SCIENCE AND THE STRUGGLE FOR RELEVANCE

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Science and the Struggle for Relevance

Wetenschap en de strijd om relevantie

(met een samenvatting in het Nederlands)

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Chapter 1.

Introduction

1.1 Background, general aim and research questions

The relevance of science is evident yet elusive. Today's major societal issues like ageing, climate change and depletion of natural resources are so complex that they call for substantial research efforts. But how to organize these effectively in order to optimize their practical impact? Given the intrinsic uncertainty and unpredictability of academic research activities, both the possibility and desirability of steering its content are contested. Scholars in science and technology studies discuss the relevance of academic research in the context of debates about changing science systems. They point to profound changes in the dominant visions on the societal relevance of science, and also in the interactions of universities with other organizations expressing these visions. Natural scientists themselves are ambivalent about relevance. Promising societal benefits can help them to acquire necessary resources, but it can also restrict their autonomy and creativity, which count as central values in their work.

The intriguing and important question when and how science can be said to be relevant is the topic of this thesis. The object of study is the way university researchers deal with potential societal benefits of their work, or how they *struggle for relevance*. I will investigate changes in such struggles in eight fields of natural science. I will analyze the similarities and differences among the dynamics of these different fields, and reflect on possible explanations for the changes observed.

With this study of the relevance of science I aim to contribute to the understanding of transformations in the knowledge infrastructure, as discussed in a large and expanding literature. Influential scholars, university managers and policy makers have claimed that the position of universities in modern societies is changing radically. Due to various internal and external factors, academic research is said to be increasingly connected with practical applications (Ziman 2000, Nowotny et al. 2001). It is generally assumed that the interactions between universities, industry and public organizations have intensified (Etzkowitz & Leydesdorff 2000). Moreover, significant changes have been reported in the content and organization of science, like the growth of 'transdisciplinarity' and the broadening of scientific quality control (Funtowicz & Ravetz 1993, Gibbons et al. 1994). However, as I will show in Chapter 2, the understanding of these dynamics is still limited, due to two problems: first, the empirical evidence supporting these claims is not fully convincing, and second, the most influential concept used in this debate ('Mode 2 knowledge production') suffers from conceptual weaknesses that inhibit a proper operationalization.

The issue of relevance figures prominently in the literature about changing science systems. One of the central claims in this literature is that research practices are changing in the sense that research agendas are increasingly oriented at producing societal benefits, or, in other words, that the relevance of science is increasingly defined in terms of specific products or policy solutions (see Chapter 2 for a systematic review). This literature points to the relationship between science and its societal context as an explanation for the increasing societal orientation of research agendas. In this thesis I address these issues by investigating changing struggles for relevance. To what degree and in what ways do individual researchers struggle for relevance in their daily work? How has the meaning of relevance changed in different scientific disciplines? Are struggles for relevance facilitated or complicated by the institutional environment of science? Is there a tension between societal relevance and scientific quality? And how do these struggles vary across scientific fields? By addressing these questions, with empirical data and theoretical reflection, I aim to make a contribution to the debate about changing science systems.

The main research question of this thesis is:

How to understand the changes in the struggle for relevance of Dutch academic researchers in chemistry, biology and agricultural science, in the period 1975-2005?

This question can be divided into three sub-questions:
1. What changes can be discerned in their struggles for relevance?
2. How can the similarities among the changes observed in different scientific fields be explained?
3. How can the differences among the changes observed in different scientific fields be explained?

This thesis deals primarily with *academic* knowledge production, that is, research conducted at universities and not in public research institutes, industrial laboratories or other knowledge-intensive organizations. The current debate about transformations in the knowledge infrastructure does not deal with universities only. For example, it is claimed that the interactions between universities, industry and the government are intensifying (Etzkowitz & Leydesdorff 2000), and that firms, think-tanks and consultancies play an increasingly important role in the production of knowledge (Gibbons et al. 1994). However, because a shift towards Mode 2 would have important implications for the university and its relationships with its societal environment, I chose the university as an empirical entrance point. Indirectly this approach can yield insights in the changing roles and positions of other organizations in the knowledge infrastructure as well.

In the remainder of this chapter I will first provide a further specification of the idea of struggles for relevance (Section 1.2). Next I will elaborate on the link between my research and literature about changing science systems, in particular the notion of Mode 2 knowledge production (1.3). In Section 1.4 I will conceptualize the idea of struggles for relevance in terms of the 'credibility cycle', a 'science-society contract' and a 'research system'. Finally I will present my general methodology (1.5) and sketch the structure of this thesis (1.6).

1.2 Struggles for relevance

The starting assumption of this thesis is that academic researchers *struggle for relevance*. Researchers will always encounter a certain pressure to position their work in a broader framework. To a lesser or greater extent, they need to make sure that their work is valuable to society, either directly or indirectly. The idea of 'struggles for relevance' was coined by Rip (1988) in an analysis of changing science systems. He observed that with the emergence of 'a new layer of institutions, explicitly oriented to "missions", to programming, to strategic mobilization' in the 1970s and 1980s also a shared repertoire for judging relevance had emerged. This development gave rise to struggles for relevance, 'on top of struggles for fundability' (Rip 1988, p70). In this thesis, I continue with the idea of 'struggles for relevance' in a generalized sense: I consider these struggles for relevance as a universal aspect of academic research with different manifestations over time and place. Struggling for relevance can involve attuning one's research agenda to the needs of societal stakeholders. Another form of this struggle is the active transfer of knowledge to potential users. But, in principle, basic research that does not directly address external knowledge needs may also be relevant, depending on what counts as 'relevance'.

Indeed the notion of relevance does not have a fixed meaning, but is historically and socially situated. Building on constructivist approaches in science and technology studies (Latour 1987, Yearley 2005), I conceive the meaning of relevance as a product of social interactions among scientists, policy makers and other stakeholders. In general, relevance refers to the possible (societal) benefits of science. In the course of this thesis, it will become clear that these benefits can come in many different forms, ranging from broad cultural values to the development of specific products or the creation of spin-off companies. After all, 'society' is a complex aggregation of numerous individuals and organizations, which each may benefit from scientific research in different ways. Science can contribute to health care, to environmental policy making and to industrial innovation, for example. Moreover, even within one sector the potential benefits of science can vary greatly, and need not be appreciated equally by the various actors involved. Agricultural science can help farmers to raise the productivity of their meat production, to enhance animal welfare, or to decrease their environmental impact.

