When Modern Economics Learned French

Jacques Drèze and the Foundation of CORE

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Abstract: *The Center of Operations Research and Econometrics* (CORE), founded in 1966, was one of the channels through which economic modelling practices were imported from the U.S. to Europe. Officially modelled after the *Cowles Foundation for Economics Research*, it rather reflected Jacques Drèze's broad experiences in the U.S. during the 1950s when modelling techniques were not yet anchored in disciplines. CORE gained international reputation however, through the rather exclusive community of Neo-Walrasian economists represented by Werner Hildenbrand, Jean Gabszewicz, and Gérard Debreu. After this community has modified the disciplinary divisions at CORE, the influence of CORE on continental economics happened mainly through disequilibrium economics that still stands for a "French accent" in modern macroeconomics. At the same time, operations research and econometrics prospered at CORE without receiving much attention by economists. This essay tells the story of how CORE changed continental economics through the unique career path of its founder, Jacques Drèze.

Key Words: continental economics, disciplinary formation, Neo-Walrasian economics, disequilibrium economics, Jacques Drèze.

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> Il n'est peut y avoir d'équilibre humain, d'harmonie de l'être, sans un effort d'intégration, sans une reconquête de l'unité. C'est seulement par la critique des fondements que l'on peut revenir de la fragmentation des savoirs et des pratiques vers l'unité vivante d'une position d'existence. (Drèze et al. 1974: 37)

Cowles replicated

French economics was a late bloomer in history. While in Britain and in the United States the experts of optimizing economic life have inhabited academia all way down the 20th century, French economists still after WWII either debated legal matters in law departments or pursued individual projects at the margins of engineering schools. The Ministry of Education established a separate *licence ès sciences économiques* not before 1959 (Fourcade 2010: 192). There surely were individual economists who made important contributions to economic thought (Dupuit, Walras, Allais, etc.), but in terms of the institutionalization of economics, seen from the point of view of the U.S, France after World War II seemed pre-historic.

One of the institutions that changed this situation was the *Center for Operations Research and Econometrics* (CORE), founded in 1966 under the initiative of Jacques Drèze. It was one of the main channels through which economic modelling practices arrived from the U.S. to the French-speaking world, and, to a significant extent, to continental Europe at large. As Gérard Debreu witnessed

> "By the end of the 1960s, CORE had become the major research center in mathematical economics outside the United States. Since then, it has remained at the edge of the exponentially expanding universe of mathematical economics and game theory, operations research, and econometrics" (1991: 5).

Just as the *Cowles Foundation* at Yale University stands for the transformation of American economics (Düppe and Weintraub 2014a), so does CORE stand for the transformation of European economics from a literary-oriented to a model-oriented social science. Indeed, CORE was founded in the very image of Cowles: "The reference model for CORE came from the Cowles Foundation", Drèze wrote. "The initial ambition to replicate Cowles on a modest scale was largely fulfilled" (2006: 2).

This essay qualifies the nature of this replication. First, in terms of the history of ideas, the U.S. origin of the mentioned modelling techniques, as Galison (1997) would say, were the "trading zones" between the disciplines of engineering, business, mathematics, economics, and statistics. It was a unique point of time when Drèze travelled in 1954 to various sites where these modelling techniques prospered. He was socialized in an environment where pragmatic orientation of technical innovations rather than disciplinary divisions gave structure to research in the social sciences. However, the same techniques transplanted to Europe, would soon give rise to increasing specialization of the involved disciplines. Thus, CORE tells us about the fragmentation of knowledge that describes the second half of the 20th century in general (Rodgers 2011).

Second, the replication of Cowles resulted in a *local culture* of economic knowledge. CORE aimed at a contribution to the U.S. body of knowledge, however, it would become influential in economics by participating in the introduction of a "French accent" into macroeconomics - disequilibrium economics. The internationalization of European economics goes hand in hand with the creation of a local profile. Ironically, since U.S. scholars could easily neglect this continental approach to macroeconomics, the import of U.S. knowledge created a hierarchy between the two epicenters of economic knowledge.

Third, the replication of Cowles was not mainly a matter of importing ideas, but of research practices: economic models are produced in collaborative research environments that replace the individual scholar as the unit of production of knowledge. Regular research seminars, discussion papers, co-authorship, and peer review was the norm at CORE. Drèze himself had more than 60 co-authors throughout his career. In this vein, Robert Aumann famously called CORE "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" (1989: 6). As warm as its womb has been, CORE was always a pass-through of researchers, such that it would never ossify to a specific "school of thought" but rather hosted "waves of research teams". As these researchers returned to their home institutions, CORE research standards spread through large networks. These networks represent today an identifiable *continental economics*.

This essay thus tells the story of how CORE changed European economics. Rather than surveying the scientific contributions of CORE, the three mentioned elements of the replication of American economics will be narrated through the unique career path of

CORE's founder, Jacques Drèze.[2] Without reducing an institution to the will of a single mind, his career tells us of the many contexts, places, and communities that had to be bridged for this "replication" to take place. It tells us how cultured is the production of formal models in the social sciences.

Banque Drèze

It begins with *Banque Drèze*, a small local bank in a small town in the French speaking East of Belgium, Verviers. Jacques, born in 1929, was the son of Jules Drèze who owned half of its capital stock, for which he was personally liable. His father wished his son to take over the bank, but young Drèze was hesitant; he dreamt of becoming a forest warden. At the end of WWII, he graduated from high school and enrolled in philosophy in order to gain time for making up his mind. The plan was to join his older brother's apartment in Leuven. But this was not going to happen. At a student's congress in Switzerland, a hike in the mountains, a wrong step, and his brother fell to death. His father suffered particularly from this loss as his health was already weakened from a period in German's war prison. Thus Drèze decided to stay with the family and work at the bank.

[2] There exist several surveys written by members of CORE on Drèze's and CORE's (perceived) contribution (D'Aspremont 2008, Dehez 2015, Cornet and Tulkens 1989, and most detailed the annual research reports). The present focus on economics and in particular Neo-Walrasian economics is not to downplay the contributions to other fields in economics (e.g. public economics) or to other disciplines. The emphasis derives from the fact that Neo-Walrasian economics was decisive for the process of disciplinary differentiation at CORE and its influence on continental economics.

It was there that he acquired working knowledge about trade and finance. He recalls highlights such as a trip to Finland where he negotiated a barter agreement such that a local firm in Verviers could trade its weaving mills against pig iron to be sold in Belgium; at an internship he learned about "electronic accounting equipment", such that *Banque Drèze* was the first in Belgium to use a computer; and he also served as mediator for a labour conflict. It was these sorts of experiences that would make him say, in hindsight, that his sense for economic affairs including his sense for the relevance of uncertainty was conceived. The time at the bank

> "remains in my mind as the starting point of discovering the complexity of the world and of economic decisions....I don't know what would have happened to me if I had been trained as an economist without that background at the bank. Early on I did not make this connection. My dissertation that was extending Savage's model to state dependent preferences was very abstract. However, why did I feel motivated to pose such questions?"[3]

While working for *Banque Drèze*, he also enrolled in a *license* in economics and business at the nearest university in Liège though the curriculum hardly engaged him. It was a set of unrelated fields including history, geography, mathematics, statistics, accounting, and chemistry. Only two of the courses he would later still call economics proper. In spite of his absence, he did well in the mostly oral exams and graduated in 1951 with *la plus grande* distinction. "I managed to graduate without suspecting the existence of a scientific

[3] All quotations by Drèze, if not otherwise indicated, are taken from a personal conversation.

discipline of economics" (Drèze in Dehez and Licandro 2005: 431). Thus Drèze wanted to learn more and began searching a job – in India:

> "I was investigating whether I could find a job in India given my training. I wanted to discover the world. It was not an easy matter. When I went to pick up my diploma which I would also need for applications for jobs in India, the clerk who gave me the piece of paper told me 'I see you have good credits, could you possibly be interested in going to the US?'"

Thus, instead of going to India, Drèze applied for the CRB graduate fellowship for a PhD in the U.S. Few applied, and he got accepted. Learning from other Belgians who had studied in the US that economics is very different there, he decided for economics. He also had to choose a university, and went to an economics teacher asking for advice. Enthusiastically, this teacher recommended Columbia because of John Morris Clark, without knowing that Clark had been retired already before WWII. But Columbia would turn out a lucky choice.

Journeyman years

In summer 1952, Drèze arrived in New York. He had two months before classes started to get used to air and language. In preparing classes he went over Stigler's textbook on price theory, the "first encounter with serious economics". Stigler's class on price theory was

indeed the first obligatory class he took. As for optional courses, he took mathematical economics by William Vickrey that included game theory and linear programming. It was thus Vickrey who first introduced him to modelling techniques, and who would become his PhD adviser.

After his first year of classes, for holidays back in Belgium, he had the idea of a thesis topic after he read a paper that would become the first of the two building blocks of his notion of economic knowledge: Friedman and Savage (1948), that is, decision theory under uncertainty treated in a Bayesian fashion. He thought that he could develop their ideas about insurances and lotteries regarding the shape of expected utility functions.

