From uncertainty to macroeconomics and back:

An interview with Jacques Drèze,

by

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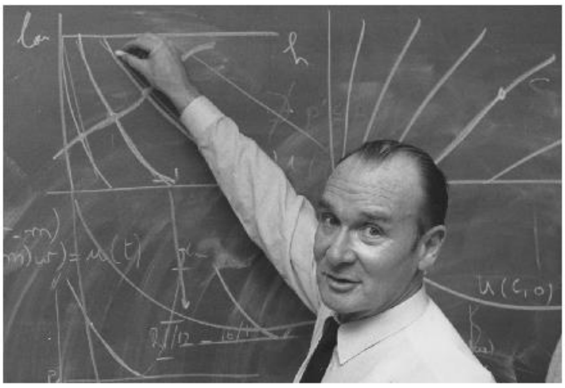
Introduction

Jacques Drèze was born in Verviers, Belgium, in 1929 and completed his PhD in Economics in 1958 at Columbia. His contribution to economics is exceptional, opening up new paths of research in various areas including general equilibrium, decision theory, game theory, econometrics (in particular Bayesian econometrics), followed by contributions to macroeconomics and economic policy. Drèze has been president of the Econometric Society, as well as associate editor and co-editor of Econometrica; founding member and first president of the European Economic Association; president of the International Economic Association, and honorary member of the American Economic Association and the National Academy of Sciences. He has received 15 honorary doctorates, including one from the University of Chicago. He has engaged in the promotion and development of research and higher education in Europe, being a founding member of the Centre for Operations Research and Econometrics (CORE) and of the European Doctoral Program in Quantitative Economics (EDP). He has also actively participated in the most pressing economic problems in Europe.

The interview mainly aims at inviting Jacques Drèze to explain what have been his major contributions and what avenues he suggests for future research in macroeconomics. The interview has been conducted sequentially. It started in October 2002 at the European University Institute. The long material collected along these highly illuminating talks was polished and condensed in the following pages (unfortunately, we were obliged to cut many interesting parts). They are organized in the following way. The first section refers to Jacques' initial experiences in Belgium followed by his study of economics in the US. The second section is devoted to the unity underlying an apparent diversity in his research interests. The third section is devoted to his more recent contributions to macroeconomics, which may be stated as "incomplete markets drive multiplicity of equilibria, calling for active economic policies." The interview concludes with his contribution to the promotion of research and, in particular, research institutions across Europe.

Both Omar Licandro and Pierre Dehez were students of Jacques Drèze. Dehez and Drèze co-authored several papers, some of which are mentioned in the interview. References to Drèze's published papers were abbreviated by inserting numbers referring to the appended selected bibliography. The numbers come from the complete bibliography – which includes 3 line summaries for each entry – available on the CORE website: www.uclouvain.be/core



CORE office, late eighties, illustrating second-best wage rigidities...

Licandro: Different stories were told at the time I was student at the University of Louvain concerning the reasons that motivated you to study economics: your banking experience, your contacts with unions, your studies at the University of Liege. Were these initial times important for your intellectual development? What have you learned from them and how they have influenced your research?

Drèze: When I graduated from high school in 1946, I enrolled for a degree in philosophy at Louvain. The plan did not materialise, due to the accidental death of my older brother a few weeks before commencement. This was a very hard blow for my parents, and I decided to stay with them, go to work for my father, serving as a secretary, chauffeur and assistant – but mostly keeping him company and providing the stimulation of introducing me to his trade. At the same time, I enrolled for a degree in business and economics at the nearby University of Liege.

My father was a small-town banker, in a one-industry town: textiles and textile machinery. His business was very modest. But these were the years of post-war reconstruction, and my father's customers faced all sorts of new financial problems. As I had no fixed duties at the bank, and had progressively acquired a basic understanding of finance, I went on a number of special assignments that were very instructive – like finding in London counterparts for forward transactions on foreign currencies, negotiating in Finland barter agreements to enable a local firm to pay for textile machinery with pig iron, raising equity capital for a small family-owned firm, or even serving as mediator for a labour conflict.

So, by the age of 20 or 21, I had come to grips with a set of real economic problems of some sophistication. This was more challenging than the curriculum in business and economics at Liege, where I was not attending classes anyhow. In these early post-war years, modern economics had not yet come to Liege. I managed to graduate without suspecting the existence of a scientific discipline of economics.

London embankment, 1949, selling sterling forward on behalf of Belgian wool washers.

Dehez: Then, the American experience came. How did you decide to go the States, and to work for a PhD? Why did you go to Columbia?

Drèze: When I graduated from Liege, the University urged me to apply for a fellowship to study in the US. University authorities were disappointed to find few Liege students among the bursaries of the Belgian American Educational Foundation, and chased up graduates with honours as potential applicants. One of my Liege professors explained that the best economics program in the US was at Columbia, where John Maurice Clark taught. Little did he realise that Clark had retired nine years earlier... Actually, I was lucky, because Columbia in the early 50ies had some excellent faculty members, including three that I worked with more closely: my adviser George Stigler; my thesis supervisor William Vickrey, and Abram Bergson (of welfare-function fame).

I did a standard first year, culminating in the PhD qualifying examinations. At the beginning of my second year, Stigler took issue with my plan to spend the year at Columbia preparing for the field exams, prior to my impending military service in Belgium. Stigler told me: “Do not go back to Belgium having attended only Columbia; to become an independent thinker, you must listen to people who disagree with us; take the field exams as soon as possible and then visit some other university or universities before returning to Belgium.”

That is the best advice I ever got. I followed it with enthusiasm. I took the field exams in January, with four fields (theory, mathematical economics, welfare and cycles) that Stigler described as four names for theory, and proceeded to Cambridge, Mass., for the spring term.