In more fundamental areas, the term 'relevance' can also refer to the significance of a particular contribution in relation to a major scientific problem; this may be called 'scientific relevance'. In some cases, scientific and societal relevance can be clearly distinguished as two separate characteristics, but often there is no clear boundary between them. Many scientists conceive 'the advancement of science' as the most important social or political implication of their research (Small et al. 2008). For example, when one would ask a biochemist about the societal relevance of his work, he could explain how his research activities make an important contribution to the understanding of metabolic pathways. The possibility that this understanding – in turn – could be used by medical researchers to develop new therapeutic strategies, is then taken for granted. This thesis deals primarily with (struggles for) *societal* relevance. Investigating the cognitive progress of scientific fields is beyond its central focus. Still, scientific relevance will receive some attention as well, because it is often linked to societal relevance.

Why would scientists strive for relevance? First, positioning one's activities in a broader context can give personal satisfaction[1]. Unravelling the fundamental principles of aquatic ecosystems can be fun simply to satisfy one's own curiosity, but may become even more interesting if one knows this enhanced understanding can help to fight the declining biodiversity. Moreover some scientists experience the successful commercializing of scientific inventions as a proof of their personal scientific quality (Jain et al. 2009). In this way making a contribution to a larger goal could enhance one's labor satisfaction. Second, considerations of potential societal benefits can also help to legitimate one's work to the outside world (van Lente 1993). It is much easier to justify spending large amounts of time and money on scientific activities, if they are likely to yield benefits for society. A political scientist interviewed by Lamont (2009) argues that social scientists should pay close attention to policy implications of their work because

> *'… it's the only way that we can justify what we do ... my bias is towards things that are going to make people's lives better ... Because I think intellectuals are leeches if they don't do that.' (Lamont 2009 p. 179).*

The third possible motivation is strongly related to the second: research of high relevance (in all its different forms) can provide access to funding and other valuable resources. Expected societal benefits have played a role in research funding since the emergence of modern science (Martin 2003, Rip 1997). Strong alignment of one's research agenda with the needs of external parties can help to get funding, either directly or indirectly. In some cases, firms or other organizations directly pay for academic research addressing their own knowledge needs; in other cases their interest helps to receive support from innovation-oriented policy instruments (Zomer et al. 2010). Moreover, in some fields stakeholder interactions provide access to valuable knowledge, datasets or other research materials (Boon & Broekgaarden 2010).

1 Personal preference is known to be a very important criterion in scientific problem choice (Cooper 2009).

If relevance is so attractive, one may wonder why it would be an object of *struggles*. There are at least two reasons why striving for relevance is not a straightforward exercise. First, scientists often struggle with relevance because relevance is not always for free. Researchers may face difficulties when trying to reconcile the need for relevance with other plans, ambitions and values. Jain et al. have recently shown that even scientists that engage in commercial activities still cherish their traditional academic identity (Jain et al. 2009). Research issues that are considered highly 'relevant' by others do not always appear to be the most promising in terms of acquiring personal satisfaction, producing high-impact scientific publications and obtaining peer recognition. This potential tension is particularly important, as it seems that academic researchers experience a growing pressure for excellent performance in terms of scientific productivity, resulting from the rise of performance evaluations over the past few decades (Steele et al. 2006, Hicks 2009). Earlier studies have shown that it is not uncommon for academic researchers to work with a double agenda: a public image of their work that selectively highlights the potential contribution to the goals identified by their funding source, and a personal strategy oriented at individual goals in terms of career advancement or other personal gains (Morris 2000, Leišytė et al. 2008). This latter agenda is often more concerned with fundamental research questions, as these are considered more challenging (and motivating) and offer better prospects on prestigious publications and on developing a successful academic career.

Second, struggles for relevance appear when the meaning of relevance is not fixed. Beside a struggle *with* relevance, there is also a struggle *over* relevance. The potential benefits of science are subject to speculation as they are never certain beforehand. Moreover, due to the plurality of norms, values and worldviews, there are often disagreements about what should be considered as the benefits of particular research activities. What one person considers a benefit may be perceived as a threat by someone else. For example, the question whether government should invest in genetic modification technologies is highly controversial (Borrás 2006). Apart from disagreements about the specific benefits, there is also an ongoing dispute about the degree to which research activities should be oriented at (short term) societal objectives (Ziman 2003, Lorenz 2008, Gibbons 1999, Böhme et al. 1983). In peer review panels evaluating research proposals there are often disputes about the relative importance of 'social significance' as a selection criterion (Lamont 2009). In negotiations about research contracts or research programs scientists can be expected to try to convince their stakeholders that their work – even if it has a rather fundamental nature – is very promising with regard to long term societal objectives. In order to protect their 'academic freedom' researchers may prefer to connect to long term goals, while stakeholders may want them to address their short term needs (Leišytėet al. 2008). Eventually, this dispute often boils down to the question what 'relevance' means, or about the mission of academic research.

Based on these considerations I propose the following tentative definition: the *struggle for relevance* is the combination of the efforts of scientists to make their work correspond with ruling standards of relevance and their efforts to influence these standards of relevance. Depending on the dominant standards of relevance, the possibilities for scientists to optimize the relevance of their work may include attuning their research agenda with the needs of societal stakeholders and transferring the knowledge to potential users. But they can also employ rhetorical strategies to present their research in such a way that it can be argued to fit with dominant standards of relevance (van Lente & van Til 2008). The second aspect of the struggle for relevance (influencing standards of relevance) becomes manifest when scientists participate in public debate or negotiate with specific stakeholders about the benefits that can be expected from their research. By articulating specific benefits of their work they influence the general view of the value of their field or discipline, or even of science as a whole. For example, the creation of an 'Innovation-oriented Research Program' in the Netherlands in the area of genomics contributed to the idea that this field has a high economic potential, and that it is of strategic importance for national competitiveness (van Lente 2006).

