Scientific progress in economics:

An analysis of Nobel Prize motivations.

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Preliminary version, English not checked. All comments welcome.

June 2016
Abstract

This article uses the material that is published by the Nobel prize committee about the contribution of the Nobel prize winner in economics to evaluate various ideas about the structure and progress of science. The material covers the years 1969-2015.

The contributions can be divided into six basic categories:. 1. The founders of the new paradigm (with mathematical modelling and econometric estimations). 2. Methodological development, with the subcategories improving econometrics, improving mathematical methods and introducing new empirical methods. 3. Crossing the borders of the research area, with subcategories applying the paradigm outside the core areas and integrating economic analysis with knowledge from other fields. 4. Introducing a new perspective that affect the subject as a whole and it is argued that there has been four such new perspectives (1) the concept of transaction cost and making institutions endogenous, (ii) game theory and strategic interaction instead of anonymous market forces, (iii) the importance and rationality of expectations and (iv) the importance of asymmetric information in the economy. 5. Developing knowledge within a specific fields, and here there are a number of subcategories for different field like growth economics, financial economics, macroeconomics, etc. 6. Developing new tools for practice, e.g. input output analysis.

The Nobel prize texts give strong support for the model of scientific progress developed by Larry Laudan and his rather flexible concept of Research tradition. At every point in time there are certain basic assumptions about the phenomena under study (e.g. rationality, individualism in economics) and beliefs about the best methods to use (e.g. mathematical models and econometrics in economics), but there are also continuous changes in each part. In economics assumptions about rationality has been modified and laboratory experiments have been added as a legitimate method. There is also room for individual researchers to depart from specific parts of the core assumptions, which can be exemplified with laurates like Coase and Williamson that were skeptical to mathematical modelling but kept assumptions about rationality and individualism. If a research tradition is rather flexible then it can change in such a way that no Kuhnian scientific revolution occurs. The texts also give some interesting lessons for those who want to change economics of today: Be tenacious, speak the language of the dominating tradition and develop your ideas by showing that the new idea withstands the criticisms of the normal scientists of the day.

The Nobel prize texts takes it for granted that one central aim of economic theory is to find the truth. Theories point to relations and mechanism that is believed to be explanations of observed facts and patterns. Economists have typically two complementary ways of arguing that it is rational to believe in a certain hypothesis and that is (1) building credible mathematical models and prove that the hypothesis is true in the model and (2) empirical studies in the form of (primarily) econometric analysis and laboratory experiments that support the hypothesis.
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1. Introduction

Ever since Thomas Kuhn’s path breaking book "The structure of scientific revolutions" (Kuhn 1962/1970) science and scientific development has been discussed in terms of paradigms and concepts like normal science and scientific revolutions. During the 1970s Imre Lakatos developed a modified version and described scientific development as a series of "research programs", each with a specific "hard core" of assumptions and guidelines about how to carry on research (Lakatos & Musgrave 1970). In later works Kuhn focused more on the role of "exemplars", i.e. important works that were used as role models for ordinary scientists. Another important work (Laudan 1977) criticized the rather inflexible structure of paradigms and research programs and argued that over time what was once seen as a core assumptions could be replaced while other parts remained. What Laudan called "research traditions" can go through rather radical stepwise changes, but not necessarily go through any dramatic scientific revolution of the type Kuhn described.

The first question in this study is which of these models that fit the development of economics best. Is it possible to identify a stable hard core in the form of substantive and methodological assumptions that cannot be changed without a revolution, or is it a field where more continuous change occurs?

Another important philosopher of science - Ian Hacking - argued in the book "Representing and intervening" (Hacking 1983) that progress in physics can be explained by the complementary activities of three kinds of researchers. The first group is the Speculators that came up with dramatic new ideas. The second group is the Calculators, who take the ideas from the Speculators, built mathematical models and derive testable implications from these models. The third group is the Experimenters, who invents and constructs the sophisticated equipment necessary to empirically investigate the implications derived.

The second question in this study is if it possible to find different complementary activities and types of scientists in economics, related to the ones Hacking described.

In economics there is a number of classical debates and questions concerning scientific development and what characterize good science. In the 1950s much of the debate concerned Friedman's article "The Methodology of positive economics" (Friedman 1953) and the idea that theories shall be evaluated by their predictions and not by the realism of their assumptions. Paul Samuelson on the other hand discussed development in terms of a series of successive approximations, where unrealistic assumptions step by step are replaced by more realistic ones in order to make the conclusions more credible (see e.g. Samuelson 1963). Another classical work is Leontieff’s article "Theoretical assumptions and non observed facts" (Leontieff 1970) discussing the conflict between "rigor and relevance" in economic. There is a recurring discussion about whether economics puts too much weight
on rigor and mathematical models and thereby loses relevance for understanding real world problems.

The third question in this study is therefore to which extent the development of economics are consistent with these different views: Are current theories better at predicting? Are they more realistic? Is it true that theories are becoming more rigorous and less relevant, or are they becoming both more rigorous and more relevant? And in what sense are they becoming more rigorous?

Laudan (1977) proposes that scientific progress should be discussed in term of problems solved, problem solving effectiveness and indirectly problem solving ability.

The fourth and last question in this study is therefore to what extent it can be claimed that economics today have solved more problems and have better problem solving ability than economics 50 years ago.

The main difficulty with all these interesting questions is that it hard to know how to answer them. Which methods and strategies can be used, and what data are relevant? This study is based on the conjecture that analyzing Nobel prize motivations can help us answer them. The Nobel prize is given for important scientific contributions and as it highly esteemed it should be representative of the view of the profession, even if there always are some controversies about whether the right winner was chosen. All the hypotheses presented above have implications for what we should expect to characterize the Nobel prize winners. For example, if there is a hard core of substantive and methodological assumptions that must be followed, then we should expect that all winners follow them. If it is important with different types of researchers, then we should be able to classify Nobel prize winners into a small number of rather homogenous groups of researchers. Or more generally: If we classify what type of contribution the Nobel prize winners have made, we could see what is important for scientific progress in economics, e.g. in terms of problems solved and problem solving ability. As the Nobel prize have been awarded 47 times, and that there are on average around 50 pages of official material for each prize, there is a considerable material to use.

The structure of the article is as follows. In the next section basic information about the process of selecting a Nobel prize is presented, together with a discussion about the limitations of using this method which of course gives an inside view of scientific progress. Section 3 describes the data used and the more specific methods of analysis. Section 4 gives a theoretical background and discusses the nature of paradigms and research traditions a little more in detail and also relates the view in this article to earlier views about paradigms and research programs in economics. Section 5 and 6 presents results in the form of a

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1 This claim is hardly controversial, but if some evidence is asked for it can be mentioned that the speech held by the Nobel prize winner is published in American Economic Review. If the prize was not seen as important and relevant such a high ranked journal would not publish the speeches.
classification of the contributions of the laurates into a number of rather homogenous group and also presents the results from a more quantitative content analysis of the motivations. In section 7-10 the material is analyzed from the perspective of the four research questions above. Conclusions are summarized in section 11.

There have been a few studies about the Nobel prize in economics (Lindbeck 1985, updated 2001, and Chowdhury 2010). The main difference between the current study and the earlier ones is that in the current study the focus is on answering specific questions concerning the nature and development of economics using information about the Nobel prize winners as data. The earlier studies are more directly focusing on which fields the laurates have been active, which countries they come from etc. There is a similarity in that they also classify the Nobel prize winners into different categories.²

2. The Nobel Prize in Economics

2.1 Background

In Alfred Nobel’s will from 1895 he decided to give most of his wealth to a foundation that every year should give out five prices: physics, chemistry, medicine, literature and a peace prize. The first prices were awarded in 1901 (http://www.nobelprize.org).

In 1968, Sveriges Riksbank (Sweden's central bank) established the Prize in Economic Sciences in Memory of Alfred Nobel. The Prize is based on a donation received by the Nobel Foundation on the occasion of the bank's 300th anniversary. The Prize in Economic Sciences is awarded by the Royal Swedish Academy of Sciences, according to the same principles as for the original Nobel Prizes. On the Riksbank website it is written that: "The prize is awarded every year to a person or persons in the field of economic sciences who have produced work of outstanding importance." There is a rule saying that the prize cannot be shared by more than 3 persons (Lindbeck 1985, 2001).

2.2 Procedure

The procedure for selecting the Nobel prize winner is as follows according the Nobel Foundation webpage. A more detailed insider description can be found in Lindbeck 1985, 2001). Nominations to the Nobel Prize in Economics can only be made by those that are specially invited to nominate by the Economic Sciences Prize Committee. The committee consists of 6 members selected by and from the Swedish Academy of Sciences. They are selected for a three year period and the members are typically professors of Economics at

² Their classifications differ considerably from the one presented later in this article but the differences will not be commented as this is not important for the purpose of the article.
leading Swedish Universities. The committee presents a recommendation in the form of a short-list and then the Academy makes the choice. The following diagram from the website summarizes the process and more details can be found on the website.

*Figure 1: The selection process*

Over the years there have been various criticisms of the Nobel Prize in Economics (see e.g. Luyendijk 2015). Is economics really a science comparable to physics and chemistry? And why is there no prize in other social sciences? Does this imply that economics is seen as more scientific than other social sciences? Is the selection of winners more based on ideological preferences than scientific contributions? Given the purpose of this article these discussions are however of minor importance. The aim here is neither to evaluate the scientific status of economics (compared to other sciences) nor to evaluate the "real" contributions of specific Nobel prize winner given an outside perspective, but "only" to see what can be learned from analysing Nobel prize motivations given the three research questions described in the introduction.

### 2.3 Limitations

The Nobel prize winners are in practice chosen by leading economists. This means that what is described in this article is an inside view of scientific progress. A very different approach is to take an outside view and there is for example a number of studies that looks at economics through the eyes of different general philosophies of science. Hutchison (1938) e.g. evaluates from the perspective of the early positivists, while a number of studies starts from the philosophy of Karl Popper or Imre Lakatos (see e.g. Blaug, 1992), and evaluates whether economics or some part of economics is scientific or is progressing given the views in these philosophies of science. There is of course room for both types of studies and the focus in this article on an inside view does not mean that external views are of less importance.
This article is also an inside view in the sense that the author is economist, and actually started to study economics in 1971 just a few years after the first Nobel prize and I have been following the development of economics over the whole period covered in the study. I think it would be very difficult for a non-economist to understand what the Nobel prize winners actually have done and why what they have done is seen as an important contributor, but I will not argue this point further here.

The way the Nobel prize winners are selected leads to selection bias in at least two dimensions, related to the fact that the prize is only given to living economist and also after a considerable time after their initial works. Lindbeck (1985, 2001) says that they have used "the test of time" and always waited a number of years to be sure that the contribution was of lasting importance. This means that economists that die young and/or make their contributions when they are old, have a very small chance to get a Nobel prize. Frank P Ramsey who died at the age of 27 is one example. Had Morgenstern and von Neumann lived longer it is very probable that would have got a prize for the work in game theory. Had Fischer Black and Amos Tversky not died in their fifties, they would of course have shared the price with Scholes and Kahneman.

Important contributions by economists who die relatively young are, however, typically developed further by other economists: Nash, Selten and Harsanyi got the first game theory price instead of Morgenstern and von Neumann. Mirrlees developed Ramsey’s theory about optimal taxation and got a prize for this. The selection bias described above has therefore probably not led to a biased picture of scientific progress in economics, even if does not reflect what all important economists have done.

