The Integration of Search in Macroeconomics: Interviews with David Andolfatto, Peter Diamond and Monika Merz

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The Integration of Search in Macroeconomics: Interviews with David Andolfatto, Peter Diamond and Monika Merz [1]

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Abstract

Peter Diamond, Monika Merz, and David Andolfatto must all three be credited for having integrated a search perspective in macroeconomic theory. In a previous paper, "The Integration of Search in Macroeconomics: Two Alternative Paths", we set ourselves the task of analyzing and comparing their respective contributions. To support our study, we conducted interviews with them. The present paper is an edited transcript of these three interviews.

Keywords: Search and matching models, Diamond, Lucas, Andolfatto, Merz, Real Business Cycle models, Matching function, Unemployment

JEL codes: B21, B40, D83, E24, J64

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INTRODUCTION

Macroeconomics and labor economics are two fields which experienced a scientific revolution more or less in parallel. In the first case, the transformation implied a change in the object of study from unemployment to business fluctuations, accompanied with a radical methodological shift (De Vroey 2016). As a result, unemployment, in the Keynesian days the overarching *quaesitum* of macroeconomics, almost disappeared from macroeconomic theory. In the second case, the change involved a shift from a non-neoclassical, institutional and empiricist vision of the field to one based on neoclassical principles, in particular the equilibrium discipline, and on new trade technology assumptions. Such a situation cried out for some piecing together. Peter Diamond was the first economist setting himself such a task with the aim of thereby reviving Keynesian theory. He achieved it in a famous paper published in 1982 in the *Journal of Political Economy*, "Aggregate-Demand Management in Search Equilibrium" (Diamond 1982). About a decade later, Monika Merz in a *Journal of Monetary Economics* paper entitled "Search in the Labor Market and the Real Business Cycle" (Merz 1995), and David Andolfatto in an *American Economic Review* paper, entitled "Business Cycles and Labor-Market Search" (Andolfatto 1996), addressed it again, this time with the purpose of enriching RBC modeling through such an integration.

Our paper, "The Integration of Search in Macroeconomics: Two Alternative Paths" (Danthine and De Vroey 2016), provides an in-depth analytical comparison of these different attempts, which actually amounted to two as Andolfatto's and Merz's approaches were very close. In the process of revising our paper, it became clear to us that we had to interview the main protagonists of our story to comfort some of our claims and tie some loose ends. Since these interviews are instructive *per se*, we have decided to reproduce them in a separate paper.

Peter Diamond was an undergraduate at Yale and evolved in the circle of J. Tobin, A. W. Okun and W. Brainard. Afterwards he moved to M.I.T. for his Ph.D. He held a position at Berkeley before moving back to MIT, where he is now a professor emeritus. He has written very influential papers in public economics, especially on social security, but also on an array of different topics. While he could have earned a Nobel prize for his work in other fields, he was one of three recipients of the 2010 Nobel Prize for his contributions to the analysis of markets with frictions search frictions. He was interviewed by Michel De Vroey on May 28, 2015.

David Andolfatto graduated from the University of Western Ontario. His advisors were P. Howitt, G. McDonald, and S. Williamson. He has written influential papers in different subfields of macroeconomics. He is currently at the Saint-Louis Federal Reserve Bank. He was interviewed via Skype by Samuel Danthine on June 19, 2015.

Monika Merz earned her Ph.D. from Northwestern. She wrote her thesis under the supervision of L. Christiano, D. Mortensen, and M. Watson. She has written papers in macroeconomics and labor economics. She is currently full professor at the department of economics of the University of Vienna. She was interviewed via Skype by Samuel Danthine on September 4, 2015.

AN INTERVIEW WITH PETER DIAMOND

About the 1982 search externality paper [2]

MDV. Your 1982 search externality paper is considered as a contribution to macroeconomics and for that matter as one to Keynesian macro – it was included in the Mankiw/Romer New Keynesian Economics volume.[3] What led you to move from microeconomics to macroeconomics?

PD. I think you're asking the wrong question. I found search very difficult. It was a very different experience than the work I did in the sixties, where it was very clear how I set things up, and do things with it. Just for a little bit of autobiography, as I was an undergraduate math major. I took the principles and intermediate classes. And Yale of course was a Tobin school very much. Okun was there I spent the summer working for Koopmans. I was at coffee all the time with Okun, and Tobin and Brainard. I was a Keynesian from the beginning, let me just put it that way.

