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A More Plausible Story

By Eric Bartelsman*

*Eric Bartelsman is professor of economics at VU University Amsterdam. His research interests focus on the sources of productivity growth, both from a micro and macro point of view. Recently, his work is on international comparisons of productivity and resource reallocation at the firm level.

Will the recovery lead us to the same level of output we would have achieved, had the global financial crisis not occurred? Or, will we converge to the same growth rate of potential GDP, but with a permanent gap? The questions are important for policymakers in countries such as the US, where projected increases in the debt-to-GDP ratio leave the country vulnerable to foreign creditors, or in our own European lowlands, where we are faced with the long-run challenge of an aging population.

The current debate is about how to forecast long-run GDP (growth) following a downturn. In practice, such forecasts are made by Central Banks, government budget offices, or international organizations such as the IMF and the OECD. I agree with the main tenets of a recent working paper by Cai and Den Haan (2009): the official macro forecasts leave much to be desired, and academic economists should provide the methods to improve this state of affairs.

My purpose in this short column is to complement Cai and Den Haan by suggesting a research agenda for improving our ability to forecast long-run growth. The workhorse method of forecasting continues to be extrapolation of time-series data by assuming that they are generated by identifiable stochastic processes. The art of macroeconometrics should be to find the appropriate disaggregation, such that the component time series are indeed the outcome of discernable ‘data-generating processes’.

My first suggestion is for researchers to gain a better understanding of the system of national accounts (SNA). While for current GDP estimates the US Bureau of Economic Analysis places most weight on data from the expenditure side, the bookkeeping system also builds up GDP from the income side and the production side. For tracking the cyclical movements around a Keynesian recession, it does seem sensible to analyse the time paths of final expenditure components that add up to GDP. For understanding the effects of structural policies on GDP, or for understanding issues related to long-run growth, the expenditure-side disaggregation of GDP may not be the most illuminating.

The income-side components of GDP provide a different view of the underlying economic processes. The rapid rise of labour income in the financial sector in recent years may or may not have raised some eyebrows. But, if national accountants had adjusted their measures—in accordance with the spirit of the SNA—to account for the changes in riskiness on the balance sheets of banks, then measured GDP may well have been one percentage point lower. I expect this loss to be permanent.

For long-run GDP forecasting, the production-side approach seems more relevant. In a recent working paper with Z. Wolf, we assess the forecasting power of multivariate time-series models of GDP, factor inputs and productivity. In general, the forecast for productivity is extremely poor—even over relatively short horizons. My next suggestion is therefore for macro forecasters to find data that better match theoretical models of productivity and income growth. In my view, relevant components include time series of the productivity frontier, movements of firms towards the frontier, and reallocation of resources between firms with differing productivity. I think that the financial crisis will leave a permanent mark by reducing risky investment needed for growth of the productivity frontier. Further, I fear that the current neglect of structural policy reform may reduce the contribution of reallocation to long-run growth for some time to come. While I presently have no metric with which to judge my wild guess that these impacts of the crisis will reduce long-run growth by 1/2 percentage point, at least I have a more plausible story than claiming that GDP evolves according to a process that can be identified by looking at its own history.

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Development economics and poverty mapping

An interview with Chris Elbers

Chris Elbers is Desmond Tutu Chair Holder of the Faculty of Economics and Business Administration of the VU University Amsterdam. The chair is part of the Desmond Tutu program on Youth, Sports and Reconciliation, which aims to strengthen cooperation between the VU University and partner institutions in South Africa. Elbers has been with the VU University Development Economics group since 1994, where his work has focused on impact evaluation and poverty measurement. TI Magazine sat down with him shortly after his inaugural lecture on October 14.¹

You began your inaugural lecture as Desmond Tutu Chair by criticizing what you called typical economic reasoning when giving policy advice: relying only on theoretical ideas, and being uninterested in empirically testing hypotheses. How big of a problem is this?

I think that the problem is widespread. People have called it the Ricardian vice, the idea of ruthlessly applying some kind of equilibrium, rational behaviour concept and arguing to a certain outcome. It basically says that we are always very close to an equilibrium state. For instance, there cannot be any large bills left on the sidewalk because they would have been picked up. Or there can be no serious misalignment of expectations with true distributions, because people would milk the suckers who are wrong.

While I think that the arguments are often elegant and rather appealing—especially to economists who are trained in looking at equilibrium phenomenon—they still have to be tested. In the inaugural lecture I mentioned Jacobs and de Mooij (2009), who argue that the marginal cost of public funds exactly equals one. Their reasoning is that if it were not so, politicians would adjust the tax level to perfectly balance the inefficiency of tax collection with the advantages of income redistribution. While that is an interesting idea,

it’s not enough if the issue is really going to be important. In that case, you have to set up a program to test it properly and find out whether it’s true.

*Early in your career you also did quite a bit of theoretical work. Were you more interested in theory back then?*

Yes, very much. I have always enjoyed digging into theory, and it’s still intellectually pleasing to discover some insight that is not directly presented to you from a textbook or an article. How relevant that is, I don’t know. Here’s an example: A couple of years ago I was working on a joint paper on environmental policy and trade. We were able to apply an existing model to this case, and by basically changing some names we obtained a particular result. Yes well, what does it mean? At best, it means that this is a point that needs some attention from policymakers or researchers.

Theory can lead to certain conclusions or ideas— but they’re just ideas. Most economic theory turns out in the end to be irrelevant— but this is probably true for theory in most disciplines.