There, I could attend the seminars of Samuelson, Leontief and Haberler, as well as interact with younger people like Daniel Ellsberg (I was probing for a thesis topic related to uncertainty). May and June I spent at the Cowles Commission, where I met Marshack, Koopmans and Debreu, as well as Houthakker, Beckman or Telser. Next I attended summer school in Ann Arbour, where the Survey Research Centre had set up the large database of the Survey of Consumer Finance and was pioneering microeconometrics. Klein was there, also Jim Morgan, and of course George Katona, the psychologist who had founded the Centre.

In the meantime, I had in December 1953 heard Franco Modigliani present the life-cycle model, which I found very convincing. But lifetime income is far from certain. I wrote to Modigliani, stating a potential interest in extending the model to uncertainty. At his invitation, I visited him in Pittsburgh in May 1954. Our conversation lasted six hours, during which we understood the difference between immediate and delayed resolution of uncertainty, with application to savings decisions – the root of a paper published... in 1972.

With lifelong friend Franco Modigliani in Rome, 1992, after Baffi Lecture.

This was my first personal experience with progress in research. It was also the start of a life-long association with Franco Modigliani, which was to prove influential for my own career.

So, thanks to a 3-months deferment granted by the Belgian army, I spent the fall of 1954 at Carnegie, associating with Modigliani and Miller, but also Herbert Simon, Charnes and Cooper, Cyert and March or Jack Muth and the team developing Linear Decision Rules. I took there a course in multivariate statistics, to make up for the absence of any econometric teaching at Columbia, and discovered Operations Research, which proved valuable when I did my military service as an OR specialist (sic) working for the Quarter Master General.

Licandro: This brings us to the diversity of your contributions to our science: from economic theory to economic policy, from econometrics to operations research, from general equilibrium to game theory, from micro to macroeconomics. Why have you decided to extend your research in so many different areas? What are the connecting themes behind this variety of research subjects?

Drèze: There are actually two quite distinct answers to that question: the first brings out the substantial unity underlying an apparent diversity, whereas the second accounts for the residual diversity. As an economist primarily interested in real world problems, my theoretical interests have been driven in part by the substantive theme of allocation under uncertainty, in part by the persistent desire to integrate theoretical advances into a unified approach, namely general equilibrium theory - a field which I taught at Louvain for 25 years.

Regarding uncertainty, my interest originates in early work related to decision theory. Thus, my Ph.D. thesis was entitled "Individual decision-making under partially controllable uncertainty". It dealt with two extensions of the model of individual decision in games against nature as developed by L.J. Savage in the *Foundations of Statistics.* The extensions concern state-dependent preferences and moral hazard. The relevance of these extensions is clearly brought out in the application to safety that I pursued in the early sixties (12).

Dehez: That is indeed an interesting application, which I enjoyed developing further with you 20 years later (59)! Can you explain a bit?

Drèze: In 1960, two French engineers were wondering how much should be spent on investments enhancing road safety. So they tried to define the economic value of a life saved. They suggested measuring that economic value by the future income of a potential victim –a consideration also retained in the compensation to the heirs of the 9-11 victims-, and stumbled on the question: should the value of future consumption be subtracted, in order to appraise society's *net* loss? I realised at once that this very question pointed to the basic flaw of the approach: people want to survive and consume, not starve! Going back to the root of the problem, I introduced what is known today as the "willingness to pay" approach to valuing lives in safety analysis. How much would an individual be willing to pay in order to reduce his probability of accidental death? That is for the individual to decide, given his resources on the one hand, given the subjective importance he attaches to survival on the other hand. That subjective value is not reducible to objective calculations; also, it is diminishing in the size of the probability gain. Road safety being a public good, individual willingness to pay should then be aggregated as in the Lindahl-Samuelson theory of public goods.

Dehez: Indeed, my dear Watson! But how does that relate to your thesis?

Drèze: When the "state of the world" is either life or death, it is clear that preferences among "consequences" are state-dependent! Also, when the decisions aim at enhancing safety, it is clear that the probabilities of the states are not given, but depend upon the chosen "course of action". So, Savage's model is not suitable; it must de extended to state-dependent preferences and action-dependent probabilities. These were the very extensions pursued in the thesis. Note that the literature is loaded with models where agents maximise expected utility over actions that entail not only consequences but also variable probabilities. I was after the axiomatic foundations of such behaviour.

Dehez: Did your thesis already provide these foundations?

Drèze: The thesis dealt with a model with three states of the world only. The generalisation to n > 3 states, calling for more advanced tools, came in instalments, first in 1961 (8), then in the definitive formulation of 1987 (76), somewhat simplified more recently (123). Also, I conjectured in the thesis that the logic of subjective expected-utility maximisation also applied to games of strategy. That conjecture is proved, at long last, in current work with Robert Aumann - a clear illustration of continuity of research interests, if I may say so.[1]

Dehez: You are jumping over 45 years! What came after the thesis?

Drèze: While working on the thesis, I started working with Franco Modigliani on savings decisions under uncertainty (28). I soon realised that many other chapters of microeconomic theory similarly called for extension to uncertainty. My volume of collected papers, *Essays on Economic Decisions under Uncertainty* (B2), consists of several parts: individual as well as public decisions, market equilibrium, consumer as well as producer decisions, human capital and labour contracts – a breadth singled out for praise by John Hey in his kind review of the book. My alertness to the need of spelling out the extensions to uncertainty of many economic models is no doubt rooted in my early exposure to practical business situations: coping with uncertainty was part of the daily life of my father and of his customers.

Dehez: The collected papers appeared in 1987. You are again jumping ahead! Can you single out a few intermediate landmarks?