As this definition indicates, there are both individual and collective struggles for relevance. Individual scientists try to make their research relevant in various ways. But also research groups and complete fields and disciplines compete with each other for relevance. For example, spokespersons of catalysis will publicly argue that their field deserves more support thanks to its contribution to process innovations in chemical industry. Struggles about the meaning of relevance can also be found on both levels. On a collective level, leading scientists can influence ruling standards of relevance by participating in public debate or negotiating about specific research programs. But also individual research proposals and scientific publications influence dominant conceptions of relevance, as they articulate (new) pathways by which societal benefits can emerge from research activities. In this sense, the efforts individual scientists make to increase the correspondence of their work with a ruling standard of relevance also affect this standard. In this thesis both individual and collective struggles are addressed, but the most detailed analysis is provided of struggles on the individual level, due to the focus on actual research practices.

1.3 Mode 2 knowledge production

After this introduction to the object of study of this thesis, let us now move up one level of abstraction to the broader scholarly debate it intends to contribute to. This investigation into struggles for relevance fits in the context of a debate about transformations in the knowledge infrastructure, in particular regarding the possible shift from Mode 1 to Mode 2 knowledge production, which has been claimed in the literature (Nowotny et al. 2001, Gibbons et al. 1994). An analysis of struggles for relevance above can provide empirical insights into the degree to which such a development is taking place in the Netherlands. Moreover the outcomes of my case studies may also inform the debate with more general insights into (the factors influencing) the dynamics of scientific fields. The struggle for relevance as conceptualized above relates in particular to two Mode 2 attributes, reflexivity and quality control. As will be shown in Chapter 2, these are two of the most controversial aspects of Mode 2 knowledge production. Neither of these attributes is supported by convincing empirical evidence. Moreover they seem to be in conflict with the increasing importance of bibliometric quality indicators, which tend to narrow down the idea of scientific achievements to strictly academic aspects.

Reflexivity refers to the degree to which researchers are sensitive to the broader societal implications of their work. With their claims about the rise of Mode 2 knowledge production, Gibbons et al. argue that this sensitivity is increasing. Some critics, however, have argued that this trend is probably limited to a small number of policy-relevant scientific fields (Weingart 1997). Moreover, it seems plausible that the awareness of (possible) societal impact is present when scientists interact with societal stakeholders in order to acquire funding, but whether this awareness remains active during the complete research process remains open for empirical investigation. Reflexivity is an important aspect of the struggle for relevance. Making an effort to enhance the societal benefits obviously starts with awareness. So by investigating the changes in struggles for relevance, this thesis implicitly addresses the issue of (the reported growth of) reflexivity.

Quality control can be defined as the set of procedures and criteria that constitute the 'selection mechanism of problems, methods, people and results' (Gibbons et al. 1994 p. 32). These mechanisms can be assumed to play a crucial role in struggles for relevance. Practices of quality control, such as funding instruments and evaluation processes, can be seen as the institutional environment of academic research, which I conceptualize as part of the academic *research system*. They provide pressures and incentives to individual researchers to focus on particular problems, rather than others. The specific criteria guiding these mechanisms determine to what extent it is important to optimize the societal benefit of one's research, or to *struggle for relevance.* Gibbons and his co-authors argue that the traditional forms of academic quality control are increasingly accompanied by novel forms which involve non-scientific actors and which are based on criteria concerning the societal value of scientific research. However, there are also indications that academic quality control is increasingly ruled by bibliometric criteria, which tend to overemphasize the scientific rather than the societal value of research activities (Weingart 2005, Scott 2007). Interactions with possible knowledge users do not seem particularly beneficial for one's academic career (Goldfarb 2008)(van Rijnsoever et al. 2008). What is the actual weight of societal criteria in relation to other quality criteria, especially in processes other than funding allocation? By investigating the changing research system and its effects on individual struggles for relevance, I will shed light on the question whether novel forms of quality control are of increasing importance.

Disciplinary differences

One of the main deficiencies in the literature about transformations in the knowledge infrastructure is the lack of differentiation among scientific disciplines. Some of the most influential writings about this topic (Ziman 2000, Gibbons et al. 1994, Slaughter & Leslie 1997) fail to properly distinguish among disciplines (see Chapter 2 for a systematic literature review). It is well-known that science, technology and innovation studies in general disproportionally draw on evidence about the natural sciences, with a particular emphasis on a few 'hotspots' like high-energy physics, nanotechnology, genomics, biomedicine and other biotechnology fields. This situation carries the danger that observations about a limited number of (overstudied) fields are generalized to the whole science system, without taking into account the characteristics of individual fields or disciplines. Obviously, observations about natural science cannot unproblematically be extrapolated to the social sciences and humanities. But also within the natural sciences there are significant differences in the social organization, cognitive content and types of stakeholders in society that will influence the struggle for relevance (Bonaccorsi 2008, Whitley 2000, Becher & Trowler 2001). One of the intended contributions of this thesis is to enhance the understanding of such disciplinary differences and the way they moderate transformations in the knowledge infrastructure.

1.4 Theoretical framework

This thesis presents investigations into struggles for relevance in Dutch university research. To facilitate this empirical analysis, an adequate theoretical framework is needed. At first sight, the concepts of 'Mode 1' and 'Mode 2' may seem attractive, as these concisely summarize several putative trends in contemporary science systems. However, these concepts will provide insufficient support for my analysis, as they rest on a weak theoretical basis (see Chapter 2).

My theoretical starting point is a distinction between the agency of scientists and the structures patterning their behavior. In line with structuration theory I assume that these structures are both the medium and the outcome of research practices. Structures are the rules and resources drawn upon in the production and reproduction of social action, but they are at the same time the means for system reproduction (Giddens 1984). This assumption also resonates with actor-network theory in which structures are regarded as relational effects that recursively generate and reproduce themselves; succinctly summarized by John Law as 'social structure is not a noun but a verb' (Law 1992).