*Did you decide to move to the Economic and Social Institute (ESI) of the VU University in 1984 because the position gave you the chance to do more applied work?*

The move was partly due to the fact that I had been working for quite a while at the institute where I was at the time, so I felt the need to move on. It was also a nice switch to a more relevant kind of research, where I needed to link my technical skills to real applications. At the time, doing applied general equilibrium analysis was very popular as an alternative to macroeconomic models for policy evaluation. I have never regretted this step; it was really a good thing to do.

*Was there a particular reason why you later moved to the VU economics department?*

Jan Willem Gunning, who recruited me to the institute, moved to the faculty to become Professor of Development Economics, and he was building the staff around him. The move was attractive for me, because it meant going a little bit more into academia. There had been quite some room at the institute to do research that was funded by the faculty, rather than by some external source. At a given point, much of that work was shifting back to the faculty itself.

*You mentioned in your inaugural lecture that your success is linked directly to that of the poverty-mapping project. Could you explain what poverty mapping is?*

Basically, the challenge is to come up with more detailed statistics than the data would normally allow. It’s a bit of magic that is possible only if you combine several sources of data.

In development economics, you have something called Living Standards Surveys, which allow for a detailed look at the goings-on in a household— what they consume, very often what they earn, and all kinds of household characteristics. These are small surveys because of cost and logistic limitations, and they give a picture at a rather high level of aggregation. But policymakers would really like to have more detailed information with which to build their policy.

What people started to do is to use census data to paint a detailed picture of local standards of living. Census data does not contain direct information on consumption, but some countries started playing with “rule-of-thumb indicators” of living standards: how big the house is, for example, and how many rooms there are, what kind of material is used and so on. This gave them a wonderfully detailed picture of the population— if you would believe the index they were using. That happened in Ecuador. At that point, awareness began to grow about how completely artificial such an index is.
Poverty mapping allows us to do better by combining both types of data. Census data from a typical developing country contain many things we know are intimately correlated with consumption levels, such as housing characteristics and so on. The Living Standards Surveys have information about the correlation between these core variables and consumption for the sampled households. We use the variables in the census as predictors, to come up with predicted consumption for households not covered in the Living Standards survey.

It is a nice invention. When it comes to doing relevant work, I think that this is the most relevant thing I have done and will probably do in my life.

Your work was published in Econometrica (Elbers et al. 2003), and poverty mapping has been widely used by the World Bank. But there has also been criticism. An independent review on World Bank research (Banerjee, Deaton et al. 2006), and also Deaton and Tarozzi (2009), argue that standard errors are in fact larger than those the World Bank presents. How do you view this criticism?

Deaton clearly dislikes poverty mapping, and was extremely critical in this World Bank independent review. This is remarkable, really, because he was consulted very often during the development of the details of the poverty-mapping procedure. But it has to be said that there are others who are very positive about poverty mapping. The statistical community has been very positive, as has Martin Ravallion, who is I think the most influential researcher working at the World Bank.

The serious issue here is that with poverty mapping, you are applying patterns in the data to domains where you have no direct observation. You are basically assuming that the residual in your survey is more or less distributed similarly in the other non-surveyed parts of the countries.

Data from a Living Standards Survey are clustered, meaning you have a group of ten households interviewed in one place, ten interviewed in another, and so on. That helps, because it gives you an idea about the differences that exist between locations—even after you control for many observable variables. We model these differences as random effects, which we usually apply at the level of the census enumeration area, which is, say, a group of 100-200 households. If you want to predict poverty for a group of 10,000 households, that gives, say, 50 independent random effects—and they cancel out a little bit.

The obvious objection made by Deaton and Tarozzi is that you have only thinly sampled clusters in your survey, and each location effect could just be representative of the effect of a bigger area. Say, for instance, that you didn’t use 50 random effects to build up your district estimate, but five; that would certainly increase the standard error of your prediction.

It is basically an empirical matter. The difficulty is that you don’t often have observations where you can check—although in some rare cases you do. We have a really nice data set from Brazil that can be used to check whether this makes a lot of difference. Fortunately, we were vindicated in that particular case, but that is just one case.

My position is that the only way we can resolve this is by knowing the correlation of location effects of two contiguous enumeration areas. Say we made a double selection of clusters—so that every time we draw an enumeration area, we could also pick a neighbouring one as well. Then we could study the degree of correlation that exists between the location effects of two neighbours, and easily fit that into the whole procedure.

What is the objection to doing so? It’s funding. You would still want to have a picture of the whole country, so it would amount to doubling the number of clusters, which would basically double the cost of the survey. I’ve been pushing for this—so far, unsuccessfully. But perhaps one of these years I will succeed.

Do you expect a change in the focus of your work as a result of receiving the Desmond Tutu Chair?

One thing is that I’ll be working more in the area of program evaluation. I’m currently involved in a project measuring the impact of budget support in Zambia. The idea of budget support is that donor funds go directly to the recipient government, without many strings attached. There is some monitoring of the budget process, but essentially the funds just go into the government’s general budget. People want to know how effective this is, and Jan Willem Gunning and I have elaborated a proposal for the evaluation of budget support. It’s a bit scary, in the sense that it is certainly not clear yet whether this is a feasible approach that will yield credible results.
I also plan to do some more work on risk, growth and the role of insurance with Jan Willem Gunning. That’s an area in which there is a nice interplay between economic theory and empirics.

As for South Africa, I’ll be working with a number of South African PhD students. One has begun already; she is a brilliant student and will be looking at the efficiency and effectiveness of the education system in South Africa. She has been working with Servaas van der Berg, a famous South African economist. So far, she’s mainly been doing regression-based analysis, and I’d like to also get her started using project evaluation techniques.

There should also be an opportunity to work with the South African government, which is now in the process of setting a new poverty line. Martin Ravallion has done a lot of work looking at the theoretical underpinnings of poverty lines, and this could be a chance to apply certain aspects of his approach to the case of South Africa.

