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### Jim's View: Patience vs Urgency

In the previous two essays in this series, I chronicled the extraordinary impact that the basic medical sciences have had on the human condition over the past century, and attempted to identify the key ingredients that enabled scientists to produce such transformative outcomes. This third (and final) essay in this series will appraise the prospects and challenges for sustaining such productive basic sciences in current societal environments.

This is important, because in recent years there has been a growing emphasis on both translational research and on highly organized “top down” projects involving networks of multiple (and even many) laboratories designed to take advantage of new scalable data-gathering technologies. Unfortunately, these developments have come largely at the expense of individual investigator-initiated basic science, and now even threaten to overwhelm it. Certainly, we need both applied and large-scale science, but when overall resources are limited or declining, it is all the more important to articulate the value of fundamental research to the public – be it family, friends, disease interest groups, funding agencies or politicians.

To anticipate, it is my view that the major factor determining our global capacity to innovate in public health is how we balance the *patience* needed to establish and maintain the stable multi-generational cultures needed for great basic science against the *urgency* of short-term needs.

Scientists in the United States enjoyed generous, growing long-term support for basic science in the wake of World War II and the subsequent Cold War, permitting the dramatic successes just chronicled. However, the traditional American model of funding young scientists independently requires exponential budgetary growth, and this is no longer sustainable.

So far, the USA has failed to transition to a new steady-state model that retains the key ingredients for success. Instead, the USA is shifting from patience towards urgency: funding is increasingly top-down, targeted, directed at large projects, shorter term, and unstable. Meanwhile, investigator-initiated basic science projects are provided with sub-threshold budgets because of the desire to spread the remaining funds around, all the while maintaining the official fiction that the projects are fully funded. *Almost 80 years after the historic transplant of the culture of fundamental European science, will the historically pragmatic American host ultimately reject this vital graft?*

As we learned from the German history in the 1930s, it is far easier to destroy an established scientific culture than to build it in the first place. It took several decades for Germany in particular, and Europe in general, to recover momentum from the precipitous loss of scientists and scientific culture in the second world war era. Happily, with solid investments in individuals (rather than programs) Europe regained its position gradually if not explosively. However, chronic underfunding (including in the UK) with some notable exceptions (notably Germany and Switzerland) has limited the degree of success. On the positive side, the traditional European university structures offer some institutionalized sustenance by providing baseline stability through local governmental support, at least for the senior scientists.

Superimposed on this, EU-based funding models are increasingly important sources for research funding, and these often demand international collaborations for essentially political reasons. *Will the*

*positive development of trans-national funding schemes allow European science the intended benefits of scale; or will they potentially stifle innovation by forcing unnatural partnerships that squash individual creativity in favor of fashionable top-down translational programs?*

The most dramatic changes today are happening in China, where many young scientists trained in leading laboratories in the US and EU are repatriating to an environment enjoying an explosive increase in funding for research with a laudable emphasis on early independence. However, urgency seems to be decisively crushing patience in China. A short-term reward system based on current fashion (Cell/Nature/Science/impact factors) and an excessive focus on essentially applied or derivative research will likely retard the establishment of the kind of deep and stable culture that fosters truly fundamental science. *Will a society built on five-year plans muster the patience to develop a culture of scientists capable of true originality and innovation?*

Personally, I remain hugely optimistic about basic science and the unlimited power of the scientific method to improve the human condition. I am inspired by the words of Eduard Buchner, the founder of biochemistry, over a century ago in his Nobel lecture: “We must never let ourselves fall into thinking *“ignorabimus”* (“We shall never know”) but must have every confidence that the day will dawn when even those processes of life which are still a puzzle today will cease to be inaccessible to us natural scientists” (1).

## References

1. Eduard Buchner – Nobel Lecture. NobelPrize.org. Nobel Media AB 2019. Tue. 7 May 2019.  
<https://www.nobelprize.org/prizes/chemistry/1907/buchner/lecture/>