Building on sociological theories of science, the further theoretical foundation of this thesis consists of two basic assumptions. First, in accordance with the constructivist tradition of science studies (Latour 1987, Yearley 2005, Knorr-Cetina 1999), I regard science as a social activity. This implies that the progress of science should be seen as the product of the socially embedded actions of scientific researchers, including both interactions with research materials and instruments, and interactions with other researchers and their wider social context. My second general assumption is that scientists are involved in a continuous struggle for resources. Building on resource dependency theory (Oliver 1991) and principal-agent theory (van der Meulen 1998, Braun 2003), I assume that scientists depend on their environment for crucial resources like funding and legitimacy. Scientists compete among each other for resources, in the form of reputation (Whitley 2000) and credibility (Latour & Woolgar 1986).

Based on this theoretical foundation, I will develop and use three central concepts in this thesis to analyze struggles for relevance: the credibility cycle, the science-society contract, and the research system.

Credibility cycle

To conceptualize the agency of individual scientists in their struggles for relevance, that is, their attempts to make their work correspond with ruling standards of relevance and to influence these standards, I will use the credibility cycle (Latour & Woolgar 1986). This model explains how struggles for reputation influence the daily work of individual scientists[2]. Its starting assumption is that a major motivation for a scientist's actions is the quest for credibility. Scientists invest time and money expecting to acquire data that can support arguments. These are written down in articles, which may yield recognition from colleagues. Based on this, scientists hope to be able to receive new funding, from which they buy new equipment (or hire staff) which will help to gather data again, etc. Conceived in this way, the research process can be depicted as a repetitive cycle in which conversions take place between money, staff, data, arguments, articles, recognition, and so on (see Figure 1.1). The struggle for relevance can be seen as part of the credibility cycle. Producing or promising societal benefits may catalyze some credibility conversions, e.g. the acquisition of funding. However, relevance could also be in conflict with credibility, for example, when more fundamental research activities lead to more prestigious publications than application-oriented projects.

The precise form of the credibility cycle can be subject to change. In this thesis I use this cycle as a heuristic for analyzing academic research practices. This choice implies the assumption that this model gives a valid and universal representation of the general pattern of scientific agency. During my research, however, I will also explore whether changes in academic research practices give reason to modify or expand this model.

Some aspects of the credibility cycle also vary across scientific fields. For example, in fields characterized by a high strategic task uncertainty there is no consensus about intellectual priorities (Whitley 2000). This implies that scientists can earn recognition for a more diverse set of contributions than in fields with a low strategic task uncertainty.

2 The notion of the credibility cycle will be futher elaborated in Chapter 3.

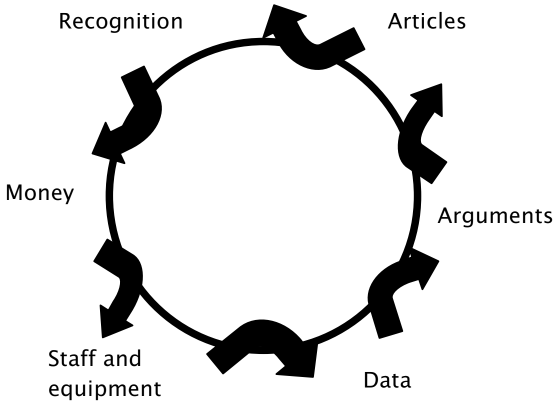

Figure 1.1 The credibility cycle, adapted from Latour and Woolgar (1986)

The struggles for relevance are primarily located within academic research practices. However, because 'relevance' is intrinsically connected to the societal context of science, struggles for relevance are also shaped by structures extending beyond the immediate research environment. As I have indicated in Section 1.2, the meaning of 'relevance' is the product of interactions of scientists with the wider world. Moreover, external forces stemming from public policy and knowledge needs of industry (or other societal stakeholders) influence the degree to which scientists care about practical utility of their work. For these reasons, changes in the struggle for relevance cannot be understood without taking into account the dynamics of the organizations in which science is conducted, the institutions governing it, and the public debate about the value of academic research. In the following chapters I will investigate the changing structures acting as rules and resources for university research from two different angles, using the concepts of a science-society contract (Chapters 3-5), and a research system (Chapter 6), respectively. Although both approaches shed light on the structures influencing the agency of academic researchers, the first emphasizes discursive structures, while the latter emphasizes institutional structures.

Science-society contract

The first concept used to study social structures influencing the struggle for relevance is the idea of a 'contract' between science and society (Martin 2003, Elzinga 1997, Guston 2000). This concept can help to grasp the changing societal discourse about academic research. My notion of a contract refers to the whole of all implicit and explicit agreements between science and societal parties[3]. It arranges what science should do (its identity), why it should do this (rationales), and what are the appropriate conditions for science to function well (see Figure 1.2).

3 The notion of a contract between science and society will be further elaborated in Chapter 3.

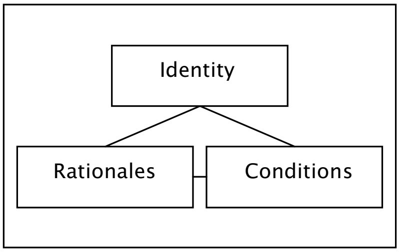

Figure 1.2 General composition of the science-society contract

The identity of science, as specified in this contract, refers to its task, which can be defined in its most general form as *the provision of relevant research outcomes*. Science's task is to produce knowledge and to deliver it in forms like papers, patents, artifacts or educated people. Obviously the precise type of knowledge science is expected to produce and the form of deliverance vary over time and across disciplines. The second issue addressed in my conceptualized contract is why science deserves support. Many different (combinations of) arguments are possible, but most of them relate to concepts of societal relevance. Academic research is often regarded as a necessary condition for sustaining a system of higher education. Other prominent rationales for supporting science concern its practical outcomes in terms of commercial products and knowledge of general interest (like expertise which can support decision making or biological understanding which may lead to medical innovations), either on the short or on the long term. But basic science can also be seen as a cultural good. The third element of the contract contains agreements about the conditions under which scientists work and deals with aspects like the scientific reward system, career opportunities and processes of research agenda setting. Although utterances by scientists, policy makers and other stakeholders form the entrance point for studying this contract, it can also provide insights into the changing institutional environment of academic research.