MDV. May I ask you what you mean by 'being a Keynesian?'

PD. That the Arrow-Debreu model doesn't have the ability to address business cycle questions. I'm sure you do know Lucas's essay 'My Keynesian Education'.[4] He says very clearly that there are phenomena that just do not fit with the paradigm he had at the time. So I started out with a very clear sense that, because Debreu was so stunningly clear, there was no getting bogged down in having to do the mathematics. In part, I was at the level needed in the fifties, it was more than adequate. So I was very sensitive to the inadequacies or the incompleteness of the Walrasian paradigm, and the Arrow-Debreu version in particular. The element that seemed to me the most important was the failure to really distinguish the future from the present. The literature that Grandmont summarized in his book on temporary equilibrium was an attempt to get at that, but it didn't work very well.[5]

Part of it is making Keynes dynamic, part of it is recognizing that there is something going on that Keynes is describing which is absent in the Arrow-Debreu model and the problem was to

[2] Diamond (1982).
[3] Mankiw and Romer (1991).
[4] Lucas (2004).
[5] Grandmont (1988).

figure out what and why. The first pass I had resulted in my paper on the stock market.[6] The way that paper got written was, I didn't have the stock market in mind, I didn't have anything concrete in mind. I had in mind the idea that with incomplete markets, the market mechanism wouldn't be determining resource allocation. So, the question I asked myself is, think of other models that have incomplete markets and in which you could analyze resource allocation. It wasn't, build a model to address any explicit economic questions. It was a theory-oriented paper. The other thing that was happening in the sixties was the analysis of tâtonnement and the general issue of how does the Walrasian allocation happen. At MIT, I was a student of Frank Fisher. Frank gave a reading course, just for me and the late Steve Goldfeld**.** We were second year students and it was really a reading course in interesting cutting-edge theory questions, ones that might give you an idea for a thesis. That was spring of 1962. So, what I got from Frank was the description of his image of what tâtonnement was about, which was to find a mechanism that will converge to the competitive equilibrium. Of course, in 62 this was before Scarf came up with a counterexample.

What struck me, and I don't know where it came from, was the idea that that was the wrong question. The right question is: if you had a plausible mechanism, which tâtonnement isn't, what would it converge to? That led that my first search paper (Diamond 1971) in which what search converged to became the center of the analysis. If you read the paper, it is all about the dynamics, and so for me the challenge was to set up tractable dynamics and then see where it went. So, that was the question I was asking from the beginning.

That paper was relatively easy to write. The problem I faced after that was that I wanted to get richer around equilibrium and I wanted to break away from the price setting version and I did a lot of wheel spinning until I settled in on the law and economics question. I actually spent several years taking classes at Harvard Law School, thinking to explore law and economics. I wrote a few papers, none of them matter, but it was really to expose myself to a different perspective, and different questions. Then, I realized that this was a question that was not easy yet tractable. There was an audience for the answer because there was a law and economics community. Eric [Maskin] who was an assistant professor here at the time – we were teaching public finance together – was willing to work on it with me.[7] Those three papers were the next step from the first one, just in terms of the dynamics of the resource allocation process. Eric chose not to continue working on the topic because he wanted to focus on the game theory subject he was doing and I continued doing partial equilibrium, which that was, of the labor market. But, all the time, what I wanted was something that would address the macro question, but I had no vehicle for doing it. I don't know how much earlier it started, but I spent a summer trying to find a model I could solve, and finally I found the right simplification. What I was trying to do with the 1982 paper, and the reason the labor

[6] Diamond (1967).
[7] Diamond and Maskin (1979,1981).

market is modeled as it is, is that I wanted to have a model that would show the basic Keynesian point. When Keynes attacks Pigou, it's around sticky wages not being an adequate explanation of the business cycle, it's aggregate demand. So, in order to completely show that you don't need a labor market problem to get something that would be Keynesian, I set up that model with no labor market.

MDV. So, I'm wrong in saying that you turned to become Keynesian, you were so already. What happened is that you turned to produce a Keynesian paper.

PD. Yes, and that was in the back of my mind.

MDV. That was in the back of your mind, nonetheless you accept that there is a turn in the type of paper.

P.D. Absolutely, there is absolutely a turn in the kind of paper, but the turn came from the theoretical breakthrough, of finding a way to address it.

MDV. Still, the motivation of the paper doesn't come out much when reading it.

