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**PREFACE**

As some of you may already know, I have accepted the position of Research Professor in “Growth and Coordination” at the University of Groningen. I will continue my directorship of NAKE until the end of the millenium, however, which is why the NAKE Secretariat has also moved to Groningen. We thank **José Dijkzeul** for her tireless efforts on behalf of the NAKE students and fellows and wish her all the best for the future. Jose will continue to work at the University of Amsterdam, where they are now looking for a new position for her.

“The queen is dead. Long live the queen!” With the move to Groningen we not only have a new location but also a new secretary. Ms. **Janna Mesker** has agreed to be the new NAKE secretary. She has commenced her new job on March 1, and her address and further details can be found on the inside cover page and below. Janna will combine her job with the secretariat of the LNBE (the business economics network). In doing so we hope to achieve a further streamlining of the activities of NAKE and LNBE. Janna was already involved in two joint NAKE-LNBE Workshops, viz. the December 1995 Utrecht workshop on “The economics of finance” and the June 1997 workshop in Groningen. She thus brings a considerable amount of experience to the job.

In this *NAKE Nieuws* you find the best reports on two (of the three) lecturers of last December’s workshop in Nijmegen. **Luc Moers** (University of Amsterdam) gives his impressions on ‘The Methodology of Empirical Macroeconomics’ by Kevin Hoover (Universit of California, Davis). Last but not least, **Theo Leers** (Katholieke Universiteit Brabant) writes on ‘Modelling Bounded Rationality’ by Ariel Rubinstein (Tel Aviv University).

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### **COURSE QUESTIONNAIRE 1998/1999 UTRECHT PROGRAMME**

In the middle section of this *NAKE Nieuws* you find a removable course questionnaire plus a listing of all courses that are offered by the Fellows of the network. Between 16 and 24 of these courses can be scheduled for inclusion in the Utrecht teaching programme for 1998-99. In order to design a well-balanced programme, it is important for me to know the potential clientele for the different courses. For this reason I would like to ask all (potential) participants to fill out the questionnaire and return it to the NAKE secretariat **before May 1, 1998**. The outlines of (almost) all courses can be found on the NAKE Home Page. Suggestions for new courses are, of course, also welcome. The new teaching programme for the academic year 1998-99 will be announced some time in June 1998 on the NAKE Home Page.

**Ben Heijdra**

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## Kevin Hoover

### The Methodology of Empirical Macroeconomics

Report by Luc Moers, University of Amsterdam

#### 1. Introduction

This is a report on the lectures given by professor Kevin Hoover (University of California, Davis) during the Netherlands Network of Economics workshop in Nijmegen, 8-12 December 1997. Hoover tried to show what methodologists can teach (empirical macro)economists.

As an introduction, his first lecture was devoted to a paper by Pissarides (1992), to illustrate some methodological problems from the perspective of a specific example which can be considered very typical of ‘state of the art’ macroeconomics. A first very typical aspect of this paper is that it starts from a ‘stylized fact’, observed in the real world, in this case the observed persistence in the time-series data of unemployment. That is: deviations from trend unemployment tend to be serially correlated and therefore unemployment shocks tend to persist for long periods of time. The key idea of the paper is that unemployed workers lose skills while unemployed, which gives rise to a ‘thin market externality’. Thus, a shock which increases unemployment makes the average skill level of the pool of available workers drop, this lowers the expected profits of hiring a worker, which in turn leads firms to decrease their job supply and the original shock to unemployment to persist. As is often the case in economics, the idea is obvious, but its formalization less so (which makes that Hoover sees some truth in the joke that ‘an economist is someone who looks at the world, sees something that is true, and asks whether it would work in theory’). To formally explain the stylized fact, Pissarides (1992) employs an overlapping-generations model, with cohorts of identical agents who live for two periods, where searchin workers and firms are randomly matched once. Very typical is also that this is an explicitl microfounded optimization model with representative agents, and that very simple (search) functions must be assumed to be able to solve the model at all. Finally, some empirical implications are treated in the paper. Here, it is very typical that the empirical equations are not strongly implied by the theoretical ones, that a causal structure is imposed, and that the testing itself is not done.

It is not hard to think of some methodological problems with the above, and this is exactl what Hoover did. He gave an impressive list of methodological problems in the subject of macroeconomics which can be found using this specific paper (working ‘bottom-up’). Getting

back to it every now and then, in his next three lectures, he went more deeply into the following categories: firstly, problems concerning macroeconomic laws; secondly, problems concerning microfoundations of macroeconomics; and thirdly, problems concerning causality in macroeconomics. In the final lecture he came around with a general overview of the most important 20<sup>th</sup> century contributions to the philosophy of science properly, which has wider implications for all scientific subjects, including macroeconomics (working ‘top-down’). This line of structure will be followed in the coming sections, after which some concluding remarks will be made.

## **2. Are there macroeconomic laws?**

To be able to treat the question whether there are laws in macroeconomics, in his second lecture, Hoover started off with a short summary of the traditional view of explanation in the philosophy of science. This view is called the ‘deductive nomological explanation’ (DNE) or the ‘covering law model’ (ascribed to Mill, as so many in the past both a philosopher and an economist). It has a definition of a law (Greek: ‘nomos’) as ‘a true universal generalization wrapped up with counterfactuals and subjunctives’. Counterfactuals imply that if A were (had been) the case, then B would be (would have been) the case, where in fact A is not (has not been) the case. Subjunctives imply that if A should come to pass, then so would B, where it is left open whether or not A will in fact come to pass. Essentially, it is the wrap-up that distinguishes universal laws from accidental generalizations. Hoover brushed the problem of what are suitable counterfactuals and subjunctives aside to define a scientific law as ‘a true universal generalization which is unaccidental’. In the DNE then, there is an ‘explanandum’ deduced from ‘explanans’ by a law, which can also be cast in a probabilistic framework.

Hoover went on referring to Cartwright (1997), who emphasizes that a law is a universal generalization valid only under special circumstances, which need to be specified instead of just assuming ‘ceteris paribus’. To her, a true generality is an unwarranted leap of faith. She stresses that we need ‘nomological machines’, which generate laws. They are established arrangements of different, but related capacities and specified shielding conditions which give rise to the regularities. In this view even for example Newton’s law is something having capacities which are different under different special circumstances. Applying it to macroeconomics, Cartwright (1997) argues that here too there are nomological machines and that macroeconomic models are blueprints of these. This methodology is echoed most extremely in the real business cycle approach (RBC), as for example illustrated by the following statement from Kydland and Prescott (1996, p. 83): ‘Earlier in this article, we quoted the Lucas (1980) definition of ‘theory’ as being an explicit set of instructions for building an imitation economy to address certain questions and not a collection of assertions about the

behavior of the actual economy. Consequently, statistical hypothesis testing, which is designed to test assertions about actual systems, is not an appropriate tool for testing economic theory’.

