My research strategy[1]

Peter Diamond[2]

A key question for any researcher is what to work on. Integral to answering is the method of search for an idea that might become a good paper. This is particularly an issue for many students starting on their theses. When I talk with students just starting out, or after a period without a successful start, I spell out multiple ways of getting started that I have used, rather than presenting a dryer, abstract list. Generally, students coming to me are trying to write theory papers, and it is my experience with getting started on theory papers that I relate in this essay.[3]

A key part of that strategic process, and also of the tactics of completing and presenting papers, is trying to figure out how interesting an actual result, or a conjectured result, might be. My movements across different research areas and between basic applied theory and policy analyses have elevated for me the ongoing importance of strategic planning. This essay reports my memory of how I proceeded strategically over the last 50 years, both before and after I recognized a need to think directly about these choices.[4] Over time I have become aware of the diversity of research approaches that work at different times and for different people and the unevenness in the quality of advice I have given on this issue.[5] So this is one researcher's story, not one researcher's advice, a potted history from my memory of early conscious and not conscious choices.

[1] Forthcoming in *Eminent Economists II - Their Work and Life Philosophies*, Cambridge University Press, New York 2011, edited by Michael Szenberg and Lall Ramrattan.

[2] I am grateful for comments on drafts from Nick Barr, Angus Deaton, Avinash Dixit, Jim Poterba and Joel Yellin.

[3] Choosing research topics is only part of how one approaches one's professional life. And how one leads a professional life is only part of how one leads life. I have written about what I thought would be useful and interesting to readers, not other issues like the importance of taking responsibility and working hard at making one's department and university a good place to be a student and faculty member, and the critical importance of being part of and taking care of one's family.

[4] This essay is about picking topics. For more discussion of my thinking in writing various papers, see Giuseppe Moscarini and Randall Wright, "An Interview with Peter Diamond," Macroeconomic Dynamics (2007) 11:4:543-565.

[5] For example, I remember telling the young Bob Hall that Ricardian equivalence, which he had proved, was not interesting, a view I hold to this day. Bob's memory includes a reference I do not recall, as he emailed me: "Actually, you told me something much more pointed and intelligent: Ricardian equivalence

I started graduate school at MIT in mathematics in 1960. I was taking both math and economics classes as I tried to choose a field, having majored in math at Yale and also having enjoyed both economics classes (principles, intermediate theory and general equilibrium, taught by Gerard Debreu from the new Theory of Value) and a summer RA job with Tjalling Koopmans. Koopmans hired me to do math for him. He asked me to provide an example of a function with certain properties. Being lazy, or maybe just preferring more abstract thought, I produced a class of functions rather than grinding out a particular one. I had no more thoughts about my response than that I had done what was asked. But Koopmans found real interest in the class of functions, which I had not considered. And generously elevated me to a co-author of the 1964 Econometrica paper that followed. I have had the same experience on the other side, as a graduate assistant (Saku Aura) did an assigned calculation in a particular way that then led to results that were worthy of publication as a joint note in the AER, 2002. The general message, for me 50 years ago, and for communication to graduate students, is think about the interest in what you find (stumble over?). I have sometimes put this to students as thinking about what theorems, if true, would be interesting. I have no algorithm for telling what might be interesting. It seems to be an intuition that is built up out of reading and listening to what is well-received, and what isn't. And reactions to individual papers vary considerably with location, one of the reasons why it is important to spend time in several places as a student and a young researcher. I find no point in proving uninteresting theorems, although there can be interest in the methods of proof of an uninteresting theorem. That has just not been part of my set of interests. (I don't find a theorem interesting just because it is hard to prove.)

I liked the MIT economics classes better than the math classes. And I was better at them. I was not interested in pursuing more and more general settings for the same basic theorem, which I took to be the heart of the real variables class I took. So I became an

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was both well known and wrong. The first from Ned Phelps's 1965 book [Fiscal Neutrality Toward Economic Growth, New York: McGraw-Hill Book Co., 1965], which I dutifully consulted and found you to be right. The second from your own work, fresh at the time, in the OLG framework." Either way, there is no doubt that following my advice greatly reduced Bob's nevertheless very high citation level.

economist. One of my formative experiences as a student was a reading seminar, in the spring of my second year, that Frank Fisher gave to me and my classmate Steve Goldfeld. This was really a search for possible thesis topics. Each week we were assigned something to read followed by a discussion. The only assignment I remember now was Arrow's <u>Social Choice</u>. Frank told us there was a mistake in the proof and to find it. We did. But what mattered was that it highlighted a way to read – always looking for what might be wrong, preferably without losing sight of what is interesting in a paper. I attribute much credit to that seminar for what I have considered a very fruitful way to read.

The first two essays of my thesis came effortlessly. One drew on my work with Koopmans, framing the same infinite horizon choice problem differently in a way that led easily to theorems. That became my job market paper and was done, apart from polishing, early in the fall. Right after, I read the thesis of T N Srinivasan, with whom I had shared an office at Cowles and a drive across the country in 1960. I thought of a way to prove an additional theorem in his model, and had a second thesis essay done.