Drèze: In 1953, Arrow wrote a path-breaking paper, introducing states of the world and an event-tree as the primitive description of exogenous uncertainties for general equilibrium analysis – a topic soon picked up by Gérard Debreu in *Theory of Value*. That was exciting. But it called for interpretation. How do subjective probabilities (and state-dependent utilities, for that matter) affect prices for contingent claims? My paper on *Market allocation under uncertainty* (21), largely conceived during my visit to the University of Chicago in 1963-64,

[1] See « Assessing strategic risk », by R.J. Aumann and J.H. Drèze, DP 361, Center for the Study of Rationality, Hebrew University of Jerusalem, May 2004.

establishes the martingale property for contingent prices, an important result further generalised in 1979 by Harrison and Kreps. Yet, complete insurance or asset markets are an abstraction, no doubt essential for theoretical understanding, but devoid of empirical realism. Thus, securities traded on all US primary and secondary markets account for a mere 7% of GDP! So, incomplete markets are the rule. That observation was in the foreground of my thinking on uncertainty through the sixties. When I discovered Peter Diamond's path-breaking paper on *The role of a stock market in a general equilibrium model* (1967), I immediately sought to extend his analysis –limited to ray technologies, so that firms only choose investment levels- to more general technologies. I could not replicate his efficiency result, and eventually proved the opposite: stock market equilibria need not be efficient, not even constrained-efficient, under incomplete markets (33)! And this, even though each firm is adopting a production plan that is Pareto efficient from the viewpoint of its shareholders, and the stock market is competitive!

Dehez: You are referring now to the so-called "Drèze criterion" for firm decisions under incomplete markets, when profit maximisation is not well defined. Did you pursue that theme further?

Drèze: In several directions. Some work extended the criterion to more complex decision structures within the firm (67, B3). Other work applied the same analysis to non-profit organisations (38) or to labour-managed firms (40, B3). A joint paper (85) establishes the generic inefficiency of stock-market equilibria in a general model. Right now, I am extending the so-called "Drèze-criterion" to many periods, thereby integrating the concern voiced by Grossman and Hart, but using more general assumptions.

Dehez: Another instance of continuity in your research interests! From what you have said so far, it seems that your persistent interest in uncertainty has taken you in a variety of directions, confirming an inclination towards diversity...

Drèze: I must indeed plead guilty on that score. It is not without ground that my friend Agnar Sandmo likes to introduce me as a "Jacques of all trades". But remember: there is also a persistent quest towards integration. If I look back at the major developments of our thinking about uncertainty over the 50 years of my professional career, I trace their origins to three interacting disciplines, namely: statistical decision theory, individual decision theory and general equilibrium. Familiarity with statistical decision theory, especially the work of Abraham Wald, was a clear source of inspiration to both Jimmy Savage in his work on decision theory, and to Ken Arrow in his work on general equilibrium. I was active on both fronts. And there is yet another offspring of that interaction, in which I too became involved, namely Bayesian statistics.

Dehez: How did that come about?

Drèze: The significant development of Bayesian statistics over the past half-century owes a lot to the pioneering research of the fifties at Harvard Business School by Pratt, Raiffa and Schlaifer. In 1958, I was hired by Université Catholique de Louvain to teach statistics, econometrics and OR. In statistics, I was following their lead and expounding Bayesian techniques. This was a natural approach for someone immersed in decision theory. Going from decision theory to Bayesian statistics to the economics of uncertainty was a natural route (27). But the econometrics of the time, centred on simultaneous equations, was classical. Hence my students, who went from Bayesian statistics to classical econometrics, faced a breach of continuity. So I went to work and wrote in 1962 a paper on the Bayesian analysis of simultaneous equations, a paper that was never published as such but was rather influential and paved the way for my own later work (34, 39, 41) and that of my students Morales, Mouchart, Palm and Richard. That explains my involvement in Bayesian econometrics, and the birth of what has sometimes been referred to as “the Louvain Bayesian School".

Dehez: So, you have been active on all three fronts just mentioned. You mentioned current work with Aumann. There were earlier forays into game theory with him...

Drèze: I do not regard myself as a professional game theorist. Bob and I did two earlier papers (37, 75) combining his technical expertise with my economic interests. Also close to my heart is a paper with Yossi Greenberg on “hedonic coalitions” (53), Uncertainty to which brings in preferences of the players over the identity of the other members of the coalition in which they belong – a natural concern, as every member of an economics department knows! I wish that I could some day get back to that interesting topic...

Dehez: Please do not bring in the future... We are not done with the past yet! Are we done with the recurrent theme of uncertainty?

Drèze: If I may add a final note: there is a direct link from uncertainty to macroeconomics; every macrotheorist realises that today. In my own thinking, the link materialised via price rigidities. Let me read to you the footnote 1 of my 1975 paper on *Existence of an exchange equilibrium under price rigidities* (36): ”The present note was motivated by research in progress on the rational aspects of wage rigidities and unemployment compensation, viewed as a form of income insurance for which market opportunities offer no substitute”. Said research in progress matured progressively (91, 95), leading to my joint paper with

Christian Gollier on *Risk sharing on the labour market and second-best wage rigidities* (101), and to other papers on reconciling risk-sharing efficiency with productive efficiency on the labour market (125, 131). But in the meantime equilibrium under price rigidities had attracted the attention of macroeconomists, and the recession initiated by the oil-price hikes of the seventies had gained momentum. Prompted by these real concerns, my research interests veered towards macroeconomics. But here again, uncertainty matters, under incomplete markets. Incomplete markets not only provide the rationale for wage rigidities just mentioned; they also account for the volatility of investment and aggregate demand, which is central to macroeconomic fluctuations. My current research on *The macroeconomics of uncertainty and incomplete-markets* (124) brings together my concerns for uncertainty and macroeconomics, restoring again the unity of apparently diverse themes. And it adds the dimension of endogenous, macroeconomic uncertainties. So, macroeconomics brings me back to uncertainty, which closes the loop.

With Robert Aumann in Louvain-la-Neuve, 1986, celebrating the 20th anniversary of CORE, which Aumann described as “a unique breeding ground: a place where cross-fertilization leads to the conception of new ideas, as well as a womb – a warm, supportive environment in which these ideas can grow and mature.”

Licandro: So you are claiming again underlying homogeneity... We will come back to wage rigidities and macroeconomics. But first, let me ask “Jacques of all trades” how he became interested in labour management?

Drèze: It all came from being a Professor-at-large at Cornell University, and being assigned the office of Jaroslav Vanek during his sabbatical. One day at lunch the department head, T.C.