Hoover showed to have problems with this account. According to him, nomological machines may exist in macroeconomics, but models like in the RBC literature or Pissarides (1992) are not their blueprints, because they are too far from reality. The usual reply to this is of course that these models need to be seen as metaphors, ‘as if’ the economy behaved like them. Hoover argued that this cannot do, however, because it leaves open how a particular model relates to rival models and to the empirical data. Actually, the standard regression diagnostics check whether the data came from some nomological machine. Although economic language suggests a lot of laws (law of demand, Engel’s law, Okun’s law etcetera), Hoover thinks they are less strong than the laws in for example physics, or the exact sciences in general, for that matter. His basic point is that in economics the relationship between stylized facts, theoretical models and empirical tests is much looser than in the exact sciences. Showing clearly that ‘Pissarides’s (1992) equations are surely no laws and have an exceedingly loose relationship to the data’, he illustrated this nicely with a comparison to the covering law model of falling objects. Therefore, Hoover concluded this lecture dubbing economic laws ‘less like laws and more like robust empirical generalizations’.

### **3. Does macroeconomics need microfoundations?**

Starting his third lecture with a quote from Lucas, who ‘looks forward to the day when we no longer have the prefixes micro and macro’, Hoover questioned the current attitude that all good macroeconomics must be microfounded. Some examples: Pissarides (1992) clearly shows a macromodel which simply looks like a micromodel and ‘micro is now even infiltrating undergraduate macroeconomics textbooks and policy models’.

Hoover followed the historical path along which this situation has been arrived at. The earliest economics is ‘home economics’, practiced by the Greeks. However, the earliest name for economics as the subject known today is ‘political economy’, which is largely macro. As an illustration, Hoover noted that ‘everybody knows of the ‘invisible hand’ in Smith, but it is actually very hard to find inside *The wealth of nations*’, arguing that ‘individualistic’ claims are not that strong there, or at least not as strong as others have interpreted them. Also, the earliest empirical work in economics is largely macro. In the middle of the 19<sup>th</sup> century there was a discernible shift towards a more individualistic approach, but the focus was still more on markets as a whole. The final shift of emphasis towards individualistic economics, according to Hoover, occurred only at the end of the 19<sup>th</sup> century, in French writings. Subsequently, the discussion about the relative explanatory power of micro and macro started to develop. Many economists adopted ‘methodological individualism’, saying ‘there is no well-grounded explanation of

macrodata without modeling individual behavior' (although ironically, at the time, most Anglo-Saxon economists did not really choose between macro and micro). By 1930 most economics was micro. In the 1930s, of course, in Keynes there was the beginning of modern macroeconomics. Hoover emphasized that Keynes actually talks a lot about the micro level, but only in service of macro explanation: 'in essence, Keynes stressed heterogeneity'. Because micro still dominated, it was only natural that economists started to wonder about the relation between micro and macro in Keynes, which ended in the current 'reduction of macro to micro'.

Hoover nicely showed that this micro versus macro discussion has analogies in other sciences. In physics for example there is the quantum mechanics (micro) and the relativity theory (macro), with no unified theory explaining both levels and thus no clear relation between them. In biology for example the relation between mind (macro) and brainstates (micro) has also not been resolved. The last example is kind of ironic to economists, 'for biologists want to reduce mind to brainstates, because they do not want 'intentional states' to be fundamental at the micro level, while economists want to reduce macro to micro, because they want intentional states to be fundamental at the macro level too'. This shows most strongly in the 'Lucas (1976) critique', arguing that an economic model can only be stable if it is built in a general equilibrium context around the 'deep' parameters, tastes and technology.

Hoover argued that this approach is not so much based on methodological individualism as its proponents argue, but more on 'ontological individualism', saying 'the only real entities in economics are individuals'. 'The fear is that if gross domestic product (GDP) is modeled as independent from the fact that it comes from individuals, this somehow means logical independence between GDP and individuals'. Hoover stated this is an unwarranted fear. For one thing: different distributions of individuals can go with the same GDP. Borrowing the term 'supervenience' from biology, he explained macro as supervening on micro, leaving room for macro entities to behave differently than micro entities, but acknowledging a relationship between the two. The most fundamental aggregation problem cannot be ignored. Drawing on Kirman (1992), Hoover showed that the conditions needed to be able to aggregate micro to macroeconomics are so restrictive that they will be never fulfilled in reality. As an example, micro Cobb-Douglas maximization does not tell anything about macro Cobb-Douglas maximization and the other way around.

Thus, Hoover argued that 'the representative agent model does not solve the problem, but rather ignores it: the modern representative agent is essentially a homogeneity assumption'. As an 'idealization' it may work if distribution effects are not so important, but this can only be known empirically. There is then no principal logical necessity to microfound macromodels and certainly not to accuse all macro that does differently of 'ad hocery'. 'What happens to micro matters for macro, but macro has its own analytical aspects: macro cannot be euthanized like Lucas would like to see'.

#### 4. Causality in macroeconomics

The final category of methodological problems in macroeconomics which Hoover illustrated with reference to Pissarides (1992) as a specific example concerned causality. Pissarides (1992) puts an implicit causal structure in his model, which is however not tested for. For ‘the man in the street’ causality usually implies some sort of control over an instrument, which causes something else to happen. However, economists generally do not identify causality with control, but rather with ‘Granger causality’, in essence just a probabilistic form of ‘post hoc ergo propter hoc’. Hoover declared beforehand that he is sympathetic to the view of the man in the street.

According to Hoover, the most influential account of causality in the history of philosophy is with Hume (just as Mill both a philosopher and an economist). Hume argues that the only things we sense are immediate sensations or changes of past sensations, that is: all knowledge is empirical. He views a cause as continuous, as preceding effect, and as necessarily connected to that effect. To him a cause is not in the object, but only in the human mind. Hume’s account of causality is deterministic and only admits probabilities as a matter of ignorance of the mind. He sees law-like relationships with robust correlations as causes. But any econometrics textbook of course always warns that correlations are no causalities. A causality is symmetrical and transitive, but a correlation is symmetrical and intransitive. Simon and Rescher (1966) stress the ‘rock of contraposition’: if A implies B, then the negative of B implies the negative of A. This does if A causes B, but not if A only correlates with B, and of course fits with the common-sense view of a cause as ‘what makes things happen’. This is certainly not in Hume, but that is a logical consequence of his position that all knowledge relies on sense impressions. If you deny the latter, and Hoover would do just that, you need not go along with Hume. Moreover, a careful reading of Hume, according to Hoover, shows that his view of causality as a philosopher is not his view as an economist; as the latter he did take the exact same position as the man in the street. This goes nicely with Granger’s (1980, p. 331) statement that ‘... philosophers are not constrained to look for operational definitions and can end up with questions of the like: ‘If two people at separate pianos each strike the same key at the same time and I hear a note, which person caused the note that I hear?’ The answer to such questions is, of course ‘Who cares?’’.