> "I had discussed this a little bit with Stigler. Could one try to estimate this function from information about people who buy different kind of lottery tickets? … So I wanted to visit the Belgian public lottery administration … and learnt very soon that it would not be possible because the first principal of this lottery organisation is secrecy."

During the same summer, he also learnt about a conference organised in Paris by Maurice Allais that was attended by Arrow, Samuelson, and Savage. He was not there, but would get hold of the proceedings including Arrow's seminal paper on uncertainty (Arrow 1952). He came back from this summer with a firm interest in modelling uncertainty in decision theory, specifically insurances.

But Drèze's actual intellectual profile only gained shape thanks to an advice from Stigler, his tutor, for his second PhD year: Like masters once told their apprentices, Stigler

told him to leave Columbia and travel to other places in order to learn from others who disagree with him. And thus, after his field exams in January 1954, Drèze had the chance to spend the year at several key-universities in the U.S. where all sorts of modelling techniques prospered. He went to Harvard, MIT, Pittsburgh, Cowles, Michigan, and Carnegie. He got to know people as diverse as Wassily Leontief, Paul Samuelson, Tjalling Koopmans, Jacob Marschak, Milton Friedman, George Katona, Lawrence Klein, Franco Modigliani, Herbert Simon, Abraham Charnes and William Cooper, among others. He also could exchange with other students and learn of their notions of economics. These sites, most of them rather at the margin of the Ivy League, were not choices made by Dréze and he was not aware of its selection. Most other departments would have been more conservative in their methods. This year was thus a unique experience that made Drèze different from others of his generation. During this year, his eclectic image of economic knowledge was shaped; an image open for collaborations with psychologists, business, engineering, and statistics. As special as this experience was, Drèze would not see the scientific change going on, but presumed to learn about 'normal science'.[4]

At MIT, he attended a seminar by Samuelson who was "extremely accessible. Somehow we got on an easy relationship including playing tennis." Samuelson would call upon him later for several professional tasks. At Harvard, he met Daniel Ellsberg, another economics student. He could also talk to Oskar Morgenstern who came to give a talk. They discussed expected utility, the Allais paradox, Modigliani's life cycle hypothesis and saving

[4] "I still feel today that it is a major deficiency of the US education that graduate training in almost all cases is done at one place and students are not encouraged to visit other places, especially those who do a PhD." Later Drèze would require from his PhD students at CORE to spend one year abroad. When Gabszewicz in 1966 spent a year in Essex, however, he was grateful for Lipsey allowing him to spend most of that year at CORE.

decisions. He met Leontief with whom he had a long conversation about the same topics. Leontief was interested and eager to learn, Haberler less so.[5]

From Boston he planned to go to Chicago. On his way, there was Pittsburgh, and thus the opportunity to meet Modigliani, which would turn out to be the most important stop-over of his career resulting in a paper and in a lifetime friendship (Drèze and Modigliani 1972). He heard Modigliani presenting his life cycle model at the annual AEA meetings in December 1953 and wrote to him concerning uncertainty questions that are relevant for life cycle decisions. Modigliani was ready to talk to him.

> "On my way from Cambridge to Chicago, I stopped in Pittsburgh and visited Modigliani on a Saturday morning. By noon we had discovered the property that earlier resolutions of uncertainty are generally preferable, which is a simple property, but which is at the root of analysing savings and their uncertainty and portfolio choices…. That is what I presented at Cowles."

The paper he gave at the Cowles Commission was the first research seminar he gave and thus the debut of his career as researcher. Cowles was a lively place. Next to Beckman, Houthakker, Herstein, Radner, and Marschak, Tjalling Koopmans, research director, left

[5] At Harvard, and elsewhere, Drèze would also learn about the Red Scare, the stoke of fears of communism, that was at its height during the early 1950s. For a general interest journal in Belgium, *La Revue Nouvelle*, he wrote an essay on education and democracy in the U.S. Generally fascinated, he reflected on topics such as segregation, financing of education, the role of religion, American pragmatism (being against the values of higher education), and also noted absurd elements of anti-communism: "Un comité bien intentionné du Middle-West a voulu proscrire l'usage dans les classes de "Robin des Bois", parce que les réfractaires y vivent en régime collectiviste!" (1955: 364).

the greatest impression because of his "global approach and exceptional personality". The person he got closest with was Lester Telser. Also Debreu came to his seminar, gave comments, but they would not get more acquainted. There was little they could talk about since Drèze was not at all into general equilibrium theory (GET) at this point. During his stay at Cowles, as well as for the next 15 years, he would elaborate his model of decisions under uncertainty in stock markets. His paper on market allocations under uncertainty (1971) has been largely conceived at Cowles. Apart from short visits, this would remain Drèze's only longer stay at Cowles. Considering the notion that CORE would be modelled after Cowles, the visit made little impression compared to what would follow in Carnegie.[6]

In fall 1954, Drèze followed an invitation from Modigliani to visit Carnegie on a three months fellowship. Even if Carnegie was not a research center, but a business school with an economics department, it made a greater impression than Cowles. Carnegie gathered an exceptional amount of economists that would coin Drèze's notion of intellectual collaboration: Herbert Simon, Franco Modigliani, Charles Holt, Abraham Charnes, William Cooper, Peter Winters, and John Muth. It was only there that he encountered operations research. He must have learned of Carnegie's engagement with the military (Klein 2015) but recalls rather the context of a business contract with *Pittsburgh Plate Glass* which gave rise to optimising quadratic functions under linear constraints and uncertainty. Even if the model of organizing CORE came from Cowles, the model of

[6] During the summer months, Drèze went to the University of Michigan to the *Survey Research Centre* for a summer school. Next to Lawrence Klein, he met the psychologist George Katona, someone not many who were associated with MIT and Cowles came in touch with. His work on micro-econometric analysis of economic behaviour resembled well Drèze's interests. "The summer school in Michigan was not really part of my mainstream interest at the time but of my education in general: applied econometrics, micro econometrics and some economic psychology".

combining fields came from Carnegie. “There was economic theory, econometrics, operations research, game theory. In a sense corresponding exactly to what CORE became.” Also in terms of its inclusive atmosphere, Carnegie would remain his model.

> “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” (Drèze in Dehez and Licandro 2005: 447).

Drèze liked Carnegie, and Carnegie liked him. In 1957, after two years at the Belgian army where he advanced operations research methods, he received an offer to return to Carnegie as a visiting Assistant Professor for two years. Freshly married, the couple left to the U.S., and Drèze could finish his PhD thesis entitled “Individual decisions-making under partially controllable uncertainty”. It extended Savage’s *Foundation of Statistics* to state-dependent preferences and moral-hazard). The end of the PhD being imminent, there was the question if the couple stays in the U.S.

> “Carnegie had a dean named Bach. On the day that our oldest son was born in Pittsburgh, he called me and he told me: ‘we realise that you consider going back to Belgium, so we want to make you an offer: the day that you become doctor, you may become associated professor with tenure at Carnegie’. He thought it was a very smart move. However, that gave me information that made it easier to go back to

> Belgium because I learnt that I could come back ... We planned to go back to Belgium for two years; if I saw a prospect of a career there, we would prefer that, mostly for family reasons."[7]

Drèze quickly found a job as a part-time *chargé du cours* in Leuven, at the bilingual and still unified *Katholieke Universiteit Leuven* (KUL)/*Université Catholique de Leuven* (UCL), and as a part-time researcher in Brussels at the *Centre d'Etudes de Recherche Operationnelle* at the *Université Libre de Bruxelles* (ULB). Gaston Eyskens in the economics department in Leuven, soon prime minister whose son spent a year at Columbia, believed that econometrics should be taught in Leuven, just as Tinbergen did in Rotterdam. And so Drèze started teaching in September 1958.

Back in Belgium

The difference between the academic atmosphere in the U.S. and continental Europe was vast. European universities were inward looking, and little internationally oriented. This was true for France, Germany, Italy and Spain, and also Belgium economics was dominated by "mandarin" professors (Maes et al. 2000). Major university reforms in favour of more fundamental research were still to come in the early 1960s (Ibid: 2000: 95).

[7] "If we had lived in the US we would have raised our children as Americans ... That would have meant a cultural gap vis-à-vis our own children and we did not feel attracted by that." Notwithstanding, his children became very international. Notably, his son Jean went to India to work on poverty issues with Amartya Sen.

> "The problem on the continent at large was that universities were essentially local. They taught in their local language, they hired their own graduates, had no visitors and basically no sabbatical leave. They lived in small self-perpetuating communities and they had suffered by the whole war period during which there were no contacts with the US or with the UK and no contacts across national borders. This provincialism explains that there was very little research of world level significance done on the continent at that time."

Having learned about the value of working in teams, Drèze found himself alone. "Basically I had no colleagues and it became soon clear to me that if I wanted to stay I must have colleagues... and that they would have to come from outside". In a laudatio on Drèze, Licandro compared Drèze with Don Quichote:

> "At the start of his career, he took the risk of going back to Belgium, his native country, to contribute his efforts to modernizing teaching and developing research in economics. In his first years at the *Université catholique de Louvain* (UCL), like Don Quixote in the harsh steppes of La Mancha, he had to fight against what seen from a certain distance looks today like windmills, although at the time they took on the appearance of giants" (2002: 5).