PD. No, the focus on the writing was to make the model as transparent as possible and the connection to motivations, doesn't help in that. And then of course, I went back to the labor market in my work with Blanchard.[8]

MDV. So your dissatisfaction with Keynesian theory, was lack of micro foundations

PD. … very much so …

MDV. And the fact that the only way in which Keynesians had been able to translate Keynes's message was to assume wage rigidity.

PD. Right and I thought that was a mistake, as did Keynes.

MDV. In discussion of what is now called the Diamond, Mortensen, Pissarides model (e.g. Albrecht 2011), your 1982 article is presented as a special case of that model. I find such a characterization misleading because it sweeps the originality of the coconuts model under the rug. Do you agree with me?

PD. I'm not aware of that kind of writing, I haven't seen it. But I view my 82 paper as something different and something that did not generate a literature. What I had hoped would happen, didn't happen.

Business cycle theory

MDV. So, your Keynesian inclination was prior to your enrolment at MIT.

PD. Yes, and certainly my education at MIT was consistently Keynesian, but again on the research side, that was a time of large econometric models. The macro theory being taught

[8] Blanchard and Diamond (1989,1990, 1992).

was primarily growth theory, which I never thought of as relating to the business cycle issues. I thought of that as microeconomics done in aggregates. But there was no good business cycle theory among Keynesians.

MDV. You say that the 1982 paper was triggered by your preoccupation with the business cycle, but when looking at it, one sees nothing about business cycles in it.

P.D. The thrust of the paper was to show that you could have aggregate demand problems without sticky wages. I don't believe that sunspots are the driver, I've never believed that. After that paper was published, people asked me, 'Could you get a cycle?' My answer was that, of course, you could. The paper with Fudenberg was just to show what to me and to a number of other people was obvious.[9] In fact, it was a paper that I resisted spending the time writing, because I thought it was obvious from the earlier paper. I think it was Tobin who said to me more or less that it was obvious. Because once you have multiple equilibria, you can do all sorts of things and it was just to show you could put them together sort of like that.

MDV. Were you in contact with the people at Penn doing sunspots and self-fulfilling prophecies, Shell and Cass or Azariadis?

P. D. I would say yes and no. Shell has been a personal friend since he was on the faculty here at MIT, and so I was always aware of what he was doing, Cass again was a friend, not a close one like Shell was. Azariadis I had only a little contact with. So I was aware of them but we weren't interacting. There is a difference between what you learn reading a paper and when you need a conversation to fully understand. I felt I understood the relevance of those papers for what I was doing, and that didn't warrant extended conversations. Let me go back and repeat something I said which is relevant. I used sunspots because there was a theoretical vehicle for getting the model to behave a certain way. I never believed that it was realistic, while they kind of believed it. So we were in different places on the use of this mathematical mechanism. The specific point I try to make is that the advantage of simplicity is that it underlines your point and clarifies what it's role is, whereas if you were thinking about the economy more generally you're naturally be drawn to more complexity.

MDV. Have you ever thought about introducing heterogeneity in the search externality model?

PD. First let me say that heterogeneity and expectations is absolutely essential for understanding the capital market, both the capital market in the business cycle and the capital market in steady state. But again remember the point I'm trying to make, which is to take off the paraphernalia. Rational expectations are there precisely because I was making a point that there is still this issue and so if I had had heterogeneous expectations, it wouldn't be making the point. But back then the idea was to impact the profession. If I had tried to introduce

[9] Diamond and Fudenberg (1989).

heterogeneity it would have been, I think, counter-productive for the central point of that paper. However, heterogeneity is central to my current thinking and research agenda. I haven't started on yet, but I've been thinking about it. I've spent the last two springs in New York, dividing my time between NYU and the New York FED and I've written one paper with one of the New York Fed search labor economists and we have another one that will be coming out that will take another six months.[10]

Relation to Lucas

MDV. In your Wicksell Lectures (Diamond 1984) you seem to have wanted to present a model that would be a rival to Lucas's model. Is that true?

PD. I think yes is the answer. I had long been very aware of Lucas's work which is so well done, and very aware that my basic perception of the economy and his are different, and yes so there is always very much a sense of intellectual rivalry.

MDV. Can this difference in perception be captured by saying that you hold a Keynesian and he laissez faire vision?

PD. Yes.

MDV. In our paper [Danthine and De Vroey 2016] we write that, treading Keynes's footsteps, your aim is to model inefficiencies. Do you accept such an account or do you refuse it on the ground of being a positive scientist?

