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TEACHING RESEARCH--THE SUCCESSFUL LEGACY OF RJK

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INTRODUCTION

Ray Krizek has had a profound and positive influence on my academic career. At the time I was his student, however, I was not sufficiently mature to realize that his approach to doctoral student supervision would be so beneficial. Working as part of "Team Krizek" was a big challenge, and yet it provided many opportunities that later proved to be very valuable to me, both as a post-doctoral researcher and as a professor. Dr. Krizek taught us how to write proposals, plan and carry out the work, and publish the results—in short, to do all the things necessary for a successful career in research and academia. We didn't know it at the time, and we certainly didn't call it mentoring, but that's what it was. Dr. Krizek was a very effective mentor for his doctoral students.

While the Krizek method may not work for all students and all professors, it appears to be more successful than the other common approaches to graduate student supervision and mentoring—especially for producing successful researchers and professors. In order to provide some perspective on the RJK method, other approaches to dissertation supervision are described below along with their advantages and disadvantages.

Many times, Ray Krizek would tell us students what it takes to succeed as an academic; his ideas were often illustrated by examples of current and past students as well as his own experiences. Five pieces of advice that I particularly remember are given in the paper. Finally, I close with a few remarks about the legacy of RJK.

DIFFERENT APPROACHES TO THE SUPERVISION OF DOCTORAL STUDENTS

There are a number of ways professors supervise their doctoral students. Although I suspect that most of us would deny that this applies to us, the approach usually taken seems to be more a reflection of the professor's personality and style than any conscious attempt to find a procedure that is most appropriate for the individual student. If the student can adapt to the professor's style, then the PhD student will probably complete a dissertation successfully.

At the risk of overgeneralization or glaring omissions, the approaches to PhD supervision that I have observed in my academic career seem to fall into four categories: hand-holding, sink-or-swim, impossible dream, and the RJK method.

1. The "hand-holding" approach

This approach is common in big multi-year projects involving several student researchers. Because the work needs to be well organized and well coordinated, it may even involve post-doctoral researchers or other permanent departmental and research staff. In this case, the student is given a fairly detailed plan for her dissertation research. The original proposal was written by the professor or other former students and staff, and there rarely is any involvement of the student in establishing

the scope of work or the technical approach. The student only has to be moderately successful at doing what was proposed, write it up, and voila! A PhD.

In many cases, there are frequent, often weekly, meetings with the professor just to be sure things are on track. The meetings may involve others on the project team and may include members of the student's PhD advisory committee. Occasionally even the sponsor's technical
representative is involved in these meetings.

Proponents of "hand holding": Well, you probably know some…

Upside: if the student stays on track and does not have any serious technical difficulties, then she gets the degree with a minimum of pain and uncertainty. Research students learn to be good team players.

Downside: the fresh PhD does not directly learn about proposal writing, research planning, or how to work independently.

2. *The "sink or swim, hands-off, you are on your own" approach*

In this approach, the professor meets with the student and they both agree on a topic that should make a suitable thesis. Then the student attempts to do the research with minimum input from or interaction with the professor. Many times, the student gets frustrated with a lack of obvious progress and little positive feedback, and he may become "in absentia" and go get a real job.

Now, in some cases, absentia students do actually manage to complete a dissertation. They may be motivated by pressure from their spouses, employment contracts, university imposed time limits, etc. Then after years of little to no communication, the student suddenly appears with a thesis in hand and he expects/ hopes/demands that it be approved as is. What happens if it is not acceptable to the professor or to some of the committee members? What if the thesis needs substantial revisions, or worse additional research? What if the time limit is exceeded?

Proponents of "sink-or-swim": Famous Professors

Upside: for Famous Professors who are often very busy, there is a minimum of effort and time involved. For the student, IF he succeeds in writing and successfully defending a thesis, then he is really able to work on his own, generate his own ideas, and will likely become a successful academic. But no thanks to Famous Professor…

Downside: Especially with absentia students, the quality of the research is often poor, and graduation rates appear to be very low. The geotechnical world used to be littered with students of Famous Professors, students who were very bright and capable but who never quite finished and often were bitter about their experience with Famous Professor.