In line with the structuration perspective, the 'conditions' specified in the science-society contract act as rules and resources in the conversions of different forms of credibility. These conditions represent institutions that enable and constrain the agency of individual researchers. The criteria used by research councils to distribute their funding, for example, contribute to the possibilities of scientists to turn recognition into money. But shared views about the identity of science or the rationales for supporting science also act as discursive structures that influence the behavior of individual researchers in their credibility cycle. For example, public opinion on the importance of disinterestedness may inhibit the development of close contacts of medical researchers with pharmaceutical firms. In their turn, research outcomes can also influence or reproduce the contract, for example when they inspire visions on possible future applications that give rise to new rationales for public investments in research.

Research system

The other angle from which I will investigate the structural conditions of academic research is by scrutinizing the institutions that are part of the 'research system' (see Figure 1.3). Following Rip and van der Meulen (1996), I regard a research system as consisting of 'research performers (individuals, groups, institutions), other organizations and institutions, interactions, processes and procedures' (Rip & van der Meulen 1996 p. 345)[4]. This system contains universities, related research institutes and funding agencies, but also governmental organizations, firms and intermediary organizations to the extent that they are part of the institutional environment. Knowledge users are also part of this system, as they express expectations about desirable research outcomes, and they sometimes provide research funding. The selection mechanisms which Gibbons et al. (1994) have termed 'quality control' can also be regarded as part of the research system. Altogether, this institutional environment provides research organizations with incentives and constraints to conduct (particular kinds of) research. In this way it gives meaning to the notion of relevance, and it can either stimulate or inhibit scientists to optimize the societal benefits of their work.

Similar to the 'conditions' of the science-society contract, the institutions of the research system give shape to certain conversions of credibility. They function as rules and as resources for the 'agents' in the credibility cycle. Simultaneously, funding bodies probably take into account the outcomes of research practices when formulating their future priorities. In this way, research practices can strengthen these institutions, but they can also neglect them and put them under pressure. So the research system can be seen a structure that shapes research practices, but that is at the same time (re)produced by these practices.

Research systems may vary significantly across scientific fields. Depending on its cognitive content, each field has its own stakeholders in society, ranging from environmental NGOs to multinational firms and from farmers to governmental policy makers. These knowledge users provide different pressures and incentives to academic scientists, exerting stronger or weaker forces on their activities. In some fields there are traditionally intensive interactions with (potential) knowledge users; in others they are more distant. Another relevant dimension is the degree of strategic task uncertainty (Whitley 2000), or the relative 'divergence' of the search pattern in a given field (Bonaccorsi 2008). In fields with a low task uncertainty and a convergent search pattern the research system reflects clear intellectual priorities. If the strategic task uncertainty is high, however, the different actors in the research system disagree about research priorities and activities diverge in various directions.

4 The notion of a research system will be further elaborated and applied in the analysis of agricultural sciences in Chapter 6.

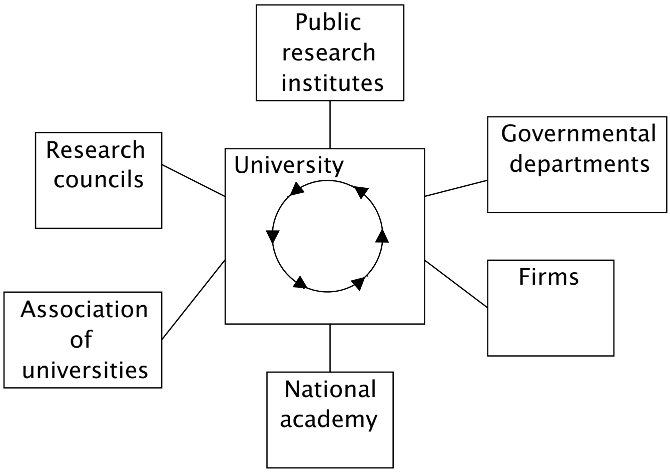

Figure 1.3 The credibility cycle embedded in an academic research system

In this thesis, the main focus will be on the influence of the discursive and institutional structures on the credibility cycle. My main object of study is the struggle for relevance as performed in the credibility cycle. I study the dynamics of the research system and the science-society contract primarily in order to explain the changes in the struggle for relevance. This implies that the influence of changes in the credibility cycle on the structural conditions as represented by the research system or the science-society contract is beyond the central focus of this thesis.

1.5 Methods

The general research strategy of my investigation of changing struggles for relevance is a case study approach (Yin 2003, Eisenhardt & Graebner 2007). This strategy seems appropriate because my aim is to contribute to the *understanding* of transformations in the knowledge infrastructure. This thesis focuses on three scientific disciplines in the Netherlands. My purpose is to generalize my findings to theoretical propositions about changing science systems, rather than to empirical claims about broader populations. As has been noted in Section 1.3, one of my intended contributions is enhancing the understanding of the differences among disciplines regarding the changing relationship with their societal context. A comparison of various disciplines can be expected to yield fruitful material to this end. I have conducted three case studies, each dealing with one scientific discipline in the Netherlands. My theoretical sampling (Eisenhardt & Graebner 2007) of disciplines is inspired by Stokes' typology of scientific research (Stokes 1997). The two dimensions making up this quadrant model are the relative commitments to considerations of use and to the quest for fundamental understanding, respectively (see Figure 1.4).

Both seem highly significant in relation to struggles for relevance. For disciplines that are traditionally strongly inspired by considerations of use it is probably much easier to show the relevance of their work than for those lacking such a tradition. The degree to which a discipline is concerned with a quest for fundamental understanding can be expected to correlate with potential barriers for 'relevant' research, for example, when there is a strong tradition of academic publications.

