

## ADVICE TO MID-CAREER RESEARCHERS

*We are starting a new series to provide advice to mid-career researchers. There are a number of programs that SIGMOD organizes for researchers at the beginning of their careers (PhD Symposium and the like) and senior people do not (or should not) need much help. There are considerable challenges for those who are about to transition from an early researcher to a more senior role. In academia, these are people who are about to get tenured that comes with starting to think of moving from shorter-term research objectives to longer-term ones. In industrial research, this corresponds to the transition from participating in projects to initiating and leading them. As a community we don't seem to talk about these challenges much. That is the gap this series attempts to fill. We will get the views of senior researchers from diverse backgrounds and diverse geographies. We will continue as long as we find original advice and the views are not repetitions.*

*M. Tamer Özsu  
University of Waterloo*

### Mid-Career Researcher, huh? What just Changed?

Laks V.S. Lakshmanan, University of British Columbia, Vancouver, BC, Canada

You just got promoted to Associate Professor. Like most things in life, whether joys or sorrows, the joy of this accomplishment will not last forever. However, that doesn't mean that you should not look back and reflect on years of hard work and tenacity that you have put in which have earned you this promotion, so first of all, congratulations! Take a moment to savor this accomplishment. On the other hand, it would be a mistake to not ask the question, what just changed about me. Let's see. You now have tenure and you have been promoted to a senior rank. In one sense, that translates to less stress, but in another, you do have to wonder whether it necessarily does mean less stress. On the flip side, you should also take advantage of the opportunity to ask, what are some new freedoms I have just earned. The stress component is driven by partly knowing, but also partly being unsure of, the expectations from a newly minted Associate Professor. The freedom component stems from knowing that you are now tenured, which hopefully means that you can embark on more daring, high risk projects, even if you don't feel like you know quite how to negotiate the trade-off between risk and impact.

#### Research Strategy

##### *Depth over Quantity*

One of the questions to consider is whether you should change your research strategy. How critical is it for you to choose bite-sized problems that lend themselves to clean technical solutions with a relatively quick

turnaround that is appropriate for the next SIGMOD or VLDB deadline? You have no doubt heard that with becoming a senior researcher comes an expectation that your choice of problems and projects place depth and impact over quantity of publications. But what does it mean concretely, in terms of how to align your choice of projects and problems with the major deadlines that you value? It can be daunting to make a sudden transition where you seemingly don't care about the next deadline and are instead focusing on a "long term vision". The truth is you cannot, and in fact, don't want to, *ignore* those deadlines, not for too long anyway. What I have found worked well for me is diversification of projects – some focusing on short-term research output and some on longer term goals. You end up sending fewer submissions to immediate deadlines, strategically splitting your time between your immediate and long-term priorities. This may not necessarily work for everyone. For example, you may have a long stream of pending projects from your pre-tenure efforts (and this is a good thing!) and taking them to fruition may be your top priority. In that case, you may want to ease into a diversification-based solution more gradually over a period of time. It is important to be not overly caught up with a "publish a lot or perish" mindset and instead ask what are some exciting things you learned from the new project (which may be completed or in progress) and how that can make a difference to something or some area. Be patient with yourself and have faith in your ability. After all, you didn't make it this far by accident. It can be helpful to

talk to people – including your senior colleagues in the department, your letter writers, as well as senior researchers you met through conferences and other events. Talking with people with various levels of experience can not only be a source of ideas for how to manage the transition to senior researcher but even open new doors for collaboration.

### ***How many areas at a time?***

A second, but related question has to do with focus and patience. Achieving impact does not happen quickly and certainly not with one or even a small number of publications, unless you are lucky. It often takes persistent, focused, and sustained effort lasting several years to generate impact and visibility for a theme of research you have been devoting your energies to. Does this mean you want to begin by focusing on *exactly one area*, for let's face it, time is finite. And in that finite window, as a senior faculty, your department may have just started to reward you with additional responsibilities, including leadership positions in certain areas of teaching and/or service. In addition, new reviewing and/or editorial responsibilities may have started to land on your lap or soon will. As such, your time is seriously constrained. You have to make a tough decision between choosing one area to focus on and choosing several, possibly at the expense of depth and impact. Or so it seems. For some, working on one area with laser focus for the next  $k$  years is perhaps the right choice. I have found once again that diversification with some finetuning worked well for me. Specifically, choosing a small (e.g., 2-3) number of areas, keeping one of them as the “central” area and devoting a majority of my research time to that area with enough left over which can be meaningfully invested in the other areas has been a rewarding experience. For me, when I got tenured and promoted, this meant keeping Query Optimization for Datalog (aka deductive databases) as the central area with some non-trivial investment in other areas including logic programming, object-oriented databases, and managing data with various forms of imperfection such as incompleteness and uncertainty. The initial part of such journey can feel like one is visiting different islands on different days (or weeks, depending on the frequency with which you meet with your graduate students) as it did for me. I have always been fascinated with mathematical logic: logic is what drew me to computer science in the first place.

Before long this led me down the path of seeking extensions to Datalog endowed with features capable of addressing the aforementioned functionalities. In sum, during the initial part of the transition to senior researcher, the model that worked well for me was to deliberately divvy up my time between one clear central area and a few other carefully chosen “satellite” areas.