Realizing that we live in a probabilistic world, nowadays there is a strong tendency to work with probabilistic accounts of causality. A first thought that comes to one’s mind is that A causes B if  $P(B|A) > P(B)$ , where for example A is ‘taking an aspirine’ and B is ‘headache ends’. Hoover worked from this thought, carefully showing the restrictions needed to be put on it, to get to a more proper probabilistic account of causality. For a start, the above is observationally equivalent to  $P(A|B) > P(A)$ , which is of course a very unattractive position. Therefore, it is better to say that A causes B if  $P(B_{t+1}|A_t) > P(B_{t+1})$ , because this is not observationally equivalent to  $P(A_{t+1}|B_t) > P(A_{t+1})$ . This is at the heart of Granger causality. From here Suppes (1979) defines: ‘if A precedes B and

$P(B_{t+1}|A_t) > P(B_{t+1})$ , then A is a prima facie cause of B'. However, Hoover stressed, there are still problems with this account, mainly the fact that common third causes can exist. As an example: the money supply rises during Christmas sales, before Christmas, but of course money-supply growth does not cause Christmas. To overcome this sort of problems there is the 'common cause principle': Suppes's (1979) definition must be restricted further with  $P(B_{t+1}|A_t+C_t) = P(B_{t+1}|C_t) > P(B_{t+1})$ , so that C 'screens off' A. But this still leaves third interfering causes possible. Again Hoover used an example. From chemistry it is known that ceteris paribus taking birth control pills increases the chance of a thrombosis. At the same time taking birth control pills reduces the chance of pregnancy though, and pregnancy in turn is positively related to the chance of a thrombosis. This last effect dominates, so in practice we observe that taking the pills reduces the chance of a thrombosis (indirectly), while we 'know' it should increase (directly). This illustrates that as a further restriction some sort of 'contextual unanimity' is needed.

The above also serves to show that the theoretical structures imposed are primary to the probabilities observed. However, according to Hoover, this need not cause 'relativism': 'if you assume that some true parameters and causes exist somewhere out there, then you can find the causal structure to things you do not know', nicely summarized as 'no causes in, no causes out'. It is 'known' for example that the central bank can control the short-term interest rate with its open-market operations ('cause in') and this can be used to investigate the causal relation between money and GDP or still other causalities ('cause out'). Thus, we simply have to live with putting some prior causes in. Hoover's account of causality is more restrictive than Granger causality. Practically, he proposes to look at regime changes which are 'known' to affect just one side of a correlation and then check the parameter stability of both causal orderings of the correlation (with recursive regressions): the 'right' causal ordering should remain stable, while the 'wrong' causal ordering should break down. 'Harnessing with historical and institutional prior 'knowledge', this way causalities may be determined, but this cannot be done with statistical analysis alone'.

## 5. Pragmatism, realism and the practice of macroeconomics

Contrary to his previous lectures, Hoover's final lecture was not focused on a specific category of methodological problems, but came most close to a crash course in 20<sup>th</sup> century philosophy of science properly, working from the famous historical line Popper-Kuhn-Lakatos.

Popper goes along with Hume in acknowledging that verification is impossible, but stresses that falsification is possible. Therefore, he sees science as a never-ending process of conjectures and

refutations, that nevertheless becomes more and more focused. As separate from the context of discovery, Popper concentrates on the context of justification, so that he has little to say on what constitutes a 'good' theory. A theory can even be a 'black box', like represented by vector autoregressive models in economics. This is the main reason why Popper has fallen somewhat out of favor with philosophers. Thus, according to Hoover 'Popper is not so much wrong, but rather off track', because his reasoning is only given the theory.

Here Kuhn comes in. Kuhn distinguishes between 'normal science' and 'scientific revolutions'. During normal science a main theoretical 'paradigm' restricts the sort of theories that can be put forward. This can go on until there is too much evidence put forward against this paradigm, at which point a scientific revolution occurs: a shift of paradigm, because it has become untenable. Thereafter normal science can go on again, within the new paradigm. Between two paradigms Kuhn sees a radical discontinuity, with strict 'incommensurability' ('they cannot really talk to each other'). Hoover does not believe in scientific revolutions in this strict sense of Kuhn, who takes them as given: 'In essence Kuhn's view of science is too linear'.

In this respect Hoover likes Lakatos better, who, at one point in time, distinguishes competing 'scientific research programs' at various stages of their life cycle. Each research program has a 'hard core' and a belt of positive ('where to go') and negative ('what to avoid') heuristics. This view implies that falsification does not necessarily lead to the abandoning of a research program, unless there is a 'better' program available. Hoover is critical of Lakatos as well though, because he thinks the heuristics have no practical relevance for the scientist (economist), or are at least not very restrictive: 'I do not think that I would not do something because it would not get accepted within a research program'.

Both Kuhn and Lakatos are still 'antifoundational', in the sense that in their views there is no absolute truth in the sense data nor in the logic (theory). In this respect, both were preceded by Quine (1961, p. 44) who writes in a beautiful paragraph: 'As an empiricist I continue to think of the conceptual scheme of science as a tool, ultimately, for predicting future experience in the light of past experience. Physical objects are conceptually imported into the situation as convenient intermediaries - not by definition in terms of experience, but simply as irreducible posits comparable, epistemologically, to the gods of Homer. For my part I do, qua lay physicist, believe in physical objects and not in Homer's gods; and I consider it a scientific error to believe otherwise. But in point of epistemological footing the physical objects and the gods differ only in degree and not in kind. Both sorts of entities enter our conception only as cultural posits. The myth of physical objects is epistemologically superior to most in that it has proved more efficacious than other myths as a device for working a manageable structure into the flux of experience'.

Hoover's basic criticism of all this comes from the fact that he does think there is a certain truth in the sense data and logic. He is a 'realist', as could have already been discovered from his previous lectures, and he sees too much 'relativism' in the above views. Moreover, Hoover argues

that a careful reading of Quine (1961) shows that even with him it is not 'anything goes', but reality does put certain restrictions on science. He cites the American 19<sup>th</sup> century 'pragmatist' Peirce as his most important influence. Peirce has stressed that it is the way of fixing beliefs that matters. They may be beliefs, but they can start from things we truly do not doubt. Thus, Hoover disagrees with the popular statement that 'if you torture the data long enough, it will confess': 'In fact, when we do empirical work, we feel the resistance of the data to our molding of it to our beliefs'. With Peirce, Hoover agrees that we will never know truth, but it exists and there are some things we are so sure about that it is simply unwise to ignore them, at least for the time being: 'When we walk into a wall, we know the wall will resist, which is a belief that is better held on to and from which other things can be found'.