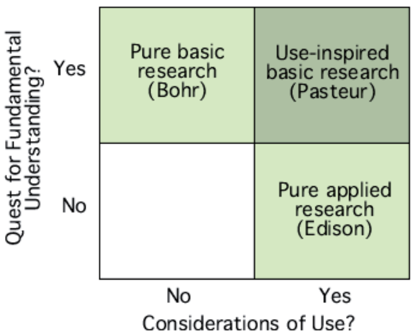

Figure 1.4 Stokes' quadrant model of scientific research (Stokes 1997 p.73)

Stokes' dimensions should be seen as continuous rather than dichotomous scales, as research practices can show commitment to these two goals to varying degrees (Stokes 1997 p. 72). Disciplines seem too diverse to be classified neatly in one of the four boxes. Still the disciplines I have selected for empirical analysis, chemistry, biology and agricultural science, traditionally vary significantly along Stokes' two dimensions. They were selected based on their variation along these dimensions around 1975, the starting point of my analyses.

The main share of Dutch academic chemistry can be positioned in Pasteur's Quadrant as it is inspired by both the quest for fundamental understanding and by considerations of use. Industrial and academic chemists are known to form dense networks, especially in the Netherlands (Tijssen & Korevaar 1997, Homburg 2003). Still, academic chemistry has a strong academic orientation and is also committed to producing fundamental knowledge about compounds and reactions. This is illustrated by the rich tradition of excellent scholarly publications by Dutch chemists (Moed & Hesselink 1996).

Compared to chemistry, most biology was traditionally less directly concerned with practical applications, especially before the rise of biotechnology. The possible applications of life science research range across several societal contexts such as agriculture, environmental policy and health care, but these have traditionally not strongly steered its academic research agenda[5]. Biology shares with chemistry its quest for fundamental understanding.

Agricultural sciences are (almost by definition) connected to agricultural practice. In the Netherlands, the Ministry of Agriculture has always served as the main funding source for most agricultural research. Moreover, the Dutch agricultural sector is traditionally known as a dense network with strong interconnections among research, education and extension activities (Leeuwis et al. 2006, Maat 2001). More than the other two disciplines, agricultural research has traditionally been oriented at delivering practical outcomes. Most fields of agricultural science do not have a strong tradition of scholarly publications. Their main goal has not been to gain fundamental understanding but to contribute to the productivity enhancement of Dutch agriculture.

For two reasons I have limited myself to natural sciences. First, I can build on a substantial body of secondary literature. Second, the focus on natural sciences enhances the policy relevance of this study as these represent the most costly parts of the academic science system. There have been numerous calls for more studies of the dynamics of social sciences and humanities and some of the issues discussed in this thesis may create specific problems in these disciplines (Boomkens 2008, van Hemert et al. 2009). However, by limiting myself to the natural sciences I create a body of evidence on a set of disciplines that share enough characteristics to be fruitfully compared.

In each case, the discipline was studied over the period between 1975 and 2005. I chose 1975 as the starting point because this marks the beginning of governmental science policy in The Netherlands (Salomon 1980, Blume 1985), which is generally considered as a key-event in the changing relationship of academic science with its societal context, and because around this time civil society started penetrating the science system (Rip & Boeker 1975, Grin 2010).

Within each case I have selected two or three scientific fields (see Table 1.1) to represent the breadth of each discipline in terms of possible societal stakeholders. A part of the structural conditions are shared among the fields within a particular discipline. Some research councils and evaluation processes are organized on the level of disciplines. In a sense, however, the various fields can be regarded as separate case studies as well. In each disciplinary study (Chapters 4, 5, and 6) the fields show very diverse dynamics, and comparing the observations on different fields will turn out a rich source of information and understanding. In the final chapter (Chapter 7) this will even lead me to the conclusion that comparing individual fields is more fruitful than comparing aggregated disciplines.

5 The first research assessment of biology stated that most research could be qualified as purely scientific, not explicitly designed to lead to application (Biologische Raad, 1980, Biologisch onderzoek in de subfakulteiten biologie en instituten voor fundamenteel biologisch onderzoek, Amsterdam). In the mid-1970s, the main biological research council BION (Stichting Biologisch Onderzoek Nederland) only funded 'purely-scientific' research which addressed questions of 'purely fundamental nature, not inspired by any immediate societal issue' (BION, 1975, Jaarverslag, Den Haag, p. 3).

Table 1.1 Fields selected within each case study and their stakeholders in society

Discipline	Fields	Main stakeholders
Chemistry	Catalysis Biochemistry Environmental chemistry	Chemical industry Biotech industry / Medicine Environmental policy
Biology	Paleo-ecology Toxicology	Oil industry Environmental policy
Agricultural science	Animal breeding and genetics (ABG) Animal production systems (APS) Cell biology	Animal breeding firms Farmers, agricultural policy (Veterinary) medicine

Data for the case studies have been drawn from in-depth interviews and documentary analysis. For the credibility cycle analysis of changing struggles for relevance semi-structured in-depth interviews with 47 academic researchers were carried out[6]. The respondents' ranks ranged from PhD-student to full professor and they were employed at five different universities in the Netherlands (see Table 1.2). They were asked questions about their current and past research activities, their personal motivation, and their experiences and strategies concerning funding acquisition, publishing, scientific reputation, and performance evaluations. Using NVivo (qualitative analysis software), I coded the interview transcripts in accordance with the different steps of the credibility cycle.

Table 1.2 Distribution of 47 respondents over fields, universities and academic ranks

Catalysis (9) Paleo-ecology (8) Toxicology (7) Biochemistry (6) Environmental chemistry (5) Animal breeding and genetics (4) Animal production systems (4) Cell biology (4)	Utrecht University (18) Wageningen University (12) University of Amsterdam (11) VU University Amsterdam (3) Radboud University Nijmegen (1) Eindhoven University of Technology (1) Leiden University (1)	Retired full professor (6) Full professor (13) Associate professor (10) Assistant professor (6) Post-doc researcher (5) PhD-student (7)

My analysis of the changing structural conditions of academic research is based on documents (listed in the Appendices of Chapters 4, 5 and 6) in combination with interviews with scholarly experts, and representatives of firms, professional organizations, research councils and the government. The documents were collected based on prior knowledge of the authors, tips from interviewees, and the 'snowball method'. The selection includes governmental policy documents, reports and strategic plans of research

6 Stefan de Jong and Floor van der Wind conducted some of the interviews for the case studies of biology and chemistry, respectively.

councils, foresight studies, evaluations and other important publications about the disciplines addressed. The findings from these documents were triangulated in interviews with the experts and stakeholders mentioned above.