PD. Obviously, I'm a positive scientist in the sense that I believe in empirical confirmations, and all of that. But I think that for me a lot of the interesting economics is the connection to policy, and where there aren't inefficiencies, there isn't the same type of connections.

MDV. So, you don't protest?

PD. No, I don't protest. At the heart of macroeconomics there is the desire to understand inefficiencies and what can be done about them. And don't forget my primary teaching, my whole career is about public finance, so one of the first things you learn is that there is pigovian taxation and externalities.[11] That is a gem of an example of going to policy from recognizing, empirically recognizing, shortcomings.

MDV. There are also many similarities between Lucas and you…

PD. That's right. Well, we both believe in microfoundations.

MDV. Why didn't you pursue that rivalry?

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[10] Diamond and Şahin (2015).
[11] Diamond (1973).

PD. Because I didn't find a way to do something that was satisfying. You no doubt looked at my book, *On Time.*[12] What struck me when I completed it, was how interesting my discussion of time was in the micro half of the book, and how uninteresting my remarks were in the macro half of the book. That's because I had a much better conception about the market's work on the micro dimension than on the macro dimension. I have a quote from Marshall that I use in talks – I don't know whether I have ever used it in written down – where Marshall says, 'in good times firms are producing as much as they can, they're restricted only by their productive limits. In bad times they are always concerned about spoiling the market.' I use that quote because it says the organization of the economy is different in good times and bad times. We've used the term business cycle theory, repeatedly. I don't do business cycle theory, I never have, if by business cycle theory you mean the theory that explains the turning points. What I do is trying to explore a theory drawing the distinction between good times and bad times. If you think about Keynes, it's really what Keynes was doing. There are times when you need fiscal stimulus, or monetary stimulus and times when you don't.

MDV. What about Kydland and Prescott's way of taking up Lucas's model?

PD. I think I use the Marshall quote to say that, if you believe Marshall, the fundamental paradigm of Kydland and Prescott is misguided, because it is based based on the premise that there are these constants over time, which are the parameters in the DSGE model, and Marshall says no. In these models, there are shocks and there are reactions to shocks, but the underlying parameters of the elasticities of responses are the same. The problem that the representative agents or multiple agents are solving is the same problem in good times and bad times. And I read Marshall as saying, No, you've got to be modeling market interactions differently in good times and bad times. If that's correct, then the Kydland/Prescott agenda is a mistaken agenda.

MDV. If there is one mistaken agenda that has taken off, and a good one which is just virtual, what do we do theoretically?

PD. From the theoretical viewpoint, somebody will come along at some point, and put the pieces together. Who knows when? Probably somebody young, right?

MDV. According to you, are new Keynesian economists, like Gali or Woodford, going in that direction now?

PD. Again, it's the attraction first of sticky prices and wages, and secondly the adequacy of the modeling conception that goes under the term frictions. It is a friction piece. [By contrast] in Minsky's work you're seeing the legacy of nominal contracts. This means that firms behave differently in good times and in bad times. If you have a threat of bankruptcy, then

[12] Diamond (1994).

that becomes your dominant motivating factor and in good times you don't have that. I mean it fits with Marshall,

MDV. Minsky may be right but he was unable to construct a consistent model.

PD. Yes, there is no model.

MDV. You are a person who wants a model, don't you?

PD. Yes.

MDV. So you prefer to be mute when you have no model.

PD. Well, I'm not mute in the sense that if I were doing a keynote address, I might talk about the need to do that. I'm only mute on presenting models because I don't have a model.

MDV. What are the prospects of such models seeing the day of light?

PD. There is an ongoing search literature where people are doing the consumer market along with the labor market. And it may be the degree of complexity around that is such that you can't do it with theory the way I do it. There'll be rich computerized models that will be necessary to do that. Any Keynesian believes that the output market and the labor market are both key ingredients in the description of the economy in good times and in bad times. It's a simultaneous equations system and the difficulty is the complexity of getting both of them going. The trouble is that, if you need search elements in both markets, there is a limit to how simple you can be and therefore I don't know if the answer is a breakthrough in technique so you can handle both markets or the answer is that we have to be simulating. The advantage that Kydland and Prescott literature has is that they have defined things in terms of the variables we normally measure.