This statement needs to be qualified. In fact, there was little *scientific* resistance. There were no debates in which Drèze had to convince his colleagues of the virtues of a certain

methodology; no paradigm clashes of beliefs how economics should be done. The obstacle was less the scepticism about modern modelling techniques but rather the institutions that are implied by these techniques. There was general openness for new methods in economics, but the provincialism created little structure to adopt them.

Drèze had to face explicit methodological suspicion only in individual cases. In Leuven, one dominant figure who was against the use of mathematics in economics was Léon Dupriez.[8]

> "(Dupriez was) a very complex figure and intelligent man, well trained in philosophy and some economics. He had founded the *Institut de Recherche Économique* where he essentially was doing micro-econometrics. He was collecting data from business firms to make forecasts of economic conditions in a theoretical framework inspired mostly by Walras… He was a general equilibrium person but without formalism… He was rejecting emphatically macroeconomics, and had not encouraged econometrics as such… I send him long comments on his book, *Philosophie des Conjonctures économiques*, but never heard back from him."

[8] Dupriez's "Des mouvements économiques généraux" (1947) was a highly appreciated and widely cited book in Belgium during the 1950s. Dupriez attracted grants from the Rockefeller Foundation thanks to his Hayekian inspired business cycle research. Drèze supervised his first PhD student jointly with Dupriez, Henry Tulkens. The same closed spirit Drèze encountered with Étienne Sadi Kirschen, known as the creator of national accounts and as a neo-Keynesian economist of a Samuelsonian kind. "Kirschen was like Dupriez, convinced of the quality of his own work and for whom the rest was of no interest or value." For more on Dupriez and Kirschen, see Maes et al. 2000.

There was one Belgium econometrician, instead, who shared Drèze interests: Jean Waelbroeck. He taught econometrics at Brussels and was soon befriended with Drèze.[9] Waelbroeck would later become a CORE member and helped setting up CORE as an interuniversity center of ULB, UCL, and KUL. Waelbroeck's colleague at ULB, Kirschen, would try to prevent this agreement.

Drèze's integration in the French scene happened without much effort thanks to the European roots of the Econometric Society. CORE would later not only stand for an Americanization of European economics, but also a diffusion of the economics of the so-called "French engineers" – a group of individuals that lacked an institutional framework in which their work could be coordinated and prosper. Maurice Allais was teaching at *École Nationale Supérieure des Mines*, Edmond Malinvaud was at the *Ecole Nationale de la Statistique et de l'Administration Economique* that was rather descriptively oriented; Marcel Boiteux became the chief executive at *Electricité de France*; Pierre Massé was *Commissaire General* in the planning office; Jacques Lesourne was an engineer running a consulting business. The French engineers fit the old model of the individual scholar receiving truth by contemplation rather than conversation: "Maurice Allais did not need colleagues; he did not need to talk with people, he could do it all on his own. He never tried to be surrounded by others or create a research unit." Nevertheless, this group of economists was in charge of the European basis of the Econometric Society. After Drèze

[9] Waelbroeck has been at MIT between 1965 and 1966. Together with Kirschen he founded ASEPELT in 1961, the *European Scientific Association for Medium and Long Term Forecasting*. Waelbroeck was a founding member of Project Link with Lawrence Klein, a statistical model of the OECD countries. According to Maes and Buyst, "Waelbroeck and Drèze managed to overcome the old ideological rivalries between freethinkers (University of Brussels) and Catholics (University of Leuven/Louvain)" (2005: 80).

came back from Carnegie, only a few weeks later, he attended the 1958 European summer meeting in Bilbao:

> "I had submitted a paper written by Charles, Drèze and Miller (1966) and it had been accepted for presentation. I knew nobody... One of the first presentations was made by Lesourne, I believe. At the end I made a remark which essentially amounted to saying 'you make a very restrictive assumption and it looks feasible to extend it in this direction'. After I made this remark, a person got up and said 'the remark that has just been made deserves attention because I have a letter by Kenneth Arrow, who has seen the paper, and asked me to present exactly the same remark here'. So I felt a bit flattered"

The person who was supposed to hand over Arrow's remarks was Malinvaud. Drèze was thus immediately taken into the French community. His position in the Econometric Society was settled even before he had an English publication; in 1963, he would become associate editor of *Econometrica*, replacing Malinvaud as the French-speaking editor.[10] A year later Drèze published a survey article on post war French contributions, thus giving U.S. economists access to the French literature, among others, to Boiteaux's theory of second-best pricing (1964). "On the very day when the journal came out – I was in Chicago – I was visited by (Milton) Friedman and (Harry) Johnson both of which told me 'I have read your paper'". U.S. econometricians had an instinctive appreciation of European

[10] When the society was created, French was one of its two official languages such that *Econometrica* had a French speaking co-editor, Malinvaud at the time. Strotz knew Drèze well, and naturally chose for Drèze as co-editor.

scholars as they contributed to the foundation of the society (Schumpeter, Frisch, Bortkiewicz, etc.), an appreciation that did not correspond with an international orientation of the French engineers. They were scholars working alone and there was nothing in sight that could have changed this situation. It is impossible to conceive that CORE could have been created in France, at the Sorbonne, at *Ecole Normale* or any other prestigious university.

Drèze had equally important contacts with the Netherlands, specifically to the Econometric Institute in Rotterdam founded by Theil in 1956, two years before Drèze's return. Its reputation preceded and favoured Drèze integration in continental economics. CORE would later be the replacement and makeover of this institute. Theil has just published his *Economic Forecasts and Policy* (1958); he called his approach to econometrics Bayesian, though, according to Drèze, it was "Bastard Bayesian" as he used mixed estimations (2006b: 82). Operations research also had its presence. Simultaneously to Simon, Theil worked on quadratic programming techniques. With Theil, Drèze organized activities for the Econometric Society, of which Theil was president in 1961. Apart from Theil, other permanent scholars were Anton P. Barten, John C.G. Boot, Dirk B. Jochems, Teun Kloek, and Cornelis van de Panne (Drèze 2006). Jan Tinbergen was present too though stayed in the economics department working mainly in development economics. Like CORE later, the Econometric Institute had a working paper series, and hosted several visitors during their sabbatical such as Arthur Goldberger, Arnold Zellner, Franklin Fisher, and Marc Nerlove. Drèze never stayed in Rotterdam for a longer period, but frequently was present for giving or attending seminars. In one of the seminars Drèze gave in Rotterdam, he managed to recruit a first student, Michel Mouchart. Mouchart liked the U.S. style Drèze

represented, embarked on a Masters and PhD with Drèze, and in 1963 would become his teaching assistant. The institute gave Drèze hope for a future in Europe; indeed, it became the European model for CORE:

> "Without Rotterdam's Econometric Institute, we (Drèze and his wife) might well have given up on Europe... The spirit and activities there were reminiscent of the excitement at Carnegie, on a more modest scale… A mix of local researchers, including young ones, and foreign visitors; a mix of research diversity and concentration on interrelated topics; seminars, discussion papers and reprints; and close links between research and teaching programs; with a research centre serving as host for these activities and providing the congenial environment, both scientific and human…The example of what could be achieved with limited means inspired confidence in the future of economic research in Europe" (Drèze 2006b: 80f.).

Drèze's confidence was strengthened when in 1959 negotiations about a sponsoring agreement with *The Institute of Management and Science* (TIMS) began in Rotterdam. Drèze knew from William Cooper from Carnegie, chairman of TIMS, that the institute is interested in sponsoring a small European center for operations research and quantitative methods of management.[11] Drèze spoke about these plans with Theil, who, to the surprise of Drèze, had already submitted a proposal to TIMS without mentioning it.

[11] Other members of the TIMS Advisory Committee were Abraham Charnes, Roy Radner, Russell L. Ackoff, Maurice G. Kendall, and George Kozmetsky.

In spite of the contacts in Paris and Rotterdam, Drèze felt relatively lonely in Belgium and eager to reconnect to his U.S. colleagues. Thus in 1962, after three years passed, Drèze visited for eight months North Western University. Robert Strotz was there, Charnes had come from Pittsburgh, and Robert Eisner was working on macroeconomics. During this time his third son was born with a dangerous malformation (hydrocephalus). Treatment in the U.S., specifically in Chicago was more advanced than in Belgium. George Shultz, the dean of Chicago's business school, learned from Merton Miller about Drèze's situation and made him an offer at "any time for whatever length". For the coming five years, Drèze was thus on and off in Chicago. Shultz was related to the Ford Foundation, and would turn out vital for CORE's funding.

But Drèze did not give up on Belgium. In November 1964, he held a speech on the modernization of UCL, pleading for higher quality standards, appreciation of fundamental research, interdisciplinary cooperation, acceptance of English, more professors, and especially the modernization of the catholic heritage of the university – all in the service of society and man at large. He noted "une difficulté particulière a notre temps: les sciences sociales atteignent à peine leur majorité scientifique" (1965: 591). With this speech, Drèze took on the role of the modernizer of the social sciences at UCL, and he would indeed receive support from the rector in this project for the coming years.