3. Data and method

3.1 Data

The official material published about the Nobel prize that is used in this study can be divided into the following categories (see appendix 1 for details).

1. The short motivation. When the prize winner is presented there is a one sentence motivation of why the winners get the price. This is in the following called "the short motivation" and is available for all of the 47 years covered in the study (1969-2015).

2. The press release. From 1972 a Press release was presented at the same time as the winner was announced. The length of this press release has varied as can be seen in appendix 1. Shorter press releases were introduced in 2000 when the Academy started to publish a special document with information to the public about the contribution of the winner.
3. *The speech at the award ceremony*. This is also available for every year and there are some variations in length but not as much as for the press release. There is typically a large overlap between the press release and the speech at the award ceremony.

4. "*Information for the public*". This started in 2000 and is usually a 4-5 page non-technical document.

5. "*Advanced information*". This started in 1995 and is a much longer document and more technical document, typically 20-50 pages. This document has become considerably longer in recent years.

All the information above is official in the sense that it is available through the website of the Nobel Foundation and there are no named authors for the texts.\(^3\) The motivation and the press release are however also published in the Scandinavian Journal of Economics (formerly Swedish Journal of Economics) and then together with:

6. *One or several articles about the contribution of the Nobel prize winners*, and this is also a material used in the study. When the Advanced information have become longer the similarity between the Advanced information and the articles in Scandinavian Journal of Economics have increased. The articles are written by internationally well-known researchers and not by members of the selection committee. These articles are commissioned by the prize awarding committee and used during the selection process, but for some reason they are not mentioned on the website. A list of these articles in chronological order can be found in appendix 2. Note that they typically are published the year after the award was presented.

### 3.2 Method

The basic method used is different forms of Content analysis (see e.g. Krippendorf 2013).

In order to identify different types of contributions the main source was the short motivations, the press release and the speeches to the laureates as this was the most official material where the committee motivates the award. Formulations about motives and the laureates view on issues related to the research questions above were marked and collected in an Excel-file, typically 5-10 sentences per prize.

The next step was the classification of contributions: What kind of things can you have done in order to get the Nobel prize? When the work started there was a general idea, based on theories about paradigm and research traditions that this could be things like laying the foundation for a new paradigm, develop the scientific methods and show that more and more phenomena can be explained within the paradigm. In practice a trial and error

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\(^3\) The only exception is the first Advanced information in 1995 where Lars E O Svensson, then a member of the committee, is named as author.
procedure was used where a preliminary classification was tested against the formulations in the analyzed texts and if there were prize motivations that did not fit into the pattern, the classification was modified. Using this trial and error procedure and going through the motivations a number of times the result was the categorization presented in Table 1 below.

Table 1  Categorizations of contributions

1. Founders of the new paradigm.

2. Methodological development, with subcategories improving econometrics, improving mathematical methods and introducing new empirical methods.

3. Crossing the borders of the research area, with subcategories applying the paradigm outside the core areas and integrating economic analysis with knowledge from other fields.

4. Introducing a new perspective that affect the subject as a whole.

5. Developing knowledge within a specific fields, and here there are a number of subcategories for different field like growth economics, financial economics, macroeconomics, etc.

6. Developing new tools for practice.

In some cases a specific laurate or a specific prize has been categorized into more than 1 category. An early example is Paul Samuelson who both is described as laying the foundation for a paradigm (category 1 above) and as a developer of mathematical modelling in economic theory (category 2 above), but also in category 5 for his work in microeconomic theory (revealed preference theory).

In order to find answers the research questions two strategies was used. The first was a to check if the prize winners were described in such a way that it was consistent with one or the other of the different statements tested. The second was to go through all of the material, looking for more direct statement related to the research questions.

As what one observes in a text change when more texts are read all material was gone through several times in order to get a consistent treatment of the material.

A small quantitative content analysis was also carried out. In this part of the analysis the only material used is material that is rather constant over time, and this is the short motivations and the speech to the laurate. These texts were the simply copied into one word file for the short motivations and one word file for the speeches. The search function was then used to...
highlight the words and get a total count. This was then inspected manually to see that the word was used in relation to what the laureate had done and also to see in how many of the individual motivations that the word was used and not only the total number of uses. In the table below the words analyzed are listed. A check was also made to see if the word paradigm was used in these texts.

Table 2  Words in the quantitative analysis

<table>
<thead>
<tr>
<th>Problem</th>
</tr>
</thead>
<tbody>
<tr>
<td>Method</td>
</tr>
<tr>
<td>Model</td>
</tr>
<tr>
<td>Analyze/analysis/analytical</td>
</tr>
<tr>
<td>Understand/understanding</td>
</tr>
<tr>
<td>Explain/explanation</td>
</tr>
<tr>
<td>Cause/causal</td>
</tr>
<tr>
<td>Predict/prediction</td>
</tr>
<tr>
<td>Forecast</td>
</tr>
<tr>
<td>Theory/theoretical</td>
</tr>
<tr>
<td>Empirical</td>
</tr>
</tbody>
</table>

3.3 Reliability and validity

There is a large literature on the content analysis discussing different types and their limitations (see e.g. Krippenberg 2013). The qualitative content analysis carried out here are what Hsie and Shannon (2005) call "Directed content analysis" which they describe as: "The goal of a directed approach to content analysis is to validate or extend conceptually a theoretical framework or theory." The starting point for the analysis described above was from this perspective to test whether the motivations and formulations found were consistent with certain general views about scientific progress.

The main problem concerning reliability concerns the qualitative content analysis. Would another researcher pick out the same formulations as the crucial and important ones? Would another researcher classify the contributions in the same way? Sometimes several persons in a project have done the classifications independently of each other to increase reliability (see e.g. Krippenberg 2013 for a general discussion about reliability) but in this case there were not resources available to do that. The issue of reliability will therefore be
left to future researchers. Readers who do not feel comfortable with the result presented here will have to do the analysis themselves and argue for alternative conclusions.4

The main problem in the quantitative part concerns validity. Perhaps the words used more reflect the person or persons writing the speeches than what the laurates have done? This means that one should not put any weight on small differences and individual cases. The word theory/theoretical is e.g. mentioned in 42 of the 47 motivations, but it could just as well have been used in the remaining ones also. Theory/theoretical is e.g. not used in the short motivation and speech for Ronald Coase, but in others texts he is described as laying the foundation for transaction cost theory, e.g. in the material about Oliver Williamson.

4. Theoretical framework: Paradigms, research programs and research traditions

4.1 Description

Since Kuhn´s influential work "The Structure of Scientific Revolutions" (Kuhn 1962) a scientific field at a certain point in time has often been described as a specific paradigm with a number of parts, e.g. certain fundamental assumptions that are not questioned. A few years later a modified version of Kuhn´s theory was presented by Lakatos (Lakatos & Musgrave 1970) where scientists are described as working within a specific research program, with a certain "hard core" and a certain "heuristic" that guides researchers when problems shall be solved.

Kuhn´s description of what a paradigm really is changed over time as described in several of the works in Devlin & Bokulich (2015). Instead of discussing in terms of paradigms, concepts like "disciplinary matrix", "exemplars" and "lexicons" are used (Devlin 2015,p 158). In a disciplinary matrix there are conceptual, theoretical and methodological elements.

Other authors have discussed similar ideas. Laudan (1977) uses the concept of research tradition: "In brief, a research tradition provides a set of guidelines for development of specific theories." (p 79) He criticizes both Kuhn and Lakatos with the argument that their concepts are too static and inflexible and describe his alternative concept more in detail as follows (p 78-79).

4 All summary files mentioned above are available from the author: the Excel file with selected formulations, the word file with the short motivation and the word file with all speeches in order to simplify reproduction of the analysis.
1. Every research tradition has a number of specific theories which exemplify and partially constitute it; some of these theories will be contemporaneous, others will be temporal successors of earlier ones.

2. Every research tradition exhibits certain metaphysical and methodological commitments.

3. Each research tradition goes through a number of different, detailed (and often mutually contradictory) formulations and generally has a long history.

Dolfsm & Welch (2008) describe paradigms in terms of a "set of rules and routines" (p. 1088). Solomon (2011) described paradigms as "defining both the questions of interest and the appropriate evidence" and a paradigm also has "a core of technical results and successful exemplars that have been extended over time." (p. 455).

From the perspective of this study the important idea is that at a certain point in time there are fundamental ideas about the nature of the subject matter and about the best way to explain and to carry out studies. There are certain "shared commitments".

An important contribution in science should then be possible to relate to these fundamental ideas and be seen as a specific development in relation to the paradigm at the time when the work was carried out.

The different views about the flexibility of the paradigm/research programme/research tradition will be returned when the first research question will be answered below.

4.2 Paradigms in economics

When the first Nobel prize was awarded in 1969 the presentation speech started with the following line:

"In the past forty years, economic science has developed increasingly in the direction of a mathematical specification and statistical quantification of economic contexts."

It seems rather uncontroversial to say that an old "hard core" in economics consisted in ideas about rationality, and that the development of the economy can best be understood if the analysis starts from the decisions of households and firms (or in the case of macroeconomics at least assumptions that seem consistent with such rational decision making). Looking at economics in the 1970s, the new parts were then that a theoretical study should contain an analysis of a relevant mathematical model and that an empirical study typically should consist of a statistical analysis of "hard data". The aim of the empirical study was to test a theory and this was done by estimating coefficients in the statistical model and to see if these was consistent with the hypothesis in the theory or not.

Marchionni (2013) discuss "field-specific explanatory standards" (p 334) and writes:
"As is well known, in economics a good explanation is commonly held to be one that shows how the phenomenon to be explained results from the actions and interactions of rational agents."

Schmalensee (1991, p 15) writes:

"There is no doubt that economics has experienced revolutions in the last century; these include at least the emergence of macroeconomics as a core field and the methodological triumph of mathematical analysis as an 'engine of inquiry'."

There has been a number of works using the concept of paradigm to describe economic theory and economic development. Many however use the term paradigm in a more narrow sense than the one here, where a paradigm/research tradition should include both substantive and methodological rules. Stiglitz argues in his Nobel lecture "Information and the Change in the Paradigm in Economics" (Stiglitz 2002) that information economics represents a fundamental change in the prevailing paradigm (p. 460) and that the incomplete information paradigm will replace the current paradigm based on full information. But from the perspective of a broad paradigm based on rationality, individualism, mathematical models and econometric test, then introducing incomplete information models is a change within the paradigm and not a new paradigm.

Harstad & Selten (2013) discuss how the bounded-rationality model can be competitive and supplant the dominant paradigm. They however presuppose that the new paradigm should fulfill exactly the same criteria and look like the current dominant paradigm. But if they are doing the same thing as the old paradigm and "only" replace one assumption with another then this would be the same thing as Stiglitz discusses, and it would also in this case be more reasonable to call it a development within he rather broader economics paradigm sketched above. The same conclusions holds for Mange (2015) who discusses the development of asset price theory from efficient market theory to imperfect information and behavioral models as a paradigm shift. A very narrow interpretation of paradigm can also be found in Cook (2003) who writes "In this paper the Kuhnian analysis of scientific revolution has been applied to the observed movement within econometrics from the textbook approach to the LSE methodology." (p 72)

Dolfsma & Welch (2008, p 1086) writes:

"Indeed, the history of economics as a science has seen a number of paradigmatic shifts. One might think of the marginalist revolution, Keynesianism, monetarism, and new growth theory, to name just a few."