AN INTERVIEW WITH DAVID ANDOLFATTO

S.D. I want to talk to you about, your *American Economic Review* paper, "Business Cycles and Labor-Market Search".[13] Let's start with the context. You did your PhD at Western Ontario in the 1990s. Your advisors were Peter Howitt and Glenn. MacDonald. Can you talk about the state of the department and how did Minnesota, Rochester ideas influence Western?

D.A. I was a graduate student there in the 1980s, 1987 to 1991. I was aiming to have Bruce Smith as the third member of my committee, but he had departed Western by 1990. Steve Williamson arrived at about the same time and he graciously agreed to serve as the third member on my committee. My most lasting impression of the department at the time is the amount of intellectual energy that was packed into those walls. I've never been in a place with so much energy, intellectual energy. It wasn't always the most collegial place. Some of

[13] Andolfatto (1996).

the battles that took place there are legendary, but as a student I kept away from that stuff. And you can tell by the composition of my committee, it didn't respect any boundaries that existed between different groups. The Rochester and Minnesota schools had a significant influence on the place. The core "macro" courses at the time (they were labeled "dynamic general equilibrium theory" I and II) were taught by Jeremy Greenwood and Greg Huffman, respectively. Greenwood was from Rochester and Huffman from Minnesota, so they naturally infused the young grad students with "freshwater" macro. And then Peter Howitt returned from sabbatical leave from MIT in 1988 and he taught a second year topics macro course that I took, dealing with search theory. In fact, Peter has a very nice paper that I think is under-appreciated, called "Costly search and recruiting," which is a partial equilibrium set-up but which embeds practically all of the things that Pissarides was doing at the time.[14] Peter also taught us the 1985 Pissarides and the 1987 Pissarides models.[15] For me, these were eye-opening papers. I remember thinking hard about how I might combine these "saltwater" ideas with the "freshwater" tools I had learned the year before. It wasn't immediately obvious at the time how to do it. One problem was that my freshwater tools were developed for representative agent economies, while the search models naturally entailed a lot of heterogeneity. Getting all that to hang together in a DSGE model was not so obvious. I recall talking to Randy Wright many years later, and him saying to me that he and Ken Burdett and others had tried to do the same thing, but they just couldn't figure out how to get the search in the model of physical capital in a dynamic general equilibrium model. Thankfully, I didn't know Randy at the time because if I had, I think he would have convinced me that the task was impossible.

S.D. The whole problem of heterogeneity.

D.A. Yes. We didn't have the analytical or computational tools to handle that type of heterogeneity where the state of the economy includes an endogenous distribution of economic traits. The breakthrough for me came in a 45-minute session I had with Peter Howitt. I didn't seek very much guidance at the time, but at one point I was just stuck and needed to talk to him. I recall Peter graciously inviting me into his office, where I sat down and explained to him my problem. He then stood up, walked over to his chalk board, and started to sketch out elements of a model, piece after piece. And I remember scribbling these notes down furiously with Peter saying things along the way like "Oh my god, how lovely! Look at how this works and oh, look…" I just stared at him and I recall thinking to myself "Are you kidding me"? He just sketched it all out like it was already living there in his brain. At the end of it, I realized that I could think about handling the problem of heterogeneity by assuming an insurance market, insuring people against the idiosyncratic risks associated with search markets. I can't say I was entirely satisfied with the assumption, but at least I knew

[14] It was jointly written with McAfee (Howitt and McAfee 1987).
[15] Pissarides (1985, 1987).

how I might solve the model. I discovered later that other people were making these large family assumptions with the same effect, but I didn't know about that at the time.

S.D. You knew about Rogerson's and Hansen's papers, didn't you?

D.A. Yes, I did, but I guess it just hadn't occurred to me until then how to connect that to my search model. I wasn't aware of the trick of assuming a large family, say, the way Lucas did. It all sounds so obvious now, but at the time, I didn't know. In any case, to make a long story short, I assumed an insurance market to rid myself of the heterogeneity problem and then I basically smashed together a plain vanilla RBC model with the Pissarides 1985 model, which I submitted as a term paper to Peter. That was in the fall of 1988 and I don't know if you want me to go on with the history of that paper. I can if you want.

S.D. Yes, please do.

