

Preface

In this century, the average life span of man is predicted to increase much beyond that already achieved in the last century. This change will have a major impact upon services provided by the state or purchased by individuals and includes for example, health care, education and pensions. Providing state-of-the art health care to all is, however, probably unachievable. One potentially positive way forward would be to fund translational research and bridge the gap between scientists (such as social, biological, environmental, population) and clinicians to translate discoveries in the “field” into effective sustainable interventions and treatments, reducing the burden of disease across all societies.

Prostate cancer is the commonest adenocarcinoma in men and an increasingly important international clinical problem. Mortality remains effectively unchanged in the past 100 years and advances in management since androgen deprivation therapy was introduced in the 1940’s have been few. This stands in marked contrast to the remarkable explosion of knowledge in the basic sciences. Why these contrasting successes should be so different is unclear but raises an important question, “What elements in the cycle of translation from bench to bedside are restricting the development of effective new diagnostic and therapeutic interventions for prostate cancer”? We suggest that there are several restriction points including lack of cross talk and networking, polarised approaches to research, territoriality, poor understanding of the natural history of the

disease and lack of funding/general support for translational research. These factors, along with the manner in which academic rewards and benefits are traditionally allocated appear to mitigate against collaboration.

This book is an attempt by like-minded individuals to address some of these challenges by bridging the gap between bench and bedside and help to overcome the restriction points. We have asked clinical and scientific colleagues to each explain their specialist areas for complementary colleagues — but specifically in a manner best suited to enhance understanding of often very complex areas. It is hoped that by improving knowledge outside our own individual areas of expertise, communication will be enhanced and collaborations increased to the ultimate benefit of patients.

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Bacon, Francis

The men of experiment are like the ant, they only collect and use; the reasoners resemble spiders, who make cobwebs out of their own substance. But the bee takes the middle course: it gathers its material from the flowers of the garden and field, but transforms and digests it by a power of its own. Not unlike this is the true business of philosophy (science); for it neither relies solely or chiefly on the powers of the mind, nor does it take the matter which it gathers from natural history and mechanical experiments and lay up in the memory whole, as it finds it, but lays it up in the

understanding altered and digested. Therefore, from a closer and purer league between these two faculties, the experimental and the rational (such as has never been made), much may be hoped.

Francis Bacon, *Novum Organum*, Liberal Arts Press, Inc., New York, p 93. (5)

Szent-Györgyi, Albert (1893-1984) b. Hungary

Basic research may seem very expensive. I am a well-paid scientist. My hourly wage is equal to that of a plumber, but sometimes my research remains barren of results for weeks, months or years and my conscience begins to bother me for wasting the taxpayer's money. But in reviewing my life's work, I have to think that the expense was not wasted. Basic research, to which we owe everything, is relatively very cheap when compared with other outlays of modern society. The other day I made a rough calculation which led me to the conclusion that if one were to add up all the money ever spent by man on basic research, one would find it to be just about equal to the money spent by the Pentagon this past year.

Albert Szent-Györgyi, *The Crazy Ape*, Grosset and Dunlap, New York, 1971, p 72.