Making the Right Move to Senior Researcher: some challenges and hints

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I have been working on research in data management for the last 40 years. I like my job and my research institution (Inria, the French national research institute for computer science), which have offered me great opportunities to learn a lot, do good work, get to know smart and nice people and overall feel useful. However, since the early days of my mid-career, the research environment, including academia and industry, has certainly become more complex, making the move from junior (or pre-tenure) researcher to senior researcher quite challenging. Based on my experience, I review some of the main questions and challenges and give some hints on how to deal with them. I'll sometimes use stories and anecdotes to illustrate the point.

Let's start with a basic question: *why do you want to do* (*or keep doing*) *research*? This is an important question you should reflect on, since research requires major personal investment. I have heard many different answers, e.g., I want to make the world a better place, I like the freedom given by my job, it is fun (e.g., programming), I want to be the best, … Whatever your reason is, the motivation must be very strong and profound (not just guided by your ego), in particular, to deal with ups and downs in funding, projects and results. The question may also help you to consider careers outside of research. After all, if you survived as a junior researcher, you have developed proficiencies such as creativity, autonomy, rigor, reliability, communication, etc., which will be invaluable in many other domains.

The first challenge in rising to a senior researcher is the necessary shift from "woodcutter" to "forester". In the mid 1980s, as a junior researcher at Microelectronics and Computer Technology Corporation (Austin, Texas), during one of my yearly performance reviews, my boss said to me something like: "So far, you have been a great woodcutter, taking one problem after the other and giving it a nice solution. But it is now time to move on as a forester, looking at the overall forest, so you can choose the best trees by yourself." This was a great advice, which required a radical shift in the way to do research. A junior researcher is often given research directions to work on, with full control over the solutions and implementation details. The shift to becoming a forester requires working on problems for which you may not have all the knowledge and skills to solve. Thus, you will have to learn new scientific methods and move outside of your comfort zone. You should also work with experts from other fields, which requires much humility and patience as you realize they know so much more in their field.

The next challenge is to shape a research agenda. This is the hard part as a serious project typically involves several other researchers and students, so you will also have to turn into a team leader. You may also choose to work on several unrelated projects, which will be even harder as it may lead to dispersion of attention and effort. I've always preferred to focus on a single, ambitious (high-risk) project, perhaps decomposed into several smaller projects, as it yields more overall coherence and synergy between people working on different pieces. A good example is building a new database system (a big project), with smaller projects such as query engine, transaction manager and storage engine.

There are two approaches to come up with a project: reactive and proactive. The reactive approach, e.g., proposing a project to answer a call for funding, is easier as most of the burden of identifying the research directions may have been done by the people who wrote the call, but may not benefit from all your creative potential. The proactive approach is harder as you need to identify a real, challenging problem that you can solve. This may take time, talking to many people, e.g., close colleagues as well as people from applications and industry, to understand the domain and state of the art. For instance, to come up with my Zenith project on scientific data management, I spent much time talking to colleagues in machine learning and data mining, as well as to scientists, e.g., plant biologists, to understand

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their requirements in data management. The proactive approach yields more freedom than the reactive approach, as you are not bound by specific calls, and may have higher impact, e.g., by setting a completely new research direction. And you can either find generic calls for funding, or apply to specific calls by adapting the project to fit in.

To lead the team in charge of the project, you will need to develop management skills. This takes time as just taking a management class is not going to be enough. You will also have to learn how to deal with smart but very different people and have them work together as a unified body. Books and courses will help, but real experience will be critical. At the same time, you should be careful not to lose technical skills, which are useful to communicate with the other researchers, students and engineers who will do the actual work. One way to do this is to take your share in the project development, e.g., do programming, which may be challenging but also fun. In a world controlled by technology, technical skills will always be an asset to realizing your own vision and not to be fooled by techie woodcutters. Many years ago, I was teaching databases at a top engineering school in Paris (a so-called "grande école" in France). A student was complaining that it was too technical, which is true: databases is a very technical subject. So, I asked: "But aren't you supposed to learn engineering, which is quite technical?". And the student gave me that answer: "Yes, but I want to be a manager". Then, I continued with the advice I just gave you.

Having a great research project is a first step, but not enough as you will also need the right people with complementary expertise to help you along the way. Within your organization, collaborating with colleagues from other teams will be much more productive and more pleasant than competition. It will also save you much pain and time lost in all kinds of useless fights. Collaboration is also more and more international, leveraging diversity of ideas, practices and resources. International cooperation is also a good way to obtain funding, e.g., the European Commission is instrumental in supporting big projects in several countries, even outside Europe. Furthermore, some research institutions have very strong international programs, e.g., Inria's associated team program that funds tight collaboration with a foreign team for up to five years. To succeed in collaboration, you must develop a solid network of collaborators, both within and outside your organization, and keep nurturing it. This takes time and patience as you will have to learn to work with different people. In the long run, it is very rewarding and pleasant, and some collaborators become best friends.

Once you have the right project and the right people, you need to produce results, yet another major challenge. I advise producing high-quality papers, always favoring quality and long-term impact over quantity. Thus, it is best to avoid the LPU (least publishable unit) strategy. Not only does it generate just too many papers to get read and referenced, it is also counter-productive if you look for promotion at a serious research institution, where for instance, you will be asked to give your top 5 papers that will get read by the selection committee. Publishing impactful papers is hard and takes time, as extensive validation and often reproducible results are required. Producing software is also important, not only to validate the research results but to deliver artefacts that other researchers can use and build on, e.g., open-source software. Some highly successful projects even go the extra mile of creating a user community around the software, with much long-term impact. For instance, the Ingres DBMS project at UC Berkeley in the mid-1970s has been the basis for the PostgreSQL community that has thousands of developers today. Another example is the Pl@nNet platform for plant identification developed in my Zenith team with plant scientists from other French institutions (CIRAD, INRAe and IRD), which has millions of mobile phone end-users world-wide.

A final challenge is to be a role model for people around you, which means behaving ethically. The ACM Code of Ethics and Professional Conduct is an excellent guide for ethical decision-making. In a world that is more and more controlled by algorithms and data science, it is important to act responsibly, with a good understanding of the impact of our work on people's lives and planet Earth. This raises many questions on how to make algorithmic decision-making fair, accountable and transparent (FAT), as discussed at the VLDB 2018 panel on data and algorithmic ethics.

Let me end with the common issue of searching for the elusive perfect place to do research. I have met many

researchers during my career who, even though they were relatively happy with their job and organization, were always looking for the next better place. Of course, there are many good places, both in academia and industry, and doing the right move at the right time, e.g., at the end of a project, should be quite beneficial for your career. However, searching for the perfect place to work may be endless and illusive. Let me illustrate with this nice tale. There was an old man sitting at the entrance of a city. A stranger comes and asks: “I have never been to this city; what are the people who live here like?” The old man replies: “How were the people in the city you came from?” The stranger says: “Selfish and mean. That's why I left.” The old man continues: “You will find the same here.” A little later, another stranger approaches and asks: “I have just arrived, tell me what are the people who live in this city like.” The old man replies: “How were the people in the city you came from?” The stranger says: “They were good and welcoming. I had many friends.” The old man says: “You will find the same here.” Someone who watched the scene asks: “How can you give two completely different answers to the same question?” The old man replies: “Well, each of us carries its own universe.” Thus, you may want to consider changing the way you look at your current situation, before considering moving to the next place.