1.6 Outline of this thesis

The general aim of this thesis is to contribute to the understanding of transformations in the knowledge infrastructure by investigating changing *struggles for relevance* in three scientific disciplines (in the Netherlands). The middle part of this thesis (Chapters 2-6) was written as a collection of self-standing papers, which have been submitted to different journals (and one edited volume).

The first step of my itinerary is a systematic review of the current literature on transformations in the knowledge infrastructure, which is presented in Chapter 2. This review served to acquire a general overview of the debate about changing science systems, and it was conducted before I had chosen a specific research question. In fact, the outcomes of this literature study have formed an important input for the choice of my research topic and formulation of research questions as presented in the current chapter. In this review the notion of 'Mode 2 knowledge production' (Gibbons et al. 1994), was chosen as a starting point, because of its prominence in this debate, both in academia and policy circles. Chapter 2 systematically reviews the reception of this concept in scholarly literature and compares it to seven alternative 'diagnoses' of changing science systems, such as the 'Triple Helix' and 'Post-normal science'. The review is concluded with a general research agenda for scholars in Science, Technology and Innovation Studies, dealing with the three most controversial features of Mode 2 knowledge production: transdisciplinarity, reflexivity, and quality control. This general agenda has strongly inspired me in formulating the central research question of this thesis, which has already been presented above (Section 1.1):

How to understand the changes in the struggle for relevance of Dutch academic researchers in chemistry, biology and agricultural science, in the period 1975-2005?

As I have argued in Section 1.4, this question deals in particular with the Mode 2 attributes reflexivity and (novel) quality control.

After this literature review, my next step is a further conceptualization of these issues in terms of my theoretical framework. Chapter 3, 'In search of relevance', develops the first two building blocks of this framework: the science-society contract and the credibility cycle. This chapter also presents a brief case study to illustrate the usefulness of these concepts. It will explore how changing concepts of 'relevance' in the science-society contract influence scientific practices, with a particular focus on the institutions governing the credibility cycle.

The following three chapters use and enrich (elements of) this framework in the analysis of case studies of Dutch chemistry, biology, and agricultural science, respectively. Each study addresses similar questions about the changing struggle for relevance. The specific research questions in each case were chosen based on their urgency for this particular discipline in relation to existing literature and current developments.

Chapter 4 presents a more elaborate study of the chemistry case. It gives a more detailed analysis of the changing science-society contract, highlighting in particular the changing funding sources and the rise of performance evaluations. Further it explores the consequences of these developments for the struggles for relevance of researchers in three different fields of chemistry, guided by the question whether practical applications have become a source of credibility.

Chapter 5 addresses the influence of changes in the contract on the actual behavior of academic researchers and the content of their work. It focuses in particular on multidisciplinary collaborations in paleo-ecology and toxicology. Multidisciplinarity is one of the characteristics often discussed in the debate about changing science systems. It can be seen as a weak type of (and a necessary condition for) transdisciplinarity, one of the five attributes of Mode 2 knowledge production. This chapter analyzes the changing structural conditions influencing Dutch biologists using the contractual perspective as presented in Chapter 3. Next, it investigates how these conditions provide pressures and incentives for multidisciplinarity in two different biological fields: paleo-ecology and toxicology.

The central question of Chapter 6 is: how do changing institutions influence academic research practices? This chapter deals with Dutch agricultural sciences, in particular with three fields of animal science. These fields have experienced a highly dynamic societal context. Dutch agricultural sciences are under pressure to change the content and the organization of the research in order to contribute to more competitive and sustainable agricultural production. In this chapter the changes in (institutions of) the agricultural research system and their consequences for the daily work of Dutch animal scientists will be analyzed.

The thesis is concluded with a general discussion: in Chapter 7 I summarize the main findings and compare them across my three case studies. I relate my empirical findings to the debate about transformations in the knowledge infrastructure, discuss the theoretical contributions of my research and provide recommendations for science and innovation policy.

Chapter 2.

Re-thinking new knowledge production: A literature review and a research agenda[7]

Abstract

This paper offers a systematic reflection on the Gibbons-Nowotny notion of 'Mode 2 knowledge production'. We review its reception in scientific literature and compare it with seven alternative diagnoses of changing science systems. The 'Mode 2' diagnosis identifies a number of important trends that require further empirical effort, but it suffers from severe conceptual problems. It is time to untie its five major constitutive claims and investigate each separately.

2.1 Introduction

Science systems are said to be in transformation. The last two decades various studies have pointed to a variety of changes, such as an increasing orientation of science systems towards strategic goals (Irvine & Martin 1984) and the production of relevant knowledge (Gibbons et al. 1994, Böhme et al. 1983). A variety of approaches to understand, explain, and, perhaps, extrapolate such trends have emerged, but none of them is uncontested. Probably the most famous account of a transformation is the concept of 'Mode 2' knowledge production. This notion refers to a set of putative changes that are introduced in *The New Production of Knowledge* (Gibbons et al. 1994). The book sketches the emergence of a research system that is highly interactive and 'socially distributed'. The basic argument is that, while knowledge production used to be located primarily in scientific institutions and structured by scientific disciplines, its locations, practices and principles are now much more heterogeneous. Mode 2 knowledge is produ-

7 This chapter has been published as Hessels, Laurens K., and Harro van Lente. 2008. Re-thinking new knowledge production: A literature review and a research agenda. *Research Policy* 37:740-60.

ced 'in the context of application' by so-called transdisciplinary collaborations. Moreover, scientists are more reflexive and they operate according to different quality criteria when compared with the traditional disciplinary mode. The new mode of knowledge production has been coined 'Mode 2' and it is not believed to replace Mode 1, but to supplement it[8]. Table 2.1 gives a summary of the basic claims in a well-known format.

