodities and businesses are finding themselves in these mature or declining phases. Given the international production trends and efficiency of sugarcane and production, it would be fair to suggest that sugarcane grown for the purpose of sucrose alone finds itself at maturity or even declining.

By definition, this would imply that significant innovation would be required to break out of this conundrum. There is ample evidence to suggest that innovation is pivotal for the success to introduce new products (better value capture from the feedstock) or significant enhancements in quality, or reduction in the price of the feedstock.

INNOVATION

Successful businesses are led by CEOs and senior management who realise that innovation is pivotal for their success and, hence, ensure a culture that is supportive of it. In contrast, many fail because the management emphasis and business model stifle innovation.

Innovation is about embracing new technologies, improved methods, meeting changing customer demands or needs, and better systems and processes. Business innovation is only possible if supported by senior managers and the business leaders.

Innovation should not just be a catch phrase in the value statement of a business. Innovation relies on a constant evaluation of creativity by everyone all the time. It is not something that is available on demand. More often than not innovative ideas are born from informal discussions away from the normal work environment.
Senior management

One of the reasons why some companies are good at innovation is the extent to which senior management is involved in the creative process - or more accurately, are not involved. The secret to successful innovation seems to be to keep senior managers as far away from it as possible (Amble 2010).

Many senior managers have difficulty accepting the most fundamental natural law of innovation - the only certainty is uncertainty. For most managers, operational excellence striving for +90% defect-free performance is a key performance indicator. In operational excellence environments, managers are rewarded for stamping out variance. This is in stark contrast to R&D, which can result in 90% failure rates. With innovation, ambiguity and change are the dominant forces, and is inherently messy and inefficient.

Companies that are solely structured around minimising variation and accurate forecasting cannot be innovative. To be innovative, you must be willing to accept failure and inaccurate forecasting.

The experience is that many senior managers spend endless time trying to find reasons why something will not work rather than helping to design methodologies that will make innovative ideas work. This could be in part linked to an underlying protection of individual reputation.

Innovation and peer review

Peer review is an important quality-control tool in R&D. For managers that often lack the expertise to judge the quality of work in research projects, especially in the early stages where no product or application can be measured, peer review is the most valuable tool. Hence, there should be encouragement for scientists to publish their research results in high-standing scientific journals. Measuring success rates are valuable to judge the quality of research projects.

However, it is important to differentiate between the values of the peer review of completed work versus grant application or project development (Spier 2002). There are many examples where papers, initially rejected by one or more journals, eventually make it to press and then radically changed the way we think and operate.

Without doubt peer review at the proposal stage is not well suited for truly innovative investigations (Spier 2002). If it is at the grant-proposal stage, the rejection may kill the idea forever (Rennie 2003). An editorial in Nature suggests that monolithic companies are oppressive environments for ambitious and innovative young researchers.

Innovation and project management

R&D, innovation, and indeed all creative activities can be managed using some components of project management methodology but it is key to take into account the elements of risk and unpredictability (Wingate 2015). Regardless of the level of certainty associated with R&D goals, the application of project management, enhanced by complementary disciplines, provides a powerful toolset to manage activities outside of what is considered a traditional project.

The relationship between project management and innovation is best represented by an inverted U-curve (Kavanagh and Naughton 2009). Increasing levels of project management are correlated to increasing levels of innovation up to a point after which innovation decreases significantly. As innovation is both the exploitation of existing knowledge and exploration in search of new knowledge, formal project management approaches should facilitate the former and hinder the latter.

A rigid project management approach locks team members into simply doing their jobs and nothing more. The result of this is that there is no time or opportunity to consider problems or solutions from different angles. A project schedule that prescribes every task that needs to be accomplished during the project may be great for estimating project timelines, but it leaves no room for creativity and innovation.

Project management at its core should be about doing things faster, better, cheaper. Project managers, with their penchant for detail, tend to optimise the process, rather than the result. Innovation requires continuous change, and often some exploration ‘look see’ before planning is even possible.
A case study

There are many examples but for illustration I am using the career of John Franz and the discovery of glyphosate to show how subtle the difference between environments encouraging or stifling of innovation can be. Even between divisions or departments in the same business, significant differences in innovation, culture can exist (Wikipedia 2016).

John Franz worked for Monsanto from 1955 focussing on new polymer synthesis, and the development of plasticisers and polymer flame-retardants. He transferred to the Agricultural Division of Monsanto in 1967, motivated by the department’s “emphasis on publishing, academic contacts, and the freedom to pursue ideas”. Because his background was in organic chemistry he studied and familiarised himself with the new field of plant physiology and biochemistry before beginning research. After many failures, but being allowed to pursue his ideas, he discovered the herbicide glyphosate and received $5 for his first patent from Monsanto. From 1960 to 1988, he received over 840 patents globally, including approximately 50 in the United States.