## **6. Concluding remarks**

Hoover, in my view, made it plain that methodology and (empirical macro)economics are not to be seen totally separately from each other, something which was, admittedly, already in the back of my mind. It is for example hard to deny that differences of opinion concerning methodology are at the heart of many of the debates between different schools of thought in economics. Therefore, personally, I think that some training in the philosophy of science is indispensable for an economist, and Hoover's lectures, concisely, did just that. Reading the background literature, I was particularly struck by the part from Hausman (1997), which I would recommend to anyone interested (and not interested, for that matter) in the subject.

Using the specific example of Pissarides (1992) and being both a realist and a pragmatist all contributed to keeping Hoover's lectures constructive, contrary to the to economists often excessively esoteric work in the philosophy of science in general (remember Granger's (1980) 'pianos questions' above). His main (bottom-up) points, that laws in economics are less strong than in exact sciences, that macro supervenes on microeconomics and need not principally be reduced to it, and that without 'causes in, no causes out', clearly reflecting his realism and pragmatism, to me, are well worth taking. Despite his origins in philosophy, Hoover primarily considers himself a macroeconomist, more specifically a monetary economist. Especially his lecture on the microfoundations of macroeconomics, was comforting to macroeconomists, such as myself, who are naturally in the defense in an era dominated by microeconomics. The main point I am highly critical about concerns his final (top-down) lecture, in which I found the general view of science, including macroeconomics, in some sense too 'naive'. Contrary to Hoover, I think macroeconomics fits very well in the 'Lakatosian' scheme with, maybe even overly, restrictive heuristics. Hoover may not 'not do something because it would not get accepted within a research program' (above), but I am not so sure about this of other people, admittedly, including myself. What is more, I find the heuristics of research

programs in economics not only of high practical relevance, but often not even questioned before worked with. This should not be interpreted as agreeing with relativism, but as a plea for transcending the borders drawn by the heuristics of a particular research program.

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## **Ariel Rubinstein**

### **Modelling Bounded Rationality**

Report by Theo Leers, Tilburg University

#### **1. Introduction and Overview**

During the NAKE workshop in Nijmegen in the second week of December 1997 Professor Rubinstein presented some chapters of his book *Modeling Bounded Rationality*. This book sheds some light on how bounded rationality can be modeled but not on the substance or conclusions of models of bounded rationality and especially its economic consequences. The aim of the lectures was theoretical and tried to provide the necessary tools for studying bounded rationality. Since the term bounded rationality has been applied to a lot of different subjects, Rubinstein pointed out that a more specific definition of bounded rationality is needed. Only models are considered where there is an *explicit* process of *deliberation* in which people reach decisions, through certain procedures.

In Rubinsteins view, the theory of bounded rationality is largely about the way people decide and it should be used to explain for example, that one person may be more able to make decisions than another person and it should try to give an explanation of for example the existence of social institutions, such as contracts.

For the development of a theory of bounded rationality, new theories of choice have to be developed, sometimes with the help of results from psychological experiments. Besides these developments, the concept of choice needs to be extended to issues such as deciding when and how to decide and what information to use. Modeling bounded rationality also requires a new notion of equilibrium, because the existing notions of equilibrium are not clear about the knowledge agents have and use in equilibrium.

Trying to shed some light on the issues raised above, Rubinstein devoted his lectures to a discussion of the first two chapters of his book (Rubinstein, 1997) which have an introductory character and later on to a partial exposition of Chapters 4 and 7 which treat some examples in a more detailed fashion.

## 2. Bounded rationality in Choice

The first lecture and a large part of the second one were built upon the first introductory chapter of his book. Rubinstein pointed out that the *Rational Man Paradigm* has been dominating the economics profession for the last couple of decades. The rational man makes a choice after answering the following questions: firstly, what is feasible, secondly, what is desirable and finally, what is the best alternative according to the notion of desirability, given the feasibility constraints?

The implicit assumption which makes the rational man paradigm so powerful is the independency of the first question and the second one. Notice that with respect to the second question, the agent may have so called *crazy* or *irrational* preferences, but this does not prevent him or her from choosing optimally. The main problem with this description of the rational man is that it lacks all predictive power. Any choice can be optimal for appropriately chosen preferences. A number of other more implicit assumptions underlie the rational man paradigm, for example, the decision maker is assumed

- to have full knowledge of the problem, a complete ordering over the set of alternatives
- to be able to solve a sometimes very difficult optimization problem,
- to be indifferent between different descriptions of the same set.

Rubinstein mentioned that an important justification for the rational man paradigm is the well-known *as if* notion. Agents do not really solve difficult optimization problems, but they act as if they do. For example, someone who spends a constant fraction of her income on the commodities she buys acts as if she maximizes a Cobb Douglas utility function. A necessary and sufficient condition for the choices of a decision maker to be equivalent to the choices of a rational man is the consistency condition. This condition requires that if the most preferred element from a certain set is in a certain subset of this set then it is also the most preferred element or optimal choice in this subset. The question now arises which procedures, different from the rational man procedure, satisfy this consistency condition.

A boundedly rational procedure that satisfies the consistency condition is Herbert Simon's so called *satisficing procedure*. Given is a set of alternatives, a set of satisfactory alternatives which is a subset of the former one and an ordering in which the elements of the set of alternatives are reviewed. The first satisfactory element that is visited according to the ordering above is chosen. Given that this ordering is independent of the set of alternatives, this procedure satisfies the consistency condition. However, as can be easily shown, if the ordering depends upon the set, the

consistency condition might be broken. Consider for example the case where there are two orderings and the following rule: if the number of elements of the set of alternatives is small, say smaller than or equal to some natural arbitrary number, the first ordering is chosen, if the number of elements of the set alternatives is large the second ordering is chosen. This procedure clearly does not satisfy the consistency condition above. Rubinstein suggests a few other procedures, that may fail to satisfy the consistency condition. They fall in three different classes

*framing effects*

Framing effects arise if the way in which the choice problem is posed influences the choice process of the decision maker. If for example the choice problem is given by a list of alternatives instead of a set a possible procedure could be: pick the element that appears most often in the list of alternatives.

