Choosing and pursuing unusual research topics

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One of the most important and maybe hardest decisions in one's career is to pick a topic on which to write a paper, and especially a job market paper. Should one write papers on many topics or rather focus on a select few? Should the topic be mainstream or can it be very unusual? Should the paper show off one's strength and capacity to do the "heavy lifting"? Should the paper be single-authored?

While these are good questions, there is not a single answer. I can only tell you about my experience. Before I tell you about my path let me say what it is I do: I work on experimental economics, most notably gender differences in competition. I also work on matching and market design. I have helped redesign the market for new Gatroenterologists, and helped introduce the signaling mechanism used by the American Economic Association for economists on the market.

My most important criterion is whether I enjoy the topic and work in the first place. Whatever we choose to work on, we spend a lot of time on it, and if we don't enjoy it, it may become painful, if not disastrous, since we can't focus on it. I cannot imagine working on something I do not care about. However, sometimes, the more I learn about a topic the more I get interested in it, so I try to keep an open mind to new ideas as much as possible.

As a graduate student, and still today, I was interested in understanding what gets people motivated and what drives phenomena we can observe in the world. I am especially interested in human interactions, what determines choices and what confines them. Because of that interest, I decided in my first year as a graduate student in Harvard to take a class in social psychology. I was very interested in behavioral economics, so I thought I better know what the psychologists have to say. This was very helpful in providing me with lots of ideas: Psychologists have their own questions and often very clever experimental designs (though often less clever theories). And while it is perfectly fine to receive research ideas from reading papers, I tend to get them through other channels. So, influenced by my psychology class, I found an idea.

Unfortunately, just because I find something interesting, doesn't always mean that others find it interesting. When we work on mainstream areas this may be an easy hurdle to pass. After all, if X papers are published on a topic in recent years, it can hardly be because no one finds it interesting. It may be harder to gauge the general interest with a new topic. A first thing to ask is: Is it important? Will the paper help change the way in which we understand a certain phenomenon? Will it have broader implications? A second important and useful tool, especially as graduate students: Talk to your advisors and your friends

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in other fields. My advisor was a bit skeptical about my job market paper idea, he thought I wouldn't find anything, though he was very supportive once I decided to work on it. I always tell this story to my students: I may not have the same intuition about what to expect, and often the research is only interesting if there is an effect. So, take your advisor's concern seriously, but in the end, it is your paper. I also talked a lot to my friends in other fields. Once I got my macroeconomics and international trade friends excited, I knew I had something interesting, something where most likely my audience wouldn't fall asleep.

So, I had my interesting if also maybe quite unusual research topic: An experiment on gender differences in competitive performance. As if being an experimental economist wasn't unusual enough…

Would an experiment on gender differences be able to turn into a good job market paper? A common advice is that a job market paper should show off one's skills, be in a way as technical as possible. As such, the "ideal" job market paper would be in a well-researched area, where it is clear that the contribution is difficult, and the additional insight can easily be judged. Unfortunately, this also often makes for sometimes quite boring job talks – unless one enjoys watching someone flex muscles… On the other hand, with an unusual research topic, and using unusual techniques, it becomes especially important to show in the analysis and execution as much "brain and spark" as possible, since it's harder to show off muscles. If a paper follows well-known research areas, assumptions may be easily justified if they are made by the whole literature. By having a job market paper on an experiment on gender differences in competition, I had to be prepared to convince my audience not only that I was following "industry" standards, but also that the whole approach of using experiments is interesting and appropriate.

When I saw job market presentations of my friends I was often a little humbled by all their muscle flexing. Here I was with my experiment that, once you have seen it, you could redo very easily. However, as I saw how interested people were, and how some control treatments would not have made it into the paper if others had tried to write a similar one, I could take comfort that, indeed, I knew a little something as well☺ The advantage of my job market paper, in return, was that I never got tired of presenting it. A lively audience makes for much more interesting talks.

So, in a way, the two approaches to job market papers (and research agendas in general) can be summarized the following way: You can either be a sprinter, showing your muscles and that in an area where many people follow similar paths you are able to outrun many of them. Or you can try to be someone who finds their own route through the economics-free wilderness, exploring new areas. The latter will keep your audience awake – though of course it's better to go exploring in interesting places. However, the latter will also make it more difficult to judge your skills, since many may not see through all the intricacies of your paper, or be able to judge the skills that you do show. It may also be that simple techniques are the right ones for the question at hand. You may also have to go the extra mile to justify your approach, why the area you are exploring is interesting, compared to the well-trodden path.

However, a single paper is not a research agenda. While I think that my job market paper would have generated substantial interest anyway, I feel interest increased because I continued working on it. After all, if I were not interested in pursuing this new line of work, how could I be sure others would be? And, more importantly, I still had many questions I wanted to pursue, and so I have been working on topics related to gender and competition since graduate school. I tested whether performance differences can account for differences in choices of compensation scheme (no) and the role of beliefs on relative ability (very important). Then I asked how these beliefs change. Why is it that women are less confident than men? And there are still many questions: Can we see the same phenomena in the field? Does the measure of “competitiveness” correlate with other traits and economic outcomes?