Just one last question. Quality Assurance Netherlands Universities (QANU) recently carried out an evaluation of economics research groups in the Netherlands. Do you know how the VU Development group did?

No, well . . .

I’ve heard you received a 4.5/5 for quality, which is very good.

Okay, yes . . . Well, we jokingly refer to ourselves around here as the best development economics group in the Netherlands. Seriously, we are a big group and I think that most economists would consider us to be somewhere near the top in the country. There seems to be renewed interest in development economics these days among students.

Something slightly strange is that many students who are interested in development economics courses at the VU are actually UvA students. We have the advantage of being able to pick out the best students. Our department has lots of room for research assistants, so you grab these students and keep them. Some of them go on to enrol in high-profile PhD programs—and although we’re happy for them, and proud, it’s also a bit sad to see them go.

Many PhD students have come directly from the TI MPhil program—four in just the past two years. As I also mentioned in my inaugural lecture, our department is a great environment to work in, being surrounded by many bright people.

References


You are an econometrician doing applied work in marketing: can you explain what led you to apply econometric methods to marketing questions? How did your research evolve over time?

What attracted me mainly was the detail that is available in marketing data. The detail can be found in having information on many different households making decisions, or having low-level sales data (that is, at the actual product level). Having such a rich dataset compels you to use more complicated models. Furthermore, the models that you work on have applied value—the analysis you do really answers a particular question. So I can work on several models and techniques without losing sight of possible applications. My research started at the Econometric Institute at Erasmus University.

As part of my Master’s thesis I did an internship at ROBECO. I had a marketing topic for my research there, where we determined the risk profile of people. My colleagues at ROBECO were not really used to using econometric techniques on the information in their client database. This was my first encounter with marketing econometrics. While working on this project, I became interested in “quantitative marketing models”. During this period, my thesis supervisor, Philip Hans Franses, suggested that I become a PhD student. I had to think about it a little bit, because I did not know if academics was something for me. Actually, at the time I thought I was relatively young to go into business—and there was too much I did not know about—and decided to do a PhD. During my PhD I liked the field so much that I...
What are the particular demands of marketing scholars, compared with other scholars in economics and management science? And, in terms of approaching problems, what do you think are the major differences between marketing and other fields?

These differ according to the scientist’s perspective, which can be methodological or more theoretical. I can say something only about the methodological part. In terms of methodology, a marketing scholar needs to be creative in finding the question that is interesting to study and identifying what kind of modelling techniques should be developed to answer the particular question. Apart from that, there is a need to combine different subfields of econometrics. The question of interest can involve many issues at the same time—for example, endogeneity, a panel model, and a dynamic structure that you want to describe. In this sense, a marketing scholar needs to know about all of these different aspects. A second challenge facing a marketing researcher is not to get too focused on the technical matters. You have to be able to also explain things in a relatively less technical manner than you would have to do for a study in an econometrics journal, for instance. So you could say that being able to sell your method is an important skill of a marketing scholar. In the recent years, the marketing field has become more and more quantitative, and the models in marketing have become increasingly complex. This means that there is more interest in technical matters—although the practical side of things is important. If you develop a new technique, you need to explain it well enough to reach the entire audience of the marketing field.

How wide is the field of marketing? Although people traditionally think of supermarkets, many other players—such as insurance companies, funds with ‘good targets’, political campaigns, pension funds with early retirement decisions—use advanced marketing strategies based on individual decision making. Is there a common element in all of these subfields?

The field of marketing is really broad. It is not very easy for me to define marketing, but all of the studies that we carry out deal with the choices that people make. The data we use for this purpose can be aggregated data for a particular region, or it can be household-level data. But in the end it is always some sort of consumer behaviour that we model. This behaviour could be the choice to buy a product, or the choice to vote for a particular party. Hence, the behaviour could be influenced through price or promotion—or through political campaigns. Similar kinds
of methods and techniques can be applied to both of these problems. In this sense, marketing may not be the correct label for the research I do.

How valuable do you think econometric and statistical models are for marketing managers in the field? Do they use these models (or at least the results from the application of such models), or is such use mainly confined to academia? How strong do you think the link is between marketing scholars and practitioners?

Models are important for managers, but academics are always ahead of the applied work in the field. We see managers using some applied models, but not the most advanced ones. What is often important for managers is to be able to understand the models themselves. So they need to understand how the models work, and some of these models are just too complicated for them. However, this does not mean that managers are not interested in the things we do. In fact, we have many contacts with firms. These contacts give us access to the data of firms, which enables us to apply our methods to interesting questions—and we can then answer the questions of these managers using new methods.

To what extent do you think that marketing and economics influence each other—for instance, with insights from behavioural models and firm/consumer multiproduct decision models?

The link between marketing and economics goes both ways. Economics influences marketing, in the sense that marketing models mainly start with some form of economic model. Economic models have become increasingly realistic, and they oftentimes provide the basis for econometric models. For example, when we observe a market and a new development happens in the market—perhaps a firm changes its policy or a new brand is introduced—you need the economic models to assess the effect of it on what the competitors will do. Marketing, however, also influences economics. Understanding and describing decision-making is, after all, a large part of economics. Marketing is an area in which we can observe many decisions being made. Such observations allow us to test theories, and to say something about how people arrive at their decisions. I think that marketing is a good field in which to apply economic models—and developments in the marketing field influence economics.

Do you think that marketing research also contributes to modelling innovations such as those in econometrics and statistics, or are marketing scholars more empirically driven—i.e., interested mainly in applying econometric models to study specific research questions?