But given the broad definition of the paradigm in economics presented above then at least monetarism and new growth theory should be seen as developments within the paradigm. The separation into macro- and microeconomics might be seen as a more fundamental shift but as it occurred before the period studied here it will not be discussed further - and later
macroeconomics was returned to the old paradigm when the demand for microeconomic foundations was seen as important!

Turner et al (2001) discuss whether Ecological economics should be seen as a new paradigm or a new perspective and concludes that the latter is a more reasonable view. The focus on the dynamics of the physical system and new questions about the long term feasibility of economic growth are seen as new, but it is argued that this can be seen as a complement to traditional theory and could be integrated in the standard paradigm (p 46).

Several articles discuss paradigms within a specific research area. In Stiglitz & Greenwalds book "Towards a new paradigm in monetary economics" (2003) it is argued that there should be a shift from focusing on money from the perspective of transaction to a perspective where credit and information asymmetries are in the center. Pieterse (2005), e.g. discuss "paradigm making and paradigm breaking" in development economics, but it mostly seem to concern specific theories about what causes a certain phenomenon.

The question of what really is a paradigm shift and what are developments within a paradigm is of course not specific for economics. A detailed discussion from the field of economic geography can be found in Sunley (2008), but the main point that is important for the rest of the article is that a paradigm or research tradition should be seen as something "broad": based on rather general assumptions and methodological recommendations, or as Laudan summarizes it (p. 81):

"a research tradition is a set of general assumptions about the entities and processes in a domain of study, and about the appropriate methods to be used for investigating the problems and construction the theories in that domain."

5. Results (1): Classifications of contributions

The contributions have, as described above, been divided into 6 different types, and here each will be presented in a little more detail and the laureates belonging to the category will be listed. Key formulations from the official motivations are presented in the tables below. In a few cases formulations from the articles in the Scandinavian Journal of Economics have been used and then that is explicitly mentioned. If the laureate also is listed in some other category there is a number after the name showing if this is the first, second and third time the laureate is listed.

5.1 Founders of the new paradigm

As quoted above, when the first Nobel prizes were awarded there was a belief that economics had entered a new era with a more scientific approach. In the motivations of the
first three Nobel prizes there were explicit formulations about that these economists had been important for creating what we could call the new paradigm in economics, with mathematical modelling in theoretical economics and econometric estimates and hypothesis testing in empirical economics.

Table 3 **Founders of the new paradigm**

<table>
<thead>
<tr>
<th>Year/name</th>
<th>Contribution (from motivations)</th>
</tr>
</thead>
<tbody>
<tr>
<td>1969 Frisch/Tinbergen (1)</td>
<td>Their aim has been to lend economic theory mathematical stringency, and to render it in a form that permits empirical quantification and a statistical testing of hypotheses. One essential object has been to get away from the vague, more &quot;literary&quot; type of economics.</td>
</tr>
<tr>
<td>1970 Samuelson (1)</td>
<td>Generally speaking, Samuelson's contribution has been that, more than any other contemporary economist, he has contributed to raising the general analytical and methodological level in economic science.</td>
</tr>
<tr>
<td>1971 Hicks/Arrow (1)</td>
<td>Hicks used traditional differential analysis as a mathematical tool. When later more modern mathematical methods began to be introduced into economics, Arrow used them to study the properties of general equilibrium systems. SJE: Hicks’ contribution consists in logic and power in the methods.</td>
</tr>
</tbody>
</table>

All of these also made more specific contributions mentioned in the motivations and therefore they are also listed under more specific categories below.

### 5.2. Methodological development

The methodological developments have been subdivided into three categories: Development of econometrics/econometric modelling, development of mathematical modelling and introduction of new methods.

#### 5.2.1 Development of econometrics/econometric modelling

Eight Nobel prizes have been categorized as primarily given for further development of econometric techniques, but it should be observed but many of these economists also have contributed to the development of the substantive theories that are evaluated by their new methods. Lucas has e.g. also been included also as contributing to macroeconomics, and it could be argued that this should also be done for Klein [1980] and Sargent/Sims [2011]. In the information for the public one can e.g. read: "Some of Sargent’s contributions were

\[ \text{[5]} \] will be used when the figure refers to the year that a prize was given.
solely methodological, although he has also applied the new methods in highly influential empirical research” (p 3). Similar formulations can be found for almost all of the winners listed below.

When the prize has been shared by several persons, they have sometimes been individually assigned to different types of contributions. Hansen [2013] is an example of this when his contribution explicitly was said to be to have developed the statistical tools necessary for testing the other laureates theories.

<table>
<thead>
<tr>
<th>Year/name</th>
<th>Contribution (from motivations)</th>
</tr>
</thead>
<tbody>
<tr>
<td>1980 Klein</td>
<td>For the creation of econometric models and the application to the analysis of economic fluctuations and economic policies.</td>
</tr>
<tr>
<td>1989 Haavelmo</td>
<td>For his clarification of the probability theory foundations of econometrics and his analyses of simultaneous economic structures.</td>
</tr>
<tr>
<td>1995 Lucas (1)</td>
<td>Lucas’s pioneering work has created an entirely new field of econometrics, known as rational expectations econometrics.</td>
</tr>
<tr>
<td>2011 Sargent/Sims</td>
<td>Have developed methods for answering questions regarding the causal relationship between economic policy and different macroeconomic variables such as GDP, inflation, employment and investments.</td>
</tr>
<tr>
<td>2013 Hansen</td>
<td>Hansen developed a statistical method that is particularly well suited to testing rational theories of asset pricing.</td>
</tr>
</tbody>
</table>

5.2.2. Development of mathematical modelling

There are two prizes that it seems uncontroversial to put in this category and that are Debreu [1983] and Allais [1988]. For several of the game theoretical contributions it also seems correct to say that one very important part of their work was to develop mathematical tools, and that they saw themselves primarily as mathematicians. Here Nash [1994] has been included but it could be argued that one should also include Aumann [2005] and Shapley [2012].
Both Debreu and Allais used their mathematical skills to develop microeconomic theory (general equilibrium theory), but it seems to correct to say that their substantive contribution to the theory was not as important as their introduction to the development of more stringent mathematical methods.

Allais is a special case in the material in that his work had very little direct influence on economics, and one can in the motivations read: "Even though his fundamental research has been relatively little known beyond the French-speaking sphere, Allais has had a far-reaching indirect impact through younger French economists who have been strongly influenced by his work."

<table>
<thead>
<tr>
<th>Year/name</th>
<th>Contribution (from motivations)</th>
</tr>
</thead>
<tbody>
<tr>
<td>1983 Debreu</td>
<td>Gerard Debreu symbolizes the use of a new mathematical apparatus. SJE: At an even more basic level, the axiomatic approach used by Debreu and his colleagues has been adopted by the profession as a standard mode of economic analysis.</td>
</tr>
<tr>
<td>1988 Allais</td>
<td>The foremost contribution of Maurice Allais was made in the 1940s when he continued to develop Walras's and Pareto's work by providing increasingly rigorous mathematical formulations of market equilibrium and the efficiency properties of markets.</td>
</tr>
</tbody>
</table>

5.2.3 Introduction and development of new empirical methods

One important argument by Laudan against Kuhns’ version of the paradigm concept was that the latter was too static. From that perspective it is interesting to note that it is possible to get a prize for introducing completely new empirical methods that initially was rejected by the scientific community, in this case laboratory experiment. Beside the prize to Smith in 2002, the price to Roth [2012] was also included here as he also was important in the development of laboratory experiments. The quotation below for Roth is from the Advanced information.
Table 5  Introducers of new (empirical) methods

<table>
<thead>
<tr>
<th>Year/name</th>
<th>Contribution (from motivations)</th>
</tr>
</thead>
<tbody>
<tr>
<td>2002 Smith</td>
<td>For having established laboratory experiments as a tool in empirical economic analysis, especially in the study of alternative market mechanisms</td>
</tr>
<tr>
<td>2012 Roth (1)</td>
<td>Controlled laboratory experiments are frequently used in the field of market design. Vernon Smith shared the 2002 Prize for his work in experimental economics. Alvin Roth is another major contributor in this area.</td>
</tr>
</tbody>
</table>

5.3. Crossing the borders of the research field

5.3.1 Applying the paradigm outside the core area

When there is an established research tradition that has been used for what can be called standard economic issues, one type of contribution is to show that the same framework can be applied to other more non-standard issues. The examples here concern using the framework for analysing politics, family formation, crime and punishment and economic history. Four prizes fit into this category and Becker is in the SJE-article called "one of the most outstanding of the imperialists".

Table 6  Using the framework outside the core area

<table>
<thead>
<tr>
<th>Year/name</th>
<th>Contribution (from motivations)</th>
</tr>
</thead>
<tbody>
<tr>
<td>1982 Stigler</td>
<td>For his seminal studies of industrial structures, functioning of markets and causes and effects of public regulation. SJE: But one can argue that his work on regulation has been the most important.</td>
</tr>
<tr>
<td>1986 Buchanan</td>
<td>The first contribution from Public Choice theory, that probably is the most widely known outside the academic world, is the extension and use of traditional economic micro-theory in the studies of the political system, the public administration, and interest organizations.</td>
</tr>
<tr>
<td>1992 Becker</td>
<td>For having extended the domain of microeconomic analysis to a wide range of human behaviour and interaction, including nonmarket behaviour.</td>
</tr>
<tr>
<td>1993 Fogel &amp; North</td>
<td>For having renewed research in economic history by applying economic theory and quantitative methods in order to explain economic and institutional change</td>
</tr>
</tbody>
</table>
5.3.2. Integrating knowledge from other fields into economic analysis

If the category above can be seen as economic imperialism where economists try to take over neighboring territories, the current category is almost the opposite. Here the contribution is to take knowledge from other fields and integrate it into economic analysis. This can concern knowledge from political science, from business administration, from philosophy and/or from sociology and psychology. Four prizes fit into this category.

Table 7 Integrating knowledge from other areas

<table>
<thead>
<tr>
<th>Year/name</th>
<th>Contribution (from motivations)</th>
</tr>
</thead>
<tbody>
<tr>
<td>1974 Myrdal &amp; Hayek</td>
<td>For their penetrating analysis of the interdependence of economic, social and institutional phenomena</td>
</tr>
<tr>
<td>1981 Simon</td>
<td>He starts from the psychology of learning, with its less complicated rules of choice and its more moderate demands on the memory and the calculating capacity of the decision-maker. SJE: Surely, if ever there was a case where interdisciplinary strengths have borne fruit in economics, this is it.</td>
</tr>
<tr>
<td>1998 Sen (1)</td>
<td>By combining tools from economics and philosophy, he has restored an ethical dimension to the discussion of vital economic problems.</td>
</tr>
<tr>
<td>2002 Kahneman</td>
<td>For having integrated insights from psychological research into economic science, especially concerning human judgment and decision-making under uncertainty</td>
</tr>
</tbody>
</table>

There are a few cases where similar formulations have been found, but then not as a main contribution. In the Advanced information in 2001 concerning Akerlof it is written that he "has been innovative in enriching economic theory with insights from sociology and social anthropology".

