Jordi Galí (CREI, UPF and Barcelona GSE))

Jordi Galí was born in Barcelona in 1961 and graduated with a Llicenciat en Ciències Empresarials and a Master in International Management from the Escuela Superior de Administración y Dirección de Empresas (ESADE), Barcelona in 1985 before obtaining a PhD in economics from the Massachusetts Institute of Technology in 1989. He taught economics at the Graduate School of Business, Columbia University between 1989 and 1993, before accepting a tenured position at New York University in 1994 where he was promoted to Professor of Economics in 1999. He currently holds four academic positions: Director and Senior Researcher, Centre de Recerca en Economia Internacional (since 1999 and 2001 respectively), Professor of Economics, Universitat Pompeu Fabra (since 2001), and Research Professor of Economics, Barcelona Graduate School of Business (since 2009).

Professor Galí's research focuses on the causes of business cycles and on optimal monetary policy, and he is regarded as one of the main figures in New Keynesian macroeconomics. His most-cited articles include, 'How well does the IS-LM model fit postwar data?', *Quarterly Journal of Economics* (1992), 'The science of monetary policy: a new Keynesian perspective', *Journal of Economic Literature* (1999), co-authored with Richard Clarida and Mark Gertler, 'European inflation dynamics', *European Economic Review* (2001), co-authored with Mark Gertler and David López-Salido, 'Optimal monetary policy in open versus closed economies: an integrated approach', *American Economic Review* (2001), co-authored with Richard Clarida and Mark Gertler, and 'Monetary policy and exchange rate volatility in a small open economy', *Review of Economic Studies* (2005), co-authored with Tommaso Monacelli. He is also the author of *Monetary Policy, Inflation and the Business Cycle: An Introduction to the New Keynesian Framework* (Princeton University Press, 2008).

Professor Galí received the Yrjö Jahnsson Award from the European Economic Association in 2005, and was elected as a Fellow of the Econometric Society in 2004. His current editorial duties include being Associate Editor of the *International Journal of Central*

Banking (since 2005), *Journal of Economic Perspectives* (since 2005), and *American Economic Journal – Macroeconomics* (since 2007).

I interviewed Jordi Galí at his hotel in Cambridge, Massachusetts, where he was attending the NBER Summer Institute conference. It was the early evening of Wednesday, July 14, 2010.

BACKGROUND INFORMATION

What was your attraction to economics?

What drew me to economics is hard to say. It was a bit by fluke. I didn't know what to study in college. In Europe, we specialize very early on, so I had to choose a field. I didn't have any strong preferences, but I went to a business school called ESADE in Barcelona, and that's where I discovered economics. I thought it was really interesting. It was also a time in Europe, but particularly in Spain, when inflation and unemployment were both very high. The unemployment rate in Spain got close to 25 percent, and that problem was at the center of policy discussions. I thought that economics could solve it, and that led me to graduate school at MIT.

As a student, which professors were most inspirational or influential and why?

If I have to name one, it would be my advisor, Olivier Blanchard, at MIT, and then all the people there from whom I took courses related to macroeconomics, including Stan Fischer, the late Rudi Dornbusch, and Julio Rotemberg. Also, Danny Quah was doing macroeconometrics at that time, and introducing serious methods that have become widely adopted by macroeconomists. And Bob Solow, who was still teaching in the macro sequence, was also very influential.

But I would say that working as a research assistant for Olivier Blanchard and Stan Fischer on their book, *Lectures on Macroeconomics*, which was published by MIT Press in 1989, was probably one of the most rewarding and influential experiences. I had to go over all the chapters, so I really had to immerse myself in the material of the book, and make suggestions and possible changes. The book may be a bit outdated now insome respects, but its spirit still permeates a lot of modern business cycle theory and modern macroeconomics.

Why did you decide to pursue an academic career?

What were the alternatives? I like the freedom that academia gives you, but that's something that I have discovered ex post. I don't come from a family with anyone in academia. At the same time, it's demanding and challenging. I like it. Even If I had not been an economist, I think I would want to be a professor in another field.

As a researcher, which colleagues have been most influential and inspirational mentors?