The questions we study in marketing lead to a demand for models with specific features—models that can, for example, deal with differences between individuals, or that can be used to define groups of similar individuals. Next, we often want to tie such differences to some observable variables. This aspect is rather unique to marketing, since data is usually available for many individuals. We see that the tools to deal with these kinds of problems, such as Hierarchical Bayes models, are growing in popularity. Many developments in this sense actually come from a marketing application—where people are dealing with an empirical question, but at the same time they need to develop some specialized technique for the question at hand.

Do you think that econometric models are able to replace managers for some decision-making tasks, or would you say that it is the manager (with his or her managerial intuition and experience) that, in the end, makes the difference between a good or a bad decision?

It is not going to happen soon. I think we are at the point where we have to convince managers that it is good to get advice based on models. Next, they can use their own knowledge to adjust this advice, if necessary. The main reason for this is that the decisions that the managers make are on a rather high and general level. For instance, decisions about a product introduction are not made on a daily basis. In a firm, such decisions are taken after many meetings, and the decision-making process takes a long time. Only when each decision is not extremely important can you rely on an automated system. One situation in which the replacement you mentioned is possible (and necessary) is the online world. For example, websites like Amazon apply techniques to process the search and clicking behaviour of consumers in order to target special offers through email or to suggest products to buy. This is all automated, and is done using econometric models. But in general, automated systems are not applicable to many managerial decisions.
How do you think that the advancements in information technology affect econometricians—particularly those in marketing science? For example, the availability of large databases, as well as advances in computer technologies, especially for the researchers using Bayesian methods—how did all these achievements affect marketing research? How do you see this progress?

One aspect of information technology relates to the development of online applications. I think we will see more websites that are adaptive. The behaviour of the consumer will be recorded, and the suggestions made by the website, or the way in which the site can be viewed, will be updated according to this information. Apart from that, we have larger databases. Nowadays, every company collects data about everything. In some cases they have millions of records. The companies often do not differentiate what part of this data is useful; they simply collect all data available. The challenge for marketing people is to do something useful with this huge dataset. While such databases are often large, they frequently do not contain enough information on each individual. In order to say something about a particular person, we also need to use the information about the other individuals. Bayesian methods are ideally suited to problems like this. A second aspect of information technologies is the increasing computing power needed to apply these methods. Most of the time, the models we make are just small enough to get results in a couple of hours or a couple of days. Every time the computers become faster, we come up with more complicated models that are more detailed and involve more individuals. In that sense, computing power is still a limit on the things we are capable of doing. With more powerful computers we can also make the analysis more general.

What lines of research in quantitative marketing do you think will be more pronounced in upcoming years? Can you envision relatively more promising research lines in marketing that will attract significant attention? Which of the newly emerging fields particularly interests you?

Many different marketing subfields should become more important in the future. The analysis of online behaviour is one of these. There is also a trend to generalize findings. People used to study one set of products in one area or in one country. The tendency nowadays is to, for example, analyse multiple products over several countries in order to be able to assess the results for a new product, or a different country. The challenge is to make a general model that can explain the differences between the regions, which places higher demands on the techniques that are used. A final thing is the goal of being able to say something about a new situation. This could be a new product in the market or changes in the structure of the market, such as new competitors. In order to explain the results of this change, we need a rather strict connection to economic theories. We need to use the econometric model on this data and extrapolate it for the future event. This cannot be done by regressing sales on prices, as this does not provide advice for something that happens outside the scope of the data.
Any novel combination of otherwise independent research lines raises the question whether the new, interdisciplinary approach yields insights that could not otherwise be obtained using existing methodologies.

Law (L), Economics (E) and History (H) have a long history of fruitful interconnection. By performing some combinatorics on L, E and H, one obtains a large set of possible interdisciplinary approaches. To keep the exposition compact, let us use X→Y to indicate the study of Y using methods borrowed from X. Legal History (including both H→L and L→H), Economic History (E→H) and History of Economic Thought (H→E) are three well-established disciplines. More recently, the Economic Analysis of Law (E→L) has applied methods borrowed from economics to unveil the behavioural effects of legal rules; vice versa, Law and Finance (L→E) has looked at (company) law as a way to explain the economic performance of (financial) markets.

All of these areas of research are now an integral part of the curriculum of lawyers, historians or economists, and their role in bringing novel ideas into the social sciences is beyond question. Therefore, it seems desirable to perform some additional combinatorics: that is, to explore further possibilities for interdisciplinary research. This contribution sketches the importance of research combining Law, Economics and History—with specific reference to the Fiat Lex Vidi-project.

The Fiat Lex project
This project aims to take account of theories developed in the fields of legal scholarship, history and economics when addressing questions concerning the evolution of law and lawmaking institutions. Research by Douglas North (North 1990) and Avner Greif (González de Lara, Greif and Jha 2008) first aroused interest for the effects and determinants of institutional change. Bringing in methodology and results from legal history enables us to shift the core of the research slightly—from institutions in general to the specific institutions that make law (courts, assemblies, customs) and to the law itself.

The goal of the Fiat Lex project is to understand why lawmaking institutions change over time and what effect these changes have had on the laws created by them.
The goal of the Fiat Lex project is to understand why lawmaking institutions change over time and what effect these changes have had on the laws created by them. Roman law is still considered to be an important (at times, the most important) building block of western legal systems—within both the civil law and the common law tradition. Roman law spans a period of over a thousand years, and its study relies on a centuries-long tradition of scholarly attention. It is by far the most studied and best-documented ancient legal system.

By looking at the evolution of Roman lawmaking institutions and tracking their effects on the final product of the law, one can learn much about the present.