In 1965, a new perspective opened up when it became known that Theil would leave to Chicago. The future of the Rotterdam Institute was open. Drèze managed to convince TIMS to transfer the money for the econometric institute to Leuven, and thus to create an Econometric Institute according to his own tastes. Leuven was pleased to match the terms that Rotterdam has offered - a working budget and two assistant positions that could be

spent for visiting professorships. In fall 1965 the transfer was accepted.[12] As Drèze would write in the opening document of the Center: "This Center, devoted to the progress of Management Science through research, education and related activities will follow the tradition of similar institutions sponsored by TIMS in Rotterdam from 1961 to 1966 and in Mexico City" (report 1966).

The foundation of CORE

With the prospects of the TIMS grant, Drèze tried to connect to the researchers and teachers at the economics, business, and engineering faculties that in some way or the other would embrace formal modelling techniques. The computing center offered the physical facilities (and access to the IBM 360 and 1620). The president, Henri Florin, a known Dutch mathematician, and director Jean Meinguet arranged this deal. They agreed to postpone the construction to add a floor for CORE.

From the Flemish part of the economics department, economist Paul van Moeseke and the econometrician John van Waterschoot were members of the first board. Like Drèze, van Moeseke had a PhD from the U.S., indeed from Yale University and was strongly supported by Tjalling Koopmans. He was interested in economic theory, linear programming, and in the Walrasian tradition. As apt he has been for CORE, he would not

[12] Next to the TIMS grant, CORE received other external research grants: from the Belgian productivity Agency on "the use of mathematical models in the social sciences", and the Belgian National Science Foundation on "general equilibrium under uncertainty", as well as a business contract on programming.

easily integrate in the CORE research group.[13] His wife was from Latin America, and he was commuting between the U.S., Belgium, and Latin America. He would remain at CORE until 1971.

Also the engineering school provided support. Next to Hendrik de Meulder and José Paris, there was Guy de Ghellinck who taught operations research, and would be associated with CORE on a long-term basis. Ghellinck did actual research, which was not a given at the time. "He was a very reserved person and he had a problem convincing himself that what he was doing was worth publishing". Also the business school provided board members. Next to H. van den Borre, there was the first treasurer of CORE, F. Juckler, and Frans van Winckel, executive director of CORE, both associate professors in operations research.

Many of these first board members mainly helped to have CORE recognized as a joint initiative of the French and Flemish universities but did not take part in CORE research. That is, without external hires CORE would only exist on paper. The university paid for two external hires, Anton P. Barten from Rotterdam, a disciple of Theil, and a younger German economist, Peter Schönfeld. But were internationally oriented, but they were not yet internationally known. Schönfeld would leave soon, and Barten would spend a large part of his career at CORE. He would be the second research director replacing Drèze

[13] "When he (Moeseke) became a CORE member, on the first opportunity he announced that he wanted to be research director of CORE. The CORE has statutes explaining how the research director is appointed and he was not appointed. He took that very badly; he went to the rector of the Flemish university to tell him to pull out of CORE and left for New Zealand. That is the one unfortunate human relation story that I know of in the life of CORE".

(1970-1974). TIMS funds were used for two visiting research fellows (with a total of 2000 dollars per year), Jan Mossin from Norway and Wlodziemierz Swarcz from Poland.[14]

In order to develop the international dimension, Drèze used his U.S. contacts. The first U.S. visitors in 1966-1967 were Albert Ando from Pennsylvania, and Merton Miller from Chicago. The year after, Gordon Kaufman from MIT came, then Jack Hirshleifer from UCLA and Robert Wilson from Stanford. Also the seminar series was a way to connect back to the U.S. In the first two years, there was only one, a Thursday seminar. Roy Radner from Berkeley and TIMS member gave a talk on "planning under uncertainty". Then, among others, Tjalling Koopmans, Hirofumi Uzawa, and Carl Christ came to give talks. In January 1968, Debreu gave his first talk.

Most visiting positions were research positions without teaching obligations, as CORE did not offer a degree, though the courses given by their members, in English, allowed for the presence of students at CORE. The list of student research assistants of the first two years is noteworthy as some of them would be committed to CORE for their entire careers: Stephan Gepts, Jean Jaskold-Gabszewicz, Pierre Pestieau, Dirk Vanwijnsberghe, Paul Hespel, Maurice Marchand, and Henry Tulkens. They all got initiated to modern economics by taking courses from Drèze. Drèze used Samuelson Solow Dorfman on linear programming as a textbook, which was *the* model textbook for combining operations research and economic theory. As Pierre Pestieau said about the atmosphere among these assistants: "It was clear that we are part of something new, we were arrogant, and proud of belonging to a venture that is going to be successful. CORE was very different from other

[14] For the most time, there was always some Eastern European presence at CORE also because that was the interest of the Ford foundation. But these scholars were never central to CORE's agenda.

departments" (pers. communication). Arrogant towards others, when it came to go to the Thursday seminars he would hide under his desk – the formal training would not be sufficient to follow. When Pestieau in 1967 went to Cowles to pursue his PhD, coming from CORE increased his credentials. He became the teaching assistant of Koopmans, and was trusted to translate his *Three Essays*. "Cowles was similar, older, more mature, better known, but it was the same sort of arrogance of the Cowles member towards the economics department at Yale. Cowles was the big brother of CORE" (ibid.).

Part of the innovation of CORE was also the administration that was in favour of giving as much time as possible to research, which was not common practice in other departments. CORE was set up like a small business enterprise in basic research, Maes and Buyst observed (2005: 80). Sheila Weyers began working at CORE in 1968 first as a librarian then as secretary. She immediately got the sense of "excellence" at CORE. The international orientation was unique at the university, but she did not identify this style as an U.S. imitation.

Despite the early reputation, the direction into which CORE would develop was not yet set. The perhaps most formative event for CORE's further development happened soon due to one of the first PhD students of Drèze: Jean Gabszewicz. Born in Zaire, his father polish and his mother Belgium, Gabszewicz started studying law and then economics in Leuven in 1955. In 1962, he took one of Drèze's first classes in mathematical economics, jointly with Henry Tulkens. Gabszewicz got deeper into GET, specifically after reading Aumann's seminal paper on core convergence and the continuum of traders (1964). He indeed was the first French-speaking person who got into this literature. There was nobody who could have guided him – neither Mouseke, nor Barten, nor Drèze (who testified that it

was Gabszewicz who introduced him to GET). Who then could examine his thesis? A local mathematician in Leuven approved of it, but his judgement turned out to be incorrect. Questions of the core being at the center of the group around Debreu in Berkeley, Werner Hildenbrand, an associate professor at Berkeley, got a hold of a draft of this thesis, and found a mistake: the separation theorem only holds in finite spaces, but not in the space of all squared functions (L2), which has no inferior point. The need for mathematical expertise became apparent at CORE. Hildenbrand became part of the thesis committee, and Drèze managed to hire him starting fall 1968. The thesis was defended the same year, and Gabszewicz received an appointment upon graduation. With the arrival of Hildenbrand and Gabszewicz the golden years of Neo-Walrasianism at CORE began; five years that roughly matched the time of a substantial grant from the Ford Foundation.

It was during these years, in September 1970, that Drèze gave a presidential address at the Econometric Society. It resumed his own research interest as a blueprint of the CORE research program: decision theory in the line of Savage's *Foundation of Statistics*, French marginal costs pricing, Bayesian econometrics, and stochastic sequential optimization programs. That is, his speech pleaded for the complementarity of economic theory, econometrics, and programming thanks to one shared scientific interest:

> "At the cost of over-simplification, let me suggest that positive economics should assist in defining the set of acts (that is, the range of alternatives and their consequences conditionally upon the states); that normative economics should assist in defining the utility; that statistics and econometrics should bring empirical

observations to bear upon the assessment of probabilities; and that techniques of mathematical programming should assist in finding an optimal act" (1972: 2f.).

Though Drèze mentioned the Arrow-Debreu model in the context of uncertainty, general equilibrium theory was not yet a corner stone of his overall image of economic knowledge. But with the arrival of GET at CORE, as we will see in the next section, his belief in intellectual cooperation will be severely challenged.