Most of my co-authors have been very influential, but if I have to name one, it would be Mark Gertler, whom I met when I went to NYU. He's the one who got me interested in monetary policy and monetary theory. Together with Rich Clarida, who was also very influential, we worked on four or five papers in one year. In a sense, I shifted fields within macro as a result of that, and ten years later or more, I do work that is still strongly influenced by that line of research.

Someone who I haven't worked with directly as a co-author, but who has been very influential because of the work he's done, is Mike Woodford. Even when I was starting as a researcher, and I hardly knew him at that time, his papers were always very influential. I like his research style very much.

What would you say are the main differences between working as a researcher in Europe versus the US?

It depends on where you work. But one thing I can say is that in economics there are certainly many more places in the US where it's relatively easy to pursue an academic career and keep doing research at the frontier. In Europe, there are some places where you can do roughly the same, so you don't feel at a disadvantage, but there are fewer places; I would say no more than ten. And in macro, my area, there are probably six or seven that would be comparable to good places in the US. So, if you don't happen to be in one of those places, then I think it's harder to keep in touch with what is being done at the frontier, and to remain connected with the top researchers. But the same is true if you were in the US at some small college lost in the middle of the woods; it's not so much about Europe. I feel very fortunate in this respect, because when I've worked in the US and in Europe, I've been in institutions that are very much part of the circuit (an elite group of economists).

GENERAL THOUGHTS ON RESEARCH

There is an increasing emphasis at many economics departments on applied research. Is this true at CREI?

Yes. I work at a relatively small institution, so I would say, compared to the rest of the profession, we may have some theoretical bias as an institution, but this may have to do with the fact that the law of large numbers doesn't apply. But I clearly see the trend that you mention towards more applied research. In macro, for instance, it's very hard these days to publish a paper, or to present a paper at a seminar, that is purely theoretical. It has to have some empirical support. That was not true in the past. You had purely theoretical papers and some applied papers, and sometimes the connection was very weak between those doing the empirics and those doing the theory. Now it's blended. It's clear that theory just for the sake of it gets little attention, unless you come up with some revolutionary concept. But that happens very seldom.

What do you see as the value of pure versus applied research in economics?

Both presumably should be valuable to society to the extent that they deal with real-world problems. If they don't deal with real-world problems, then I won't defend them. But you can have good, theoretical research that points to some hypothetical mechanisms that could explain some interesting and important real-world phenomena, and so then it's up to empirical researchers to validate the existence of those mechanisms and their importance.

How would you describe the dialog between theory and empirics in economics?

It's hard for me to think of empirical research that is not subject to some discipline that is provided by the theory. And theoretical work is useless unless you subject it to the discipline of the data. So, theory and empirics go hand- in-hand in my view, and this is becoming more and more prevalent.

How would you characterize your own research agenda and how has it changed over time?

I've always been very interested in business cycles and economic fluctuations as a phenomenon. To me, it's still really puzzling that economies fluctuate as much as they do, and I've always wanted to understand that. When I was in graduate school, the so-called real business cycle school of thought was very influential and viewed those fluctuations as being not necessarily undesirable or inefficient. They were interpreted as the optimal responses to shocks that were hitting the economy. In a sense, all my research since those days has sought to challenge that hypothesis and to try to understand what kind of imperfections in markets could lead to the fluctuations that we observe, and could make those fluctuations undesirable so that there is room for policy to attenuate the losses or the costs generated by those fluctuations. So, originally, most of my research was done in the context of real models, and I was emphasizing the role of imperfect competition, and also thinking of economic fluctuations as the result of what we call sunspot fluctuations, which is essentially multiple equilibria. More recently, especially after my work with Mark Gertler and Rich Clarida, I have devoted more attention to monetary models, and have focused on the role that rigidities in prices and wages may play in making those fluctuations either unnecessarily large or undesirable from many respects, and in providing a role for the government to dampen those fluctuations.

Do you think it is important to have broad research interests?

Yes, but I think that's becoming more and more difficult. There's a force that pushes you towards specialization as the only way to remain close to the frontier. Some people manage to do that without specializing, and I can only feel strong envy for them.

As a consumer of research, I try to maintain that broad perspective and to keep track of what people do, even in areas outside macro, but I don't even attempt to make novel contributions to those areas as a researcher.