*the tendency to simplify decision problems*

Decision makers tend to simplify decision problems to save deliberation resources for example. This can lead to a procedure of the following form. Given a set of alternatives and a predetermined ordering, take the first, the middle and the last element of this set of alternatives according to the ordering mentioned above and choose the most preferred of these three alternatives. Rubinstein stressed that such a procedure might be valid when somebody wants to borrow a book from a library.

*the search for reasons*

Sometimes the reason to make a certain choice depends upon the decision problem itself. A procedure recognizing this would be the procedure that picks the element from a set which dominates most other elements in the set, where domination is determined by some arbitrary partial ordering.

None of the procedures pointed out above satisfy the consistency condition. A large amount of experimental evidence supports the claim that people use procedures as the ones described above. Much of these experiments have been performed by Kahneman and Tversky. One experiment in Tversky and Kahneman (1986) shows that the outcome of an experiment is different if it is posed in terms of losses instead of in terms of gains. One possible explanation could be the fact that people are risk-averse with respect to gains and risk-loving with respect to losses. Another experiment in the same paper shows that when subjects have to choose between different lotteries a number of them chooses a lottery which is strictly dominated by another lottery. One possible reason for this choice could be that subjects cancel out certain aspects of alternatives that are similar in order to simplify the decision problem and base their decision upon the remaining aspects. Huber, Payne and Puto (1982) considered the following experiment. Subjects could choose between alternatives from the set  $\{A,B,C\}$  or from the set  $\{A,B,D\}$  where  $C$  is strictly dominated by  $A$  and  $D$  is strictly dominated by  $B$ . Most subjects that had to choose between  $A$ ,  $B$  and  $C$ , chose  $A$  and most subjects that had to choose

between  $A$ ,  $B$  and  $D$  chose  $D$ . This could be explained by a procedure which picks the alternative dominating most other alternatives.

### 3. Modeling Procedural Decision Making

The remainder of the second lecture and the largest part of the third lecture were devoted to modeling procedural decision making. One way of simplifying decision problems is by using the concept of similarity. This idea is described in Rubinstein (1988). Similarity can simplify decision problems by cancelling out aspects of different alternatives that are approximately equal or similar. Some experimental evidence suggests that this may be the way decision makers decide. Consider the following experiment (Kahneman and Tversky, 1982). Agents can choose between two lotteries, a lottery giving a prize of \$3000 with probability 0.25 and a lottery giving a prize of \$4000 with probability 0.20. Most subjects choose the second lottery. This might be the case because these subjects regard the probabilities 0.20 and 0.25 as similar and the prizes of \$3000 and \$4000 as not similar. However, when confronted with the choice between a lottery which gives a prize of with probability 0.8 and a lottery which gives a prize of \$3000 with probability 1 most subjects choose the first one. In this case they neither perceive probabilities or prizes as similar and therefore they use another criterion, such as risk-aversion, to decide. The choice of the second lottery in the first decision problem and the choice of the first lottery in the second decision problem contradicts the von Neumann-Morgenstern independence axiom.

Consider lotteries of the form  $L=(x,p)$  where there exists a probability  $p$  of receiving a prize  $x \in [0,1]$  and a probability  $1-p$  of receiving nothing. Rubinstein defines a similarity relations on probabilities and prizes. A similarity relation has to satisfy certain properties, such as reflexivity and symmetry. However it does not have to be transitive. For a formal representation of these similarity relations it is referred to Rubinstein (1997). Given a certain similarity relation Rubinstein proposed a procedure which consists of the following steps. Firstly, domination is checked, if both the probability to get a prize as well as the prize are noticeable higher in one lottery, then this lottery is chosen. Secondly, if the first step is not decisive, similarity is checked, if both prizes and chances show similarity then if one of both is noticeable higher the lottery having this is chosen. Finally, if these first two steps are not decisive then the third step comes into being. This step is not specified in the model.

Two questions emerge from this procedure. Firstly, given some similarity relation, are the decisions implied by the first two steps consistent with the optimization of any preference relation? Secondly, how does the procedure restrict the set of preferences that are consistent with this procedure?

The answer to the first question is affirmative. Concerning the second question it can be shown that all preference relations that are consistent with the first two steps of the procedure are close to each other, in other words, the first two steps of the procedure almost uniquely define the preference relation.

#### **4. Modeling Limited Memory**

After having extensively treated the first two chapters of his book, Rubinstein now turned to the fourth chapter titled *Modeling Limited Memory*. He announced that the second part of his set of lectures is devoted to the modeling of limited memory and bounded rationality in games (Rubinstein 1997, Chapter 7).

He started by explaining his interpretation of memory: what someone knows at a certain date about what he or she knew at a previous date. Especially the case of imperfection of knowledge which he calls imperfect recall is treated. A decision problem with imperfect recall refers to a situation in which someone successively has to carry out several actions but is restricted by limits to his or her memory. If the someone above represents an organization the limits can be interpreted as communication problems between members of this organization.

Mostly, restrictions or limits on the (number of) strategies available to economic individuals or organizations arise from difficulties solving the underlying models. So these restrictions and limits are not based on plausible assumptions with respect to the real world environment. To analyze decision making with imperfect recall Rubinstein introduced the model of an extensive decision problem.<sup>1</sup> This decision problem describes the order of decisions that someone may have to take in the course of a particular situation. So not only the order of the different moves is given but also the amount of knowledge that someone obtains at any moment at which he or she has to make a move.

Formally, Rubinstein defines a decision problem which can be finite or infinite and can be accompanied by perfect or imperfect information as a five-tuple. Firstly, there is a finite set of histories which are interpreted as possible sequences of actions that are taken by the decision making individual or occur by chance. A history is called terminal if there are not at least two actions available to the decision making individual or chance. Secondly, there is a subset of the set of non-terminal histories where only chance determines the way the situation evolves. Alternatively, this set contains histories which are not under control of the decision making individual. Thirdly, the decision making individual has beliefs about the probability distribution of the histories which are determined only by chance. Fourthly, the decision making individual has an utility function which assigns a payoff to any

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<sup>1</sup> Such an extensive decision problem is just a conventional extensive game with only one player.

terminal history. Finally, there is a set of information sets, which is a partition of the set of non-terminal histories under control by the decision making individual. Rubinstein (1997, p.65) assumes that "...the sets of actions available to the decision maker at all the histories in the same information set are identical.

Rubinstein interprets the partition of the set of non-terminal histories under control by the decision making individual into information sets as a description of the knowledge of the decision making individual.<sup>2</sup>

Now Rubinstein explains his notion of strategies, which differs from the notion of strategies used in the game theoretic tradition. The latter notion of a strategy requires that actions are specified after histories that are not reached if this strategy is actually followed. The former notion of strategies is a more natural one according to Rubinstein. He calls this notion *a plan of action*: actions are only assigned to histories reached with positive probability. In this context an extension of the notion of a strategy in game theory is also of importance, the behavioral strategy. A behavioral strategy is a rule of behavior which assigns behaviors to the information sets present in the problem faced by the decision maker. This assignment can be random.