By looking at the evolution of Roman lawmaking institutions and tracking their effects on the final product of the law, one can learn much about the present. On the one hand (learning about the past: L,E,H→H), the economic study of Roman law caters to the demand of historians for a clarification of the legal-economic mechanisms operating within the ancient world (Frier and Kehoe 2007), an endeavour that has already proven to be able to produce remarkable results (for instance, Parisi 2001 and Temin 2001). In this sense, legal scholarship and economics are used alongside other modern disciplines, such as physics, chemistry or archaeology, in order to shed light on the past.

On the other hand (learning from the past: L,E,H→L,E), Roman law can be used—with some caution, due to the lack of econometrically tractable data—as a natural experiment about different modes of lawmaking. The existing literature has shown that there is both a demand and a potential for research that draws lessons from the past in order to understand the complex interaction between legal and economic principles (for instance, Hansmann, Kraakman and Squire 2006, Levmore 1986 and Malmendier (forthcoming)). The important next step, which is the aim of this line of research, is to examine the essence of the lawmaking process and to understand what determines the emergence of certain lawmaking institutions—and then to investigate what impact this in turn has on the legal system. Although such questions have been put on the world’s research agenda by the Legal Origins project (La Porta, López de Silanes and Shleifer 2008), a comprehensive answer is still missing.

The fact that the law evolves in reaction to both external events and internal dynamics (see, for instance, Greif and Laitin 2004, and Roe 1996) is now generally accepted; yet, the exact nature of the dynamics of legal change remains elusive. One question that remains is whether one can compare different lawmaking institutions (say, courts and assemblies). In other words, can we confirm that one lawmaking institution is in some way better than another—and if so, according to which benchmark(s)? Addressing these questions means investigating whether lawmaking institutions have a systematic, predictable effect on the product of the law—and if so, what effect. Looking at a past where different types of lawmaking institutions were simultaneously present might shed some light on these modern conundrums.

Case law vs. legislation

Most commonly, the relevant economic literature considers two modes of lawmaking: case law versus legislation. This approach naturally brings to mind the dichotomy between common law and civil law jurisdictions, but to limit the analysis to this dichotomy would not do justice to the complexity of the reality. Rather, both common law and civil law countries adopt a mix of case law and legislation.

Roman law cannot be classified according to modern criteria as a common law or civil law jurisdiction (this distinction emerged much later); rather, the Roman legal system relied on both case law and legislation.

A teaser of the results

In a study on the emergence of the corporate form (Abatino et al. 2009), we examine the legal framework that made depersonalisation of business in ancient Rome possible, and we compare it with the modern corporate form. Depersonalisation of business (that is, the possibility for an enterprise to operate as a separate entity from its owners and managers) is a crucial step in economic development. The Roman legal system developed an early form of de facto depersonalised business entity—exhibiting all of the distinctive features of modern corporations (continuity, direct agency, limited liability, and entity shielding)—by means of the judicial evolution of specific remedies in favour of
In this case, the focus of the analysis is on the influence that the distribution of political and economic power has on the functioning of a lawmaking institution.

While still a great deal of work has to be done in order to better understand the institutional dynamics governing lawmaking, there is a clear convergence of interests among legal scholars, economists and historians. It is at the intersection of these disciplines that breakthroughs may be expected.

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References


Tinbergen Institute Medal

The Board of Tinbergen Institute established in 2008 the ‘Tinbergen Institute Medal’. The Chairperson of the Board, Jeroen Kremers, announced that this medal would be awarded to honour those persons who are or have been of great service to the Institute.

The first TI Medal was awarded to professor Maarten Janssen, director of the Institute from 2004 until 2008, and the second to professor Jaap Abbring, Director of Graduate Studies from 2004 until 2008.
**Letters from Alumni**

life after the PhD thesis defense

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*A lot less remote*

Ronald Wolthoff*, University of Chicago

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When University of Chicago professor Roger Myerson was awarded the Nobel Prize in October 2007, I had just entered my final year as a PhD student at the VU University Amsterdam. Although I was more familiar with his work than with the work of his co-winners, I did not know much about him. Chicago seemed like a remote place.

Around the same time I had to start thinking about my future. My advisor, Pieter Gautier, encouraged me to go to the international job market. I applied to several places and decided to go to the meetings in New Orleans. It was definitely a fascinating experience: the desperate attempts around mid-December to find a hotel room (still many thanks to the unknown person who suddenly cancelled his online reservation); the four days in a city invaded by 9000 economists; the interviews with four people around a bed, while the fifth one tries to convince the cleaning lady that they will really need only two more minutes to kick me out.

In the end, I got a fantastic offer: a two-year postdoc position at the University of Chicago. The Windy City suddenly seemed a lot less remote. I’ve been here for a bit more than a year now, and have enjoyed every second. The academic environment is truly amazing, with seminars at virtually any moment of the day and opportunities to speak with an impressive number of prominent economists. Of course it is typically harder to arrange a meeting with somebody here than it was in Amsterdam, but often the intensity of the discussion is compensation enough for this drawback.

The main lesson that I have learned in Chicago is that the research question and the motivation are crucial. Everybody here can do IV, solve Bellman equations and program complicated models. What distinguishes the best from the rest is the question they answer and the way they sell it in their paper and presentation. Hopefully, I am slowly developing some intuition for that as well.