The Golden Years of Neo-Walrasianism

Big politics mattered for the rise of CORE. In the second half of the 1960s, The Ford Foundation went through a major change when McGeorge Bundy became President and David Bell Vice President. This meant a shift in mission towards a strengthening of democracies around the world, specifically in Europe. Similar to the big grants that Ford gave to American think tanks such as Brookings or RFF, Ford wished to sponsor European initiatives in the social sciences. As Craufurd Goodwin, one of the board members working on a European Brookings, recalls: "Ford support was part of a general effort to make government more efficient and democratic in Europe, but CORE was not perceived as a particularly important part of this."[15] There was another Ford program headed by the young

[15] Message to the author, June 4, 2015. There is another interesting explanation of the Ford grant in Mirowski: "What was needed was something like RAND for Europe; but political considerations dictated that it could not be sited in England, France, or especially Germany, and the recruitment of ideal personnel was proving refractory. This situation provides the background for the formation of

business school dean from Pittsburgh, Marshall Robinson. He wished to modernize American business schools away from the case study method and toward the Carnegie Mellon model. Drèze's initiative would thus be the perfect fit for Ford. Drèze did believe in exactly the same objectives of the university, to render society and government more rational and democratic, that is, less totalitarian, less absolutist, and also less "hippy", as he would promote in his 1974 essay "les finalités de l'université". More importantly, his contacts with, and admiration for Carnegie Mellon would clearly attract Ford's attention. George Shultz from Chicago helped in setting up the deal, and CORE would win the grant: 400.000 U.S. dollars for the years between 1968 and 1973.[16]

The Ford grant might be considered the actual beginning of CORE as it allowed for a substantial increase of visits and positions that, in turn, increased its international reputation. In terms of age, specifically early stage scholars launching their career profited from this growth. In these years, CORE's faculty was extremely young - Drèze was in his early 40s and one of the oldest. In terms of fields, certainly all groups experienced growth and made important contributions, but the formative group that reconfigured research practices at CORE were mathematical economists: the time of the Ford grant coincides with the Golden Years of Neo-Walrasianism at CORE.

In fall 1968, the beginning of the third year of CORE, Hildenbrand arrived as university hire. He immediately connected to like-minded scholars. Visiting research

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[CORE] in 1965, conveniently located near the NTO directorate in Brussels, under the charismatic leadership of Jacques Drèze" (2002: 490).

[16] The Ford grant covered half of CORE's budget. The university matched the grant by providing new buildings and further six positions, three Flemish and three Francophone. As further funding sources, next to TIMS, CORE received funds from the then recent *Fonds de la Recherche fondamentale Collective*. Also the *Societés Reunies d'electricité du Bassin de l'Escaut* financed a project on the use of computers in business.

associate positions were given to several PhD students, to Truman Bewley from Berkeley, to Birgit Grodal from Copenhagen, and to Hildegard Dierker from Heidelberg (her husband Egbert came in spring 1969 for a visit). Another PhD student from Berkeley was Volker Böhm who has been present on and off since 1968. Wilhelm Neuefeind, Andrew Postlewaite, and Bernard Cornet would work on their PhDs as CORE visitors during these years. David Schmeidler belongs to this list, who came in 1970 after graduation under Robert Aumann in Israel, as well as Jerry Green who came for one year in 1971 after graduation under Lionel McKenzie in Rochester. This clique of young and mathematically excited students was supported by an important local mathematician at UCL, Jean-Francois Mertens who became research associate in 1968. He was a mathematician who met Debreu's standards, and would become for CORE what Steve Smale has been for Berkeley (Düppe 2012). Mertens, like the other PhD students were in their mid-20s, and writing up their thesis: Mertens graduated 1970 in mathematics under José Paris, Grodal 1970 under Werner Fenchel in Copenhagen, Bewley 1971 in Berkeley under Calvin Moore, and Volker Böhm received his PhD from Berkeley under Debreu in 1972.

Senior fellows arrived shortly after. In 1968/69, Karl Vind from Copenhagen came as Visiting Research professors. In 1970/71, Dieter Sondermann and Alan Kirman came as visiting professors. Alan Kirman, previously John Hopkins, was teaching with Waelbroeck at ULB, and stayed associated with CORE until 1974. Short visitors included David Gale, Reinhard Selten, Duncan Foley, and Herbert Scarf, among others. The year 1971-1972 might have been the peak of this wave when both Debreu and Aumann came as visiting research professors. Aumann was "immediately captivated by the dynamism of the place and the people". It became his "European home" (Aumann 1989: 5).

This was a critical mass of scholars that would establish the international reputation of CORE in economics. CORE became the mecca of mostly European trained but U.S. oriented mathematical economists that shared the same intellectual values as, and admiration for, one scholar, Gérard Debreu, and one book, his *Theory of Value* (1959). In Hildenbrand's telling words:

> "Let me recall the situation of mathematical economics in 1968. Mathematical economists at that time, and, I emphasize, in my view, were a well-defined group of people. Whatever was the academic background of the members of that group, the *absolute authority* in all questions of doubt was the *Theory of Value* … The consensus on the right methodological positions and the judgement what is important in mathematical economics created a unique atmosphere in the first years at CORE" (Hildenbrand, in Cornet and Tulkens 1989: 60).

Granting absolute authority to the *Theory of Value* is to say that questions regarding the mathematical structure of the general equilibrium model (rather than its economic content) were the center of excitement. Mertens and Hildenbrand, for example, worked on the "upper hemi-continuity of the equilibrium set correspondence for pure exchange economies" (1972). Or Debreu, jointly with Schmeidler, worked out a proof of the "Radon-Nikodym derivative of a correspondence" (1972). Mathematical economics did no longer mean the same as what Drèze once learned from Vickrey; it became quite distinct from economic theory. While mathematical economics used advanced mathematical techniques in order to axiomatize the mathematical structure of one general theory, economic theory

aimed at developing a plurality of theories of different market structures by mathematical tools. Drèze might not have signed Hildebrand's lines quoted above: "I never considered myself a mathematical economist. I am an economist, and of course you do need mathematics for much of what you do in economics".

The most important topic of these years was related to the concept of *the core* of the economy and its convergence with a competitive equilibrium. Core convergence was studied under conditions of economies with indivisible commodities, with a continuum of goods, and a continuum of agents, so-called "atomless" economies. Drèze was involved too. With Gabszewicz and Gepts he worked on the continuum of agents and commodities showing that in this case the core does *not* converge (1969). With the core being a game-theoretic concept, it was in CORE's womb of mathematical rigour that perfect competition modelling and game theory, initially opposed to one another, came closest in history.

There are several notes to be made about the impact of this wave on CORE's identity. Neo-Walrasian economics comes with an image of knowledge that, due to its axiomatic character, is discursively closed. Neo-Walrasian economists were the most exclusive community that existed at the time, diametrical to the times when 20 years earlier Activity Analysis whirred between mathematics, economics, business, and engineering (Düppe and Weintraub 2014b). Even if there is nothing within Neo-Walrasian economics that involves a hierarchy of forms of knowledge (it does not promote a methodological language to pronounce such judgements) it is almost incapable of inclusion, and in this sense *elitist*. Also the secretary, Weyers, understood this. She noted a "snobbism of abstraction compared to more applied work".

> "This group was a *bagarre de coqs*, a rooster fight. Everyone wanted to be considered the best … The atmosphere was tense because everyone wanted to be right, everyone wanted to impress Gérard Debreu. Gabszewicz and Hildenbrand were both very energetic about that."

The crucial implication of this intellectual attitude that one was not inclined to intellectual cooperation with other fields. None of the scholars listed above applied methods of operations research or econometrics. CORE was founded in the spirit of collaboration, but the research group that established its standing was incapable of being submitted to "complementary aims". Neo-Walrasianism was the beginning of the differentiation of disciplines at CORE, and thus in stark contrast to the integration of disciplines that Drèze presented in his Presidential address. The complementarities Drèze envisioned were hollowed out under the influence of Debreu.

The most visible sign of this change was the CORE seminar. Until 1969, there was one main CORE seminar, a Thursday seminar. Right after the arrival of Hildenbrand, a *specialized* seminar in mathematical economics was set up. It took only a year before the other fields did the same. Since then, there is a Monday seminar in mathematical economics, a Tuesday seminar in Operations Research, and a Wednesday seminar in econometrics, with only a minority of staff visiting more than one seminar. After this divorce of regular exchange, the differentiation of intellectual cultures between the fields advanced rapidly. Cooperation became not only more and more difficult, but also less and less necessary for the individual careers of the young researchers.

Note that even if this group was united by the adherence to Debreu's *Theory of Value*, there was not always agreement. Gabszewicz emphasized, in hindsight, his disagreement with Hildenbrand regarding the nature of perfect competition modelling. Having learned his economics from Drèze, the conditions under which there is *no* convergence of the core and a competitive equilibrium was for him a way to understand imperfect competition and uncertainty (Gabszewicz 1999, Drèze et al. 1972). But the extent to which such divergent epistemic interests were lived out is not obvious. They might not have been confronted at all given that mathematical discourse gives little space for debating one's intuitions. Yet to some extent, compared to the Berkeley Monday seminar, the CORE Monday seminar might have been more politicised. For those of the few scholars from other fields who visited the Monday seminar, mathematical economics was politically tinctured. They thought, like many outsiders did, that perfect competition modelling reflects free market institutions rather than, as Debreu did, an autonomous mathematical framework specific to economic theory in general. The econometrician Mouchart, for example, sometimes visited the Monday seminar. He knew measure theory and topology very well, but could hardly identify with how this group used it.

> "Economics, for them, was like a chess game of an old champion: fun, but not serious. The idea of proving that competition is efficient and improves the well-being of the population is just funny. For an econometrician this is simply contrary to observation… But saying so would have been a taboo. How could we object to Hildenbrand or to Debreu?" (Mouchart, personal communication)

Despite the potential debates about the nature of economic theory and the choice of methods, typical for the Neo-Walrasian community, there was little methodological debate (see Düppe 2011, Düppe and Weintraub 2014b). Again, Mouchart:

> "I recall a discussion with Gabszewicz about the interaction econometrics and economic theory. The issue was really the scientific status of economics – to what extent economic theory was a way of understanding observation and the real functioning of economic life, and how observation and modelling interact. Nothing. No response. Theory is theory."