Do you think having a broad set of tools is more important?

Yes, I would say so. Having a broad set of tools can be useful both for empirical work and theoretical work, but it's more useful in the sense of allowing you to remain in touch with frontier research in a specific area.

Do you think there is any difference in the types of work done by researchers at different stages of their careers based on tenure concerns, publication requirements or other pressures? Should there be a difference?

I can certainly see there is a difference. When you start out, you need to make a big splash. You certainly want to try to publish in top journals, especially if you're in a top institution, otherwise there's no way you will draw much attention in the profession, and you may not be promoted. So, I think there's a bias, and it's good because you have people who are taking risks and exploring in order to write papers, some of which will be highly influential. That's how the profession makes progress.

And then different people pursue different avenues. Some people keep working on extensions of what they did when they were starting out as assistant professors. Others, once they get tenure, are happy to explore perhaps even other fields. But it's true that the papers that get more cited are the ones that are written at the early stage of people's careers. That's what I see as a general rule. Sometimes, I think it's similar to what you see in music. Look at some rock bands. They keep coming up with new albums, but the really good ones were put together at the beginning of their careers. Of course, there's a selection bias; if they hadn't come up with good work early on in their careers, they would probably have given up and we wouldn't know about them. The same is true in economics.

In the end, do you think the profession has helped to bring out and shape your research for the best?

I think there's a trade-off. The way the profession is organized is good because it keeps you alert and tense, and working hard to come up with new things, as opposed to relaxing.

It's competitive. The not-so-good part of it, which hasn't necessarily affected me negatively, is that if you're not working in an area of research that is hot at some point in time, you may be ignored. One consequence is herd behavior; suddenly, most people start working in these few areas. One may have a very good idea, but because it does not have an immediate application to those hot areas, one may not explore it further. I don't think that's good.

But I'm not complaining. I've been lucky because the areas that I was interested in at each point in time, and that I really wanted to work on, were areas that people were paying attention to. They were all central issues in macro; not marginal or exotic ones.

IDEA GENERATION

Where do you get your research ideas?

That's hard to say. I have this feeling, which is hard to prove or verify, that I get most of my research ideas while I sleep, and some of them are still in my mind when I wake up. I don't try to work while I sleep, but the brain keeps functioning.

I know for sure that most of my ideas come very early in the morning, when I shower or have breakfast. That's a time when I feel very alert. But this is for ideas that involve small thinking; tricks that you could use to model one thing or another, or an alternative way to do some empirical tests.

When it comes to the big ideas that somehow shape your whole career, those have been with me all the time, and the fact that they remain in my mind is what keeps me working on them. I mentioned some of them to you early on, like the extent to which imperfections in different markets may be at the root of economic fluctuations, and whether or not government should do something about them. I cannot get those off my mind.

At what point does an idea become a project that you devote resources to?

Maybe you test it with your colleagues and see how they react. Then, you can elaborate a bit, do some toy modeling, or some very simple, rough empirical work to see if there's some promise. Then, you present that in an internal seminar at your institution. In any of those stages, there's a possibility of just discarding the whole thing if you see that it doesn't fly. But if it passes those stages, you try to write the paper, and that's when you make a commitment to devoting some serious time and resources.

IDEA EXECUTION

What makes a good theoretical paper?

It's one which brings to life some new mechanism that people had not thought about before. It's not necessarily one that makes use of a model that you can take to the data immediately. It could be a toy model that is useful to point to that particular mechanism, or possibly to a counterintuitive result.

Can you give an example?

The work of (Greg) Mankiw and (George) Akerlof' and (Janet) Yellen from the mid-80's pointing out that small menu costs (the cost of adjusting prices) can have large first-order welfare consequences for society.[1] I remember reading those papers when I was a student. They were purely theoretical papers using very simple models, but they were very powerful and extremely influential. Their points are now part of our baggage; they are embedded in any of the monetary models with nominal rigidities that we use.

What makes a good empirical paper?

One would be a paper that points to an empirical observation or phenomena or relation that no one had come across before, that is somewhat surprising, and that cannot be explained easily with current models. That's what I would call an inspirational paper, because it's likely to trigger a lot of research trying to account for this, either in the context of existing models or with new models.