D.A. Well, what happened was that after writing that paper I put it aside to concentrate on my course work and field exams. Then, sometime in 1989, Joel Fried came back from a sabbatical year he had spent at Northwestern and he caught me in the computer room asking me what I was working on. So I told him and he went, "Interesting. Well, you know, that sounds a lot like something I saw Dale Mortensen working on. You should write to him." I sent Mortensen my term paper and asked him whether he would kindly send his paper to me (there was no email at the time, of course, we did this by hard copy and snail mail). Eventually I received his paper, just his paper, no letter or anything like that. So I didn't know what he thought of my paper. But when I read his paper, my heart sank immediately because I saw that, for all intents and purposes, we had written the same paper. I mean there were differences, he didn't have physical capital in his model but, you know, it was still very similar. I took Dale's paper to Peter's office and said, "Well, I guess, I'm going to throw away my paper, because Professor Mortensen beat me to the punch". But Peter just looked at me and said, "No…I don't think so. You keep working on your paper, it'll be fine." So, I did and I made it the first chapter of my thesis. I presented it at the NBER Summer Institute in 1993, I think. After my talk, people came up and told me how much they liked the paper, also asking me whether I had seen a very similar paper by Monika Merz presented there the year earlier. No, I hadn't! Of course, Monika was a student of Dale's. I had no idea whether Dale had talked to her about my paper or not, but I suppose it's not surprising to find more than one person working on a similar idea. And Monika had integrated search into an RBC model in a very elegant manner.

S.D. She used the large-family construct to solve the problem of heterogeneity.

D.A. Yes, that's right. She sent her paper to the *Journal of Monetary Economics* and I sent mine to the *American Economic Review*. I figured out that one referee was Dale Mortensen. I didn't have to guess who the second referee was. The journal mistakenly included some

correspondence thanking Larry Christiano for his report (I never revealed this to Larry, but I guess I should someday.) John Campbell, the editor, seemed tentative but willing to give me a shot to please the disgruntled referee. After a long time, I finally resubmitted the draft and after another long time, I heard back from Campbell, who said something like, "Well, it looks like the referees are still divided, except they've now reversed positions. Your revisions have satisfied the one referee, but incensed the other." I finally managed to satisfy both referees and the new editor, Ken West, accepted it in 1995. It was like five, six, seven years, after I started working on it. Monica's paper came out in the *Journal of Monetary Economics* before mine did. As for Dale's paper, I had no idea for many years what happened to it. When I once asked him about it he said, "Oh, that thing, well, it wasn't as good as yours, so I sent it some obscure Spanish economics journal." I thought that was a pretty generous thing for Dale to do on behalf of Monika and myself. I think it was published in 1990, or something like that.

S.D. It was published in 1992 in *Cuadernos Economicos*. Was that paper a chapter of your thesis?

D.A. Yes.

S.D. How did it match up with the other chapters?

D.A. I had two papers in my thesis. The second one was a paper on technology diffusion, imitation, innovation, coauthored with Glenn McDonald.[16] It was really quite a separate thing. There was growth and we were interested in explaining these S-shape profusion patterns that we thought we saw on the aggregate data, especially the diffusion dynamic that would cause spurts of economic growth, followed by slowdowns. The common theme between the two chapters was the presence of frictions that were lacking in standard neoclassical models. In the first paper it was the search friction, in the second paper it was a learning friction. The underlying idea was that people had to exert effort to copy the ideas of others, with the difficulty of imitation proportional to how widely an innovation had already diffused in the economy…

S.D. Were frictions a new thing at the time?

D.A. Embedding them in quantitative dynamic stochastic general equilibrium model at the time was a new thing, for sure. Of course, there had been pioneers who blazed the path. John Whalley, who was also at Western, was doing very interesting computational work in static general equilibrium models. I did see an early attempt at calibration exercise in Alan Blinder's PhD thesis of all places. He was clearly thinking about computational dynamic general equilibrium models at the time.

[16] Andolfatto and McDonald (1995).

S.D. In the 1980s Howitt wrote several papers aiming at pursuing the line opened by Diamond in his coconut model.[17] Did Howitt mention Diamond's paper in his class?

D.A. Yes, he did, but, for some reason I don't have a strong recollection of that paper influencing me very much. The paper that really influenced me was Pissarides's 1985 paper, which I mentioned above. If you take a look at my *American Economic Review* paper, you will see that is basically Pissarides's model, tweaked up a little bit, embedded into a real business cycle model.

S.D. I asked my question because Howitt declared in a lecture given in 1986 at the Canadian Economic Association that macroeconomics faced an important crossroad, one fork of the road being RBC modeling, the other transaction externality modeling *à la* Diamond, expressing the hope that the latter might win the day.[18] But you came to him with a project for a paper adopting the opposite line. How did he respond to that?