At that point I thought finding things to work on was easy. I was in for a nervous-making surprise, as months went by without a glimmer of a good idea for another chapter, an experience repeated, as mentioned below, during my first leave from teaching. Well into the spring, Bob Solow, my supervisor, suggested I read W. E. G. Salter's book on technical change and cost reductions. I recognized that the approach could be turned around into a growth model and managed to complete my third thesis essay just barely in time for a June, 1963 degree. Only when googling while drafting this essay did I learn anything about the book other than its being handed to me by Solow: "In 1960 Cambridge University Press published Salter's thesis as *Productivity and Technical Change*. M. M. Postan described it as 'one of the most elegant exercises . . . in the theory of investment and innovations to come out of post-war Britain'." (http://adbonline.anu.edu.au/biogs/A160198b.htm) Again, there was a research lesson in how to read – look for analyses that can be transferred, what is sometimes called intellectual arbitrage.[6] Of course, not all transfers are interesting.

I was interested in theory, both micro and macro, and chose public finance as my other main field. At my first job at Berkeley, I started out teaching all three. It was the year-long public finance class for economics majors that interested me most. What became a 1965 AER paper started out as a lecture in that class, as I tried to understand and convey the central issues in the analysis of the public debt, moving beyond the analyses of Franco Modigliani and James Buchanan. And my starting place in the initial optimal tax papers I wrote with Jim Mirrlees occurred, literally, in the class room in my first year at MIT, as I lectured on deadweight burdens and had the idea of minimizing them.[7] The process of going from my calculation of first order conditions for deadweight burden minimization and derivation of the optimality of aggregate efficiency in a one-consumer economy to a joint paper with Jim Mirrlees addressing a many person economy was relatively quick, as the results, in essentially their final form, were presented at US and European Econometric Society meetings in 1967.[8]

Not surprisingly, I am not one of the researchers who view the relationship from teaching to research as teaching solely interfering with research. Rather, as indicated, I have found teaching, both undergrad and grad, to be a prime stimulus to research. No doubt this is related to how I approach teaching. I have often used preparation and classroom time as an opportunity to develop my own approach to standard issues, or to push forward on topics that strike me as worth more development. When I prepare a handout, I am looking for a good way to present, which is often not a summary of some existing paper. With this approach there is a chance of discovering something new. Some

[6] A clear expression of arbitrage at work is the title of my 1972 AER paper with Menahem Yaari, "Implications of the Theory of Rationing for Consumer Choice Under Uncertainty."
[7] Modeling deadweight burdens in a way that led to this use was another example of a move from classroom preparation to a published paper (1974, Journal of Public Economics, with Dan McFadden).
[8] Jim was already thinking about optimal tax before we began collaborating. He had set an optimal tax question on the economics tripos in 1967.

students like this approach – seeing a piece of a research process. Many find it less satisfactory, and certainly harder to decipher.[9]

Indeed, the least productive research period I had was during my first leave, 1965-6, with very little teaching, at Churchill College, Cambridge. This was a repeat of the frustrating time I had in the midst of writing my thesis. Shortly before going to Cambridge, I formulated the model that became my 1967 AER paper on the stock market. This was an unusual process for me. I wanted to explore the allocation of resources in models of an economy with uncertainty and without the complete set of Arrow-Debreu markets. My plan was to write down different allocation processes until I came to one that would look interesting to explore in detail. The first one I tried led to the paper. Fruitlessly attempting to find results beyond the very special assumptions that resulted in constrained efficiency fed my frustration, and the very limited amount of teaching threw up no alternative ideas to draw me away.[10] While a teaching load can be so heavy as to interfere with doing research, I am very far from viewing the optimal amount of teaching to be zero even from the narrow perspective of research, not counting the pleasures of teaching.

To this point, my research choices were one-off, with no sense of having or needing a strategy. That changed in the early 70's as I was having trouble finding things as interesting as what I had done earlier. That led to two ventures. I started taking classes at Harvard Law School, planning to write on law and economics, hoping to find some question in that realm that would be a route to what I was really interested in – the importance for resource allocation that trade happens in real time, rather than in the all-at-once way of Arrow-Debreu theory. That is, I was hoping that thinking about a concrete legal problem would lead to modeling that captured a real time process and resulted in insights that would be more generally usable. I was hoping for a modeling approach different from sequential equilibrium, which somehow did not turn me on. I

[9] Since student grading of classes started, I have never once been judged as highly as my co-teachers, Amy Finkelstein, Jon Gruber or Jim Poterba.