3. *The impossible dream*

It is tempting for both a professor and a student to try to solve one of the remaining big geotechnical problems, the solution of which would bring them both fame and fortune. These big problems require a lifetime of work devoted to the topic, and thus are unrealistic for a PhD dissertation. When the dissertation topic is so broad in scope, the student finds that after a few years' work, it is going to be impossible to ever solve the problem and complete an acceptable thesis.

I have not seen many "impossible dream" topics recently, although they were quite common in the early days of soil mechanics. (I think my own original thesis topic to relate shear strength to quantified clay fabric was an "impossible dream", even though it would have never brought us fame or fortune!)

I have seen a problem that is a corollary of the impossible dream. The thesis topic suggested by the professor and agreed to by the student is relevant, important, and considered reasonable and doable by both. But as the work progresses, major differences develop between the expectations of the professor and the ability of the student researcher to make "satisfactory progress" (defined by the professor, of course) on the topic. This disparity leads to conflicts between the professor and student researcher. The professor is disappointed at the inability of the student and the apparent lack of progress, and the student is continually frustrated by not being able to produce acceptable results.

4. The RJK method

The following comments are based on the experiences of me and my classmates during 1966-1970. I have no idea if Ray Krizek has continued to use this method with all his students, but in those days the RJK approach to doctoral supervision seemed be almost custom-designed for each student. Our individuality was recognized and respected, as were our different backgrounds, abilities, and technical interests. Students often were able to select their own dissertation research topics, although when a funded project was involved, more direction was obviously required. We often ended up in groups or teams working on related aspects of a research topic. For example, Wally Baker's Anisotropic Mohr-Coulomb study turned into work on clay fabric and shear strength by Don Sheeran and myself, and then to the theses by Tunch Edil, Salah Abdelhamid, and others.

Dr. Krizek provided us lots of opportunities to write proposals and to review and critique published papers. We were encouraged to write up any studies of a topic that might become a section or chapter in our thesis…or it might not; it didn't matter. Ray typically critiqued and edited these short papers, and then generously (in the pre-word processing era) had them typed up and duplicated for distribution to the other graduate students. In a few cases, if he felt a paper was potentially publishable, he would urge us to do the necessary additional work, suggest a "home" for the paper, and even help us with the submission process. His encouragement to work on something interesting and to write up our preliminary results was a good learning experience for those of us aiming for an academic career. (In my own case, my Northwestern-RJK experience was excellent preparation for a post-doctoral stint at the Swedish Geotechnical Institute and an assistant professorship at Purdue.)

One of the current buzzwords in engineering education is teamwork. Well, that is nothing new for the graduate students in soils at Northwestern, at least when I was a student. We considered ourselves to be part of the entire NU soil mechanics team—faculty, graduate students, and staff, such as Dominic and Hugo. As students, we were encouraged to participate along with the faculty in seminars (ours and departmental), Chicago Soil Mechanics Lecture Series, and Chicago ASCE soils group meetings. We all worked together, for example, on a big open house for the Midwestern geotechnical graduate programs in 1970. I always appreciated the fact that all the soils faculty—Professors Osterberg, Krizek, and Franklin—were faithful attendees at these events. I learned from them that, as long as I am town and no matter how busy I might be, always go to seminars, lectures, and even student presentations.

ADVICE FROM PROFESSOR KRIZEK TO ASPIRING ACADEMICS

When I came to Northwestern in the fall of 1966 to work on a PhD, Ray was still an Assistant Professor. Although he had only been on the faculty a couple of years, he already had well-developed ideas about research and academic life (perhaps nurtured during his teaching time at Maryland?) Discussions with him about technical problems often turned into conversations about what was necessary to have a successful academic career. He often used living examples—current students or recent graduates—to illustrate his points. (I often wondered what he told the other students about me!)

While I'm sorry I don't remember all of his advice, here are a few gems for those of us intending to be academics:

1. *It is "publish or perish", and be sure to also bring in research money*! Deans and Department chairs like to see lots of publications and a decent amount of outside funding from their young faculty.