If the problem allows for *absent-mindedness*<sup>3</sup> then the randomness mentioned above comes into being independently at each time the information set is visited.

So, a behavioral strategy leads to a distribution of the terminal nodes of the decision problem. Furthermore a behavioral strategy is called optimal if there exists no other behavioral strategy with a higher expected utility. Rubinstein argued that this concept of optimality is so called prior-optimality analyzed before the start of the actual problem.

After the elaboration on the extensive decision problem and the different notions of strategies, Rubinstein now defined the main concepts of the fourth chapter of his book, perfect and imperfect recall, respectively.

**Definition** A decision problem with **perfect recall** is a decision problem in which the decision making individual remembers the actions he or she took in the past and the information on the moves of chance in the problem.

**Definition.** A decision problem with **imperfect recall** is a decision problem in which the decision making individual does not (completely) remember the actions he or she took in the past and the information on the moves of chance in the problem.

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<sup>2</sup> If the information sets are all singletons the decision problem boils down to a problem with perfect information.

<sup>3</sup> Absent-mindedness is defined as the possibility that the decision making individual will be at the same information set more than once.

Rubinstein tried to shed some light on the concept of imperfect recall through some examples. In the first one the decision making agent has to choose a password out of two possibilities and then has to repeat this password to get a non-zero payoff. If she chooses the first possibility and repeats this she gets  $x_1$ , if she chooses the second possibility and repeats this she gets  $x_2$ . It is assumed that  $x_1 > x_2$ , so she prefers the first password relative to the first one. The imperfect recall is inhibited in the fact that when the times comes to repeat the password, she knows that she has chosen one possibility but she does not recall which one.

The second example is the well known anecdote of the *absent-minded driver*. This driver, in order to get home, has to take the highway and has to get off a the second exit. Taking the first exit leads into a bad area (zero payoff). Taking the second exit (the shortest way home) leads to the highest payoff. Continuing beyond this exit leads to a positive but lesser payoff (getting home via a detour). Because the driver is absent-minded (of which he is aware), he does not know when he reaches an exit whether it is the first or the second one. He cannot remember how many exits he has passed.

After thoroughly elaborating on these examples,<sup>4</sup> Rubinstein pointed out that a number of properties that hold for problems with perfect recall fail in the case of imperfect recall. The first property Rubinstein mentioned is the so called *time consistency*. This time consistency is linked with ex-ante concept of optimality as is mentioned above. In perfect recall problems the time inconsistency problem is often ignored because it appears to be insignificant, but in problems with imperfect recall this issue may be critical. The reason for this difference is the fact that in situations with perfect recall the system of beliefs must conform with Bayesian updating.<sup>5</sup> Rubinstein argued that an extension of this updating to problems with absent-mindedness is not conceptually trivial. In a decision problem without absent-mindedness a strategy is optimal if and only if it is time consistent. Unfortunately this does not hold for decision problems which involve absent-mindedness. Recall the example of the absent-minded driver mentioned above. If the driver plans his drive home in advance, he has to conclude that he should not exit when he reaches an exit, so it would be optimal for him to drive home via the detour and receive the low but positive payoff. But in the case that the payoff at the right (the second) exit equals 4 and the payoff from the detour equals 1, Rubinstein argued that if the driver would remember his strategy, he would conclude that he was at the first exit with a probability equal to 0.5. Then it would be optimal for the driver to exit because this would yield an expected payoff of 2. So Rubinstein strongly pointed out that despite the fact that nothing changed the driver is tempted to change his initial strategy once an exit is reached. So here time inconsistency arises without any

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<sup>4</sup> For a more formal representation of the examples one is referred to Rubinstein 1997, Section 4.3.

<sup>5</sup> For an excellent overview of updating mechanisms it is referred to Fudenberg and Tirole, 1991.

change in information or preferences. Notice that in this example there is a conflict between an intuitive rationality principle and maximizing expected payoff.

Besides the different implications involving time consistency, perfect and imperfect recall have also different implications for the role of randomization. In standard decision theory it is assumed that one does not make use of random devices because in real world people do not use such devices and under perfect recall randomization does not lead to a higher payoff. In particular, Isbell (1957) showed that without absent-mindedness no behavioral strategy could do strictly better than all pure strategies. But as has been shown above in the example with the absent-minded driver the behavioral strategy which equally mixes exiting and continuing outruns the optimal pure strategy which prescribes to continue at any exit. The driver would maximize his expected payoff if he chooses to continue with a probability equal to  $2/3$ . More generally, Rubinstein showed that this holds for all decision making problems which involve absent-mindedness (Rubinstein 1997, Proposition 4.3).

The discussion of time consistency above which can be summarized as follows (Rubinstein 1997, p.78): "...when a decision maker reassesses his behavior at any particular information set, he may consider changing his planned actions at other information sets as well," contrast with an alternative suggested by Strotz (1956) which assumes that a decision maker is a collection of hypothetical agents whose individual plans form an equilibrium (plan). This alternative is called the *multiselves approach*. Under perfect recall, this approach is equivalent with the more common single-self approach (see for example Hendon, Jacobsen and Sloth 1996). However, under imperfect recall this equivalence breaks down. Rubinstein showed in a very formal way why this equivalence does not hold anymore. For a representation of his reasoning it is referred Rubinstein (1997, p.79-80).

Rubinstein concluded his exposition of modeling limited memory by pointing whether the extensive decision problem is an appropriate tool for analyzing imperfect recall. For example modeling confusion is very problematic in an extensive decision problem. Rubinstein concluded that confusion (Rubinstein 1997, p.84) "...can be modeled using the tools of an extensive decision problem, I am confused about the extent of its effectivity."

After having discussed modeling limited memory, Rubinstein moved on to the final subject of his lectures: the modeling of bounded rationality in games.

## **5. Modeling Bounded Rationality in Games**

Contrary to the former subjects of his lectures, which dealt with pure decision-theoretical situations, the last subject is devoted to the incorporation of bounded rationality in games. Rubinstein distinguished three themes in the large literature on bounded rationality (in games).

- Firstly, Rubinstein pointed out some models of interaction between players in which players, when making a decision follow a procedure not consistent with the so called *rational man paradigm*. On these models Rubinstein elaborated during the remainder of his lectures.
- Secondly, Rubinstein recognized that there exists a trade-off between optimality and complexity of strategies which can influence the outcome of the interaction of some classes of repeated games.
- Thirdly, Rubinstein argued that limits on computability can impose constraints on the existence of so called rational strategies.