And then you have papers that are narrower, but can still be very influential, because they try to test a very strong tradition of a particular class of models. In those, typically the empirical work comes after the theory, whereas in the ones that I mentioned earlier, the empirical work inspires or motivates the theory that follows.

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[1] G.R. Mankiw (1985), 'Small Menu Costs and Large Business Cycles: A Macroeconomic Model of Monopoly', *Quarterly Journal of Economics*, 100, May, pp. 529-537, G.A. Akerlof and J.L. Yellen (1985), 'A Near Rational Model of the Business Cycle with Wage and Price Intertia', *Quarterly Journal of Economics*, September, 100, pp. 823-838.

Can you give an example?

I view Bob Hall's work on the permanent income hypothesis as a very clean example of the second type of papers that I mentioned. It was very influential in terms of the econometric approach to testing a theory.

For the first kind of papers, I'll mention a paper of mine, which is my favorite, from the *AER* in 1999.[2] It provides evidence suggesting that positive technology shocks have a negative impact on employment in the short run. Why did I find that result interesting? Because, if you believe it, it implies that technology shocks cannot be a dominant source of economic fluctuations. In the data, we observe a very strong, positive co-movement between employment and output over the business cycle, but my empirical findings suggest that in response to technology shocks, output and employment move in opposite directions. And there are other papers that have provided supporting evidence along the same lines.

I view my paper as a potentially useful one because it makes the reader update his or her priors about the validity of a certain class of models, not necessarily a specific model. It's very easy to reject a specific model, whereas evaluating a family of models, which share certain predictions, is more useful in helping us make progress in macro.

[2] J.Galí (1999), 'Technology, Employment, and the Business Cycle: Do Technology Shocks Explain Aggregate Fluctuations?', *American Economic Review*, March, pp. 249-271.

When you hit a "brick wall" on a project, do you continue to work on the problem or do you take a break and work on something else?

I have to say I usually don't run into brick walls, not because of my technical skills, but precisely because I'm fully aware of my limitations. I don't engage in projects that are likely to hit brick walls. I like simplicity, and I value it. I also have the feeling that simple ideas and simple projects that are novel get more attention than others that are extremely complicated.

Earlier in my career, especially when I was a graduate student, perhaps I was more into trying to show my technical skills, and so I hit lots of brick walls and dumped projects. But now I dump projects because I end up thinking they are not worth my time, or the time of potential readers; they don't make an important point, or they don't deal with an important issue. It's a question of substance.

What would you say has been the biggest change, in the course of your career, in how researchers in your field do research?

People have become much more open-minded in macro. When I was a graduate student, and in my early years as a researcher, there was a huge gap in terms of the language and tools that people were using in the Midwest versus the East coast in the US. It's amazing how the two have converged over time. So, there's been widespread adoption of stochastic dynamic general equilibrium models, but also models in which we embed all kinds of imperfections. And in monetary economics, which is mostly what I work on these days, we've seen a huge change in emphasis from trying to understand the effects of a one-time monetary policy shock to the impact of monetary policy rules.

THE WRITING PROCESS

Which aspect of the writing process do you find most difficult?

Writing the first few sentences. I tend to postpone writing the introduction because it's a bit scary; it can shape very much what the paper will become or how you will sell it.

I do write very slowly, and I don't find it particularly enjoyable. I appreciate papers that are well written, so I try to write in a clear fashion. But it does take a long time, and it's painful.

What steps have you taken to improve the quality of your writing during your career?

I haven't made a deliberate effort. When I get referee reports, they tend to say that the paper is very clearly written, and probably that's because I spend a lot of time polishing the writing and trying to make sure that it can be understood, even by someone who's not an insider on that particular topic.

Who proof-reads your writing?

When I have a first draft of a paper, I give it to a couple of research assistants, largely so that they can look for inconsistencies or mistakes or omissions.

How do you split up the writing tasks among co-authors?

The way it works best, in my opinion, is that someone takes the lead in writing a very rough first draft, and then, once that is done, you take turns. If you work with a co-author in the US then it's ideal, because someone is working on the paper around the clock. You go to bed, and someone else takes over. Here's an advantage of transatlantic co-operation!