5.4 Introducing and developing new perspectives affecting the whole scientific field

In four cases the laurates can best be described as having come up with ideas that has affected not only a specific part of economics, but economics as a whole even though it is still within the general paradigm described earlier. They have found an aspect that had been neglected in earlier thinking and have made economists look at the economy in a somewhat different way than before. In chronological order of the prizes these four new perspectives are the following
1. The first is the role of transaction costs and making the institutional structure endogenous. In traditional macro- and microeconomics the institutional structure is more or less taken for granted, but the transactions cost perspective opens up questions about why there is a certain institutional structure, including certain types of contracts, and this is of importance for all economic areas. In the article in SJE about Coase one can read: "Coase also demonstrated that the power and precision of analysis may be enhanced if it is carried out in terms of rights to use goods and factors of production instead of the goods and factors themselves..... Coase may be said to have identified a new set of "elementary particles" in the economic system". Coase ideas were then developed by Ostrom and Williamson who won the prize in 2009. In that press release one can read: "Over the last three decades these seminal contributions have advanced economic governance research from the fringe to the forefront of scientific attention".

2. The second new perspective was introduced by game theory. Instead of seeing the economy as driven by anonymous market forces, game theory shows that it can perhaps better be seen as a number of games where each actor base their actions on expectations about what others will do. Beside classical examples of oligopolistic markets, in the motivations it also mentioned that game theory can help us understand political decision makers and central bankers. Schelling is described as having "set forth his vision of game theory as a unifying framework for the social sciences." In the SJE article about Schelling it is stated that game-theoretic concepts, terminology and modes of analysis have come to dominate most areas of economics, and that it is the foundation of major parts of modern economics.

3. The third of the new perspective was the theory of rational expectations, which perhaps can be seen as having two parts: That expectations are very important for behavior and that people are rational when they form these expectations, using available information and theories. In the motivations of the prizes to Lucas [1995] and Phelps [2006] their role for introducing this new perspective is underlined. In the motivations it is stated that this is not only relevant for macroeconomic policy, but also for asset pricing and for various microeconomic models, e.g models based on game theory.

4. The final new perspective is the role of asymmetric information and moral hazard problems, and this is also relevant for a number of issues in both macroeconomics and microeconomics. In the motivations for the price to Vickrey and Mirrlees one can read “Incomplete and asymmetrically distributed information has fundamental consequences, particularly in the sense that an informational advantage can often be exploited strategically.” It is stated that this has generated a better understanding of insurance markets, credit markets, auctions, the internal organization of firms, wage forms, tax systems, social insurance, competitive conditions, political institutions, etc. In the motivation to the prize to Akerlof, Spence and Stiglitz it is said that applications have been abundant, ranging from traditional agricultural markets to modern financial markets.
Table 8  Introducing and developing a new perspective

<table>
<thead>
<tr>
<th>Year/name</th>
<th>Contribution (from motivations)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1) Transaction cost and endogeneity of economic institutions</td>
</tr>
<tr>
<td>1991 Coase</td>
<td>For his discovery and clarification of the significance of transaction costs and property rights for the institutional structure and functioning of the economy.</td>
</tr>
<tr>
<td>2009 Ostrom/Williamsson</td>
<td>Elinor Ostrom: for her analysis of economic governance, especially the commons. Oliver E. Williamson for his analysis of economic governance, especially the boundaries of the firm.</td>
</tr>
<tr>
<td></td>
<td>(2) Game theory and strategic behavior</td>
</tr>
<tr>
<td>1994 Nash/Selten/Harsanyi (2)</td>
<td>In particular, non-cooperative game theory, i.e., the branch of game theory which excludes binding agreements, has had great impact on economic research. The principal aspect of this theory is the concept of equilibrium, which is used to make predictions about the outcome of strategic interaction. John F. Nash, Reinhard Selten and John C. Harsanyi are three researchers who have made eminent contributions to this type of equilibrium analysis.</td>
</tr>
<tr>
<td>2005 Schelling/Aumann (2)</td>
<td>For having enhanced our understanding of conflict and cooperation through game-theory analysis. Aumann was the first to conduct a full-fledged formal analysis of so-called infinitely repeated games.</td>
</tr>
<tr>
<td>2012 Shapley</td>
<td>Used so-called cooperative game theory to study and compare different matching methods.</td>
</tr>
<tr>
<td></td>
<td>(3) The role of (rational) expectations</td>
</tr>
<tr>
<td>1995 Lucas (2)</td>
<td>For having developed and applied the hypothesis of rational expectations.</td>
</tr>
<tr>
<td>2006 Phelps (1)</td>
<td>He developed a new theory of unemployment and inflation that highlighted the role of inflation expectations as well as information problems in the labour market.</td>
</tr>
<tr>
<td></td>
<td>(4) The role of asymmetric information</td>
</tr>
<tr>
<td>1996 Vickrey (1)/Muirlees</td>
<td>For their fundamental contributions to the economic theory of incentives under asymmetric information.</td>
</tr>
<tr>
<td>2001 Akerlof/Spence/Stiglitz</td>
<td>During the 1970s, this year’s Laureates laid the foundation for a general theory of markets with asymmetric information.</td>
</tr>
</tbody>
</table>

If there were such a thing as a mini-revolution within a scientific paradigm, one could argue that there have been these four mini-revolutions within economics during the last 50 years.
5.5. Developing knowledge within specific fields

In this category the prize motivation refer to a specific part of economics and that the laureate has made important substantial contributions to this field. The order is chronological and related to the first time a person was given a prize in this area.

5.5.1 Business cycles and stabilization policy

As mentioned above several persons got the price for work in the intersection between the development of econometric methods and macroeconomic theory. This is clearest for Frisch/Tinbergen, Lucas and Sargent/Sims. Tobin has been included only in this category even though financial markets are mentioned in the motivation as the SJE articles underline that integration of financial markets into macroeconomic theory as more important than his contribution to financial theory as such.

Table 9  Business cycles and stabilization policies

<table>
<thead>
<tr>
<th>Year/name</th>
<th>Contribution (from motivations)</th>
</tr>
</thead>
<tbody>
<tr>
<td>1969 Frisch/Tinbergen (2)</td>
<td>For having developed and applied dynamic models for the analysis of economic processes. Constructed theories for stabilization policy and long-term economic planning.</td>
</tr>
<tr>
<td>1976 Friedman</td>
<td>For his achievements in the fields of consumption analysis, monetary history and theory and for his demonstration of the complexity of stabilization policy</td>
</tr>
<tr>
<td>1981 Tobin</td>
<td>For his analysis of financial markets and their relations to expenditure decisions, employment, production and prices.</td>
</tr>
<tr>
<td>1995 Lucas (3)</td>
<td>For having developed and applied the hypothesis of rational expectations, and thereby having transformed macroeconomic analysis and deepened our understanding of economic policy.</td>
</tr>
<tr>
<td>1999 Mundell (1)</td>
<td>For his analysis of monetary and fiscal policy under different exchange rate regimes and his analysis of optimum currency areas.</td>
</tr>
<tr>
<td>2004 Kydland/Prescott</td>
<td>For their contributions to dynamic macroeconomics: the time consistency of economic policy and the driving forces behind business cycles.</td>
</tr>
<tr>
<td>2006 Phelps (2)</td>
<td>For his analysis of intertemporal tradeoffs in macroeconomic policy.</td>
</tr>
<tr>
<td>2011 Sargent/Sims (2)</td>
<td>For their empirical research on cause and effect in the macroeconomy.</td>
</tr>
</tbody>
</table>
5.5.2 Microeconomics

Microeconomics here covers areas that are included in standard microeconomic textbooks which mean that welfare economics and general equilibrium theory is part of microeconomics. Contributions focusing on specific markets and how they work or can be designed are included, which is why mechanism design theory, asset price theories and studies in regulation are included here.

Table 10 Microeconomics

<table>
<thead>
<tr>
<th>Year/name</th>
<th>Contribution (from motivations)</th>
</tr>
</thead>
<tbody>
<tr>
<td>1969 Samuelson (2)</td>
<td>Samuelson's theory of &quot;revealed preferences&quot;, a theory which has provided economists with considerably improved tools for analysis in consumption theory. A third area where Samuelson has made great contributions is general equilibrium theory.</td>
</tr>
<tr>
<td>1972 Hicks/Arrow (2)</td>
<td>Pioneering contributions to general economic equilibrium theory and welfare theory.</td>
</tr>
<tr>
<td>1978 Simon (2)</td>
<td>What is new in Simon's ideas is, most of all, that he rejects the assumption made in the classic theory of the firm of an omniscient, rational, profit-maximizing entrepreneur.</td>
</tr>
<tr>
<td>1998 Sen (2)</td>
<td>For his contributions to welfare economics.</td>
</tr>
<tr>
<td>2007 Hurwicz/Maskin/Meyerson</td>
<td>For having laid the foundations of mechanism design theory.</td>
</tr>
<tr>
<td>2010 Diamond/Mortensen/Pissarides</td>
<td>For their analysis of markets with search frictions.</td>
</tr>
<tr>
<td>2013 Fama/Shiller</td>
<td>For their empirical analysis of asset prices. Eugene Fama and several collaborators demonstrated that stock prices are extremely difficult to predict in the short run, and that new information is very quickly incorporated into prices. Shiller discovered there was significant predictability over longer time horizons... stimulated the emergence of a new research field, behavioral finance.</td>
</tr>
<tr>
<td>2014 Tirole</td>
<td>For his analysis of market power and regulation</td>
</tr>
</tbody>
</table>

5.5.3 Growth and Development economics

Of the four prizes included here the most problematic case is Deaton as he is an example where the prize is given for three specific contributions to different areas of economics. One could have included him also in microeconomics for his contribution to consumption studies.
Table 11 Growth and development economics

<table>
<thead>
<tr>
<th>Year/name</th>
<th>Contribution (from motivations)</th>
</tr>
</thead>
<tbody>
<tr>
<td>1971 Kuznets</td>
<td>For his empirically founded interpretation of economic growth.</td>
</tr>
<tr>
<td>1979 Schultz/Lewis</td>
<td>For their pioneering research into economic development research with particular consideration of the problems of developing countries.</td>
</tr>
<tr>
<td>1987 Solow</td>
<td>Solow has created a theoretical framework which can be used in discussing the factors which lie behind economic growth in both quantitative and theoretical terms.</td>
</tr>
<tr>
<td>2015 Deaton</td>
<td>For his analysis of consumption, poverty, and welfare: Deaton helped transform development economics from a largely theoretical field based on crude macro data, to a field dominated by empirical research based on high-quality micro data.</td>
</tr>
</tbody>
</table>

For some other prize winners, contributions to growth theory and development economics are mentioned as a secondary contribution (Phelps, Stiglitz and Krugman) but it seems obvious that their other contributions are more important. North/Fogel were above included as contributing by using the economic approach outside traditional areas, but by doing this they also contributed to growth and development economics and therefore they could also have been included here.

5.5.4 International economics

There is an overlap between macroeconomics and international economics as especially Mundell illustrates and he has therefore been included in both categories.