Liu, said: “When Jaroslav can explain to me when a labour-managed firm will adopt labour-saving innovations, I will become interested – but not until then”. In the afternoon, I was sitting in Vanek’s armchair, facing a bookcase containing all the published work on labour management, and reflecting upon Liu’s stricture. As my reflections took shape, they eventually led to the general equilibrium model of labour-managed economies (40). Liu’s question is answered unequivocally by my equivalence result for competitive equilibria and labour management equilibria – a result comparable to that of Oskar Lange for planned economies. Of course, I immediately went on to consider uncertainty, and the funding of labour-managed firms. That line of research merged naturally with my interests in incomplete markets and second-best wage rigidities, as evidenced by my Jahnsson Lectures entitled *Labour Management, Contracts and Capital Markets* (B3).

Other apparent outliers came from pursuing themes linked to my mainstream research. This remark applies, for instance, to papers on stability of dynamic processes. At an early stage of research on the normative theory of the firm under uncertainty, I followed a wise suggestion of Gérard Debreu (then a visitor to CORE) and looked first at the simpler problem of efficient provision for public goods. That led to *“A tâtonnement process for public goods”* (23), the mathematics of which are generalised in (45) and applied to macroeconomic issues in the nineties (92, 118).

With Gérard Debreu in 1989, during the Sixth International Symposium in Economic Theory and Econometrics held at CORE on the occasion of Jacques Drèze’s early retirement from teaching. Debreu had tallied Drèze’s “48 coauthors, whose list goes from Aumann to Zellner”.

Dehez: You have made several other contributions to public economics, ranging from discount rates for public investment (25) and public sector pricing (64) to public goods with exclusion (52). Is that diversity within an outlier?

Drèze: Public economics is another field in which I had no formal training and "learned by doing". The initial investment came from writing a survey of *Post-war contributions of French economists* (13). That was highly educational, and introduced me to Second-Best pricing at the hand of Marcel Boiteux. My paper on *Public sector pricing in a Keynesian Regime* (64) extends the Ramsey-Boiteux analysis to an economy with price rigidities – another attempt at integrating different approaches. It was influential in convincing me that looking for the macroeconomic implications of microeconomics could be more fruitful than looking for the microeconomic foundations of macroeconomics.

Dehez: How is that?

Drèze: I was curious to see what happens to inverse-elasticity pricing rules when private goods are allocated not only by prices, but also in part by quantity constraints. While extending the pricing rules, I saw a multiplier emerge! There is a specific formula in that paper, which I interpret as a multiplier. I was not looking for anything like that. It just came out of the analysis. A multiplier was at work, in an economy where some prices are rigid, and the public authorities affect the allocation of resources through their pricing policies. This came as a surprise to me: why should a multiplier emerge in this second-best analysis? A lightning struck, and I foresaw the possibility of doing general equilibrium macroeconomics!

Dehez: Well, you were starting there from price rigidities – a natural starting point for you! I remember vividly the interest at the mid-seventies and early eighties in equilibrium under price rigidities and quantity rationing. Indeed that work has reconciled the Keynesian and general equilibrium approaches. But that interest was mostly on the European side – not surprisingly, since the seminal contributions came from you, Bénassy, Younès and Malinvaud. Why, in your opinion, did that interest eventually fade away?

Drèze: I have all along regretted the extent to which macrotheorists have privileged the special case of fixed prices over the more general case of imperfectly flexible prices with quantity rationing of supply allowed only when downwards rigidities are binding. Though, of course, I realise that the fixed-prices case has been useful to understand the variety of market configurations (classical, Keynesian, repressed inflation) and the need to allow for a mixture of these configurations at the micro level – as is done for instance in the econometric work of the *European Unemployment*

Program (B4). In a sense, I too reject the fixed prices paradigm – while remaining convinced of the relevance and significance of price rigidities and quantity rationing. My own explanation of the disregard in which so-called "disequilibrium theory" has fallen, especially in the Anglo-Saxon world, is simple. I agree with Blanchard and Fischer that *price or wage rigidities need to be explained, not just assumed!* Providing such an explanation, and testing it empirically, is prominent on the agenda of new Keynesian economics.

In my 1986 EEA presidential address (77), I claim that "increasing returns, price dynamics and uncertainty" bring along market allocations that "involve rationing in a natural way". I still regard today these features, especially the last one, as leading explanations of price-wage rigidities and the associated rationing. Once the existence of uninsurable risks is recognised, an inescapable conclusion emerges: sequential competitive clearing of spot markets can be dominated, according to second-best Pareto efficiency, by market clearing with price rigidities and quantity rationing.

Licandro: That is a bold statement! Can you outline your justification?

Drèze: Efficient risk sharing requires that the resources of every agent be independent of idiosyncratic risks and related only to society's risks. We have known this at least since the work of Karl Borch in the sixties. Under complete markets, trading in contingent claims brings that property about. But not so under incomplete markets. Thus, for a worker with no property income, risk-sharing efficiency would call for wages indexed on national income. But allocative efficiency calls for wages reflecting marginal productivities. Yet, at times of depressed labour demand, market-clearing wages would fall to reservation levels. There is thus a conflict between two dimensions of efficiency. Note that we are discussing efficiency, not redistribution.

A first-best outcome could be implemented through wage taxes and subsidies. Wage costs could be kept at marginal productivities while labour incomes would follow national income. I have pursued that theme in several papers. Edmund Phelps, proceeding from a complementary motivation, has devoted a full book (1997) to the working of the scheme. In the absence of the wage subsidies, downwards wage rigidities cum unemployment benefits provide a second-best alternative.

Licandro: Jacques, this is not obvious. Do you have an intuitive explanation of how wage rigidities may imply a second-best allocation under incomplete markets?