Pursuing such an unusual line of work that has been picked up by others has been very good for my career and also very rewarding. I really care to know how to help more women be good economists, for example, so that what I experience daily on the third floor in the economics department at Stanford, being almost surrounded by female faculty, shouldn’t remain as unusual as it is.

I have a more standard line of work as well, because at times I want to do things that aren’t that entrenched in the gender agenda. I work on market design, more specifically matching markets: I guess my “standard” topic turned out not to be that standard either. When I was on the job market, the second year courses I was happy to teach were Experimental Economics and Market Design. Most universities offered neither of these courses (which has luckily slowly been changing:) While market design isn’t mainstream, the questions I study are, or at least should be: How do people get their jobs? Where and on what terms? How does the selection mechanism work? What can be done to reduce certain inefficiencies? The field of Matching in Market Design has turned mainstream thanks to my advisor, Al Roth. He did it by continuously writing papers in that area, which for years, while well published, didn’t generate a large following. Eventually it took off and now it’s less unusual to work on matching. An unusual feature about that line of work, driven probably also largely by the fact how Al does his research, is that any research method goes: What is important is the question: theory, experiments, field data and simulations are all used and combined deliver much more powerful arguments. Every year, when we invite job market candidates, I get reminded how unusual it is for the work of economists to be centered around a question as opposed to a method. And every year I realize how lucky I am, that with my method being experimental economics, and experimenters being open minded, it is perfectly fine that only a bit more than half of my papers fall into that category.

So, coming up with a research area may be hard. I recently met a friend with whom I got my Master’s degree in Mathematics in Vienna and told her about my gender work. She said she wasn’t surprised, that I was always interested in why people did certain things, what made them choose to get a masters in math compared to one that only allows one to teach math in school (the choice that many of the few female students made). I certainly hadn’t expected to turn into an economist, and at times it is still funny to be a woman studying gender: how clichéd, in a way. But well, I am interested in the topic and who else should do it? As for my interest in matching, well, I had early on done some work on comparing market institutions. In my fourth year in graduate school, Al Roth and Paul Milgrom taught a market design course and since then I was hooked.

One aspect of my work that I find very important is that it is interesting not only to others in the same field. In fact, one of the main reasons why I stopped with math is that I wanted to be able to tell others what I do. Furthermore, especially the work on market design has found many real applications: I was actually able to participate in designing markets! I try to not just work in economics, but actually do it and apply my work to help others, which I find very rewarding.

This brings me to the last question I often receive: How about working with co-authors or advisors? Doesn’t one’s contribution get diluted? I have co-authored throughout my career: My job market paper is joint with Uri Gneezy and Aldo Rustichini, both accomplished economists when I was still just a graduate student. However, it is maybe not advisable to have a job market paper joint with your advisor. As long as the talk makes clear that the topic is (also) your research topic and you master it, I don't think it is a problem to have co-authors. Furthermore, I really enjoy working with others: it is more fun and the papers are often better. I was fortunate to have found really good coauthors. On gender I work a lot with Lise Vesterlund. And working with my advisor Al on matching was one of the most rewarding co-author experiences I have ever had. Working with your advisor, while it may be clear who does the leg work, can be a great learning experience, so I would advise you to do: I certainly enjoyed it. When people worry that their coauthor won't contribute enough I often tell them: Even if the other person only does a third of the work, it is still a third you don't have to do and the remaining two thirds may come easier as you always have someone you can bother to talk about your paper (though maybe it won't turn into a long-lasting co-authorship). Isn't it important to have a solo-authored paper? So far in my career I wrote only one, everything else is coauthored, though in different areas and with different people. It is important for me to talk to people and exchange ideas. And while some people worry their ideas may get stolen: well, it happens, very rarely though. However, talking with others, for me, definitely beats sitting alone in my office all the time.

Were there any surprises as I turned from being a graduate student to a faculty member? Teaching is fun, though sometimes more work than I expected. Maybe the biggest surprise was that I really enjoy talking to graduate students and working with them, they also make great coffee partners who are happy to listen to what I work on☺

In closing, as a final piece of advice: I guess anything goes, as long as you can justify that it is economics. Just make sure your heart is in it, so you have fun and enjoy the process. If you want to make your new area successful it may be important to stick to it (unless it's hopelessly dead). If it's not worth it to pursue it further for you, chances are it won't be worth it for others either. It is in this way that I wrote more papers on Gastro-economics than I ever thought I would: maybe I should have chosen my first matching question more carefully:) Finally, remember, as I once got told by my advisor: it is a marathon, not a sprint. And also, keep in mind: Write, rewrite and resubmit, never tire, never quit. Just make sure you have fun along the way!