One reason for why there was little debate, as Mouchart's quote suggests, might have been authority. Everyone knew Hildenbrand's negative attitude though superficial knowledge of econometrics but nobody would challenge it. Another reason was the official notion of complementarity between the fields as represented by Jacques Drèze:

> "If somebody works on programing, another on data, another on theory, why should they have methodological quarrels? I think it was accepted on the start that these disciplines brought together have their own relevance, and that they are complementary in many ways."

Potential debates, third, might also have been buried under a shared attitude towards the rest of the university. All permanent positions were formally assigned to the local departments (economics, mathematics, engineering, and the business school). While the

difference to the engineering department was rather small considering that engineering was a high status field in Belgium, the difference with the business school where graduate education was inexistent was immense. Also the economics department was only little different from what Drèze once got to know in Liège. During the first years, not a single time a member of the economics department was invited to become a member at CORE.[17]

The presence of an elite, the feeling of pulling at the same epistemic strings, and the shared attitude towards the rest of the university in a group of young researchers speaking English in a Flemish small-town was the soil for CORE's community spirit that everyone of this period enthused about. The exclusivity of mathematical economists was compatible with a highly convivial an inclusive atmosphere across fields. "I have no recollection of significant friction," Drèze recalled, "and regard the warm human relations among CORE members as an essential input to the success story" (2006: 3). Others testified similarly: "At Core there is a quality of intensity and excitement that is difficult to find elsewhere" (Aumann 1989: 120). The coffee lounge in the middle of CORE was an important point of informal conversation. Each fall there was a lunch meeting introducing new members, each winter a cooking contest of home-country dishes, and each summer a tennis tournaments – Hildenbrand played well, and sponsored a prize for mixed couples. The mostly female partners of the mostly male researchers would get to know each other in what Drèze called

[17] During the first years, Drèze had direct access to the rector regarding the hires of CORE members, but later had to negotiate every position with the single department committees. The shared attitude of CORE economists looking down on other economists increased the struggle over positions, responsibilities, and administrative workloads (a business professor faced higher teaching loads than an economics professor). As times passed, however, the differences disappeared and are no longer visible today. The first Louvain economist who became CORE member was Louis Phlips. It is common practice today. Yurii Nesterow, a Russian mathematician in operations research and active CORE member today happen to work with economists from the economics department rather than with CORE members.

"the lady's program" that his wife organized. When Wolsey arrived in 1971, there was a clique of young people quickly attached to each other: Gabszewicz, Robert Dechamps, Jean Philip Vial, and Etienne Loute. "The first day I arrived," Wolsey recalls, "the two secretaries showed me the studio; it was full of bottles lying around because the preceding visitor just left. The atmosphere from a social point of view has been very congenial. But scientifically it was different" (pers. communication).

The golden years of Neo-Walrasianism at CORE culminated in 1974 at an attempt to hire Debreu at CORE. Debreu considered the offer seriously and visited Leuven to discuss terms. At his arrival at the train station, he run into a crowd of students demonstrating for the departure of the French speaking part of the university to the French speaking part of Belgium. The uncertain future of the university, and the low salary could not be compared to what he had in Berkeley. Also, his family might not have been willing to move from California (Düppe 2012). Hildenbrand and Drèze were disappointed as they invested considerable effort into this offer. But Drèze knew how to consolidate: he thought that the presence of Debreu has no real consequences: "Debreu would not have made a difference; also Berkeley was not very different due to Debreu's presence." It is true that Debreu would not impose himself. But the preceding notes showed the indirect consequences of the presence of his students and collaborators for the identity of CORE.

From 1974 onwards, the enthusiasm about the *Theory of Value* that carried the late 1960s calmed down. The Neo-Walrasian research program arrived at its natural end when Sonnenschein, Mantel, and Debreu proved how empirically limited the axiomatic structure of GET is. For Hildenbrand and for Kirman these results were a major event; they turned to other methods. Kirman left CORE in 1974, and Hildenbrand gave up his by then half-time

appointment in 1975.[18] There were no new axiomatically oriented mathematical economists after that. Only Gabszewicz and Mertens stayed and regretted that many turned against the further axiomatization of economic theory. Specifically the problems of imperfect competition and power were still open for being structurally explored, which Gabszewicz did throughout most of his career.

Also, in 1973, the Ford grant ended. During the five years of Ford CORE expanded its permanent faculty from 6 to 21 members and had an average of ten visitors and ten research associates in residence. The university kept its promise given at the beginning of the Ford period that it maintains CORE at its *full* level of budget. When in October 1974, ULB discontinued funding after being asked for direct financial contributions, UCL showed its commitment to CORE and took over also this budget. As external funding, starting in October 1972, the Belgian government awarded a research contract on the development of computer programs for statistical decision-making and another contract on the analysis of *water pollution*. The European Economic Community funded the building of an econometric model of the EEC, and since 1974 the *Recherche Fondamentale Collective* funded a project on "theoretical aspects of externalities and public goods" and on "inflation in an international context". Passed were the times that "fundamental" was limited to questions like "the application of measure theory to the analysis of general economic equilibrium", for which Hildenbrand had received the same funds.

The five years of Neo-Walrasianism at CORE were carried out by scholars mainly scattered around Europe, but it was clearly a contribution to U.S. economics. There was no

[18] Bonn's presence would remain visible for some years through scholars such as Walter Trockel, U. Herkenrath, W. Neuefein, H. Föllmer, Volker Böhm, and S. Berninghaus.

other market for such ideas. Some European scholars appeared in fact more apt for advanced abstraction in economics than the average pragmatically oriented economist in the U.S. One might even say that at CORE the conditions for community creation of this group were better than at any place in the U.S. Considering the obstacles of the rather pragmatic sub-structure of American economics, without CORE, one might speculate if the Neo-Walrasian research program would have had the same impact on economics.

Disequilibrium economics

Once Drèze learned to appreciate GET, he would hold on to it for the rest of his life. He would later call it the second building block, next to Savage's approach to individual decisions, of his notion of economic knowledge (in Licandro and Dehez 2007). But he would clearly *not* identify with the exclusive purism of neo-Walrasian economists. Indeed, his approach to GET was markedly non-Walrasian. Given individual uncertainty, markets *cannot* be complete. Incomplete markets, *the* trademark of his and CORE's economic research during the first half of the 1970s was meant to be that form of economic theory that meets Drèze's notion of the compatibility of CORE fields. Missing markets, non-convexities in production, information problems, monopolistic competition and other market structures that deviate from the so-called Arrow-Debreu result in price rigidities. Perfect competition, for Drèze, is only one among many cases in which GET can be applied. Economic theory at CORE became a combination of the standards set during the Neo-Walrasian years with the belief that they can be equally fruitful applied beyond the

"dogma" of perfect competition. CORE would stand for a specific approach to macroeconomics. How did this happen?[19]

During the early 1970s so-called disequilibrium economics - models in which prices are not flexible or simply fixed - prospered at many places. Instead of price adjustments, quantity rationing describes market dynamics, allowing for an underemployment equilibrium. The U.S. battlefield was Stiglitz and Solow (1968), followed by Barro and Grossman (1968), in turn influenced by Clower and Patinkin (see Backhouse and Boianovsky 2013: 64 ff.). But also more mathematical theoreticians such as Roy Radner were open for a re-interpretation of the role of prices in GET (1968).[20] But this U.S. literature might have remained an intellectual pastime of history if it had not become influential in Europe, more specifically, through an emerging Paris-Berkeley-Leuven triangle between students and collaborators of Malinvaud, Debreu, and Drèze. Modern French economic theory was born in this triangle of *disequilibrium economics*. What makes this literature distinct is the simultaneous vicinity and distance between Neo- and Non-Walrasian economics.

The first who took up fixed prices with quantity rationing in the French-speaking context was Yves Younès in a seminar in January 1970 at CEPREMAP (*Centre pour la*

[19] It must be noted that CORE also became known for its research in public economics. It began with Drèze when he jointly with de la Vallée Poussin (1971) developed a mechanism for eliciting preferences resulting in the optimal allocation of public goods. Edmond Malinvaud did the same (1971) such that the mechanism came to be known as the Malinvaud Drèze de la Poussin mechanism. Drèze's young economists such as Henry Tulkens, D'Aspremont, but also Pestieau got interested in public economics too. D'Aspremont, who did his PhD at Stanford's business school with Robert Wilson, also introduced social choice to CORE.

[20] The mention of Radner is important; Radner stands for the breakup of the general equilibrium community in the U.S. between the NSF general equilibrium conference and Radner's decentralization conference series.

recherché économique et ses applications). But the paper was missing an existence proof, and it would not have been published before Younès was visiting Debreu in Berkeley in spring 1973 who gave him the tools to do so (Younès 1975: 493). Younès wrote that Malinvaud should be considered a co-author of the paper. The referee of this paper was Jean-Michel Grandmont.