Before elaborating on the first point above, Rubinstein gave two different interpretations of game theoretical solutions. On the one hand, a solution describes a particular player's behavior when he or she takes the other agents' behavior as known. On the other hand, a solution describes a particular player's behavior who is not aware of the other players' strategies but he or she reasons about these strategies from the fundamentals of the model, for example preferences and/or informational structures. Especially when bounded rationality is introduced both concepts are at loggerheads with each other.

Now, Rubinstein elaborated on the fact that players do not always behave according to the rational man paradigm. For a better (formal) understanding of this aspect of bounded rationality, the choice model originally developed by Luce (1956, 1959) was introduced. This model of choice describes behavior that is stochastic in a systematic way. Formally there is a (possibly infinite) number of alternatives and a decision making individual attaches to each alternative a certain value. He selects a particular alternative with a probability equal to the relative value of this alternative. These relative values are the so called *Luce numbers*. Rubinstein needs this model to demonstrate how the well known Nash equilibrium type of analysis can be applied to decision theory. For an example of this model it is referred to Rubinstein (1997, p.123-124).

Using the framework described above, Rubinstein tried to shed some light on the interaction between players who do not act according to the paradigm mentioned above. Nevertheless, these players still include in their decision procedures their preferences on the possible alternatives or outcomes. Now the players act as decision making individuals, who, when they have to choose from a number of actions with uncertain consequences attached to these actions, construct so called action-consequence relationships. They undertake that action with the best possible consequence attached to it. If one now assumes that the chance that a particular consequence is attached to a certain alternative is equal to the frequency that this consequence is observed with this alternative, then, given the randomness of the action-consequence relationships, this results to random behavior of the players.

Rubinstein argued that the main difference between rational players in game theory and the so called procedurally rational players, who behave according to the procedure described above is the fact that the latter ones do not start by constructing a conjecture about their adversaries' actions. But now the procedurally rational players link each of their possible actions to possible consequences and while doing this, they do not take into account any strategic considerations of their opponents. To illustrate such games with procedurally rational players Rubinstein elaborated on some simple examples which are extensively described in Rubinstein (1997, Section 7.3).

The last subject of Rubinsteins lectures was the limited foresight in extensive games, which is according to Rubinstein one of the most interesting issues in games what boundedly rational players. Limited foresight is interpreted as having a limited ability to understand extensive games. Rubinstein used the game of chess to illustrate this limited foresight. Game theoretically speaking chess is a finite zero sum 2-player game with perfect information. White and black have to move in an alternating order. The game has three possible consequences: white wins, black wins or it ends in a draw. The preferences of the chess players are naturally defined. The reason why the chess game is a finite game is inhibited in the fact that if the same position on the board is repeated three times, the game ends in a draw. Following the seminal work by Zermelo<sup>6</sup> at the beginning of this century, it can easily be proved that the chess game has a value in the game theoretical sense (see Fudenberg and Tirole (1991) for a treatment of values of games), but this value cannot (yet) be calculated nor the accompanying maximin strategies.

Rubinstein stressed that (not only in the game of chess) a maximin strategy is not necessarily the rational strategy to follow. Suppose that the value of the game of chess turns out to be a draw, then it is not always rational to play the maximin strategy because when playing against a weak opponent, one can do better by using a strategy which allows him or her to win the game. Furthermore Rubinstein pointed out that even if the value of the game of chess is known, the maximin strategies do not have to be familiar to every player or these strategies may be so complex that some are not able to execute them. Chess players play chess according to a so called check-out principle, that works as follows. They check out several game scenarios which seem most likely to occur to them and attach according to some method a value to the endpoints of the different scenarios. Doing this, players differ in their depth of inquiry, in the scenarios they think will occur most likely and in their evaluation methods. Also different players make different kinds of mistakes (which are not bad by definition).

Rubinstein pointed out that the chesslike behavior as mentioned above cannot (yet) be modeled satisfactorily in games. Via some examples which are extensively captured in Rubinstein (1997, p. 132-134), he drew the attention to the shortfalls of capturing the spirit of limited-foresight reasoning in (analytical) game theoretic models. For example future is treated as given, though it can

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<sup>6</sup> For a description of Zermelo's work and life one is referred to Heijenoort (1966).

be influenced or opponents strategies are treated as if they were known, though players do not even know their own moves.

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# QUESTIONNAIRE

## UTRECHT-COURSES 1998/99

Please circle the number of the course(s) you wish to follow next year and return the form before **16 May 1998** to the NAKE Secretariat. In principle a course can only be scheduled once every two years. The courses marked with a star (\*) have been given in the academic year 1997/98 and will therefore not **normally** be available in the academic year 1998/99. Courses in bold face are new.

### 10-week courses (4 SP = 160 hours)

|    | <i>Teacher(s)</i>                     | <i>Institute</i>   | <i>Course</i>   |
|----|---------------------------------------|--------------------|---|
| 1  | van den Berg/van Ours/<br>den Butter  | VU/KUB             | Applied labour economics  |
| 2  | van Damme/Peters/<br>Jansen           | KUB/MU             | Game theory   |
| 3* | Hartog/Teulings/Theeuwes              | UVA                | Advanced labor economics  |
| 4  | Palm/Nijman                           | MU/KUB             | Theory and application of modeling volatility in financial economics                          |
| 5  | Ridder/Wansbeek                       | VU/RUG             | Econometrics of panel data  |
| 6  | Talman/van der Laan                   | KUB/VU             | General equilibrium model   |
| 7* | Folmer/de Zeeuw/<br>Withagen/Smulders | LUW/KUB<br>TUE/KUB | Environmental problems and policy: A) A theoretical introduction<br>B) Growth and environment |