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What’s up next? I’ve come full circle now, and find myself again on the job market. Although many things now are quite different than they were two years ago (this year the meetings are in Atlanta), some things have hardly changed. As before, I seem to be endlessly polishing my paper and presentation— and again I have no idea where I will end up next year. One thing I know for sure, though: if the cleaning lady comes again, she will have to wait at least another 15 minutes this time!
computing the conditional parameters drastically in matrices to be zero, and also elements of the parameter diagonal GARCH model further restrictions. The covariance matrix without definiteness of the conditional guarantees positive estimated, and will not with the vec model is that it returns vector. A serious issue outer products of the lagged-by its lags and the vector of covariance matrix of the model, which parameterises expression is called the ‘vec’ models, the most general for multivariate GARCH Volatility (MSV) literature. the Multivariate Stochastic structures, this issue has not covariance and correlation examined in detail in the Multivariate Stochastic Volatility (MSV) literature. For multivariate GARCH models, the most general expression is called the ‘vec’ model, which parameterises the vector of the conditional covariance matrix of the returns vector, as determined by its lags and the vector of outer products of the lagged-returns vector. A serious issue with the vec model is that it has many parameters to be estimated, and will not guarantee positive definiteness of the conditional covariance matrix without further restrictions. The diagonal GARCH model restricts the off-diagonal elements of the parameter matrices to be zero, and also reduces the number of parameters drastically in computing the conditional covariance matrix. The BEKK specification guarantees positive definiteness of the conditional covariance matrix, which is essential for obtaining sensible VaR forecasts.

In the context of modelling conditional correlations rather than conditional covariances, the Constant Conditional Correlation (CCC) model assumes that the time-varying covariances are proportional to the conditional standard deviation derived from univariate GARCH processes. This specification also guarantees positive definiteness of the conditional covariance matrix. As an extension of the CCC model, the Dynamic Conditional Correlation (DCC) model allows the conditional correlation matrix to vary parsimoniously over time. The development of dynamic correlation and covariance models has proceeded at a faster pace in the conditional volatility literature than in its stochastic volatility counterpart. Two reasons for this would seem to be the development of parsimonious multivariate dynamic conditional correlation models and their relative ease in estimation. As a contribution to the development of parsimonious dynamic correlation MSV models that can be estimated with relative ease, the paper proposes two types of stochastic correlation structures for MSV models: namely, the constant correlation (CC) MSV and the dynamic correlation (DC) MSV models. The dynamic stochastic covariance matrices may be obtained easily from the dynamic stochastic correlation matrices. The structures can be used for purposes of determining optimal portfolio and risk management strategies through the use of dynamic correlations, and for calculating Value-at-Risk (VaR) forecasts and optimal capital charges under the Basel Accord through the use of dynamic covariances. A technique is developed for estimating the DC MSV model using the Markov Chain Monte Carlo (MCMC) procedure. The properties of the estimation method are examined using simulated data, and various multivariate conditional volatility and MSV models are compared via simulation, including an evaluation of alternative VaR estimators. It is found in an empirical example that MSV models that allow for time-varying correlations generally fit the data better.


Non-hyperbolic time inconsistency

Stationarity concerns a rationality assumption for intertemporal choice. It means that indifference between a small outcome received soon and a large outcome received later is preserved if both outcomes are equally delayed. Stationarity reflects constant impatience, and is equivalent to time consistency under common assumptions (“stopwatch time,” resetting the zero of time to the moment of decision). Empirical studies have found that stationarity is usually violated (Frederick et al., 2002), with impatience mostly decreasing and not constant. That is, delaying the aforementioned outcomes makes the decision-maker less impatient and more willing to wait for the large (and late) outcome. Thus, the indifference turns into a preference for the late outcome, and stationarity is violated. The most popular discount functions today, the generalized hyperbolic (Loewenstein and Prelec, 1992) and quasi-hyperbolic (Phelps and Pollak, 1968; Laibson, 1997) discount functions, were introduced so as to accommodate decreasing impatience. A drawback is that they do not have enough flexibility to accommodate increasing or strongly decreasing impatience. These restrictions make it impossible to fit data at the individual level because there will always be significant fractions of subjects with increasing or strongly decreasing impatience (Abdellaoui et al., 2007; Harrison et al., 2002; Barsky et al., 1997). The impossibility to fit data at the individual level is particularly disconcerting in view of recent advances in neuroeconomics, where typically only a few individuals can be analysed.

This paper introduces two classes of discount functions that can accommodate any degree of increasing impatience, and also any degree of decreasing impatience. Hence, they cover all degrees covered by hyperbolic discounting and allow additional degrees on top of those, giving increased flexibility at no cost (no more parameters used). The classes are the intertemporal counterparts of the CARA and CRRA utility functions from decision under risk. They generalize classes of discount functions introduced by Prelec (1989) and Ebert and Prelec (2007). Thus, it becomes possible to analyse data at the individual level. In particular, it is possible to analyse which subjects are most prone to time inconsistencies and, hence, to irrationalities in their intertemporal preferences.
References


Retirement, pensions, and ageing

Population ageing is wreaking havoc with the public pension schemes of many western countries. For most developed countries, the pension system includes promises that cannot be kept without significant system reforms. This paper constructs a model that helps to understand the macroeconomic effects of reforms that would restore the sustainability of the pension systems. Although the resulting model is simple enough to obtain analytical results, and hence provides better insight into the forces at play, it is also general enough to incorporate four main features of the data. (1) Until the mid 1990s, people tended to leave the labour force at an ever-younger age. (2) Most people retire at the earliest age at which retirement benefits are available (typically 60-62), or at least before the normal retirement age (usually 65). (3) Most social security programs (still) contain strong incentives for older workers to leave the labour force. (4) In many European countries disability programs and age-related unemployment provisions essentially provide early retirement benefits. The analysis makes use of modelling insights from two branches of the literature. First, the paper employs the generalized Blanchard-Yaari model developed in earlier papers by the authors. In this model, individual agents face an age-dependent probability of death. By allowing the mortality rate to depend on age, the model can be used to investigate the effects of a reduction in adult mortality—one of the main forces behind the ageing problem. A time-varying fertility rate allows us to also focus on the other two forces—the baby boom and baby bust. The second building block of the analysis concerns the labour market participation decision of individual agents. Following much of the literature, this study assumes that agents either work full time or not at all—and that the retirement decision is irreversible. Workers choose the optimal retirement age, taking as given the time- and age profiles of wages, the fiscal parameters, and the public pension system. From a policy perspective, the main finding is that most existing pension systems induce a kink in the lifetime income function that acts as an early retirement trap. Fiscal changes are not potent enough to get individuals out of the trap. Increasing the early entitlement age appears to be a low-cost policy measure to counteract the adverse effects of longevity and the changing demographic composition.