Grandmont graduated from Berkeley with Debreu in 1971 on the notion of a "temporary equilibrium" (1970), and naturally would become a visiting research professor at CORE in 1972 while being hosted at CEPREMAP in Paris.[21] He would apply his notion of temporary equilibrium to monetary theory (1974). He published on these themes with both Kirman and Hildenbrand, but also with McFadden from Berkeley. Grandmont was visiting CORE in 1972/1973. Discussing these themes, he wrote another fixed-price model jointly with Guy Laroque (1976). It put forth a notion of monopolistic competition, which is still one identifier of the French style in macroeconomic theory today.

It was in 1971, as Drèze emphasized, that he wrote his most well-known contribution, his existence proof of an general equilibrium with price rigidities that was published not before 1975 and that came to be known as the "Drèze equilibrium". The paper had been rejected as CORE discussion paper, and was accepted only after he considered suggestions from Grandmont (see Backhouse and Boianovsky 2013: 111). It was also only due to Grandmont that Drèze saw the relevance of this proof for Keynesian themes. Drèze himself wanted to "treat the incomplete markets with some generality", and

[21] The *Centre pour la Recherche Économique et ses Applications* (CEPREMAP) was founded in 1967, one year later than CORE. It was closely related with research on economic planning in France, and less academically oriented than CORE.

thought of wage rigidities as a result of "income insurance". Other models simply assumed fixed prices.

Also Jean-Pascal Benassy requires mention as a member of this triangle. Hosted at CEPREMAP, he came to CORE in 1973 giving a talk on the "neo-Keynesian disequilibrium theory in a monetary economy". Benassy submitted his thesis in Berkeley in 1973 that involved next to Debreu also Hansen, called *Disequilibrium Theory*. For Benassy, it was important to overcome the fictitious Walrasian auctioneer who chooses prices, which leads equally to quantity rationing and fixed-price models (1975). Disequilibrium economics, for him, is "economics without the auctioneer" (Backhouse and Boianovsky 2013: 121).

In these years, the share of visitor from France, mostly CEPREMAP, increased considerably at CORE. Paul Champsaur from INSEE (*Institut national de la statistique et des etudes économiques*) visited CORE in 1973 for a year, and Jean-Jacques Laffont with a PhD from Harvard in 1975. Other important visitors were Claude Henry, Paul Levine, Roger Guesnerie, and Tierry de Montbrial. It must have been noticed at the coffee breaks of the Monday seminars that one could speak more and more French, and more and more "Keynes". Times of "upper hemi-continuity sets" were over.

This literature culminated in Malinvaud's 1977 book *Theory of Unemployment reconsidered*. This book became so important for Drèze that Gabszewicz spoke of a "Malinvaud turn" at CORE, which did not please everyone. "I wrote a paper about this as a favour to Jacques. I liked uncertainty, but not disequilibrium. Drèze kept on saying to me that this is the most interesting thing." (Gabszewicz) With Malinvaud's presence, also the French academic hierarchy intruded CORE, which young researchers like Gabszewicz did

not approve. CORE was never elitist in the same sense as an Ecole Normale was. Also, Grandmont and Benassy had issues with Malinvaud over granting proper credit to the ideas in Benassy (1975).

It is not here the point to review this literature further (there exist excellent surveys starting from Weintraub 1977 to Backhouse and Boianovski 2013). In our context, it is interesting to ask what happened to the CORE project of modernizing European economics. Note that disequilibrium economics was motivated *from within* the Neo-Walrasian research program, trying to reform it in face of its limitations (aggregation, uniqueness, etc.). We have seen the central role played by Debreu for almost every single contribution even if he himself never contributed to this literature.[22] Many of Debreu's collaborators and students believed that the real benefit of their master's work are to be found in macroeconomics while giving up some of the Walrasian benchmarks such as the tatonnement process and the neutrality of money. Disequilibrium economics was a way of bringing mathematical economics back to economic theory.

Macroeconomists in the U.S., instead, did never really go very deep into the internal problems of Neo-Walrasian economics; they rather applied it with brute force. French macroeconomics remains more theory oriented, and the difference between microeconomics and macroeconomics is less developed than in the U.S. One of the reasons for this lack of reflection is indeed that some of the older generation of U.S. economists could not follow the mathematics! Solow, for example, regretted that the literature was not given fair treatment, and Patinkin admitted that he could not read it (see Backhouse and

[22] For Debreu himself, disequilibrium economics was a misnomer: "When you are out of equilibrium, you cannot assume that every commodity has a unique price because that is already an equilibrium determination" (quoted in Weintraub 2002: 146). Malinvaud took the same position.

Boianovsky 2013: 122). There was no send back option, as it were, when importing Debreuvian rigor to the continent. Or perhaps Debreuvianism was never really American in the first place? To be sure, Lucas did swear on Debreu, but he did so from afar and without Debreu's applause. Malinvaud and Drèze, instead, would be referred to by Debreu as macroeconomic applications of his theory.

Another reason why these models had less success in the U.S. than Lucas-Prescott type of macroeconomics, was that in France the demand for economic knowledge came from planning institutions and the management of public goods. Drèze's model had implications for unemployment that was an archetypal continental topic. When his model was subjected to econometric testing in ten different European countries that had inspired economic policy for years, it received rather critical reviews in the U.S.

Non-Walrasian macroeconomics being a marginal phenomenon in the U.S. but prevailing in France, it moved CORE away from U.S. economics. Indeed, only a few macroeconomists from the U.S. visited CORE in the second half of the 1970s. CORE became a trademark for a specific style, if not school in macroeconomics. Even if today's macroeconomics in Europe and the U.S. is DSGE modelling all way down, notions such as incomplete markets, including multiple equilibria still stand for a French accent in macroeconomics. It was by means of disequilibrium economics that modern economics learned French.

The other CORE

Non-Walrasian economists could not reverse the tendency to less cooperation between the fields at CORE that was initiated by Neo-Walrasian economists – even if it was more open for econometric testing. The influence that CORE had on economics was, ironically, an economics that became increasingly disconnected from operations research and econometrics. Given that the fields represented at CORE became more and more differentiated, it is no surprise that CORE is not only known for its economic research mentioned in the preceding two sections, but, independently and unknown to most economists, earned a reputation in mathematical programming and in econometrics. In contrast to the early Cold War in the U.S., where modeling practices prospered across the disciplines, continental Europe did not create sufficient pressure for the fusion of disciplines. When Drèze got to know the three CORE fields in 1954 it was very natural to bring them together as they were interlinked specifically through shared military funding. When activity analysis had its heydays, all three fields spoke the same language; but questions of "atoms that are not 'too' big" in mathematical economics would find little echo in applied mathematics, statistics, or engineering departments. The link between the disciplines at CORE, therefore, was of an *abstract* kind: a unity of a shared methodological vision that somewhat never really became manifest. How could Drèze have anticipated that advanced mathematical economics, once at the margin of economics, would change the core of the discipline, that operations research would move to business economics and engineering, that mathematical programming would become applied mathematics, and parts of econometrics would be taken over in mathematical statistics?

As of operations research, CORE had an immense influence on the field thanks to the presence of George Nemhauser and the two young local hires, Laurence Wolsey and

Jean-Philip Vial. When Wolsey did his PhD at MIT at the center for operation research in the late 1960s, CORE had already a name. In the 1970s, CORE achieved "mythical status" in European operations research, Wolsey witnessed, known for its contributions to discrete optimization and integer programming, and later also queuing theory and supply chain management. Nemhauser came for a first visit in 1969/1970, and later was the first outside research director between 1975 and 1977. He attracted many visitors, the best in discrete optimization at the time: Eric Denardo from Yale, Peter Hammer from Montréal, Jack Edmonds from the National Bureau of Standards, Tom Magnanti from MIT, Mike Todd from Cronell, Bob Jeroslow, Bob Bland, Bill Pulleyblank, and Rick Giles – these were stars at the time, and none of them would be related to the economics described in the preceding two chapters. They also brought their students, some of whom such as Gerard Cornuejols became equally stars in the field. Drèze, unbroken in his belief of intellectual complementarities, also contributed to it (Drèze and Moeseke 1974).

Operations research represented the minority at CORE with economists representing roughly the half of all positions. Being the smaller group, the atmosphere in operations research was different from economics, according to Wolsey: "Economics and operations research are different in the sense that, the atmosphere in the economics conference can be very aggressive. In OR, there is little competition. Our community is remarkable; we tend to get on well" (personal communication). In turn, optimization did not really interest the economists – which was impossible to anticipate in the early 1950s. When Wolsey got once interested in pricing of non-convexities, he had to learn that the two groups speak very different languages.

The history of econometrics at CORE is different. Coming from Savage and his Bayesian methods, Drèze, when teaching econometrics in the early 1960s, extended the simultaneous equations analysis of the Cowles Foundation to the more flexible Bayesian approach. Bayesian econometrics would become another trademark of CORE. For some time, CORE was the most active place in the world on Bayesian econometrics – known to some as the "Belgian Bayesian School". Drèze, too, remained active in this field (1976). Though little effect on econometrics at large, it is still used in financial econometrics.