The important learnings from this are that he went into a new field of study without any baggage, and was allowed to explore ideas and continue despite earlier findings.

CONCLUDING REMARKS

Effective R&D is at the core of ensuring sustainable sugarcane production. The lack of continued increases in productivity, and in many cases even a decline should be a concern of all key players. Undeniable is that failures in the way we manage and approach R&D have contributed to this phenomenon.

There is no evidence to suggest that the delivery of sugarcane R&D can be improved significantly by more control and better formalised research management. In fact, the international experience is that driving these processes has sometimes resulted in the opposite outcome.

It is timely for senior managers to evaluate and measure how well their institutions are setup to encourage and support innovation. A quick look at research portfolios will show whether there is adequate coverage of all elements in the technology pipeline.

It is time for us as senior R&D managers to ask whether we are:

- Understanding that consensus is a certain killer of innovation? Innovation is the ‘out of line’ idea and independent of seniority and often even field of expertise.
- Empowering the innovators, and creating space for them, and small innovative groups, to challenge the status quo? Protecting them from the dominance of the ‘no-variance’ operational excellence mindset.
- Creating a safe zone where experimentation failures are learning opportunities? Celebrate the learning that comes from trying rather than punish or penalise perceived failures, especially in early discovery work.
- Ensuring a healthy balance between the components of the technology funnel? Actively pursue partnerships and licencing agreements with the holders of potential step-change technologies.
- Recognising the significant differences in the code of conduct between technologists and managers? Respect the differences and harness the synergies that can flow from this diversity.
- Appreciating that people are key to our success? Provide them with a stimulating work environment where they can grow and develop. Give them the confidence that they are viewed differently from other resources in the business that are acquired, depreciated, and written off over time.

REFERENCES

Recherche, Développement et Vulgarisation dans l’industrie cannière : excès de gestion et déficience en résultats?

Résumé. En général, les améliorations de la productivité dans la canne à sucre ont été moins prononcées au cours de trois dernières décennies. En même temps, les coûts ont augmenté et les statistiques démontrent une baisse de rendement au niveau de la production mondiale. Comparé aux « années dorées » des nouvelles technologies et de l’amélioration du matériel génétique de la deuxième moitié du siècle précédent, il n’y a eu rien de plus qu’une optimisation des pratiques existantes durant les deux dernières décennies. Avec le développement moins rapide de nouvelles technologies, il n’est pas surprenant de constater que plusieurs industries cannières ont été plus attentives à la gestion de leurs institutions et développement et (RD&D), ainsi qu’à leurs investissements. L’effet de ce ralentissement a créé la perception qu’une mauvaise gestion des projets et programmes de recherche par les scientifiques était à la base du problème. Cela a mené à la mise en place de « vrais » gestionnaires pour gérer la RD&D comme si ce n’était rien d’autre qu’une activité « normale » de production et de vente à travers des modèles d’affaires bien établis. L’emphase a été surtout mise sur le choix et la gestion des projets et comment minimiser les risques. La recherche, particulièrement à la phase initiale, est une activité qui comporte beaucoup de risques et avec un fort taux d’échec. Les institutions réticentes à prendre des risques sont rapidement dépourvues de technologies innovantes. L’issue d’un projet de recherche de base ne pouvant pas être réellement prédéterminée, ce type de projet s’accommode difficilement des paramètres classiques de gestion tels que le budget, les indicateurs-clés et les échéances. En général, les gestionnaires préfèrent le développement (D) et la Vulgarisation au détriment de la recherche (R) en raison d’une meilleure prévisibilité et d’un taux d’échec perçu comme étant plus faible. Reconnaître que les chercheurs et les gestionnaires opèrent différemment et selon différents codes de conduite est la clé pour bien gérer la RD&D. Si cela n’est pas bien géré, des conflits éclateront entre les chercheurs et le reste de l’entreprise. Selon l’approche « systèmes », la RD&D est souvent vue comme une entité unique. Malgré les avantages évidents de cette approche, il lui arrive souvent de ne pas reconnaitre le(s) plus important(s) manquement(s) dans la chaîne de valeur. Cette pratique peut inutilement gonfler le coût du projet, ralentir son progrès et privilégier le consensus qui favorise le plus petit dénominateur commun ou les membres de l’équipe qui ont plus facilement la parole. Consensus et innovation ont tendance à avoir des objectifs opposés, vu que l’innovation exige une réflexion avec une mentalité « qui sort des sentiers battus ». En conséquence, cette approche peut étouffer l’innovation. Un examen par les pairs est un excellent moyen pour mesurer le progrès accompli au niveau des projets et leurs éventuelles sélections pour le développement. Elle ne convient pas pour la sélection de nouvelles idées innovatrices. Sans aucune amélioration évidente au niveau de l’offre de la technologie et son adoption, il est grand temps de se demander si l’approche adoptée actuellement permet d’atteindre les objectifs fixés. Pour répondre à cette interrogation, il est important de voir l’évolution globale des modèles RD&D et les tendances modernes dans des entreprises très innovantes. Plutôt que de tout faire pour que chaque projet de recherche pour le développement de la technologie offre un produit à la fin, plus d’emphase est requise pour créer un environnement novateur où tous les acteurs restent concentrés sur les principaux objectifs stratégiques et que tous les résultats de la recherche soient considérés comme des enseignements majeurs pour le déploiement futur.

