From PhD to PI

Bengt Westermark
The Swedish Cancer Society and Uppsala University
Excellent science is generated by excellent scientists working in an excellent environment
“”In order to get a big answer you need to ask a big question””

Peter Medawar, Nobel Laureate 1960
The work on viral DNA was particularly notable because it was the handicraft of Harold Varmus, who had joined me as a postdoctoral fellow in late 1970. Harold's arrival changed my life and career. Our relationship evolved rapidly to one of coequals, and the result was surely greater than the sum of the two parts. Together we decided to extend our interests beyond the problems of retroviral replication, to address the mystery of how Rous Sarcoma Virus transforms cells to neoplastic growth.
The brilliant biochemist who became a brilliant biologist

Stanley Cohen
My work throughout the years has been strongly influenced by my associates. Giuseppe Levi taught me the essential value of criticism in scientific work; Rita Levi-Montalcini helped me to determine my goals at an early stage; Salvador Luria introduced me to viruses; Herman Muller, at the University of Indiana taught me the significance of Genetics; Max Delbrück helped me understand the scientific method and the goals of biology, and Marguerite Vogt contributed to my knowledge of animal cell cultures. Perhaps more important than all this, the daily interaction through the years with a continuously changing group of young investigators shaped my work. For although I had general goals, the actual path followed by my research was pragmatically determined by what could be done at any given time, and my young collaborators were an essential part of this process. I always did as much as possible of the experimental work with my own hands, but in the later part of my research career this became progressively less feasible, both because the demand on my time increased and because the increasing technical sophistication and complexities of the experiments demanded a great deal of specialized skills.
What signifies an excellent research environment?

• Original ideas
• High quality research
• Publications in high impact journals
• Complementary groups
• Recruitment of the best scientists
• Open climate – encouraging, not repressive
• Openness, internally and externally
• Opportunities for young scientists
How to define excellent research according to ERC

• Important challenge at the frontier of the field
• Suitably ambitious objectives, substantially beyond the current state of the art
• Well conceived and organized
• Feasible approach
• Highly novel and/or unconventional methodologies whose risk is justified (high risk, high gain)
• Opens new and important horizons
<table>
<thead>
<tr>
<th>Category</th>
<th>Question</th>
<th>1</th>
<th>2</th>
<th>3</th>
<th>4</th>
<th>5</th>
<th>6</th>
<th>7</th>
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<tbody>
<tr>
<td>Relevance</td>
<td>Is the proposed research relevant to cancer? If successful, will the results contribute significantly to our understanding of cancer or fundamental biology relevant to cancer?</td>
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<td>Originality</td>
<td>Is the proposed research addressing new and interesting questions that we currently do not have answers to? Are the results of other groups working in this area likely to make this proposal redundant?</td>
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<td>Experimental Design</td>
<td>Is the proposed research project and the experiments described in the application designed appropriately to answer the questions being addressed?</td>
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<td>Applicant</td>
<td>Does the track record and scientific standing of the applicant(s), as reflected in their CV, give confidence that they will be able to carry out the proposed research project successfully?</td>
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<td>Achievability</td>
<td>Is the proposed research project achievable with the resources requested in the application and the other resources that the applicant(s) has indicated will be available?</td>
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Suitable ambitious...
Trends

- Omics: globalisation and large scale approaches regarding methods, material and scientists
- Whole genome sequencing of increasing numbers of tumor types
- Cancer epigenetics
- Subgrouping of tumors -> personalized medicine
- Experimental, high throughput (HT), systems for validation
- Orthotopic tumor models, particularly in the mouse
- Targeted drugs: R&D
- Development of HT bioassays for screening of chemical and RNA based libraries
- Cancer stem cells
- The tumor as a society
- Intervention in prevention and caring sciences
Scientists are trendy
The damped sinus curve of enthusiasm.

Trend
The Swedish Cancer Society career ladder

<table>
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<tr>
<th>Postdoc</th>
<th>Young investigator</th>
<th>Senior investigator</th>
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</thead>
<tbody>
<tr>
<td>Application, yrs after dissertation</td>
<td>0-3</td>
<td>0-7</td>
</tr>
<tr>
<td>Length of grant (yrs)</td>
<td>3</td>
<td>6</td>
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It helps if...

- you have made a successful postdoc abroad, or at least at another university, or at least in another department..
- you have a number first/last authorships on papers in high impact journals
- your PhD mentor is not on the author list on most of the post-dissertation publications…
- you have returned to research constellation, different from where you were brought up
- your research project is novel, excellent, doable…
Three keys to success

• Careful recruitment of group members (PhD students, postdocs etc)
• Collaboration
• Networking – with you as the spider…
What the reviewers say when they don’t like the project

Pedestrian

Karaoke

Kamikaze
Careless definitions

- Curiosity driven research (you do what triggers your mind)
- Hypothesis driven research (Formula 1…)
- Hypothesis generating research (e.g. fishing expedition)
- Technology driven research (“mindless sequencing”)
- Descriptive vs analytical research
What signifies an excellent application?

• A clear and novel aim
• Well thought-out methods, preferably novel
• A coherent story with a connecting thought
  Must not lose focus and become diffuse
• Sharp-wittedness, not amount, impresses

(My words fly up, my thoughts remain below: Words without thoughts never to heaven go)
Questions to be answered

• What do you want to do? Why?
• Why important?
• What is known already?
• How will you do it?
• What material?
• Why are you in particular suited for the task?
• Why is this something for the funding body?

• Who else will be involved?
• When?
• How much does it cost and why so much?
• What other sources do you have?
Worth considering

• Your application is the committee´s only instrument
• Don´t assume that the committee has the same qualification as you when it comes to your project – your description of the subject and the problem is important
• Your application is one of many
• The committee should sift out the best applications but happen to more look for flaws than merits
• Unclear points in the application lead to an unnecessary diversion of the discussion in the committee
Finally

Begin well in advance!

Ask for advice and comments from your peers and senior colleagues!

A not unusual situation: "Can you please look at my application? I must submit it tomorrow"
Thank you!