Drèze: Downward rigidity insures labour incomes against depressed market clearing wages. But there is a loss of productive efficiency when wages exceed the real opportunity cost of labour. Sufficient conditions for the insurance benefits to outweigh the productive inefficiencies appear in my paper with Gollier. To me, that argument provides the most fundamental explanation –and perhaps justification- of observed wage rigidities. It is an extension to prospective job seekers of the reasoning first developed in the seventies by Azariadis, Baily and Gordon for workers under contract. Our extension covers general equilibrium with risk-averse firms.

Dehez: Incomplete markets may have motivated your work on equilibria with price rigidities, known as "Drèze equilibria" in the literature. But motivation aside, where did the new equilibrium concept take you?

Drèze: There was some further work on efficient rationing (55), or with you on supply-constrained equilibria (63). And there was some empirical work in disequilibrium econometrics (71), leading to *The European Unemployment Program* (B4). While supervising that 10-country study, I became increasingly aware of an underlying multiplicity of equilibria. But the econometric model did not make room for that. Later, in the early nineties, general equilibrium theory confirmed my intuition: price rigidities may lead to a continuum of equilibria!

When a market is not cleared by price but by quantity constraints, as under unemployment, the extent of rationing introduces an extra degree of freedom: an equality has been replaced by an inequality. For a specific market, like unskilled labour in a given area, that is obvious enough: as demand evolves, alternative rates of unemployment are compatible with the same minimum wage and unemployment benefits. General equilibrium endogenises the demand side. The new feature is the multiplicity of macroeconomic equilibria.

My first foray in this intriguing domain, inspired mostly by early work of John Roberts (1988-1989) and Jean-Jacques Herings (1996), dates back to 1997 (113). It is surveyed in my presidential address to the International Economic Association (124). A general model, studied in a recent joint paper (132), is now being extended with Jean-Jacques Herings to the incomplete markets framework. This work illustrates the merits of the general equilibrium methodology for tackling macroeconomic issues. Although coordination failures have come to the attention of macrotheorists along other routes -partial equilibrium or macromodels, surveyed by Cooper and John (1988)- the link to price rigidities emerged from the general equilibrium analysis.

With Jean-Jacques Herings in the CORE lounge, 2003: coffee break away from the continuum of supply-constrained equilibria!

Licandro: Once again, we face a technical issue that deserves some explanation...

Drèze: Let me try. Consider first an economy consisting of one firm turning labour, supplied by households, into output. Returns are diminishing. Nominal wages are given. Households jointly supply N units of labour, collect wages and profits, and buy output. The firm maximises profits. Any level of output using no more than N units of labour defines an underemployment equilibrium, with output price equal to marginal cost. Indeed, the firm maximises profits, households optimise under a constraint on labour supply, and markets clear.

Dehez: That is at variance with the 3-good model of Barro-Grossman and Malinvaud, where classical equilibria are unique?

Drèze: With reference to that model, the equilibria just proposed define the frontier between classical and Keynesian unemployment, a locus indexed by output prices at given nominal wages. In the 3-good model, all nominal prices are fixed. I explain in my 1997 EER paper (113) how this implies a particular selection from the continuum associated with flexible output prices. Actually, the continuum is a general property. Thus, consider an Arrow-Debreu economy with two sets of commodities, say F commodities with flexible prices and R commodities with downward rigid nominal prices. Quantity constraints are allowed on supply alone, we are looking for a "supply-constrained equilibrium". When the rigidities bite, the R fixed nominal prices imply that R-1 relative prices are given. But R markets are allowed to clear through supply constraints. There is thus one degree of freedom left. It corresponds to either the overall ratio of the flexible prices to the rigid prices, or to the overall extent of rationing for the commodities with rigid prices. Walras law links these two macroeconomic variables as per a Phillips curve of sort. There remains a single degree of freedom, corresponding to the selection of a point on that Phillips curve. The question then arises: how is an element from the continuum selected? Note that competitive equilibria also exist in my economy, at nominal prices high enough.

My provisional conclusion is that the intertemporal equilibrium model must be complemented with a specification of the short-run adjustment process that links successive equilibria. That specification should embody the sources of price stickiness; it should cover the transition from one multivariate equilibrium to the next –possibly as per a tâtonnement or non-tâtonnement in prices and quantity constraints, of which I have studied some examples (92, 118); and it should perform the selection of a specific equilibrium from the equivalent of a Phillips curve, especially when the latter is multidimensional.

Licandro: Why multidimensional?

Drèze: When we move from Arrow-Debreu to the more realistic specification of incomplete markets, the degree of indeterminacy may rise –but that is really technical! Well, you asked for it... In the two-period stock-market economy with S states and J assets, J less than S, we may expect S–J+1 degrees of freedom; that is, a set of equilibria of dimension S-J+1! I have not encountered such a Phillips curve in the macroeconomic literature yet. Not surprising, given the limited popularity of multiple equilibria... But general equilibrium theory aims for generality. If your premises entail multiple equilibria, you had better find out, and face the consequences!

Dehez: Your answers to the last two questions refer to nominal rigidities. Your own interest in money is rather recent, I think. How does it fit into the broader picture?

Drèze: In joint work with Herakles Polemarchakis (127) then also Gaetano Bloise (136), we use a consistent and natural definition of a monetary economy: money balances are used for transactions; they are supplied by banks, which lend them at nominal interest rates set by themselves. Thus, we are considering "inside money", the only kind issued by central banks with balanced accounts... At competitive equilibria under given nominal interest rates, there remains in such a model indeterminacy of the overall price level. In a one-period model, one would say that all relative prices are determined, but the overall price level is arbitrary – a standard feature of the Arrow-Debreu model. In a multi-period model with certainty, the same property holds, and the inflation rates relating the price levels at successive dates are determined through a Fisher equation. That is the starting point, from which different authors proceed towards determinacy along different routes, like feedback rules or the fiscal theory.

But in the intertemporal model with uncertainty, that is with alternative states of the world at any date, there is further indeterminacy to the following extent: at any date event, the expected rate of inflation between today and tomorrow is pinned down by interest rates, but the variability of inflation rates across alternative realizations tomorrow is unrestricted. Understandably, a single instrument, namely the nominal interest rate, implies a single constraint, at each date event.