D.A. He didn't say anything, except to offer me encouragement with the contribution I was making. I should clarify that when I said that the Diamond paper did not influence my thinking, it wasn't because I thought his paper was bad. In fact, I was intrigued with the notion of coordination failure and multiple equilibria. I was already aware of Roger Farmer's work in the area. But at the time, I just don't think I knew what to do with multiple equilibria in computational DSGE models. I barely had the know-how to do what I was already doing.

S.D. Would you say that the insurance hypothesis is just a trick that you needed at the time for computational reasons?

D.A. You can call it a trick. All innovations embed a sort of trick, I suppose.

S.D. It kind of robs the essence of unemployment from the model…

D.A. Yes, definitely. But you know, at the time, the main question was really to trying to assess the quantitative importance of the propagation mechanism of the labor market search process, which is a very different hypothesis about the way labor is allocated from that of a inter-temporal substitution of labor. The Walrasian labor market just did not seem to replicate important business cycle facts. Moreover, the propagation mechanism in the standard RBC model was very weak. The propagation mechanism turned out to be quantitatively much stronger in the search model. And the search model replicated properties of the business cycle that the standard model failed at replicating. That was progress. Now, if my question had instead been directed toward, say, understanding the redistributive consequences of the business cycle, then of course the assumption of perfect insurance markets would have been completely inappropriate. But that was not the type of question I was asking at the time.

S.D. Do you see yourself first as a macro guy or search labor guy?

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[17] See, e.g. Howitt (1985) and Howitt and McAfee (1988).
[18] See Howitt ([1986] 1990).

D.A. Oh, gee, I never liked those labels. The only two labels I like are good scientist and bad scientist. I have questions, just like everyone else. And I try to answer these questions the best way I know how. Labels are irrelevant.

S.D. I was asking this question because Chris Pissarides sometimes stated that back in the eighties and nineties, he would attend labor conferences and people would state that he was not a labor economist…

D.A. Well he should have replied, I'm not interested in being a labor economist, I'm interested in being a good economist.

AN INTERVIEW WITH MONIKA MERZ

S.D. To begin with, could you tell me about the context of your PhD. at Northwestern?

M.M. At the time, in the early 1990s, when I was a PhD. student there, Northwestern was not a macroeconomics hotspot. It started becoming a macro hotspot in 1990 or 1991 only when Larry Christiano was visiting from the Minneapolis Fed. You may think that Dale Mortensen was the main influence in my paper but in fact I think my paper would not have been written without Larry. In terms of macroeconomics, the dominant but outgoing figures were Robert Eisner and Robert Gordon while Marty Eichenbaum was the upcoming personality. Yet he had little to do with the coming along of the paper. It was really Larry. He pointed out to me a topic that had been in the air at the time, namely the discrepancy between the wage elasticity of the labor supply at the aggregate and at the individual level. The former needs to be very big, but the latter had proven to be really small. The initial idea of the paper was really simple, namely that it is not enough that the wage rate allows agents to substitute between leisure and labor market participation. Additional possibilities ought to be considered, and this is where unemployment comes along. My paper was motivated by the idea that when looking for alternative channels from where households could substitute into or out labor supply, why not thinking of unemployment?

This is where Dale Mortensen came into the game. In the early 90s, he had written a paper, which eventually got published in some Spanish volume, where he laid out the possibility of introducing unemployment into an equilibrium framework. My take was to bring the different angles together. The idea that we needed to get a hold of this wage elasticity of labor supply problem was in the air at the time. Hence I don't see it as an odd coincidence that David Andolfatto and I worked on it pretty much simultaneously but independently. In fact, a closely related paper is the one by Rogerson, Ruppert and Wright ("Homework in Household Economics").[19] This shouldn't surprise you: they were also thinking about alternative

[19] In fact a set of papers by Benhabib, Rogerson, and Wright (1991) and Rogerson, Ruppert and Wright (1995).

channels for households to use to substitute not only leisure and market work but in their case it was homework, and my idea was simply to think about unemployment and market work and that was it. So, I view these papers as complement in some sense.

What else can I say? What was the role of Dale Mortensen? You should not forget that in the early 90s Dale was anything but a star. He was known among insiders, and well respected as a theorist. He was considered as a labor theorist, and that is the way he viewed himself. Later on, in private conversations, he told me that he was incredibly happy about the fact that his original ideas eventually entered mainstream macroeconomics because this is what he had originally intended without at the time having the means to do it. The big challenge was to cast heterogeneity and distribution in general equilibrium terms. This is why he and his colleagues went for partial equilibrium models. But of course he was smart enough to see the perspective for search theory to eventually move into macroeconomics.