COLLABORATION

When you work with co-authors, how do you decide whom to work with?

That's very hard and completely unpredictable. Sometimes, a project may be just an extension of work that you have done previously with a person. But when you initiate a project with someone who you haven't worked with before, that typically involves bringing together research that had been pursued separately by the two authors in the past, and realizing that there may be some gains in combining the two ideas or two assumptions or two approaches. That's a natural co-operation.

I'm a bit reluctant to write with my own graduate students, because I don't think it's a good idea to go on the job market with a paper that was co-authored with your supervisor. And it's also diverting my student's attention from his own research, which is the one that should land him a job. The same applies, to some extent, to working with one's junior colleagues.

When you do work with co-authors from outside your university, how do you interact with them (e-mail, phone, or face-to-face meetings)?

I like face-to-face at an early stage of the project, and then e-mail is fine. The good thing about e-mail is that it gives you time to think before you respond.

What are the main challenges that you encounter during collaborative work, and how do you overcome them?

When I disagree with a co-author on whether to include or not a certain section, or on whether a certain result or perspective should be emphasized more or less. How those differences are overcome is not independent of what my co-author's answer to the same question would be! I tend to be pretty flexible (I think!).

RESEARCH ASSISTANCE AND FUNDING

How do you use undergraduate and graduate research assistants?

I don't think I've ever used an undergraduate student as a research assistant. I use graduate students for proof-reading, as I said earlier, and also for programming. I like to do my own programming, both for empirical research and theoretical simulations, but I also want someone else to try to replicate my results independently. If they can't, then I may send them my codes and they can find all the mistakes.

I don't rely heavily on my research students, because I do like to see the sausage machine. But if someone can replicate my work, then that's comforting.

How important is funding for getting your work done?

It helps me finance research assistants more than anything else. I do empirical work, so I buy some datasets occasionally, and I try to keep all my software updated, so funding is good for all that. But the kind of work that I do uses mainstream, standard data, so the datasets are not particularly expensive. And, as I said earlier, I don't do very sophisticated technical work, so I don't require lots of research assistant time.

Do you have any advice for a young scholar on the funding process?

I think that research funding is somewhat biased towards people who are well established. So my advice would be to try to become a well-established researcher as soon as possible, and then things will be easier for you. But this is easier said than done!

SEMINAR PARTICIPATION AND NETWORKING

What are the benefits to attending a seminar that is closely related to your work versus one that is not closely related?

Both are useful. In the first case, I will certainly understand more, and I may be able to be more critical about the details in the paper. And the second case keeps me posted as to what people are doing in other areas. That's important. I most likely wouldn't have read the paper, and so when someone makes a nice presentation of some interesting work, I find that a very satisfactory and pleasant experience as a consumer.

How important is professional networking to success in research?

That's a good question. I think it's important, at least to get started, to draw people's attention to what you are doing. Believe it or not, I think it is more important now than in the past when the dominant form of research dissemination was through physical journals that landed on well-established researchers' desks.

How does the researcher without extensive networks succeed?

The truth is that there may be networks that operate at different levels. But what I think is really important starting out is whether your institution is part of the circuit or not. Also your senior colleagues are supposed to offer support at an early stage of your career to help you build those networks. I've seen very bright people on many occasions becoming discouraged because their work isn't getting the attention it deserves. That's why I think going to a good institution is so important.

To what extent is the absence of departmental colleagues working in one's research area a major disadvantage?

I particularly value being surrounded by researchers who are working in macro, but not necessarily in my specific research area. I don't want to be talking all the time about the area in which I work, but at the same time, I want to have good number of macroeconomists around me as colleagues so that we can have a lively seminar series and organize good conferences.

COMMUNICATION OF RESEARCH

How do you find the right balance between communicating your research at an early stage versus the close-to-finished stage?

In terms of communication outside the immediate boundaries of my institution, the first thing I will do is post a paper on my website. But I won't do that until I've presented it in an internal seminar, and perhaps even in one or two other institutions.

What are the unique challenges to giving a seminar and how do you overcome them?

Usually, when I give a seminar, I have already tested the basic idea among my colleagues and I feel on relatively safe ground. So, I don't view a seminar as a challenge, but as a way of getting feedback to help me with the paper or even to think of possible extensions.