Licandro: Of course, price level determinacy in monetary economies is a debated issue. Exactly what is your stance?

Drèze: The extent of indeterminacy just stated is both a headache and a blessing. A headache, because we all know that price levels do not jump around like puppets. But also a blessing, because indeterminacy in the abstract model leaves room for endogenous nominal rigidities to pin down price levels.

Licandro: Another enigmatic assertion! Can you explain?

Drèze: I started my Baffi Lecture at Banca d'Italia (B6) with the question: "When warfare in the gulf bids up oil prices, do you expect the prices of books or magazines to go down?" Because many prices are set at intervals, and because many are downward rigid, the answer is clear. As relative prices vary, price stickiness generates some core inflation. This is part of the short-run adjustment process selecting an equilibrium from my continuum, from my Phillips curve if you wish.

Many macromodels of the New Keynesian vein go that route – through staggered prices, menu costs and the like. I differ on two scores: the explanations of price stickiness -we talked about that; and the formal analysis of its implications -which brings us back to the real and nominal indeterminacy associated with price rigidities. **Licandro:** And the upshot for macroeconomics is...

Drèze: The upshot is both substantive and methodological. On the substantive side, I feel that coordination failures associated with price-wage rigidities – whatever the origins of these rigidities may be – have their place in macroeconomic theory and policy. I only wish that I could measure the extent of coordination failures empirically. But that lies probably beyond my own horizon. On the methodological side, I am now investigating some macroeconomic implications of microeconomics in a general equilibrium model extended simultaneously to incomplete markets, money, price and wage stickiness, but also increasing returns (82) and imperfect competition (134). The works! And a long way from the competitive Arrow-Debreu model... As we discussed, these extensions lead to multiple equilibria. And the static intertemporal model remains to be complemented by a specification of short-run adjustments, for which generality is an open challenge.

Dehez: What equilibrium concept, or concepts, are you using?

Drèze: There is always a trade-off between generality, hence scope for realism, and tractability! In models with arbitrary finite horizons, which are also the basic tool for infinite horizon analysis, the perfect foresight equilibrium of Radner is the easier starting point. It lends itself well to my extensions, and to the analysis of coordination failures. But perfect foresight is a strong assumption, especially under multiple equilibria, and I want to pursue more general formulations in the spirit of "temporary equilibrium" à la Grandmont. Arbitrary horizons then create logical as well as technical difficulties, with which I am currently struggling.

Licandro: All that seems quite remote from contemporary macroeconomics!

Drèze: Indeed. My vision is that the extended model is susceptible of encompassing macroeconomics! What I mean is: most of the models used in macroeconomics concern economies that fit within the general model just outlined. Mostly, of course, macrotheory uses specific models – and reaches specific conclusions. But a general approach adds perspective to the more specialised contributions. The clear identification of additional assumptions that lead from the general model to a tractable special case permits relating alternative specific models to each other, and facilitates transfers of results or techniques across specific models. These benefits largely account for the success of microeconomics as an integrated discipline within the broad framework of general equilibrium theory. I foresee today the possibility of integrating formally micro- and macro-economic analyses in a common theoretical framework. And I stress again the intellectual comfort of a unified approach to both fields, a comfort no doubt aspired to by students and teachers alike. Of course, I realise that this is not the alpha and omega of macroeconomics. I am all the more interested in special models yielding specific results, especially dynamic models, that I envision how they can be fitted into a unified structure.

Licandro: We have taken up a number of questions relating to macroeconomic theory, but not to policies. Does your eclectic approach to macroeconomics suggest specific policy recommendations?

Drèze: Definitely so! Not that they are particularly original, but at least they are clear-cut. They aim at coping with situations of underemployment of resources including an element of coordination failure; that is, of underemployment not due entirely to wrong prices and wages, but reflecting in addition a demand gap under incomplete markets. Investment is postponed at a second-order cost to firms but with a first-order effect on aggregate demand. Savings correspond to postponed spending not confirmed to producers. Reflating aggregate demand could sustain an equilibrium with more activity and employment, possibly at unchanged prices and wages. Here lies my different rationale for demand management policies.

Licandro: Why do you say “different”? Aren’t we simply back to good old Keynesian deficit spending?

Drèze: Wait, I am not done yet. One should be aware of the fact that coordination failures are potentially recurrent: whatever we do today, we will again be faced by a continuum of equilibria tomorrow. If a bad equilibrium comes about, we may be able to remedy it through a suitable policy, but we must realise that we may have to repeat the policy over and over again. So, a policy aimed at overcoming a coordination failure through deficit spending, a fiscal expansion, could, if repeated over time, lead to a continuous increase in the level of the public debt. That would result in another type of disequilibrium, which would call for corrective action; it would not be a sustainable policy in the long run.

That is also the reason why I have increasingly advocated coping with coordination failures, not through deficit spending or digging holes, but through socially profitable investments (114). Under price-wage rigidities, there exist investment projects that are socially profitable, though not privately profitable – if only because private wage costs do not reflect the social opportunity cost of labour. These investments will have the same merits for reflating aggregate demand as other forms of fiscal expansion, but they will not lead to instability in the long run, because the service of the debt will be covered by the returns to the investments. And they will have no reason to be offset by private savings, thus avoiding the Ricardian equivalence trap. Of course, the policy is less easy to implement: it is straightforward to decree a tax cut, it is much more difficult to engineer a profitable investment program of similar impact on aggregate demand.

Licandro: Let me press this point. You say that if there is a coordination failure, there is a role for economic policies. In a dynamic framework, as displayed by the real world, the multiplicity of equilibria will occur not only today but also tomorrow, and the day after tomorrow, etc. So agents need to coordinate their expectations not only today but also over the whole future. What is wrong with following simple policy rules?

Drèze: I do not put much emphasis on the notion of coordination of expectations. For me, it is natural that different agents hold different expectations, and surveys confirm that view. But what matters to avoidance of severe coordination failures, of severe underutilisation of resources, is a certain degree of optimism in anticipations. In that sense, one could talk about coordinating expectations on reasonably favourable outcomes.

