

On being professorly

It is hard to be professorly in most disciplines: it requires the approval of a small group of practitioners who have commanded the respect of the public. Those who meet this group's criteria are praised; those who fail are considered unintelligent; those who do not try are considered ignorant. The hallmarks of such a group are the implicitness of its criteria, and its attitude of self-importance.

I offer as examples of professorly people two of my teachers, Judith Aissen and Jorge Hankamer, people who —go ahead, look them up— have certainly “gained the approval of a small group of practitioners who have commanded the respect of the public” . They have learned to speak approvingly or disapprovingly of students' work with broad terms like “excellent” , “insightful” , and “inadequate” , but they cannot provide criteria by which work can be termed so. They have learned to conduct what they call scientific research, presumably by imitation of others who are respected in their field, but they cannot explain their process so as to teach it to others, and they do not really know what they are doing, except that it meshes well with things they have done before and things they have seen others do.

For example, Judith Aissen said that the first half of my work for her was inadequate because it did not “engage the data” . However, she conceded that my argument was correct, despite not engaging the data, and that engaging the data would not improve my argument. She could not explain her position any further than to say that unless I followed her advice, I would not get far in the department. (In fact, my questioning on this point was treated with bemused condescension.) I followed her advice, and unsurprisingly the rest of my work for the quarter was termed excellent.

Beyond that, nothing in the way of methods or methodology was presented in her course. Rather, the lectures focussed on descriptive work coupled with invention of theoretical novelties. I got the impression that in her eyes, the best work is not that which efficiently solves problems, but that which invents these novelties as creatively as possible, in a rambling fashion that deals with as many issues as possible simultaneously. However, we were never taught how to be creative in this way; I imagine that in her eyes, one must be born with these abilities.

As another example, Jorge Hankamer said in his written evaluation that my work was half unsatisfactory, and half barely passable. His only direct criticism of my work was that I should use a particular notational device he preferred. All other criticism was an unspecific “This is completely unclear and I cannot understand this.” . Since the other students I showed my work to had no problems understanding my work, I conclude that these problems are particular to him. This wouldn't be a problem for me if following his way would result in some sort of objective improvement, but unfortunately he never explained why this was so. I prodded these issues in an email to him, but he never responded to it. In any case, he did not give any explanation of what constituted clarity in his eyes, or how it could be achieved. Presumably this is because he cannot formulate such an explanation.

Another complaint was that my work did the bare minimum to solve the problem, and nothing more. Perhaps I am naive, but I think this is a merit of scientific writing. He

never offered any explanation of why doing more than the bare minimum would be good, and never explained what beyond the bare minimum one should do. Presumably this is because he has no explicit criteria for what he is looking for. Similarly, about my final paper he wrote that I described a certain problem, but provided no solution. But his course only taught methods of describing data and problems, not of creating solutions.

I do not consider this education, and I do not consider this science. This view may surprise some readers, who only know of education and science in the manner I have described above. That is, they are used to being exposed to lots of examples, getting the gist of it, and then writing papers that hopefully meet their teachers' approval. When they don't, they usually just "try harder", and either get that approval eventually, or eventually give up and pursue something else. They do not expect to be told explicitly what is required of them, they do not expect to be told explicitly how to do science, and in fact the majority of professors I know consider it a sign of immaturity or lack of mental ability to want to know these things explicitly. They do not want to "baby" their students, and take the position that students either "get it" or don't.

For many years I was content with this system, but since I have learned the value of explicitness, that something isn't worth doing if you can't be explicit about it. Thus I strive for explicitness in every thing I do. I am proud to say that in the disciplines I feel comfortable teaching, I am able to explicitly and specifically provide methods of solving problems, and criteria for evaluating and improving those solutions. In linguistics, the field of Judith Aissen and Jorge Hankamer, I am not able to do this; but I also cannot bring myself to be condescending towards someone who cannot understand what I cannot explain. And this is why I have no respect for the professorly.

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Addendum (15.06.2005). I regretted not having a quotation demonstrating Judith Aissen's take on science. In the interim I came across a perfect specimen in an old email she sent me: "I think there is more room for play in these assignments than you suggest."

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