PUBLICATION

How do you decide upon the appropriate journal to send your research to? Related, whom do you view as the readership of your research?

If it's a paper that contains a novel idea, I will send it to a top journal, but if it's a paper that's just an extension of earlier work, I will send it to a more specialized journal. I also look at the editors. If I know them, I will try to imagine whether they will be minimally sympathetic to the paper, and then it's usually a process of ruling out journals. The readers of my work are certainly people doing macro.

How would you best describe your approach to dealing with a "revise and resubmit" request from a journal? How about an outright rejection?

With pain in both cases. Each of us thinks that the rejections are unfair on many occasions, and that the revisions are quite often unnecessary, and divert you from the more creative aspect of the research process.

Do you think that the current structure of the publication process in economics facilitates or impedes scientific understanding and knowledge production?

We definitely want a filter and some quality standards. That's very important, especially for young researchers, and I think it works well, overall. If we adopt a social perspective, it's clear that the paper is getting proofed during the publication process. But in economics, referees and editors are too interventionist. They want the papers to end up looking as if they had written them. A consequence is that researchers spend too much time with revisions.

What has been your best and worst experience during the publication process?

I haven't had any terrible experiences, but I've had some interesting ones. I remember sending a paper to the *European Economic Review*, and by some bureaucratic mistake, it was handled by two editors simultaneously, which means that I got two independent editorial decisions. Each decision was accompanied by two referee reports, which invited me to resubmit the paper, but they were asking for revisions along very different lines. Eventually, I was told to focus on one of the editors, and the paper got published.

One of the chapters of my PhD dissertation was also very hard to publish, and I was an assistant professor at Columbia Business School at the time. I had maybe three or four rejections from reasonable journals, and I decided, "Look, I'm getting tired of this." So, I went to the library of the business school to look for a journal to publish my work. I found a journal *whose name I won't reveal*, which was published from Italy. I read in the instructions for submissions that they were asking for a CV. I was so desperate at the time that I said, “Well, these guys may decide to publish my paper on the grounds that I already have several publications in very good journals.” In the end, I didn’t submit it there, but, at the time, it seemed like my last resort was to send it to a journal that would accept it on the basis on my CV (*laughs*).

Do you have a good experience to share?

I’m afraid not (*laughs*).

REFEREEING AND EDITING

How do you decide whether or not to accept a refereeing job?

I have a very simple rule: Is this a paper that I would want to read anyway? If the answer is “yes”, I’ll accept a refereeing request independently of the journal.

What would you say are the benefits to refereeing?

The benefits are the same as those from reading a paper. If a paper deals with a topic that you’re interested in, and it has a minimum quality, then it may help you in your own research. But if it’s on a topic that you don’t care about, it’s better not to referee it. If it’s a disaster, you can write a very quick and easy referee report.

How has your approach to refereeing changed through time?

I don't think it has changed much. I try to be somewhat open-minded in the sense that I can see how people may have different approaches from the one I would have taken, and I try to respect that and be constructive.

How do you decide whether or not to accept an editing position?

That involves a lot of work. I was co-editor of the *European Economic Review*, and then of the *Journal of the European Economic Association*. I did it because I felt some kind of commitment to the European Economic Association. But I'm not doing it now, and I will think twice before accepting another position.

What would you say are the benefits to editing?

If you are very efficient at handling manuscripts, you can certainly be very influential in your area by publishing papers that you think are important and, thus, deserve attention. All of us, to a lesser or greater degree, try to leave a mark, and that's one way to do it.

TIME MANAGEMENT

How do you divide up your working day, both in terms of quantity and timing of different kinds of work?

That's something that has evolved over time, but I have converged to a situation in which I try to allocate the first four hours of the day – 8 am to noon - to research. I like to start early. I know that for the first hour at least, I'm the only one around, and so no one will knock on my door (*laughs*). After that, it's a free-for-all; I could be writing a referee report, preparing a discussion of a paper, dealing with administrative work, drafting a grant proposal, talking to a junior colleague about his work, attending seminars, or just taking a nap.

How do you balance multiple research projects?