[10] The impressive analyses of Oliver Hart and John Geanakoplos and Herakles Polemarchakis show how hard it was to make progress.

chose taking classes (including sitting exams) rather than just reading for a couple of reasons. I wanted to experience how lawyers learn to approach issues, to identify how I might set out differently; I like and believe in the merits of understanding a subject from its foundations; and I like taking classes.[11] While I did write a few papers in law and economics, the topics never turned into insights into the kind of dynamic allocation questions I was hoping to find my way into, and I moved on. While my work on labor market search equilibrium started with analysis of breach of contract in joint work with Eric Maskin (1979, Bell Journal), I don't think there was any significant link to the classroom study of contracts, but who knows what lurked in my subconscious.[12]

My second venture was to agree to serve on a panel headed by Bill Hsiao to examine whether Social Security finances were in as much trouble as was being claimed (Panel on Social Security Financing consulting to U.S. Senate Finance Committee, 1974-75). They were. This was the start of an interest in both basic theory and policy analysis of national pension systems that continues to this day. I found I liked doing policy. And I found that looking at policy questions fueled identification of good theory questions to model and analyze. As a public finance economist, I was naturally interested in policy (rather than becoming a public finance economist because I was so interested in policy), although that has reversed. And as a theorist more interested in constructing models to analyze questions than to getting new results in existing models, my taste ran to simplifications that seemed to preserve the important properties and so provide plausibly robust policy insights, an approach that fit with finding questions from involvement in policy discussions. My 1978 Journal of Public Economics paper with Jim Mirrlees (and several sequels) on how pension benefits should vary with the age at which they start came

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[11] I saw this first (post Ph. D.) when I attended David Freedman's graduate class on stochastic processes early in my time at Berkeley. David lectured on Brownian motion, leaving more general analyses to the TA. I do some of the same, covering simpler models in class, leaving the general theorems for the TA. I enjoyed the Harvard classes enough to take one a year for four years – property, contracts, torts and taxation. Of course taking taxation from Stanley Surrey was to improve my understanding of public finance, not to strike out in another direction. Years later I took another class there, on financial institutions.

[12] The underlying concern was with search equilibrium and the catalyzing event was reading Dale Mortensen, "Specific Capital, Bargaining, and Labor Turnover." The Bell Journal of Economics, Vol. 9, No. 2 (Autumn 1978). While search equilibrium does have trade that happens in real time, it was not the sort of model I started out law school hoping for, and was a natural extension of earlier work on search in the consumer market (1971, Journal of Economic Theory).

directly from wondering about that issue for Social Security as part of my time on the second Hsiao panel (Consultant Panel on Social Security of the Congressional Research Service, 1975-76). The particular motivating policy question made it natural to consider uncertainties in worker opportunities over time, which was (and is) a valuable question to ponder.

I stop the detailed autobiography here, as later experiences reinforced what I had learned earlier – that there are multiple ways of getting to interesting projects and it is important to have at least one.[13] I cite just two more experiences. Sharing the excitement of Eastern Europe's move from communism to capitalism, I asked Jeff Sachs for an opportunity to join in. He had me write about pension issues in Poland. Some Poles were considering imitating Chile, which led me to study Chilean pension reform, and prepared me well for the debate about Social Security privatization, that is still ongoing. Returning to teaching optimal income taxation in the 1990s, after many years of not teaching it, I decided to work out a simple example to help convey the insights from that quite complex literature. Another AER paper, 1998, followed from recognizing the potential in a classroom handout.

Beyond the activities that did lead to research, there are the ideas I chose not to follow up, which I offer to make clear that there are indeed choices. I asked Paul Samuelson for a suggestion for a thesis topic. I am guessing this was the spring of 1962. He suggested that I contrast English and Dutch auctions. Despite having studied game theory in the math department at Yale, I did not choose to follow up on this suggestion, not seeing why analyzing auctions would be interesting. In the late 60's, while working on uncertainty, I decided that incomplete markets per se was what I wanted to concentrate on, not the new

[13] On reading this, Nick Barr emailed that "At risk of tautology, there is also the strategy (largely the one I have implicitly adopted) of not having a strategy. A while back I was asked to write a short piece about Bill Phillips for the Oxford Dictionary of National Biography. As one of the authors, I got the editor's newsletters, and in one he mused about whether there were a small number of career patterns, just like there are a small number of love stories (boy gets girl, boy doesn't get girl, etc.). He went through a series of possible careers; when he got to the "unintended career", I got interested; he quoted Isaiah Berlin, who apparently said "I am like a taxi -- I have to be hailed". That rang true. It never occurred to me to work for the World Bank, but they asked me. It never occurred to me to write about pensions in China either. So presumably part of a strategy is the ability to benefit from and enjoy serendipity."

interest in drawing inferences from prices about asymmetric information. So another huge opportunity went by unexamined.

Teaching, working on policy questions, leaving subjects when diminishing returns appear to have set in, and returning to them with a fresh mind later have all served me well.[14] And there is also the elephant in the room of finding the right people to write joint papers with.

[14] Some of my colleagues might object to this conclusion in the absence of natural or controlled experiments.