Table 12 International economics

<table>
<thead>
<tr>
<th>Year/name</th>
<th>Contribution (from motivations)</th>
</tr>
</thead>
<tbody>
<tr>
<td>1977 Ohlin/Meade</td>
<td>For their pathbreaking contribution to the theory of international trade and international capital movements.</td>
</tr>
<tr>
<td>1999 Mundell (2)</td>
<td>For his analysis of monetary and fiscal policy under different exchange rate regimes and his analysis of optimum currency areas.</td>
</tr>
<tr>
<td>2008 Krugman</td>
<td>For his analysis of trade patterns and location of economic activity.</td>
</tr>
</tbody>
</table>

It could also be argued that Samuelson should have been added here as the Stolper-Samuelson theorem is mentioned in the motivations, but it was judged that the other contributions were more important.
5.5.5 Financial economics

Studies of financial markets are mentioned especially in three motivations, but the borderline to microeconomics is far from clear. Should Fama have been included in this category instead, or should all of the laurates below be included in Microeconomics? The formulations in the motivations have in the end determined the categorization.

Table 13 Financial economics

<table>
<thead>
<tr>
<th>Year/name</th>
<th>Contribution (from motivations)</th>
</tr>
</thead>
<tbody>
<tr>
<td>1985 Modigliani</td>
<td>For his pioneering analyses of saving and of financial markets.</td>
</tr>
<tr>
<td>1990 Markowitz/Miller/Sharpe</td>
<td>Markowitz: awarded the Prize for having developed the theory of portfolio choice.</td>
</tr>
<tr>
<td></td>
<td>Miller: for his contributions to the theory of price formation for financial assets.</td>
</tr>
<tr>
<td></td>
<td>Sharpe: for his fundamental contributions to the theory of corporate finance.</td>
</tr>
<tr>
<td>1997 Merton/Scholes (1)</td>
<td>Developed a pioneering formula for the valuation of stock options. Their methodology has paved the way for economic valuations in many areas.</td>
</tr>
</tbody>
</table>

5.6. New practical tools

There are three early prizes that do not fit neatly into any of the categories above but have in common that they developed more practical tools that are useful in various context. This concerns input-output analysis, linear programming (activity analysis) and the development of a system for national accounts.

For three later prizes it is especially underlined in the motivations that they have developed methods that are used to a large extent on specific markets. This concerns Vickrey and design of auctions, Roth and the design of matching models and Black/Scholes option pricing formula. They are therefore also included here even if they also are included in earlier categories. (There are of course numerous laurates whose contributions have influenced economic policy but this is not included here.)
<table>
<thead>
<tr>
<th>Year/name</th>
<th>Contribution (from motivations)</th>
</tr>
</thead>
<tbody>
<tr>
<td>1973 Leontieff</td>
<td>For the development of the input-output method and for its application to important economic problems.</td>
</tr>
<tr>
<td>1975 Kantorovitch/Koopmans</td>
<td>Early in his research, Professor Kantorovich applied the analytical technique of linear programming to demonstrate how economic planning in his country could be improved. Professor Koopmans, for his part, has shown for instance that on the basis of certain efficiency criteria, it is possible directly to make important deductions concerning optimum price systems.</td>
</tr>
<tr>
<td>1984 Stone</td>
<td>For having made fundamental contributions to the development of systems of national accounts and hence greatly improved the basis for empirical economic analysis.</td>
</tr>
<tr>
<td>1996 Vickrey (2)</td>
<td>His endeavors have provided the basis for a lively field of research which, more recently, has also been extended to practical applications such as auctions of treasury bonds and band spectrum licenses.</td>
</tr>
<tr>
<td>1997 Merton/Scholes (2)</td>
<td>Thousands of traders and investors now use this formula every day to value stock options in markets throughout the world.</td>
</tr>
<tr>
<td>2012 Roth (2)</td>
<td>Helped redesign existing institutions for matching new doctors with hospitals, students with schools, and organ donors with patients.</td>
</tr>
</tbody>
</table>

In the motivation of the price to McFadden his extensive applications of his own methods is mentioned, especially the design of the San Francisco BART system, but in the cases in the table above the role of the practical applications seems more important.

6. Results (2) What have they done - the quantitative text analysis

In this section the results of the quantitative analysis will be presented and these results will be used in the analysis in the following sections.

6.1 The short motivations

The results from the quantitative text analysis of the short motivations are presented in Table 15, ordered after how many times they were mentioned.

The most common word used in the short motivations is that the laureate has presented an "analysis" of something. This word is used in 26 of the 47 motivations. Analysis is a broad term and can cover a number of more specific activities, e.g. formulating theories and explanations, and investigating empirically. The second most common formulation is that
the contribution in one way or another concerned formulating or developing "theories". This was mentioned in 19 of the short motivations.

After these two there is a large jump to the motivations where "method" (6 prizes) or "empirical" (5 prizes) are mentioned. It can be noted that prediction and forecast is not mentioned in any of the short motivations, but also that explaining, understanding and causes are only mentioned in 1 or 2 motivations.

Table 15 Words used in the short motivations

<table>
<thead>
<tr>
<th>Word</th>
<th>Number of prizes (out of 47)</th>
<th>Total number of occurrences</th>
</tr>
</thead>
<tbody>
<tr>
<td>analyze/analysis/analytical</td>
<td>26</td>
<td>30</td>
</tr>
<tr>
<td>theory/theoretical</td>
<td>19</td>
<td>22</td>
</tr>
<tr>
<td>method</td>
<td>6</td>
<td>8</td>
</tr>
<tr>
<td>empirical</td>
<td>5</td>
<td>5</td>
</tr>
<tr>
<td>model</td>
<td>2</td>
<td>2</td>
</tr>
<tr>
<td>understand</td>
<td>2</td>
<td>2</td>
</tr>
<tr>
<td>cause</td>
<td>2</td>
<td>2</td>
</tr>
<tr>
<td>problem</td>
<td>2</td>
<td>2</td>
</tr>
<tr>
<td>explain/explanation</td>
<td>1</td>
<td>1</td>
</tr>
<tr>
<td>practical/practice</td>
<td>1</td>
<td>1</td>
</tr>
<tr>
<td>predict/prediction</td>
<td>0</td>
<td>0</td>
</tr>
<tr>
<td>forecast</td>
<td>0</td>
<td>0</td>
</tr>
</tbody>
</table>

6.2 The speeches to the laurates

In 42 of the 47 speeches the word theory or theoretical is mentioned, and on average 8 times in each of these 42 speeches. That theory is not mentioned in five of the speeches seem more to depend on chance than on any special features of these prizes, as these five are Schultz/Lewis [1979], Merton/Scholes [1997], Mundell [1999], Akerlof/Spence/Stiglitz [2001] and Sargent/Sims [2011].

Analyze/analysis/analytical is mentioned in 40 of the 47 prize speeches, and in 34 speeches the prize winner is described as having contributed to solving a problem of some kind. It can of course depend on chance and personal vocabularies, but 8 of the 13 cases where problem is not mentioned are from the last 11 years.

There are five terms that are mentioned in a little more than half of the speeches and that is some kind of methodological contribution (27 prizes), that the prize winner has developed a model (26 prizes), made an empirical contribution (25 prizes), has increased our
understanding of some phenomena (25 prizes) and has explained something (24 prizes). The prize winner is described as finding out causes of something in 13 cases, and this should be seen as related to explaining.

Prediction and forecast in mentioned in 10 and 6 cases. There is no overlap between these and there is a rather clear time pattern in this case where 5 of the 6 forecast-cases come from 1990 or earlier, but 9 out of 10 cases where prediction is used comes from after 1990. Forecast is typically used in practical contexts and as a foundation for policy, while prediction in used both in practical contexts and in the context of a whether a certain prediction of a theory has empirical support or not. The prize where prediction/predictability is mentioned most times is in the speech to Fama and Shiller where it is mentioned 7 times.

That the laureates have in some way contributed to practice or to solving practical problems are mentioned in 10 of the prizes.

Table 15 Words used in the speeches

<table>
<thead>
<tr>
<th>Word</th>
<th>Number of prizes (out of 47)</th>
<th>Total number of occurrences</th>
</tr>
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<td>199</td>
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<td>15</td>
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<td>forecast</td>
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</table>

7. Analysis (1): Paradigm vs Research traditions

The classification above of the contributions are more in line with Laudan´s more flexible research traditions than Kuhn´s and Lakatos more rigid structure.
7.1 The theoretical core

Rationality has been seen as a hard-core assumption in economics, but the prizes to Simon in 1978, Kahneman in 2002 and Shiller in 2013 are motivated by their questioning of at least the typical interpretation of rationality in terms of maximization and/or behaving according to the expected utility model. In the Advanced information about Fama/Shiller one can read:

"Many of these papers can be thought of as modifying the preference assumptions in rational-agent models,....., which illustrates a convergence between rational and behavioral models in recent research." (p 32).

In the Advanced information about Kahneman/Smith they write:

"A large and growing body of scientific work is now devoted to the empirical testing and modification of traditional postulates in economics, in particular those of unbounded rationality, pure self-interest, and complete self-control."

This means that the standard assumptions were not seen as a hard core that always should be defended.

Hands (2015) writes in an article about "Orthodox and heterodox economics in recent economic methodology" that

"while there are many economists critical of mainstream neoclassical practice, those who are, seem to be focused on particular problems, applications, and tools, rather than self-identifying with any general heterodox school of thought" (p 65).

This means that you can combine mainstream methods with a large set of different assumptions about human behavior, and this is inconsistent with a view that says that there is a theoretical hard core with rather narrow assumptions about human behavior that has to be followed.

7.2 The methodological core

Above it was argued that in the dominating paradigm mathematical model building could be seen as a core strategy in theoretical economics and econometric estimation in empirical economics, but also here the result above shows a much more flexible reality.

Focusing on theorist during the last 25 years there are a number of exceptions to the rule that a theoretical economist builds mathematical models, primarily Coase [1991], Ostrom and Williamson [2009], and to a large extent also Schelling [2005].

In the same way there are prize winners who in their empirical work do not use standard econometric methods. Vernon Smith got the prize for making laboratory experiments an
acceptable method in mainstream economics. Different forms of case studies were the primary type of empirical studies in the works of Coase, Ostrom and Williamson. In the Popular information one can read that

"[Ostrom’s] main accomplishment has been to collect relevant information from a diverse set of sources about the governance – successful and failed – of a large number of resource pools throughout the world and to draw insightful conclusions based on systematic comparisons."

In the SJE article about Williamson his empirical methods are described in the following way:

"two aspects of the methodology that Williamson proposed and practiced: (a) microanalysis of transaction detail and (b) comparative institutional analysis." (p 266).

7.3 How a paradigm is transformed

The material presented above is consistent with certain general hypotheses about how a paradigm is changed. The first hypothesis is that you should have patience and be tenacious. In the Advanced information about Smith and Kahneman it is written:

"When they appeared, Kahneman’s and Smith’s initial works were received with skepticism by the scientific community in economics. It took considerable time and much further research before their main ideas seriously began to penetrate the profession."

The reaction from researchers in a certain field, in line with Kuhn’s view of normal science, is rejection of new ideas. The normal scientist assumes that new ideas most often are wrong or based on a misunderstanding of the current paradigm. Soon the new ideas will fade. But if the researcher continues and tries to answer the objections by developing arguments and methods and continue to show interesting results, they might get accepted in the end.