Inciting protocols – How international environmental agreements trigger knowledge transfers

This paper offers a new perspective on the currently dominant view that international environmental agreements (IEAs) have little impact on local efforts to reduce international environmental spillovers. Even though IEAs in general may not be indicative of the ability to induce emission reductions directly, clear evidence is presented that the Helsinki and Oslo protocols on reducing sulfur (SO2) emissions have played a prominent role on their own. The paper explores the conjecture that innovating firms consider an IEA as a signal that their product market expands, and therefore are strongly inclined to designate patent protection of technologies employed for reducing SO2 emissions in countries that consider signing the protocol. The timing of (new) inventions and their diffusion are studied in detail by means of a uniquely constructed patent dataset on SO2 abatement technologies filed in 15 signatory and non-signatory countries between 1970-1997. The paper looks specifically at the protection behaviour of firms by means of so-called intended knowledge flows, or transfers through ‘family’ patents (see Lanjouw and Mody, 1996). A distinction is made between ‘mother’ and ‘family’ patents, as well as the panel characteristics of our dataset, to identify differences in patenting behaviour as induced by local and international regulatory interventions in both signatory and non-signatory countries. It turns out that innovating firms indeed exploit the signal provided by the protocols and designate mother and family patent applications before the protocols are actually implemented. Moreover, that these effects are found to be particularly strong in countries that cooperate through the IEAs (i.e. the signatory countries). The results in this paper suggest that firms are aware of the potential private benefits of such international agreements and exploit potential advantages of larger product markets by seeking protection in countries that participate in the protocols.

References
Starting an R&D project under uncertainty

The decision to start a Research and Development (R&D) project is one of the most challenging firm decision problems. R&D projects usually take time to complete, their investments are irreversible (and therefore represent sunk costs) and above all, they are highly uncertain. This paper contributes to the theoretical as well as to the empirical literature on R&D decisions under uncertainty.

From a theoretical point of view, the paper studies a two-stage R&D project with an abandonment option. Two types of uncertainty influence the decision to start R&D. Demand uncertainty is modelled as a lottery between a proportional increase and decrease in demand. Technical uncertainty is modelled as a lottery between a decrease and increase in the cost to continue R&D. Both lotteries become more divergent when the difference between the outcomes of the lottery increases. We relate differences in uncertainty to differences in risk premia. This makes it possible to consider a broader set of demand and supply lotteries than only the subset of lotteries that preserves the mean, as previously studied in the literature.

A potential entrant is endowed with a superior technology and threatens to drive the incumbent out of the market. The incumbent has a time lead over the entrant and can obtain the same superior technology by completing the R&D project before the entrant can enter the market. The presence of the entrant in our model provides the incumbent with additional benefits from investing in the superior technology, a strategic effect known as Arrow’s replacement effect. In order to deduce testable hypotheses, we derive under which lottery probabilities more divergent demand and supply lotteries positively or negatively affect the decision to start R&D. For empirical testing, data are used from the fourth Community Innovation Survey (CIS IV) in Germany for about 4000 firms to explain actual and planned R&D investments.

Our main results, strongly confirming our model predictions, are that for firms facing lotteries where the good state is more likely to prevail (i) a 10% increase in the degree of divergence of the demand lottery increases the likelihood of undertaking R&D by 1.4% and (ii) a change from a low- to a high degree of divergence of the supply lottery increases the likelihood of undertaking R&D by 23.3%. For firms facing a demand lottery where the bad state is more likely to prevail, an increase in the degree of divergence of the demand lottery decreases significantly the probability of undertaking R&D.

Why are residents reluctant to consult attending physicians?

Suppose you go to a hospital because you’ve been under the weather for a couple of days. A medical resident examines you thoroughly and diagnoses that you suffer from a rare, exotic disease. He proposes a heavy treatment that may lead to problems of infections and depressions. How would you react? As the disease is rare and the treatment is heavy, it is likely that you would want the resident to ask a more senior physician to have a look at you.

One good reason to ask for a second opinion is that physicians themselves are reluctant to consult other physicians in case of uncertainty. This typically applies to residents. Most hospitals have protocols describing the circumstances under which residents should call the attending physician. However, there is strong evidence that residents do not always follow protocols.

This paper examines a resident’s incentives to consult an attending physician when the resident is uncertain about the diagnosis. The paper distinguishes between two motives that may drive consulting behaviour: the instrumental motive and the image-based motive. The idea behind the instrumental motive is that a resident seeks feedback to better select the proper treatment for his or her patient. The first part of the paper shows that (i) more uncertainty leads to more consulting; (ii) a resident should call the attending physician if the resident’s diagnosis suggests a relatively extreme situation; and (iii) highly able residents seek confirmation from their superiors out of fear of being wrong, whereas less-able residents seek feedback out of fear of being right. These results explain why many hospitals use protocols to guide the consulting behaviour of residents.