Contemporary theorists, like Michael Woodford, stress that monetary policy rules aim primarily at anchoring inflation expectations. Let us transpose this reasoning to forestalling high unemployment. Suppose the government had a large portfolio of investment projects that are ready to be implemented, projects concerning public housing, urban renewal, urban transportation, high speed communication, what not. Let the government announce: should we see signs of a deep recession setting in, we would immediately release investment programs to reflate aggregate demand. If the agents believe that, they will expect economic activity to remain at levels reasonably close to full employment, in exactly the same way that they would anticipate monetary policy to keep inflation rates within a narrow band. There lies the scope for intervention offered by the continuum of short-run equilibria.

Licandro: So, your policy recommendations are definitely demand-oriented?

Drèze: When participating in policy exercises in Europe (79, 94, 103), I have been a consistent advocate of two-handed policies addressing simultaneously the demand side and the supply side. And more recently, I have advocated wage subsidies for the low skilled, as an alternative to wage floors, a point that we have discussed earlier.

This is related to coordination failures, which I trace back to price or wage rigidities. Unfortunately, the second-best analysis for wages that we discussed earlier does not take that dimension into account. There is thus an extra reason to be wary of excessive wage rigidities –while still realising that ex ante stabilisation of labour incomes is part of economic efficiency. Wage subsidies are an answer, to which I draw the attention of the profession as well as of policy makers.

Licandro: This brings us to the many debates on crucial issues for the future of Europe you were involved in. How do you explain that economists still have a limited influence on the political debate in Europe? Is there something important to be learnt from the American experience?

Drèze: There is one item on which the position paper “Growth and employment, the scope for a European initiative” (103) produced in the early 90ies by 13 Belgian and French economists, convened by Edmond Malinvaud and myself, has been influential. In very brief summary, that position paper advocated the two sets of measures that I have just reviewed with you: demand side measures in the form of public investment, and supply side measures in the form of reduced labour costs for low-skilled workers. The position paper was one of the first public documents to stress the deterioration in the market position of unskilled workers. So, for the unskilled workers, we advocated eliminating employers' contributions to social security. That was a fairly drastic suggestion, which would have reduced the cost of low-skilled labour by something like 30 to 40 %.

Of these two measures, the first has been completely ignored; it remains so that in official European circles, aggregate demand is not a preoccupation. This reflects in part neglect by economists, in part ineffectiveness at the national, as opposed to the EU level. Anyhow, our recommendation of wage subsidies at the low end of the wage scale did retain attention. Immediately, the staff of the European Commission initiated a set of simulations, which suggested that indeed the proposed measure would have a positive effect on aggregate employment, and especially on employment of the low skilled. Several countries have introduced such measures. Today, I know better about France and Belgium. The rate of social security abatements at the minimum wage is roughly 18% in France and 15% in Belgium. That is less than what we were recommending, but it is still substantial. So I feel that here is one instance where suggestions by economists have been taken seriously by decision makers.

Licandro: Are you pointing to this episode as exceptional?

Drèze: It is indeed the standard view that economists are less influential in Europe than in the US. Two comments on that issue. First, in Europe there is no economic authority comparable to the US government. Why? Because Europe is a Union, a confederation of states, so the prerogatives at the level of the Union are limited, the decision process at that level is complicated and carries limitations. Economic advisers to the Commission are remote from the decision-making body, namely the Council of Ministers. In contrast, in the US, the Chief economic adviser attends the meetings of the cabinet where the decisions are made. So there is no chain of communication, the economic adviser is right there. In addition, the cabinet in the US has much more direct authority than the Council of Ministers in Europe. In that sense, there is much less influence of economic advisers on policy decisions in Europe than in the US.

Dehez: You announced two comments...

Drèze: Indeed, there is another aspect to the question: the debate among professional economists, and the communication from the professional economists to the general public. Here again, there is a big difference between the US and Europe. It has been customary for a number of leading US economists to write columns in periodicals. Also panels regularly organised at the AEA meetings, by Brookings or the NBER, and so on, nourish the debate among economists. We do not have the same habit in Europe, even though I wish to commend CEPR and the journal *Economic Policy* for their valuable forum.

Licandro: Your contribution to the development of economics in Europe exceeds your own research: the creation of CORE, the European Doctoral Program in which some of the more famous European departments participate, the European Economic Association, of which you were the first President, are noticeable examples. Why did you attach so much importance to institutions? What were the roles of these three institutions in the progress of economic research in Europe and why is economic research still led by American universities?

Drèze: You are indeed right that my contribution to economics in Europe has consisted mostly in encouraging, promoting, facilitating the work of others rather than in my own research. In fact, if I look at it from a strictly personal viewpoint, perhaps my main contribution to economics has been to sire Jean Drèze, who has contributed very positively and significantly to development economics, and nowadays plays an active role in promoting a form of social security in India.

To get to your question, and to start with CORE, let me recount the following. I came to Louvain, Leuven in those days, in 1958, after holding my first academic appointment at Carnegie in 1957-58. I was extremely happy professionally in the Carnegie environment, that was more supportive and stimulating than anything I have seen elsewhere, and that is saying a lot for someone who has spent so many years at CORE. The situation in Belgium was extremely different. I was lucky to have an offer from Louvain and to start working there. But the stimulation and the excitement of Carnegie were of course no longer present and my immediate conclusion was: I cannot stay here unless I have colleagues. That explains why I was eager to organise a small research unit that could bring several people together.

Dehez: By “a small research unit”, you mean CORE – an understatement, no doubt.