That's hard. I may have many research projects open at any point in time, but I cannot handle working actively on several projects at one time. I try to work on one – finish a revision and put it aside – and then take the next one.

How do you balance your personal life and your professional life?

Again, this also has evolved over time due to family requirements, but now when I go home, I try to disconnect completely from work. I don't bring anything home except in special circumstances, and I have to admit I try not to work at all on the weekends. But there are things that are important, like trying to keep in touch with what goes on in the real world. So, I devote a good amount of time every day to reading the newspaper, like the *Financial Times*, or occasionally I just browse one of the monthly bulletins of the ECB or the latest *World Economic Outlook*. But it is light reading.

Your mind doesn't stop, but you need some discipline. I have young kids and I want to be able to have time for them. I cannot work more than ten hours a day, so if I work from 8:00 in the morning to 6:00 in the evening and then go home, that's fine. I wouldn't be much more productive if I stayed in the office, so I find it's perfect to disconnect and have dinner at home with the family.

To me, the most disruptive thing for my personal life is travel, especially if your spouse is also a professional because a lot of co-ordination needs to be done. But I think my wife and I have handled it well at home.

Hopefully, academics have more flexibility. After all, we decide whether or not to accept an invitation to a seminar or a conference, and we don't have bosses that tell us, "You should go there." But some people have spouses who are economists, and they even work in the same institution, have offices next door to each other, work in the same field, and co-author papers. Don't ask me how they handle that.

REFLECTIONS AND THE FUTURE OF ECONOMICS

What are the main challenges facing your research fields?

In macro, there is no way to be able to understand perfectly the issues that are of interest. The phenomena are extremely complex. So, I think we have to be humble and open-minded, and accept that we can learn from all of the alternative approaches. It won't be easy to integrate them all in a single framework, but we should be able to live with that.

What do you think of the new monetarist economics approach of macroeconomists such as Randy Wright, whom I interviewed for this volume?

I still have to digest it, I have to say. My view is that it involves much clever modeling, but with very limited use from a policy point of view. The truth of the matter is that the models that central banks and other policy institutions are currently using and developing are in the New Keynesian tradition, and they involve distortions and imperfections that are very different from the ones that the so-called new monetarist economists focus on. But I may be biased.

What are the strengths and weaknesses of your own research?

Weaknesses are much easier. As I said earlier, I like the creative aspect of research, so that may lead to a certain laziness in trying to tighten all the loose ends in a given paper. I feel some kind of a desire to move on to think about new ideas. I have to admit that is a weakness, but I can't help it.

Strengths? Something that I have inherited from my advisor, Olivier Blanchard, is that I have tried to stay away from "technical fireworks": doing something just to display some cool idea or skills. I try to stick to things that I view as important or relevant.

Do you have any professional regrets?

I read somewhere that Keynes was asked a similar question during his last days. To which he answered, "My only regret is not having drunk more champagne." Maybe I have written too many papers. I might have put less emphasis on quantity. There are some papers that I have written that the world could have lived without for sure. And I could have been mountain-biking instead…or drinking champagne (*laughs*).

Do you have any professional ambitions?

I think of myself as someone who has been ambitious in his professional life, and I'm happy in the sense that I have accomplished my goals. The one thing that makes me really happy is that I still enjoy doing research, and I remain quite productive. I write papers that I don't view as being of less quality than papers that I wrote 10 or 15 years ago. I'm very satisfied with that, and if I ever have the feeling that the work that I write gets much less attention, I will be happy to remain on the sidelines. I will keep enjoying reading the good work that is being done because I enjoy economics.

How would you describe the state of economics today? Are you optimistic about its future?

Contrary to what seems to be the general perception, even among some of my colleagues, economics is more relevant than ever because the financial crisis that we have experienced has reminded us that there are many things that we still need to understand and improve. Just a few years ago, I was more pessimistic from that point of view, especially being someone who has devoted much of his research to questions related to economic fluctuations. It was like being a medical researcher who specializes in some innocuous illness, like a minor cough. But, suddenly, issues related to macro stability are very much at the center, not only of economics, but of what matters to the world. And any advances we can make will contribute to making the world a better place. That is the ultimate goal any economist should have in mind.