Consulting behaviour is also influenced by how residents believe it affects their image: that is, how their superiors see them. Using a model in which residents are concerned with their image, this paper shows that protocols that dictate consulting behaviour do not always work. Fear of negative feedback seems to discourage employees from seeking feedback. On the flip side of the coin, hope for positive feedback is a stimulus for seeking feedback.

Moreover, the paper shows that the act of seeking feedback and the content of feedback have separate impacts on employees’ images. Finally, whether it is strength or weakness to seek feedback varies from situation to situation.

The second part of the paper shows that residents may shy away from consulting in order to conceal their shortcomings from the eyes of others. Abstaining from asking advice increases the likelihood of poor task performance, and may lead to complaints.

The third part of the paper examines whether monitoring through investigation of complaints alleviates the distortion in the consulting decisions of medical residents. Monitoring is shown to weaken the incentives of less-able juniors to distort their consulting decisions. However, monitoring leads to another distortion: It induces residents to give too much weight to their own information.

By Thijs Dekker (VU), Herman R.J. Vollebergh (Netherlands Assessment Agency), Frans P. de Vries (Stichting Management School, Division of Economics, University of Stirling), Cees A. Witagen (VU and CenterTIL, Tilburg University), Inciting protocols – How international environmental agreements trigger knowledge transfers
TI 09-060/3

By Sabien Dobbelaere (VU), Roland Iwan Luttens (SHERPPA, Ghent University, and CORE, Université Catholique de Louvain), Bettina Peters (Centre for European Economic Research (ZEW)), Starting an R&D project under uncertainty
TI 09-044/3

By Otto H. Swank (EUR), Why are residents reluctant to consult attending physicians?
TI 09-042/1
Kahneman and Tversky and the making of behavioural economics

Over the past two decades, behavioural economics has emerged as a dominant new research program in the economic discipline. The history of behavioural economics can be traced back to the faculty of psychology of the University of Michigan during the 1950s, where psychologists such as Clyde Coombs and Ward Edwards developed a new scientific approach to human decision behaviour: behavioural decision research. The focus of this new psychological discipline was the empirical question of if (and if so, when) individuals make mistakes in their decision behaviour. For instance, whether an individual taking part in a lottery always chooses the option with the highest expected value. In this approach, the rules of expected utility, Bayesian statistics and logic defined the normative benchmarks by which the experimental behaviour was compared. Contrary to his thesis supervisor Edwards, Amos Tversky became increasingly convinced during the second half of the 1960s that the behavioural decision research approach had a serious problem—because his experimental results showed individuals deviating systematically from the normative benchmarks, instead of deviating from them randomly, as Edwards thought. As the normative benchmarks were understood as being devised by (and hence as applicable to) normal healthy individuals, this implied that either something was wrong with the experimental methodology, or that something was wrong with the normative benchmarks.

Daniel Kahneman, who had a different background in psychology, offered Tversky an ingenious, conceptual solution for his problem in 1969. If the normative benchmarks are not thought of as being made by ordinary, normal healthy adults, but instead are conceived of as universal rules of rational decision-making, then both the normative benchmarks and the experimental results can be maintained by understanding human behaviour as systematically and predictably deviating from the rational choice, as determined by the universal benchmarks. This new approach formed the basis of Kahneman and Tversky’s famous research, including Heuristics and Biases (1974) and Prospect Theory (1979). Starting in the early 1980s, Kahneman and Tversky’s work was introduced first into financial economics, and later into economics generally. In particular, it offered a useful explanation for observed irrational behaviour in financial markets. On the basis of the research of the two psychologists, behavioural economics has produced a fundamental reorientation of economics. This dissertation shows that this reorientation can only be understood on the basis of an appreciation of a specific psychological research program that emerged in the 1950s.

References

Rationalised panics: The consequences of strategic uncertainty during financial crises

Like many international economic crises, the 2008 financial crisis materialized abruptly, after a protracted period of economic optimism. Financial crises often appear to be panic-like “swings in sentiment” on markets, against a backdrop of essentially unchanged macroeconomic indicators, and negligible changes in economic conditions. Even in cases where crises are blamed on deteriorated economic conditions, imbalances are allowed to persist at length until a panic ultimately erupts. In sum, crises are driven by sudden turbulence on markets, not by unstable economic fundamentals. This dissertation argues that “strategic uncertainty” — doubt in the minds of market participants about the intentions and actions of other agents — is key to understanding such sudden turbulence on markets. Strategic uncertainty arises from the interdependent nature of choices made by agents on financial markets, and has a decisive influence on the choices of agents. It may arise abruptly, even while economic fundamentals remain largely unchanged. Using epistemic game theory (in particular, the theory of global games), this dissertation investigates the consequences of this strategic uncertainty. It develops new models that explain many aspects of financial crises.

Consider, for instance, the question of why a country is more crisis-prone if it has more short-term debt. This maturity structure makes investors more dependent on each other, and thus aggravates strategic uncertainty. Therefore, more short-term debt increases the probability of a panic on financial markets.

Or consider the question of why markets tend to allow a misaligned currency peg to survive for a length of time, and then suddenly punish — late and harshly. Currency pegs are associated with political prestige, and governments take great pains defending them, typically by raising the costs for speculators. Speculators depend on each other to bring down the peg. Since they may expect a defence, they postpone attacking until the expected devaluation compensates for both the costs and strategic uncertainty they face. The more vigorous the expected defence, the longer they postpone — and the harsher the ultimate collapse. These two examples demonstrate the dissertation’s message that the risk of a financial crisis originates not from shocks to economic fundamentals, but from the different ways in which strategic uncertainty facilitates a change of sentiment. A study of the determinants of strategic uncertainty deserves a prominent place in the analysis of financial risk.

Unfortunately, recent experience shows that this message has yet to be internalised by policymakers and the financial sector.

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