S.D. At the time were you a labor theorist or a macroeconomist?

M.M. Not at all a labor economist. I have never taken a formal course in labor. I have never taken a course with Mortensen, which of course I regret.

S.D. This is surprising given your subsequent CV.

M.M. Well you know I teach labor myself nowadays. I offer a standard labor class at different levels, but I basically learned it on my own.

S.D. One big problem in getting search and unemployment and more realistic features of the labor market is heterogeneity, and working with heterogeneity was difficult. Of course, now we have the techniques and technology to do so but back then it was not the case. Both you and Andolfatto and Rogerson, used a trick –household or perfect insurance– to get rid of the heterogeneity on the labor market. How do you justify this?

M.M. As you mentioned yourself, in the early 90s we simply didn't have the tools. Krusell and Smith, and Rios-Rull had started working on that but they were not there yet. I remember Larry Christiano telling me that the alternative was either to wait yet another ten years for them to invent the tools or to develop an alternative doable route in order to get my paper done. Of course this household alternative also has its problems, some of which have been illuminated later on, but it is simply that we were not yet there. In particular, it introduces a moral hazard problem, meaning that those who are unemployed are better off in terms of utility, they get the same consumption bundle but they don’t have to work, while the others have to work but then ex-post they share the food so to speak. We were aware of them but given the technical constraints that we were facing in the early 90s, I viewed the large family household assumption as a simple straightforward route to be used.

S.D. Moving on, just for context, how did your paper integrate with the rest of your thesis?

M. M. Chapter 2 has received less attention but was published in the *Journal of Monetary Economics* in 1999. In that paper I introduced heterogeneity in the environment laid out in my first paper. But it was only ex-ante heterogeneity and not ex-post heterogeneity, again for technical reasons. At that time, that was incredibly difficult to do technically speaking. It was relatively easy to write down the model, but it was not so easy to implement it. The short cut I used was to assume that the temporary unemployed kept their liaison with their employer. As a result, I didn't have a discontinuity in the state space. Instead, I had a fully continuous and differentiable state space and thus could do simple quadratic approximation of my state space. The paper was an interplay of theory, empirics, and technical do-ability, or implementability. Of course, nowadays I would write such a paper differently. I would have the tools to introduce ex-post heterogeneity and I wouldn't need to confine myself to this linear quadratic approximation. The third chapter appeared in the *Journal of Economic Dynamics and Control* in 1997. The impetus for it was provided by Larry because he asked me to provide some microeconomic underpinning also for this heterogeneous match set-up. In a fourth chapter that was eventually published in the *Journal of Business and Economic Statistics*, I did something very straightforward and very simple. I used net flows in and out of unemployment for the United States. I questioned the hypothesis that it was really outflows out of unemployment, more than inflows into unemployment, that accounted for much of observed persistence in unemployment. This is something that Robert Shimer later on exploited. Finally the last chapter was something that Dale insisted that I'd write: an historical overview of the coming about of search and matching, and how it all developed, and who contributed when and in which sense and how this eventually got introduced into macroeconomics (Merz 2002).

S.D. How did you perceive the interaction between labor economists and macro economists back then and now?

M.M. I remember that at the time of my graduation I kept saying to Larry when it was time for him to write recommendation letters "I don't want to become a labor economist or to branded as such. I came to Northwestern to be a macroeconomist. I am a macroeconomist, and that is what I want to do". And he said to me "You don't have to be afraid, you will always be perceived as a macroeconomist but you have to be aware and watch out so as not to be caught in between those two fields". Indeed it has never been easy to walk this line between labor and macroeconomics, or any two fields. What is enough in one field is not for the other, and *vice-versa*. It wasn't easy then; it hasn't become any easier.

S.D. How do you perceive the offspring of your paper?

M.M. A lot of papers have grown out of it in lots of subfields of macroeconomics, be it international, log models, monetary (role for unemployment in a monetary context). I remember saying to Dale and Larry that the work that others and I had done had the potential

for providing a novel framework to be exploited by all of macroeconomics, and that is what has happened. Of course some papers are better than others, but that is always the case. But generally speaking, the paper has received the attention that we expected then.

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