### 5-week courses (2 SP = 80 hours)

|           |                           |            |   |
|-----------|---------------------------|------------|---|
| 8         | Backhaus                  | MU         | Recent developments in law and economic                           |
| 9*        | Beetsma                   | MU         | Topics in international macroeconomic                             |
| 10        | van Bergeijk c.s.         | DNB        | Applied policy analysis   |
| 11        | Bovenberg                 | KUB        | Fiscal policy in open economies                                   |
| 12*       | Brakman/Van Marrewijk     | RUG/EUR    | Regional economics, agglomeration and the global economy          |
| 13        | Brenner                   | RUU        | A critical view of economic theor                                 |
| 14        | Brenner                   | RUU        | Social economics: Heterodox approaches to economic theor          |
| 15        | Broer                     | CPB/EUR    | Dynamic general equilibrium modelling                             |
| 16*       | Burrell/Oskam             | LUW        | Agricultural policy analysis                                      |
| 17        | den Butter                | VU         | Macroeconomic policy modelling                                    |
| 18        | Cukierman                 | KUB        | Central Bank strategy, credibility, and independence              |
| 19        | van Damme/Gradus          | KUB/MvFin  | Topics in applied microeconomics: Deregulati                      |
| 20        | van Dijk/Boswijk          | EUR/UVA    | Econometric inference in dynamic models with integrated processes |
| 21*       | Ellman                    | UVA        | The political economy of transition                               |
| <b>22</b> | <b>Ellman</b>             | <b>UVA</b> | <b>The economics of famines</b>                                   |
| 23        | van Ewijk/van Wijnbergen  | UVA        | Economic growth and development: Macroeconomics                   |
| 24*       | van Ewijk/Oosterbeek      | UVA        | Economics of education  |
| 25*       | Furth/Van Cayseele        | UVA/KL     | Advanced industrial organisation                                  |
| 26        | Garretsen/Sterken/Van Ees | KUN/RUG    | Capital market imperfections, investment and monetary policy      |
| 27        | de Gooijer/Franses        | UVA/EUR    | Recent Developments in Non-Linear Time Series Analysi             |
| 28*       | Goyal/Janssen             | EUR        | Topics in advanced microeconomic                                  |
| 29        | de Gijssel                | MU         | Micro-economische onderbouwing van een monetaire economie         |
| 30        | Gunning/Keyzer            | VU         | Current issues in development economics                           |
| 31*       | Hartog/Theeuwes           | UVA        | Labour economics: A comparative empirical perspectiv              |
| 32        | Heijdra/Meijdam           | RUG/KUB    | Intertemporal aspects of macroeconomics                           |

|           |                           |                |   |
|-----------|---------------------------|----------------|---|
| 33        | Heijdra                   | RUG            | New Keynesian macroeconomics  |
| 34        | Heijdra                   | RUG            | The macroeconomics of monopolistic competition                      |
| 35*       | Herings                   | KUB            | Theory of incomplete markets  |
| 36        | Hommès                    | UVA            | Nonlinear economic dynamics   |
| 37*       | Houba                     | VU             | Differential games in economics                                     |
| <b>38</b> | <b>Houba</b>              | <b>VU</b>      | <b>Strategic bargaining and endogenous threats</b>                  |
| 39        | Huizinga                  | KUB            | International factor movements and international financial markets  |
| <b>40</b> | <b>Jepma</b>              | <b>RUG/UVA</b> | <b>International environmental policies</b>                         |
| 41        | de Jong, E.               | KUN            | Exchange rate determination and economic fundamentals               |
| 42*       | de Jong, F.               | KUB            | Economics of foreign exchange markets                               |
| 43        | Keyzer                    | VU             | Applied general equilibrium model                                   |
| 44        | Kloek                     | EUR            | Visualising data  |
| 45        | Kooreman/Kapteyn          | RUG/KUB        | Intertemporal Choice  |
| 46*       | Kooreman                  | RUG            | The economics of household behaviour                                |
| 47        | van der Laan/Talman       | VU/KUB         | Economic equilibrium under price restrictions                       |
| <b>48</b> | <b>Magnus</b>             | <b>KUB</b>     | <b>Model selection and sensitivity</b>                              |
| <b>49</b> | <b>Magnus</b>             | <b>KUB</b>     | <b>Optimisation methods in econometric</b>                          |
| 50        | Maks                      | MU             | Competition and Market Coordination                                 |
| 51        | Meijdam/Verbon            | KUB            | Theories of government debt   |
| 52*       | Morgan                    | UVA            | History of Economic Ideas   |
| 53        | Morgan                    | UVA            | History and philosophy of economic models                           |
| <b>54</b> | <b>Muysken</b>            | <b>MU</b>      | <b>Low skilled unemployment, job competition, and overeducation</b> |
| <b>55</b> | <b>Nijkamp</b>            | <b>VU</b>      | <b>Meta-analysis in economic</b>                                    |
| 56*       | Peters/Storcken           | MU             | Social Choice Theory  |
| 57*       | Pfann                     | MU             | Optimal investment contingency plans of firms                       |
| <b>58</b> | <b>Potters</b>            | <b>KUB</b>     | <b>New institutional economics</b>                                  |
| <b>59</b> | <b>Potters</b>            | <b>KUB</b>     | <b>Market micro-structure</b>                                       |
| <b>60</b> | <b>Reuten</b>             | <b>UVA</b>     | <b>Heterodox economics: Marxian political economy</b>               |
| <b>61</b> | <b>Ruys</b>               | <b>KUB</b>     | <b>Regulation and privatisation</b>                                 |
| <b>62</b> | <b>Ruys</b>               | <b>KUB</b>     | <b>Competition and cooperation in the non-profit sector</b>         |
| <b>63</b> | <b>Schoonbeek</b>         | <b>RUG</b>     | <b>Topic in oligopoly theory</b>                                    |
| 64        | Schoorl                   | RUG            | History of Dutch Economic Thought                                   |
| 65*       | Schram/van Winden         | UVA            | Experimental economics and the design of mechanism                  |
| 66*       | van Soest/Melenberg       | KUB            | Applied non-parametric and semi-parametric econometrics             |
| 67        | Steenge                   | UT             | Rational choice theory and governance in the public sector          |
| <b>68</b> | <b>Thijssen/Goudriaan</b> | <b>LUW/IOO</b> | <b>Efficiency and productivity analysis</b>                         |
| 69        | Uhlig                     | KUB            | Business cycles   |
| 70        | Verbon                    | KUB            | Decision-making on intergenerational transfers                      |
| 71        | Vorst                     | EUR            | Options pricing theory  |
| 72        | Vorst/van de Sar          | EUR            | Behavioral Finance  |
| 73        | de Vos                    | VU             | Bayesian views on Testing and Model Selecti                         |
| <b>74</b> | <b>de Vries</b>           | <b>EUR</b>     | <b>Risk management</b>  |
| <b>75</b> | <b>de Vries</b>           | <b>EUR</b>     | <b>Advanced monetary economics</b>                                  |
| <b>76</b> | <b>Wakker</b>             | <b>LU/KUB</b>  | <b>Prospect theory</b>  |
| 77        | Wansbeek                  | RUG            | Latent variables and methods of moments estimation                  |
| <b>78</b> | <b>Wansbeek</b>           | <b>RUG</b>     | <b>Econometric methods in marketin</b>                              |
| 79        | Weddepohl                 | UVA            | Overlapping generations models                                      |
| 80        | van Winden                | UVA            | Behavioural modelling of government decision-making                 |
| 81        | van Wijnbergen            | UVA            | Economics of Transition   |