Bayesianism was not an economics brand, but comes from logic and mathematics. Therefore, the profile of Bayesian econometrics did not only mean to represent a "school" among econometricians - which are foremost working at economics departments - but also was an open door to mathematical statistics. The two central CORE econometricians who advanced this agenda were the two Belgians Michel Mouchart and Jean-Francois Richard. Both began their career as PhD students at CORE in the late 1960s, and received their appointment as associate professors after graduation in 1973. With the two of them, there emerged an interest in the axiomatic foundations of Bayesianism that was hardly done at economics departments, even if it was open for axiomatic foundations of its own, GET.[23] After graduation, Mouchart published "A note on Bayes Theorem" (1976) that initiated a research program that culminated in the book, written jointly with the mathematicians Jean-Pierre Florens and Jean-Marie Rolin to appear not before 1990. Economists should have appreciated their use of measure theory, "fundamental" in Hildenbrand's sense. But for them, the use of econometrics required ad-hoc assumptions that are theoretically not

[23] One obvious encounter of economics and econometrics were computable general equilibirum models that became popular via the work of Scarf. Scarf shortly visited CORE in 1969 and later gave a lecture series.

legitimate. There was thus a clear stigma of econometrics being merely applied, which made interaction difficult: “Gabszewicz told me that econometrics was applied economics, and in the mouth of CORE people applied economics meant second rate economics” (Mouchart, personal communication).

One reason for this low esteem of econometrics might have been the need for delineating CORE from statistics. When Drèze arrived at Leuven, the quality of statistics as a scientific discipline, and in particular the teaching of it at the university, was low. Representing econometrics, he had to delineate his theory oriented research from the often merely descriptive ambitions in various statistics sections of the university. This attitude marked off his students. When the rector would ask CORE to take over teaching in statistics, repeatedly, Gabszewicz and Drèze would decline with reference to CORE being a center for economics.

> “At the end of the 70s there has been a meeting. The director of the university has asked all teachers of statistics of the university to meet together. We realized at that time there were like 30 to 40 people teaching statistics with practically no research in statistics, except at CORE. Most courses of statistics were not taught by statisticians. Then we had the idea that there was no systematic teaching of statistics and that we should think of forming a new department of statistics.” (Mouchart, personal communication)

But when Mouchart and Richard reoriented econometrics to statistics as applied mathematics, statistics had indeed a very high research quality. And so, while noting more

and more barriers with economists, they noted more and more complementarities with statistics and its mathematical foundations. At last in 1991, these developments resulted in the creation of an institute for statistics outside of CORE (though next door), the *Institute of Statistics, Biostatistics and Actuarial Sciences* (ISBA) founded by Leopold Siemar, Jose Paris, Jean Marie Rolin, and Mouchart. Only one econometrician did not leave CORE to join the new institute, Luc Bauwens.[24]

Notwithstanding, the founding spirit of CORE – the faith in the complementarity of various modeling techniques – remained CORE's master narrative. In the foreword of the 20th anniversary volume of CORE that entailed contributions from all fields, Cornet and Tulkens once more evoked this faith.

> "The rationale for gathering the chapters in this way, rather than publishing them in various specialized journals, is to promote interactions among economic theory, game theory, econometrics, and operations research – interactions that occur through the common use of mathematics. All of these fields belong to the decisions sciences, a domain of knowledge in which both mathematical concepts and mathematical language are the common thread supporting the reasoning" (xi)

The preceding notes have qualified this remark. That technical approaches would be a unifying force of the so-called "decision sciences" might have been true during the Cold

[24] This division, however, is only relative: jointly with this institute and the *Institut de Recherche economiques et sociales* (IRES), CORE today constitutes a broader unit of technical advances in the social and other sciences, called *Institute for Multidisciplinary Research in Quantitative Modelling and Analysis* (IMMAQ).

War at RAND and has been in fact the rationale for the creation of CORE. But its history, from the very start, is not adequately described by it.

The diffusion of CORE

After the first ten years of CORE, in the summer of 1977, CORE moved with UCL to Louvain-La-Neuve, 30 km south of Leuven, a town created because of this very separation of the French and Flemish speaking part of the university. KUL stopped sponsoring CORE already since October 1975. Nemhauser returned to Cornell, Hildenbrand resigned from the CORE Board, and Gabszewicz became the new research director. Pierre Pestieau, after 10 years in the U.S., came back to CORE and observed the following:

> "I felt that compared to the first years of CORE, the center was more mature, hosted more people, but at the same time I had the feeling that the golden years were behind. Given that I was never interested in GET, I did not miss anything. D'Aspremont, Mertens, Gabscewicz were representing the elite, even though others published more."

Also Drèze noticed the difference between the founding years and its establishment, again in comparison with Cowles: "For Cowles in the U.S. as well as for CORE in Europe, there was an early stage of unique distinction, progressively diluted as the same combination of

interests became more widespread – a natural and entirely desirable development" (2006: 2).

One reason for this dilution was certainly that CORE was for the majority of early members a temporal experience that helped launching their career mostly in their home countries. Only Belgians and those who grew up at CORE from young age would refuse offers from elsewhere (Gabszewicz refused an offer from Stanford). This meant that CORE had to renew itself continuously, but also that CORE standards quickly spread to other institutions all over Europe. CORE became very soon a model for imitation for many other centers, mainly founded by previous faculty members or visitors of CORE all over Europe.[25] Economic research in Europe, at the latest since the 1980s, is model-based research done in research teams. CORE was at its beginning.

Academic reproduction takes place not only by imitation, but also by teaching. Discussions between Drèze, David Hendry from LSE and Hildenbrand from Bonn led to the creation of the *European Doctoral programme in Quantitative economics* that was launched in 1978. Later, the program would include also the *Ecole des Hautes Etudes en Sciences Sociale* in Paris, DELTA, ENSAE, CREI, the European University Institute in Florence, and Tel Aviv University as exchange partner. CORE, last, was vital for another

[25] In 1975, CEME (*Centre d'économie mathématiques et d'économétrie*) in Brussels by Jean Waelbroeck; in 1985 the SFB303 (Sonderforschungsbereich *Information und die Koordination wirtschaftlicher Aktivitäten)* in Bonn by Werner Hildenbrand, in 1988 CentER (*Center for Research in Economics and Business*) at Tilburg with Barten as first president, also in 1988 DELTA (Département et Laboratoire d'Économie Théorique Appliquée) at École Normale Supérieure in Paris, in 1991 IDEI (*Institut D'Economie Industrielle*) at Toulouse was founded by Jean-Jacques Laffont, also in 1991, ECARES (*European Center for Advanced Research in Economics and Statistics*) at the Free University of Brussels, in 1994, CREI (*Centre de Recerca en Economia Internacional*) at Pompeu Fabra University in Barcelona, also in 1994, GREQAM (*Groupement de Recherche en Economie Quantitative d'Aix-Marseille*) at Aix-Marseille was by Louis-André Gérard-Varet (at CORE during his PhD between 1971-74).

important milestone of the transformation of continental economics: the *European Economic Association* founded in 1985 after an initiative of Gabszewicz, Louis Phlips, Jacques Thisse, and Jean Waelbroeck. Drèze was its first president and CORE hosted the secretarial office.

Note that for all these sites, CORE's melange of disciplines beyond economics was not reproduced. However, for all these centers both the appreciation of abstract economic theory, as well as the belief to capture essential institutions in models deviating from the standard Arrow-Debreu model prevails. The reverse migration from the U.S. to Europe led to an Americanization of economics (e.g. Maes and Buyst 2005), but it was also an appropriation. Continental economics is different.[26] Specialization and fragmentation that dominate the history of science in the second half of the twentieth century must not correspond with an international homogenization. Local cultures prevail.

The preceding narrative has also shown that it was not only specific ideas, but mainly a new form of the organization of research that was introduced to Europe: research teams, co-authorship, peer review, among others. Drèze has never seen the link between formal modelling techniques and the today prevailing organizational culture of economics that I have argued for:

> "*We could have been doing economic history. We could have, and it would have had the same impact*. Of course we were in a branch of economics that was about to

[26] For the case of Germany, one might want to specify: When Hildenbrand came back to Germany and influenced German economics, it was not at all in disequilibrium models – a difference that is still felt today between French and German macroeconomics (see Düppe forthcoming).

develop and we added an interdisciplinary dimension by bringing people from operations research and game theory together with economists." (emphasis added)

Jacques Drèze, in the late 1970s, became a celebrated figure: In 1976 he became honorary member of the American Economic Association, in 1978 foreign honorary member of the American Academy of Arts and Sciences, in 1980 foreign member of the Royal Netherlands Academy of Arts and Sciences. At the time of his retirement in 1989, Drèze realized a long-year wish, a one-year sailing trip with his wife around the Atlantic. After that, he thought about launching a second career. He considered working in development, for the poor, as his son did, or to go back to research. He opted for the latter with the hope that his research might benefit the poor, too. Thus, he continued research at CORE for another 25 years and not ending.

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