The second hypothesis is that it is easier to get new ideas and alternative methods accepted if the researcher has, so to speak, one foot within the accepted paradigm. Hayek and Myrdal first made contributions to standard monetary theory before broadening their perspective. Herbert Simon [1978] also made contributions to "hard areas" like mathematical statistics and operations research. Amartya Sen [1998] first wrote Collective choice and social welfare that mathematically investigated the sensitivity of Arrow’s impossibility theorem to different assumptions, before he started to write more philosophically inspired works. The fact that many of Vernon Smith’s initial works came to conclusions that supported standard conclusions in economic theory probably helped to make laboratory experiments more acceptable. In the works of Coase, Ostrom and Williamson assumptions about rationality and individualism are accepted, and there is a focus on efficiency, and this should have contributed to make their work more acceptable.
The third hypothesis is that if you come from another field and want to affect the development of economics you should learn the language of economics. In the SJE article about Kahneman one can read:

"But it is even rarer for critics from one discipline to persistently and intelligently insinuate themselves into another discipline by making an effort to use a common language and to minimize conceptual differences. Yet this is what two brilliant psychologists, Daniel Kahneman and Amos Tversky, have done since the early 1970s."

Getting an article into Econometrica definitively increases credibility among economists.

Laudan (1977, p 98) writes:

"But, perhaps more often, scientists find that by introducing one of two modifications in the core assumptions of the research tradition, they can both solve the outstanding anomalies and conceptual problems and preserve the bulk of the assumptions of the research tradition in tact."

This seems to be a good description of what has happened in economics during the studied time period and supports the following description by Hands (2015, p 70):

"John Davis, my co-editor of The Journal of Economic Methodology, and others, have suggested that the mainstream of disciplinary economics is no longer neoclassical: that the once dominant neoclassical framework has been replaced by a new, more pluralistic, mainstream which is more open to psychology, less individualistic, accommodates various types of path-dependencies, and allows for a much broader class of modeling strategies and tools."

7.4 Why there will not be a scientific revolution in economics

If a research tradition is flexible and incorporates new ideas and methods, even if it takes considerable time and effort to get these new ideas and methods accepted, then there will never be a scientific revolution in the Kuhnian sense. When various anomalies come up and competing ideas and methods are launched, they are after a while absorbed into the research tradition instead of leading to a situation with competition between paradigms where a new and competing school eventually wins over the old one. It could perhaps also be argued that Kuhn’s theory of scientific revolutions partly is a self-defeating theory in the sense that when leaders in a certain paradigm realize that there is such a thing as a scientific
revolutions, they become more interesting in reforming their paradigm and opening up to new ideas and methods in order to avoid to a future revolution.  

8. Analysis (2): Different types of researchers

8.1 Introduction

The idea that specialization increases efficiency is central in economics, and given that insight it seems a little surprising that the degree of specialization is not higher than it is. If the researcher has a problem with an econometric method in a certain context, the first idea could be to co-operate with pure statisticians and let them solve the problem. The result could be a joint article showing how to solve the problem. The Nobel Prize winners in econometrics have however typically both developed new methods and applied them. Of course they have studied statistics to a large extent, but they are working in economics department. According to my knowledge there are consultations with statistical experts in many projects, but one can imagine a much higher degree of specialization. In the same way, the economic theoreticians that have got the prize and that build mathematical models also typically building the models themselves. The cooperation between Arrow and Debreu can be seen as an exception, but Debreu also made his own analysis of economic models and was not only a hired mathematician. Hurwicz who developed matching models called himself a mathematician and showed how to solve specific matching problems, without any direct co-operation with economists.

The point so far is then that there is no clear general division of labor between statisticians, mathematicians and “pure” economists in the material. In order to understand the division of labor that occurs the strategy below is instead to describe three research styles among the Nobel prize winners and the kind of specialization that occurs within each style.

8.2 Economic research style (1)

The description of the scientific contribution of Paul Krugman seems to be an very good example of what could perhaps be called the economic research style:

A. There is an anomaly, an observed fact that earlier theories cannot explain. The large share of intra-industry trade was the anomaly in the Krugman case.

B. There is a new idea about what can explain this anomaly, in Krugman´s case the new idea is that a combination of low transportation cost, economies of scale in production and a taste for diversity can explain intra industry trade.

---

6 Maybe a political scientist could draw a parallel to political revolutions, and that other governments start to make reforms in order to avoid revolutions if they observe a revolution in a nearby country.
C. This idea is *proven mathematically to hold* given certain assumptions that are not looked upon as very restrictive.

In the SJE-article by Neary one can read

"Thus the model is consistent with the empirical evidence on intra-industry trade, and also shows that it leads to gains from trade: just as in Ricardo, there are precise predictions about both positive and normative aspects of trade, with the difference that the countries are identical so there is no role for comparative advantage." (Neary 2009 p 223)

The new models might also give unexpected implications. In the Advanced information one can read:

"It is shown that these models lead to qualitatively new and interesting effects of standard trade policies: protection could reduce domestic output, import subsidies could improve the terms of trade, and tariffs could reduce domestic prices"

After the initial model there is a number of theoretical works that investigates whether the relations hold if the initial assumptions are changed and e.g. made less restrictive and more realistic. In the Information to the public they write: "In many subsequent articles and books, Krugman himself, as well as other researchers, have endowed the theory with greater realism". Neary describes this process under the heading "Models of infinite variety". The continued theoretical work can be done by the researcher that built the initial model, but typically other researchers are drawn into the area, some perhaps because they want to show that the theory really doesn’t hold under more realistic assumption, some perhaps because they want to defend the theory and show that it actually holds also under more realistic assumptions, and some just because it is a new hot area for research.

Neary (2009, p 225) also writes: "

But revolutions, especially intellectual ones, are more successful when they encompass the old view rather than totally supplanting it. So an important step in consolidating the monopolistically competitive approach to international trade was to show how it could be embedded in a Heckscher–Ohlin competitive trade model".

D. Econometric studies *(or laboratory experiments)* are carried out and they show that observations are consistent with the theory, at least to a large extent and maybe combined with earlier theories. In the Advanced information one can read "The richness of model predictions inspired a lively empirical literature" (p 10) and that

"They found that allowing for scale economies improved the fit of the model, and that around one third of all industries could be characterized by increasing returns to scale. Similarly, Evenett and Keller (2002) conclude
that trade patterns are best explained by a combination of increasing returns to scale and factor proportions" (p 10).

There were also other kinds of empirical studies. In the Advanced information it is written (p 9): "Realistic models easily become too complex to be handled analytically and estimated by econometric methods. Instead, a vast literature of calibrated numerical models has emerged." These will be described more in relation to research style 3 below.

8.3 Economic research style (2)

The second research style can be exemplified by early winners as Kuznets and Friedman, and by a more recent winner, Williamson.

The first two steps are roughly similar to the style above. There is a (A) a problem in the dominating theories and (B) a new theory is formulated. Friedman argued that the role of the quantity of money was underestimated. Williamson argued that vertical integration can have efficiency motives (reduction of transaction costs) and not necessarily by a desire to reduce competition. The theory that can be described as an hypothesis of the form "X leads to be Y". In the SJE article about Kuznets (p 448) there is a quotation where he writes "... by a theory we mean a statement of testable relations among empirically identifiable factors, such relations and factors having been found relatively invariant under diverse conditions in time and space" (Economic Growth and Structure, p. 4)."

(C) Then the researcher goes directly to empirical investigations of the hypothesis. These empirical studies can take many forms. It can be detailed analysis of historical episodes and case studies, as Friedman, Ostrom and Williamson use. However, the theories can also be tested by the same type of econometric estimations as in research style 1. A number of such studies are mentioned in the Advanced information (p 4) and in the SJE-article exemplified with Joskow’s study of location of coal mines and electricity producing plants as a classic work. The empirical work is summarized by saying that "By now, there is a wealth of evidence showing that vertical integration is affected by both complexity and asset specificity" (p 4). As above, laboratory experiments can also be made to test the theories.

The main difference between researchers using style 1 and researchers using style 2 is that the first group of researchers builds mathematical models showing a set of "reasonable" sufficient conditions under which it can be proved that the theory holds (given Z, it can be proved that X leads to Y). Notice however that other researchers can take the ideas of e.g. Williamson and build the same type of models as in research style 1, but the point here is that this is not part of the research strategy used by the Nobel prize winner. In the SJE-article about Williamson one can e.g. read (p 275)

"While TCE research on contracting has been overwhelmingly empirical, recent formal modeling by Bajari and Tadelis (2001) on the effect of
complexity on contract design is very much in the spirit of TCE’s informal arguments about contracts."

There are some comments on this difference in the use of mathematical models in theoretical economics in the SJE text about Williamson (p 281).

"Williamson’s contributions to TCE are a counter-example to Krugman’s (1995, p. 27) dictum “Like it or not, . . . the influence of ideas that have not been embalmed in models soon decays”…….. [Williamson was] cautioning against “prematurely formal theory [that] purports to deal with real phenomena without doing the hard work of making serious contact with the issues” [1993, p. 43]."

The underlying issue seems to be how much one needs to know about institutional detail in order to build an interesting mathematical model. This will returned to in the section about "rigor vs realism" below with Tirole as an interesting example.

8.4 Economic research style (3)

This third research style differs in several ways from both of the earlier ones. The core of this strategy is that (A) one builds a model covering "the whole economy", and (B) that this model then is used to predict the effect of various events. To this group laurates one can count early winners like Tinbergen and Klein, but also more recent winners like Kydland/Prescott and Sargent/Sims that focus on building computable general equilibrium models.

If we focus on the most recent winners in this group the strategy can be described as follows:

- In order to predict the effect of a certain event or policy it is necessary to take into account interactions through several markets.

- A mathematical model is therefore build that takes such interactions into account. Over time there has been changes in the views about what should characterize a good model but if we look at the most recent winners (Sargent/Sims) and the description of their contribution there are several steps in the construction of a useful economic model.

The basic idea in the VAR model is described in the Advanced information in the following way:

"Technically, a VAR is a straightforward N-equation, N-variable (typically linear) system that describes how each variable in a set of macroeconomic variables depends on its own past values, the past values of the remaining N-1 variables, and on some exogenous shocks".
Notice that there is no specific hypothesis here of the type "A leads to B". The model is then calibrated using historical data and after that used for predictions.

In order to make more efficient estimations various restrictions can put on the model. In the Advanced information one can see that those restrictions can come from theory (assuming e.g. that persons have rational expectations) but that a number of other features can be included in the models (e.g. learning).

The models can be evaluated by their predictive abilities and in the Advanced information they write (p 14) "Forecasts with VARs have been compared with simple alternatives, such as forecasting based on univariate models or pure random walks, and have often been shown to outperform these techniques." It is however also written (p 15) "Nowadays, a new approach is gaining ground, where the prior is based on modern macroeconomic theory. That is, restrictions are formed based explicitly on how the econometrician a priori thinks the world works, expressed in the form of a model." and (p 16) "The identification task now is to impose appropriate restrictions ..... This requires knowledge of how the economy works and a method for making use of such knowledge." In the SJE article it is described how sticky prices and other frictions have been added in later models (p 1095). In the information to the public for the price to Kydland/Prescott they write:

"Although Kydland and Prescott’s first model was highly stylized, it laid the ground for a far-reaching research program. The mechanisms in subsequent analytical models have become increasingly more realistic."