Drèze: Not initially! The opportunity to start CORE arose in 1964 when Hans Theil, who had developed the Econometric Institute in Rotterdam, left for the United States. The endorsement he was receiving from the Institute of Management Science (in which my Carnegie friends were influential) could be transferred to Louvain. That was helpful in convincing the university to support a small research unit in operations research, econometrics and mathematical economics. The university would provide some premises and a small budget, with professors from the business school, engineering and economics getting together. I was coming back from a visit to the University of Chicago in 1964 with the feeling that there was a place for some outfit in Europe where Americans could spend their sabbaticals. I immediately advertised to my friends in the US that CORE would be glad to accommodate visitors. Indeed during the first two years we had visitors like Merton Miller and Jack Hirshleifer. In 1966, CORE was a small operation, but it was distinctly international.

Dehez: And then?

Drèze: Then we had a piece of good luck: my good friend George Shulz tipped us that the Ford Foundation was eager to intervene on the European scene of business and economics. The Foundation had done that in the US, felt that it had been successful, wanted to do something similar in Europe, but was eager to do it at an international level, not at the level of a single country. I could go at some length into anecdotes about how we eventually received the support from the Ford Foundation. Be it enough to say that by 1968, two years after the creation of CORE, we were partly –and temporarily- financed by the Ford Foundation. We had received adequate facilities from the University, at the request of the Ford Foundation in fact, and we had 7 or 8 visitors for the whole academic year. In 1968, the names that come to mind, besides Ton Barten and Werner Hildenbrand who had joined CORE on a standing basis, include Gérard Debreu, the late Karl Vind and Birgit Grodal, David Schmeidler from Israël, Truman Bewley, etc. CORE had become a lively place.

Dehez: CORE at the time was rather unique on the European continent – a monopoly that has eroded over time...

Drèze: I am truly gratified and proud that several other European universities have over the years emulated CORE. The contribution of CORE to economics in Europe is again less the research output produced in-house than the stimulus to others by the simple example that it could be done. The developments at Bonn, at Tilburg where CentER started as a mirror image of CORE with Ton Barten as director, at Delta in Paris, at GREQAM in Marseille, were all inspired by the CORE experience and organised along similar lines by former CORE members or visitors. So, in that sense, CORE has been very influential on the European scene.

Dehez: CORE is a research center. How did it impact on teaching?

Drèze: Inside CORE, there soon developed a debate about the advisability of having our own doctoral program. Some members of CORE were strongly in favour of doing that, both in order to firm-up university support, and because doctoral students are stimulating and helpful in research. The counter-argument said: if we cannot offer a program of the highest quality, better send students abroad and let them study, say in the US or in London. For a number of years, the two camps were holding their position and nothing happened. In 1975, there had been another debate at the CORE board, and I was mulling over the issue. Among the visitors that year was David Hendry, then a professor at LSE. In thinking about the issue, I told myself: why don't we cooperate with the LSE? Then: why stop there? Werner Hildenbrand had moved from CORE to Bonn; he was still in close contact with us and eager to cooperate. So the idea came up: why not have a joint doctoral program with LSE and Bonn? Of course, my own experience (being kicked out of Columbia by George Stigler to listen to people who disagreed with him) was not forgotten. If students engaged in a joint degree between Bonn, LSE and Louvain, with obligation to attend at least two of these institutions, they would necessarily listen to people from two different schools of thought! When I talked with Hendry and Hildenbrand, both were immediately enthusiastic. That is how the European Doctoral Program in Quantitative Economics, better known as EDP, was started in 1978. It has some 120 graduates to date. The first of these, a certain Pierre Dehez, set the standards! One of the indicators of success is that EDP has been copied and emulated by several others. These joint degrees, with obligation for the students to spend time at two institutions, are now part of the educational landscape of economics in Europe, and I regard this as a very positive development. The road towards emulating American excellence in higher education and research is the road of cooperation, pending concentration.

Dehez: Did the European Economic Association also matter?

Drèze: The idea of the European Economic Association came up at CORE in discussions between Jean Gabzsewicz and Jacques Thisse. Then Louis Phlips convened the first meeting of about 30 economists from different European countries where the project was discussed. Most of the participants soon agreed on what should be the basic features of the EEA; they decided to go ahead and launch it. It took a good start. For the first year of official activity, including the first congress in Vienna in 1986, we reached 1800 dues-paying members. To date the number hovers around 2000. That, I must say, is my disappointment about the EEA. It is today part of the economic scene in Europe. It is playing a useful role in issuing a journal of internationally recognised quality, in holding annual meetings, in organising summer schools for young PhD's and progressively in serving as a platform for the European labour market for economists. But somehow these services are not valued sufficiently by large numbers to have an increased membership. It is significant that people become members when they attend a congress. But in later years, if they do not attend the congress, they do not renew their membership, indicating that they do not value the services to individuals.

Licandro: In your CV, you use to include the long list of your PhD students, most of them well-known economists. Were they the output of your tireless work or a major input in your research technology?

Drèze: The list is not that long: 20 Louvain PhD's (there were a couple elsewhere) over twenty years (1968-1989), i.e. one per year, meaning that I would supervise 3 or 4 students at a time. It is unquestionably true that I had the privilege of supervising a majority of first-rate dissertations, by students who remained active in research and acquired notoriety. When I retired from teaching in 1989, 19 out of these 20 Louvain PhD's were active in research. Many, but by no means all of them worked in areas where I had made a research investment myself. Some of them introduced me to areas new to me: general equilibrium theory with Jean Gabszewicz in the mid-sixties, or disequilibrium econometrics with Henri Sneessens in the late seventies. In all cases, I learned a lot from them, and I remain most grateful. In 1989, I made the mistake of giving up PhD supervision, a mistake that I regret to this date. I mention this for the benefit of other early retirees. There is no doubt that my work in Bayesian econometrics or in empirical estimation of macroeconomic models with rationing, for instance, was substantially extended by my students, and enriched through interaction with them. A majority of these PhD students had taken courses from me here, so the transition to a thesis topic was natural. I would not describe it as "technological". But it illustrates the virtue of including in taught courses some visions of the research frontier. Remembering your student days and looking at both your careers, I feel gratified!

With another kind of big fish, April 1995: under way from Panama to Galapagos during circumnavigation under sail with wife Monique.

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