Mots-clés: Innovation, ‘conduit’ de technologie, ‘portfolio’ des projets, codes de conduite
¿La I, D y E en caña de azúcar: sobre gestionada y de bajo rendimiento?

Resumen. En general, algunas mejoras en la productividad en caña de azúcar se han originado durante las últimas tres décadas. Al mismo tiempo, han aumentado los costos de producción, así como las estadísticas reflejan disminución de rendimientos a nivel mundial. En comparación con los 'años dorados' de las nuevas tecnologías y el mejoramiento del germoplasma en la segunda mitad del siglo pasado, muy poca optimización de las prácticas existentes ha emergido en las últimas dos décadas. Dada una lenta entrega de las nuevas tecnologías, no es de extrañar que muchas industrias han colocado más control sobre cómo gestionan sus instituciones de investigación y desarrollo e inversiones. El resultado de esta 'desaceleración' ha creado una percepción de que la mala gestión de proyectos de investigación y programas por los científicos es la base del problema. Esto ha llevado a la introducción de gerentes ‘reales’ con la subsecuente gerenciamento de la I&D como si tratara de una producción 'normal' y un negocio de ventas a través del conocido ‘modelos de negocio’. Se ha puesto énfasis en la selección y gestión de proyectos y al mismo tiempo minimizando riesgo. La Investigación, especialmente en la fase de descubrimiento, es un proceso de alto riesgo económico y una alta proporción de los proyectos fracasan. Las instituciones que tienen una baja aceptación por el riesgo, rápidamente abandonan los proyectos de innovación de la tecnología. Por ello, debido a la incapacidad de predecir los resultados de investigación, un proyecto de descubrimiento no puede acomodarse fácilmente a temas de gestión tales como: presupuestos rígidos, definición de hitos y plazos. Los gerentes de estas instituciones prefieren generalmente D y extensión (E) sobre I debido a la mayor previsibilidad y menor posibilidad de fracaso. La clave para una gestión adecuada de I&D es un reconocimiento de que los investigadores y gerentes operan bajo diferentes códigos de conducta. Si estos no son correctamente manejados, el conflicto entre los investigadores y el resto puede provocar el fracaso del negocio. Generalmente, la I, D & E se ha convertido como una unidad con enfoque de 'sistema'. A pesar de evidentes ventajas de este enfoque, a menudo no reconocen los déficit (s) más importantes en la cadena de valor. Esta práctica puede inflar innecesariamente el costo del proyecto, volviendo lento y dependiente del consenso que tiende a favorecer el común denominador mínimo o involucrar a más voces de los miembros del equipo. La innovación puede ser sofocada ya que el consenso y la innovación tienden a ser objetivos opuestos; debido a que la investigación requiere pensar con una mentalidad denominada ‘fuera de la caja’. Por ello, la revisión por pares es una gran herramienta para medir el progreso en los proyectos y seleccionar proyectos para el desarrollo. No es adecuado seleccionar nuevas ideas innovadoras sin que se tenga una mejora evidente en la entrega de tecnología y adopción; por ello, es oportuno preguntarse si los enfoques actuales están alcanzando sus objetivos. Para abordar esta cuestión es importante mirar la evolución global de los modelos de I&D y las tendencias modernas de las empresas altamente innovadoras. En vez de tratar de asegurar que cada proyecto de investigación que pase por el embudo de tecnología entregue un producto, es necesario un mayor énfasis para crear un entorno innovador, donde todos los participantes tengan un rol y enfocados en objetivos estratégicos claves; al mismo tiempo, todos los resultados de investigación se consideren clave para el aprendizaje para futuras propuestas.

Palabras clave: Innovación (I), canal de tecnología, portafolio de proyecto, código de conducta; Desarrollo (D), Extensión (E).