The picture that emerges is then that these models are evaluated both by their predictive ability and by the reasonableness of their restrictions/assumptions. If the predictions are not very good in a certain case, the strategy in recent work seem to be to try to make the assumptions "more realistic" in some dimension that seems important, but if the predictions seem reasonable, the assumptions are seen to be "good enough". There is an implicit hypothesis that more realistic assumptions will give better predictions, and/or that empirically more credible assumptions make the predictions of the model more credible.

9. Analysis (3): Classical methodological controversies in economics

9.1 Friedman’s instrumentalism

It can first be noted that in the short motivation, the press release and in the speech to Friedman at the prize ceremony, there is not even a mention of his methodological works and the idea that the assumptions in a theory should be evaluated by their implications only.

Uskali Mäki has discussed the interpretation of Friedman’s work in a number of articles and also formulated what he calls "minimal scientific realism" (Mäki 2011) which he describes in the following way: "What these share in common is the realist conviction that there is a fact
of the matter about whether Y exists or does not exist and that science has the task of finding out whether it does. (p 5-6)". Mäki (2011) also writes

"Economists often build models that isolate simple systems from their surroundings, but they typically consider this an effective method for accessing causal mechanisms active in complex systems that are dynamically open. Moreover, they typically are realists about the mechanisms they model." (p 3).

If the Nobel Prize committee does not mention Friedman’s methodological works a reasonable conclusion must then be that they do not think that his methodological views are an important contribution and that their view instead is in line with Mäki’s "minimal scientific realism" view. Friedman got the prize because of his works where he claim that money supply actually causes inflation, and that permanent income actually explain the level of current consumption.

Here are just a few additional examples to support the claim that the committee has a realist view. In the press release when Lucas got the prize one can read:

"Expectations about the future are highly important to economic decisions made by households, firms and organizations. ….. The rational expectations hypothesis means that agents exploit available information without making the systematic mistakes implied by earlier theories."

People are not stupid and a theory that assumes that people are stupid should be replaced by one that make more realistic assumptions. The press release when Akerlof, Spence and Stiglitz got the prize start with the following words:

"Many markets are characterized by asymmetric information: actors on one side of the market have much better information than those on the other. ….. During the 1970s, this year’s Laureates laid the foundation for a general theory of markets with asymmetric information."

The motivation of the prize to Kydland and Prescott mentions their contribution concerning "driving forces behind business cycles".

In the Advanced information to the 2002 prize to Smith and Kahneman one can read: "If deviations from rationality and self-interest were systematic, ….., this would call for a revision of economic theory itself" (p 2). When Rubinstein in the SJE-article calls Nash "The Master of Economic Modelling" he writes: "The art of economic theoretical modeling is the identification of simple structures which approximately represent the process by which people reason about a situation." (p 11) and "Economic theory should deal with the real world. It is not a branch of abstract mathematics even though it utilizes mathematical tools." (p 12).

In what is called Research Style 3 above the idea that models should be evaluated by their predictive ability is not so far-fetched and Research Style 3 is close to Friedman’s view, but
as was clear from the discussion above, there is also in this tradition a need for the models
to have assumptions that are judged to be descriptively credible. But here "good enough"
can actually be interpreted at least partly as "gives reasonable predictions" and in that sense
it is a closer to Friedman’s instrumentalist views than the other research styles where the
researcher looks for actual causal mechanisms and increased understanding of what actually
explain various anomalies.

Looking at Samuelson’s idea of successive approximations it might actually be true for the
short-time development within especially Research Style 1 described above. When a new
idea is launched, the first models are of radically simplified and then models with more
realistic assumptions are developed in order to test how robust the result is. This view is in
line with an earlier study of normal science in theoretical economics (Lind 1992). But over a
longer time period, adding new ideas and perspectives (beside new methods) are the central
contributions to progress in economics, not making assumptions more and more realistic. As
will be returned to below, the economic tool box of ideas and methods grow over time, but
this can hardly be called a process of successive approximations.

9.2 Rigor vs relevance

The term "rigorous analysis" in theoretical economics can typically be interpreted as an
analysis that shows sufficient conditions for a certain relation. A mathematical model is built
and then it is proved that a certain relation holds in the model. Most often it is shown that:
"Given Z, it can be proved that X leads to Y" but it might also have the structure, "Given Z, it
can be proved that X" (e.g. that equilibrium exists).

As was discussed above, the Nobel prize has been given to researcher with different views
about mathematical models of this kind. In the SJE article about Frisch it is stated that he
was skeptical to "empty formalization" and in the article about Tinbergen that he was "never
much interested in esoteric theorizing". Kuznets, Leontieff and Friedman are other early
examples and while Williamson is the most obvious later example.

It is also clear in the material that it is seen as positive that the researcher that builds
mathematical models is interested in taking the step to practical implementation, even if it
isn’t a necessary conditions for important research. In the press release for the Vickrey prize
they e.g. write "He has not only made significant theoretical contributions, but - unlike most
excellent theorists - he has also followed up on his proposals all the way to their practical
application." In the SJE-article about McFadden, Manski writes

"McFadden's research combines conceptual contributions of great
generality with practical contributions of immediate usefulness. These
surely are the characteristics of a recipient of the Nobel Memorial Prize in
Economics."
An interesting division of labor can be found in the Shapley/Roth prize where the mathematician Shapley showed that a certain mechanism could solve a stylized matching problem, while Roth tested the mechanism in laboratory experiments and also helped with implementing new matching mechanism for several real markets.

In the Advanced information concerning the prize to Tirole they write:

"Tirole’s rigor has facilitated realism. Division of labor in the scientific community frequently encourages theorists to specialize in understanding the inner logic of new models, leaving the challenging job of confronting the models with reality to more applied scientists. As a result, theoretical work sometimes seems detached from "the real world" and "relevant practice". By contrast, Tirole has carefully designed his models to capture essential features of specific economic environments, and to highlight important mechanisms that previous applied research had either ignored or failed to articulate with sufficient precision."

Here we can see a clear relation to Williamson’s comment above that one shall wait with building models, or at least wait with drawing conclusions from the models, until one has made detailed studies of the specific context.

In the SJE article about Vickrey, Drèze quotes von Neumann saying

"... mathematical ideas originate in empirics. ... after much 'abstract' in breeding, a mathematical subject is in danger of degeneration. ... the only remedy seems to be ... the reinjection of more or less directly empirical ideas" and then adds "That remark applies a fortiori to economic theory." (p 194).

In the SJE article about Schelling it is written:

"Schelling stands as an exemplar of a research style that relies on deep integration between observations from life and theoretical thinking, where each motivates and enriches the other. Newcomers to research in economics and game theory could find no better role model."

As the discussion above about instrumentalism indicates, the main view in the material is that there is no conflict between rigor in the form of building mathematical models and relevance. Building mathematical models is a strategy that will help us understand the real world better, but of course it has to be a model that captures essential relations. And knowing what are essential relations requires institutional knowledge as the case with Tirole illustrates. One can relate this to Sugden’s discussion about "Credible worlds" (Sugden 2000) where Schelling’s model of segregation is used as one example of a model that in a number
of respects is unrealistic, but still captures what is believed to be central mechanisms behind segregation.\footnote{The interpretation of the argument about models as credible worlds is discussed in a special issue of the philosophical journal Erkenntnis (no 2 2009), including the contributions Mäki (2009) and Sugden (2009).}

If the model is credible, than the fact that something is the case in the model will be an argument for that it is also a case in the real world. If we can argue that Z creates a credible world and then proving that given Z, X will lead to Y, then there is reason to believe that X will lead to Y also in the real economy.\footnote{Notice that mathematical models also have other roles, as can be illustrated by Arrow’s impossibility theorem.} The evidential value of the result in the mathematical model in relation to the empirical hypothesis about a causal relation will depend upon how convincingly the researcher can argue that the model really captures important relations, but also on how sensitive the results are to various more technical assumptions. An extreme hypothesis was once formulated by McCloskey (1991) as a "Metatheorem on Hyperspaces of Assumptions" stating that for every model there is another model with assumptions arbitrarily close to the original model but with opposite conclusions. If that would be the case, the evidential value of the result in the mathematical model would be low, but there is of course no reason to believe McCloskey is right. Lind (2007) uses a Bayesian approach to evaluate results in models, where the proof that something holds in the mathematical model is seen as providing "new information" and that Bayes theorem then can be used as framework for evaluating how much a rational person would change their opinion about the probability of the empirical statement "X leads to Y" when they get the new information that "Given Z, it can be proven that X leads to Y".

10. Analysis (4): Progress in terms of problems solved and problem solving ability

10.1 Problems solved: What do we know now that was not known earlier?

The quantitative analysis of the speeches to the laurates showed that the word "problem" occurred in 34 of the 47 speeches. In the classification of contributions above one can see what kind of contribution they have made according to the committee and in the material about the each laurate one can read about specific problems they have solved and the kind of knowledge they have added, according to other leading experts that have written the SJE-articles. The ambition in this section is to categories the claimed contributions on a more abstract level. If the Nobel prize motivations are read from the perspective of "what do we know after their work that wasn’t known before" and think in terms of "type of things added" it seems to be possible to describe the contributions in terms of four main categories:
-adding a new concept
-showing (only) logical relations
-arguing (only) for empirical relations
-and a combination of the last two.

For each of these different types of contributions a table with examples is presented below, and the examples are chosen from different time periods. (There is here an implicit claim that there has been no clear changes in kinds of knowledge added over time.)

(1) Adding new concepts

One kind of contribution is to introduce a new concept and/or adding a new distinction. There are six clear cases where this kind of contribution is mentioned.

Table 16 Contributions in the form of new concepts

<table>
<thead>
<tr>
<th>Year/person</th>
<th>Conceptual contribution</th>
</tr>
</thead>
<tbody>
<tr>
<td>1970 Samuelson</td>
<td>Revealed preference</td>
</tr>
<tr>
<td>1976 Friedman</td>
<td>Permanent income, different types of policy lags</td>
</tr>
<tr>
<td>1991 Coase</td>
<td>Transaction cost</td>
</tr>
<tr>
<td>1994 Nash/Harsanyi/Selten</td>
<td>New equilibrium concepts: Nash equilibrium, subgame perfect equilibria</td>
</tr>
<tr>
<td>2005 Aumann/Schelling</td>
<td>Correlated equilibrium, focal point</td>
</tr>
<tr>
<td>2012 Shapley</td>
<td>Core, Shapley value</td>
</tr>
</tbody>
</table>

(2) (Only) logical implications or logically necessary conditions

In many motivations the contributions are described in terms of relations, implications, conditions, determinants and effects. In some cases it is clear that the relations involved are logical relations where a certain relation can be proved with mathematical/logical methods. In these cases there is no empirical claim, and this distinguishes these cases from the cases discussed below. Examples of such cases are made are presented in the table below.
Table 16 Contributions in the form of logical relations

<table>
<thead>
<tr>
<th>Year/person</th>
<th>Logical relations (examples)</th>
</tr>
</thead>
<tbody>
<tr>
<td>1970 Samuelson</td>
<td>Necessary conditions for relations...</td>
</tr>
<tr>
<td>1972 Arrow</td>
<td>Impossibility theorem</td>
</tr>
<tr>
<td>1983 Debreu</td>
<td>Necessary conditions for...</td>
</tr>
<tr>
<td>1988 Allais</td>
<td>Necessary conditions for..</td>
</tr>
<tr>
<td>1994 Nash</td>
<td>Solutions in certain games</td>
</tr>
<tr>
<td>2005 Aumann</td>
<td>Solutions in certain games</td>
</tr>
<tr>
<td>2012 Shapley</td>
<td>Certain algorithms lead to stable matching</td>
</tr>
</tbody>
</table>

(3) (Only) empirical relations

In other cases it is clear from the context that the prize concerns a pure empirical claim about certain relations. There is no indications in the motivations that these relations can be proved to hold given certain assumptions (even if this of course still can be the case). Some examples are given in the table below.