Even in the late 1960s, this was one of the "rules" of the academic game. Ray told us: "If you don't like the rules, then don't play the game." There are plenty of other satisfying career paths available to bright, hard-working PhDs, and most even in teaching don't require publishing and chasing research money to be successful. But if you do take an academic position at a research university, don't complain later about the pressure to publish and bring in research funds.

2. Another piece of advice—difficult for me, an experimentalist weak in theory—was to *stay out of the lab and avoid doing experimental research*, especially early in your academic career. Labs are expensive, and department chairs don't like it because you are always asking them for more money for your lab, but yet you don't have much to show (e.g., publications) for all your hard work and their money. Because it takes valuable tenure time to build a lab and develop a laboratory research program, it is difficult to show published results during those early critical years.

3. Ray also told us, only half jokingly, how to treat experimental data so it would look better than it really was. Besides plotting the data on 10-cycle semi-log paper, these "tricks" included using larger-than-normal symbols around the data points

and then omitting the data points themselves after positioning these symbols close to the curve-fit line. I'm sure there were others that I've forgotten.

4. "*There is always a home for that paper.*" Even if it has been already rejected by one or more of our traditional journals, Ray was a master at finding a place to publish a paper. My favorite (and perhaps Ray's, too?) was the *Journal of the Franklin Institute*. Another journal I discovered while working in Sweden was *Archiwum Hydrotekniki* (published in Poland and always looking for papers in English). My Swedish favorite is *Acta Polytechnica Scandinavica*.

5. When you write a proposal, have some of the research work already done. Show "preliminary results" to demonstrate the likely viability of your approach. Otherwise, reviewers will always complain: "…but how do we know the proposed approach/technique/method/procedure… will really work? ("Well, that's why we want to do the research!")

6. Finally, Ray occasionally talked about the different graduate programs in the USA, describing their strengths and weaknesses. As I recall, he favored programs with the faculty having different technical interests so that graduate students would be exposed to traditional soil mechanics and foundation engineering, as well as the then newer areas of physical-chemical properties and rock mechanics. He assumed that groundwater and seepage, soil dynamics, engineering mechanics, applied mathematics, and geology would be a part of the traditional geotechnical engineering education. In smaller programs, a balance of faculty interests was considered to be especially important.

THE LEGACY OF RJK

The program of this Symposium, as well as Ray's publications, awards and prizes, and his election to the National Academy of Engineering all attest to his amazing record of technical and professional accomplishments. It is truly remarkable for anyone to make important contributions in so many different geotechnical areas. I for one am glad Ray did not follow Prof. Ray Yong's advice, offered in 1967 or 68, and paraphrased here: Ray Krizek works in too many different technical areas! If he really wants to make a name for himself in soil mechanics, then he should concentrate his efforts and those of his students on one or maybe two specialty areas, no more!

As you know, Ray was the second President of the Geo-Institute, and as a founding member of the G-I Board of Governors, he played a key role in its formation and early development. His leadership, vision, and attention to detail were crucial to whatever continuing success the G-I has had, and he set the performance standards for all future G-I presidents.

I wrote the following (slightly edited) comments in a letter to Ray 10 yr ago on the occasion of his 60th birthday. I hope you will agree that they are an appropriate closing.

> Ray, you have a very loyal group of students who have benefited greatly from having you as their major professor. You provided a marvelous atmosphere at Northwestern for personal and professional development. I learned how to think and work independently, and yet at the same time interact constructively with fellow students and other faculty. You also taught me how to write proposals and papers, do twenty-seven things at once, and keep a large number of graduate students reasonably happy and inspired.
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> I always felt you put the students' interests first, and I have tried to follow your model with my own students. You also taught us how to work very hard and get things done, and you showed us how to outwit bureaucracy and silly rules.
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> At this time, I know you're going to look back on your many technical and professional accomplishments, especially your contribution to the entire Department of Civil Engineering at Northwestern in the leadership role you have played as chair of the department. Many of those accomplishments are very tangible and can be easily counted.
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> But I also hope you will take pride in the accomplishments of your students, which often are less tangible and more a matter of inspiration and style. Thanks for being a good role model and a good friend.

Congratulations, Ray, on your 10th, 40th, 60th, and 70th anniversaries, and many happy returns!