If you are one of those who enjoys diversity, not just in the set of people we interact and transact with, advise, and mentor, but in the set of areas that you focus on, then the next natural question is how do you choose the set? I will not presume to provide you with a prescription but would like to share with you the principles that have guided my choice. There are at least the following pressures acting on this question and not everyone will care about each of these questions equally. Which areas are “hot” in the major venues you care about? Somewhat correlated with that, which areas and skills are valued most by the industry? Which areas really appeal to you on a personal level? The answer to the first question can be helpful in strategizing your choice in such a way that you have a reasonable chance of success at publishing your (and your students’) research in the short term. I am *not* arguing that you should choose trendy topics. From time to time going against the grain can be rewarding, if somewhat challenging. However, ignoring current trends can be a costly mistake. You certainly want to be well informed of trends in current research and align at least *aspects* of your research in your chosen research areas with current trends to the extent possible and meaningful. An example can be helpful. In the nineties, as a “young” senior researcher, when I was looking for areas to diversify into, one of the “hot” areas that caught my attention was federated databases (variously referred to as multidatabases, database interoperability, and heterogeneous databases). Given my fascination for logic, I was looking for a set of problems in an area that would lend themselves to elegant and effective solution via logic abstractions. This led me down the path of developing algebraic and logical query languages capable of querying collections of databases with autonomously developed, and therefore heterogeneous schemas. Patience, hard work, wonderful graduate students, and collaboration with awesome colleagues were instrumental in shaping a multi-year journey within this area which led to several impactful publications. I did not always make the right decision.

As a case in point, thanks to a fun collaboration, I was fortunate enough to publish, what in hindsight, is an influential paper on probabilistic data management (ProbView) in 1997. As it turned out, we were well ahead of the pack that rode the subsequent wave of interest in probabilistic data management. There was limited immediate uptake of our research and in fact whenever I explained this research to our colleagues, many reacted it with “Why do we need to worry about probabilistic data? Which industry strength applications warrant it?” I wish I had had some patience and persisted with that area to develop the applications more fully. As such, we ended up working instead on theoretical aspects of query optimization over probabilistic (and deductive) databases, which was not a bad outcome from that investment. However, our contributions could have been broader, richer, and more impactful had I persisted with our original vision for probabilistic data management. An even stronger example of a bad decision where I moved on too quickly for there to be any impact is data management of spreadsheets. We had a paper on declarative querying of spreadsheets based on abstractions we had developed, which was published, again, well before its time came! I wish I had had the clairvoyance to anticipate just how important that topic would become in the future. The bottom line is, do not worry about making such mistakes. You will survive them. I certainly did. There are plenty of opportunities to achieve depth and impact throughout your career.

### **Supervision and Mentoring**

By now, you are a seasoned supervisor of students. Pre-tenure, you may have been mostly focused on accepting graduate students primarily on the basis of excellence and match, perhaps paying little attention to other aspects such as diversity. Diversity not just with respect to underrepresented groups but also differences in abilities. I am of course not arguing against going after excellence, but from time to time, helping students at various levels of accomplishment excel and meet their full potential can be a rewarding experience. To give two examples, I once recruited someone with relatively poor grades in their Bachelors. In fairness, I had one strong recommendation from someone I knew and trusted. That student went on to do an outstanding PhD with several strong publications. In the second example, a student who spoke halting and hard to understand

English approached me for doing his Masters thesis. For those who don't know, in Canada, we have thesis Masters, which is by far the most popular Masters option. I reluctantly agreed. It was the time when I had just moved to UBC and was in the process of building up my group. In hindsight, I almost missed that student. He went on to do an outstanding Masters thesis: not only did he publish a well cited VLDB paper, he was one of those rare cases to be hired directly into an industry research position without a PhD. He has continued to flourish in research. Having made the case for diversity, let me say, don't go after diversity because you can brag about it or because of political correctness or its appeal to funding agencies. Do it *if* you are convinced that it's the right thing to do. For the longest time, I have been actively recruiting and fostering more minorities and women among my advisees. I feel that I (and we) still have a long way to go. One of the joys of doing research necessarily comes from the social process of conducting research with colleagues and students. What better way to make it even more fun by boosting your group's diversity and inclusiveness?

In addition to just supervising students and postdocs, you will be expected to mentor junior colleagues, both in your department and elsewhere. Try to be giving and generous with your time as much as possible. Mentoring a research colleague (faculty or industry researcher) can be just as rewarding as graduate supervision, but at a whole new level. Rather than look upon this as part of your new “duties”, look at it as an opportunity to give back. Remember the great advisors and mentors who played an influential role in your career and share the knowledge, enriched by your own experience. Don't forget that mentoring a junior faculty colleague includes not just research but teaching too.

### **Service**

You won't be surprised to hear that your department, university, and community will have higher expectations of service from you than they did before you got promoted. While most of us may not have a great appetite for administration, you cannot avoid it. Besides, we all have to step up to the plate at some point. Try to find a portfolio that you feel passionate about. I have always deeply cared about faculty recruiting and graduate affairs. Administrative service does take up

time, however the impact you will have had on your chosen portfolio may well make it worthwhile.

Expectations from your research community can take the form of serving in leadership roles in conference PCs, journal editorial boards, organizing new workshops, and launching new initiatives within well-established conferences, to name a few. As a rule, do not accept any invitation or start an initiative for which you are unsure about either your time or expertise, both of which are critical for successful delivery. It's yet another instance of quality over quantity: fewer well-delivered responsibilities go farther than many unremarkable stints.

### **Closing Remarks**

Find ways to expose your graduate students (and postdocs) to new and exciting opportunities, be they competitions, fellowships, or high-risk collaborative projects spanning multiple disciplines. Always look for ways to recognize their talent and accomplishments. Try to move students and junior colleagues up author lists, with a higher priority for students whenever possible and reasonable. Equally important, be there for them when they face setbacks in research or other fronts, even more in your senior avatar than before you got tenured. The value of your emotional support is immeasurable. Groom students and postdocs for writing high quality reviews, report their contributions to PCs, and later recommend them to PCs. Think you want to turn each of them into a version of you. Hopefully better. ☺