Table 16 Contributions in the form of empirical relations

<table>
<thead>
<tr>
<th>Year/person</th>
<th>Empirical relations (examples)</th>
</tr>
</thead>
<tbody>
<tr>
<td>1974 Hayek</td>
<td>Market economy and possiblity of rational calculation</td>
</tr>
<tr>
<td>1976 Friedman</td>
<td>Money supply and inflation, permanent income and consumption</td>
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<td>Group interests and regulation</td>
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<td>1991 Coase</td>
<td>Transaction cost and contract form</td>
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<td>1993 North</td>
<td>Institutional structure and economic growth</td>
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<td>1998 Sen</td>
<td>Real income losses and famines</td>
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<tr>
<td>2002 Kahneman</td>
<td>Loss aversion and behaviour under uncertainty</td>
</tr>
<tr>
<td>2009 Williamson</td>
<td>Asset specificity and vertical integration</td>
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</table>

(4) Combination of logical and empirical relations

Given what was describe above as the dominating research strategy in modern economics (Research strategy 1) it should not be surprising that in many motivations it is not really clear what kind of relations that is claimed to exist. In this research strategy there is both an empirical claim (a certain causal relation) and a logical claim (this can be proved to hold given certain assumptions). Some examples are given below.
Table 16 Contributions in the form of both logical and empirical relations

<table>
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<th>Year/person</th>
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<tr>
<td>1977 Ohlin</td>
<td>Factor proportions and trade</td>
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<td>1987 Solow</td>
<td>Determinant of growth</td>
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<td>1995 Lucas</td>
<td>Inflationary expectations and inflation</td>
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<td>1999 Mundell</td>
<td>Exchange rate system and effects of macroeconomic policy</td>
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<tr>
<td>2001 Spence</td>
<td>Asymmetric information and signalling behaviour</td>
</tr>
<tr>
<td>2008 Krugman</td>
<td>Taste for diversity, economies of scale and intraindustry trade</td>
</tr>
<tr>
<td>2013 Fama</td>
<td>New information and change in asset value</td>
</tr>
</tbody>
</table>

Empirical laws and lawlike relations?

If the material about the Nobel prize winners is looked upon from the perspective of generally accepted empirical quantitative laws very little can be found.

Thygesen’s article about Friedman is interesting from this perspective. One could interpret Friedman as claiming that a percentage change in money supply leads to the same percentage change in nominal income. But Thygesen writes: "In work during the last decade Friedman has become less emphatic in his insistence that money demand is insensitive to interest rate variations" (p 64) and "Friedman sees only very limited scope for interest-induced offsets to money stock changes in velocity", but this means that there is some scope and not a one to one relation between money stock and nominal income.

In Uhlig’s article about Sargent’s contribution to economics (Uhlig 2012) one can find the following semi-quantitative formulations:

"There is now widespread agreement that although monetary policy and monetary policy shocks are a part of the story of business cycles, they are nowhere near as important as the original Friedman–Schwartz hypothesis appeared to suggest." (p 1088)

"However, a consensus seems to be emerging concerning one key question: the fraction of business-cycle variance due to technology shocks is less—perhaps even substantially so—than the estimate of 70 percent suggested by Prescott (1986)." (p 1096)

In the Advanced information concerning the price to Fama, Hansen and Shiller one can read:

"the research of the Laureates has greatly improved our understanding of asset prices and revealed a number of important empirical regularities as well as plausible factors behind these regularities."(p 1)
the first point: "By and large, the vast majority of event studies have supported this hypothesis. Some exceptions have been found, however" (p 13).

There are almost no comments about "the nature of economic relations" in the motivations and speeches. The only one found is the following from the speech to Milton Friedman:

"Unfortunately the social sciences - despite high ambitions - can never reach the hoped for exactitude. The enormous capabilities of people and governments to create new complications, new contradictions and conflicts, are inexhaustible and go far above and beyond the economists' powers to bring order into the system."

One is reminded of Aristotles words in the first chapter of The Nicomachian Ethics

"for it is the mark of an educated man to look for precision in each class of things just so far as the nature of the subject admits; it is evidently equally foolish to accept probable reasoning from a mathematician and to demand from a rhetorician scientific proofs." (Aristotle, nd)

Expecting knowledge about quantitative laws in a complex system with many interacting factors and where the system changes over time is hardly reasonable.

10.2 Problem solving ability

It seems to be rather easy to describe the development of economics as a clear growth in problem solving ability. This concerns at least the following things.

(1) Mathematical methods and strategies.

That there has been an increase in the problem solving ability of economics when building mathematical models are part of the problem solving strategy, can hardly be questioned. It is not only a matter of introducing more advanced mathematical techniques but also a development of strategies for certain general types of problem in mathematical models. In the 1997 prize to Merton & Sholes the prize was not primarily given for the option value formula as such but "for a new method to determine the value of derivatives". In the speech to Vickrey and Mirrlees [1996] it is said that Mirrlees: "solved the income tax problem in a way which has become a paradigm for a broad class of economic problems".

The problem solving ability is also related to the development of standard models that can be used to study a large class of problem. The development of the overlapping generations models are e.g. mentioned in the prize motivation for Allais. In the prize motivations for Modigliano it is written that "The life-cycle model has had a great impact on the
development of later theoretical and empirical research. It represents in fact a new paradigm in studies of consumption and saving”.

(2) *Empirical methods: econometric methods and laboratory experiment.*

As was described above, several Nobel Prizes have been awarded for developing econometric methods suitable for handling specific situations and types of data. The economists toolbox in terms of available econometric methods is obviously much larger today than 50 years ago, and the same holds for laboratory experiments where a number of standard designs are available.

In the prize motivations for Klein they write "Briefly stated: Lawrence Klein has created and established a paradigm for economic macromodels, a general pattern for their theoretical construction and practical application." As above it is not only a matter of statistical techniques but a broader set of recommendations and possible ways to handle specific situations.

(3) *New perspectives, frameworks and ideas*

In order to solve a specific problem it is necessary to have a number of hypotheses and general ideas that it is possible to start from and/or take into account.

The four "mini revolutions" described above can all be seen as such general recommendations of things that one should think about in a specific situations:

- transaction costs for various institutions and contract designs
- the role of expectations and how they might change
- the role of strategic behavior
- the role of asymmetric information.

Some of the press releases and speeches underline that the laureate has created a conceptual tool-box, e.g.

> Solow has created a theoretical framework which can be used in discussing the factors which lie behind economic growth in both quantitative and theoretical terms. (Press release)

> Along with Markowitz' portfolio model, the CAPM has also become the framework in textbooks on financial economics throughout the world. (Press release for Sharpe)
The theory of human capital has created a uniform and generally applicable analytical framework. (Press release for Becker)

This year’s Laureate formulated a new framework, the so-called Mundell-Fleming model. (Speech to Mundell)

This year’s three Laureates have formulated a theoretical framework for search markets. (Speech to Mortensen, Diamond and Pissaridis).

Jean Tirole has presented a general framework for designing such policies and applied it to a number of industries. (Press release)

In a large number of other cases one can find advice about what to be aware of in a specific case, e.g. that famines might be more related to income changes than production levels (Sen) and that poverty might be better measured by consumption than by income (Deaton).

Final comment

It should be underlined that what has been described above is the increase in problem-solving ability of economics. This does not automatically say anything about the problem solving ability of economists (in a certain country, educated in certain department, working in a certain environment, with a certain amount of knowledge and open-mindedness).

11. Conclusions

The Nobel prize texts give strong support for the model of scientific progress developed by Laudan and his rather flexible concept of Research tradition. At every point in time there are certain basic assumptions about the phenomena under study (e.g. rationality, individualism in economics) and beliefs about the best methods to use (e.g. mathematical models and econometrics in economics), but there are also continuous changes in each part. In economics assumptions about rationality has been modified and laboratory experiments have been added as a legitimate method. There is also room for individual researchers to depart from specific parts of the core assumptions, which can be exemplified with laureates like Coase and Williamson that were skeptical to mathematical modelling but kept assumptions about rationality and individualism. The study also found that within a basic framework there can be mini-revolutions where a new perspective is added. This new perspective affects the whole field but does not conflict with the basic assumptions. It was argued that during the last 50 years there have been four such mini revolutions in economics: the concept of transaction cost and making institutions endogenous, game theory and strategic interaction instead of anonymous market forces, the importance and rationality of expectations and the importance of asymmetric information in the economy. If a research tradition is rather flexible then it can change in such a way that no Kuhnian scientific revolution occurs. The texts also give some interesting lessons for those who want to change
economics of today: Be tenacious, speak the language of the dominating tradition and develop your ideas by showing that the new idea withstands the criticisms of the normal scientists of the day.

The Nobel prize texts takes it for granted that one central aim of economic theory is to find the truth. Theories point to relations and mechanism that is believed to be explanations of observed facts and patterns. Economists have typically two complementary ways of arguing that it is rational to believe in a certain hypothesis and that is (1) building credible mathematical models and prove that the hypothesis is true in the model and (2) empirical studies in the form of (primarily) econometric analysis and laboratory experiments that support the hypothesis. There are exceptions and in some cases the contribution is about showing a certain logical relation, like Arrow’s impossibility theorem. In other cases the winner has only been interested in empirical relations, like Friedman about money supply and inflation or Williamson about asset specificity and vertical integration. In the material it is however very clear that in economics there are no empirical relations without exceptions. There are today also economists who are working with various types of simulations models where predictive success is one criterion for evaluating whether certain assumptions are "good enough".

One controversy that seems important given the dominating strategy is how to evaluate whether a certain mathematical model actually is credible and captures important relations. The texts about Tirole are interesting from this perspective as it so strongly underlines the importance of institutional knowledge to be able to build interesting mathematical models.

The development of economics during the period studied here suggests a delicate balance between conservatism and open-mindedness. It takes time to get new ideas accepted, but eventually some of them are incorporated in "mainstream" economics. Most prize winners follow a certain research style but there are always exceptions. It is a challenge both for designers of educational programs in Universities, journal editors and referees to create a "reasonable" amount of diversity in order to make room for the Nobel prize winners that shall add new perspectives and methods in the future.
References

Aristotle (nd), Nicomachean Ethics. http://classics.mit.edu/Aristotle/nicomachaen.html


Hsie, H-F & Shannon, S. Three Approaches to Qualitative Content Analysis. Qualitative Health Research, 15, 1277-1288.


Mäki, U. (2011), Scientific realism as a challenge to economics (and vice versa), *Journal of Economic Methodology*, 18, 1-12,


Appendix 1 Material used

The table shows the number of words in the documents.

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Appendix 2 Commissioned articles published in The Scandinavian Journal of Economics

(up to 1975 The Swedish Journal of Economics)

**Chronological order**