Table 2.1 Attributes of Mode 1 and Mode 2 knowledge production

Mode 1	Mode 2
Academic context	Context of application
Disciplinary	Transdisciplinary
Homogeneity	Heterogeneity
Autonomy	Reflexivity / social accountability
Traditional quality control (peer review)	Novel quality control

In the decade since its launch by Michael Gibbons, Camille Limoges, Helga Nowotny, Simon Schartzman, Peter Scott and Martin Trow, the 'Mode 2' concept has gained an enormous visibility in the reflection on contemporary scientific practice. The notion of 'Mode 2' is referred to in over 1000 scientific articles[9] and seems to have influenced science, technology and innovation policies[10]. During the same period, however, scholars have written numerous critical papers to contest the claims and the use of the Mode 2 concept, some on a theoretical basis, others supported by empirical data. We think it is time for reconsideration of the idea of a science system in transformation and we will use the claims and contestations of the Mode 2 concept as an entrance point. To what extent is this concept helpful in describing and explaining current changes in scientific practice? What does it add to other approaches? What are the most relevant questions to address when one is interested in the transformation of science systems?

We will follow two routes, one direct, the other indirect. First, the indirect route is to compare and contrast the Mode 2 diagnosis with a number of alternative accounts of current changes in scientific practice (section 2.3), such as Triple Helix (Etzkowitz & Leydesdorff 2000), post-normal science (Funtowicz & Ravetz 1993) and strategic research (Rip 2004). We will address both agreements and differences between 'The New Production of Knowledge' (NPK) - the book in which the notion of Mode 2 has been coined - and the alternatives. This step will make clear about which characteristics of the science system the different diagnoses make claims and it will show to what extent the claims of NPK

8 'This new mode – Mode 2 – is emerging alongside the traditional disciplinary structure of science and technology – Mode 1' (NPK, p. 14)

9 Scopus search on January 18th 2007

10 In Canada, for instance, the creation of Networks of Centre's of Excellence aimed at 'facilitating Mode 2 networks' (Fisher et al. 2001).

agree with claims made by other authors. The second, direct route is to review and evaluate the numerous reactions to 'The New Production of Knowledge'. After a discussion of its general reception (section 2.4), a number of critical reactions are addressed (section 2.5). The main objections that we found in the literature will be grouped under three headings: criticism regarding the empirical validity, the conceptual strength, and the political value of NPK. Consequently, the strong and weak points of the original Mode 2 claims can be determined. We will conclude with a statement about the strength and suitability of the Mode 2 concept and with a list of topics concerning the transformation of science systems that deserve further study. First, however, we will summarize the two main publications by the creators of the concept (section 2.2).

2.2 The new production of knowledge: Mode 2

The notion of Mode 2 knowledge production is coined in *The New Production of Knowledge* (Gibbons et al. 1994). This volume constitutes the outcome of a collaborative research project conducted by six prominent scholars in the field of science (policy) studies: Michael Gibbons, Camille Limoges, Helga Nowotny, Simon Schwartzman, Peter Scott, and Martin Trow. The work was originally commissioned by the Swedish Council for Research and Planning, FRN, aiming to get a view on the future of universities.

The main proposition of the study is the emergence of a knowledge production system that is 'socially distributed'. While knowledge production used to be located primarily at scientific institutions (universities, government institutes and industrial research labs) and structured by scientific disciplines, its new locations, practices and principles are much more heterogeneous. To clarify this assertion the authors introduce a distinction between Mode 1 knowledge production, which has always existed, and Mode 2 knowledge production, a new mode that is emerging next to it and is becoming more and more dominant. Five main attributes of Mode 2 summarize how it differs from Mode 1 (see Table 1).

First, Mode 2 knowledge is generated in a *context of application*. Of course, Mode 1 knowledge can also result in practical applications, but these are always separated from the actual knowledge production in space and time. This gap requires a so-called knowledge transfer. In Mode 2, such a distinction does not exist. A second characteristic of Mode 2 is *transdisciplinarity*, which refers to the mobilization of a range of theoretical perspectives and practical methodologies to solve problems. Transdisciplinarity goes beyond interdisciplinarity in the sense that the interaction of scientific disciplines is much more dynamic. Once theoretical consensus is attained, it cannot easily be reduced to disciplinary parts. In addition, research results diffuse (to problem contexts and practitioners) during the process of knowledge production. Thirdly, Mode 2 knowledge is produced in a diverse variety of organizations, resulting in a very *heterogeneous* practice. The range of potential sites for knowledge generation includes not only the traditional universities, institutes and industrial labs, but also research centres, government agencies, think-tanks, high-tech spin-off companies and consultancies. These sites are linked through networks of communication and research is conducted in mutual interaction. The fourth attribute is *reflexivity*. Compared to Mode 1, Mode 2 knowledge is rather a dialogic process, and has the capacity to incorporate multiple views. This relates to researchers becoming more aware of the societal consequences of their work ('social accountability'). Sensitivity to the impact of the research is built in from the start. Novel forms of *quality control* constitute the fifth characteristic of the new production of knowledge. Traditional discipline-based peer review systems are supplemented by additional criteria of economic, political, social or cultural nature. Due to the wider set of quality criteria, it becomes more difficult to determine 'good science', since this no longer is limited to the judgement of disciplinary peers. However, this does not imply that Mode 2 research is generally of a lower standard.

In order to emphasize the width of the transformations, the authors of NPK (Gibbons et al. 1994) describe a number of developments in which they are visible such as the commercialization of knowledge, the massification of higher education and the increasing importance of collaboration and globalization. The book also includes chapters on the case of the humanities and on the institutional changes that are involved in the rise of Mode 2 knowledge production.

In 2001, three of the authors of NPK published a second book: *Re-thinking Science: Knowledge and the Public in an Age of Uncertainty* (Nowotny et al. 2001).[11] It can be read as a reaction to some of the criticisms that NPK has received. The authors of 'Re-thinking Science' elaborate the claims about Mode 2 in three directions.

Firstly, Nowotny, Scott and Gibbons relate their arguments to sociological literature.They discuss two accounts of social change that deal with the growth of complexity of society: the Knowledge Society and the Risk Society (p. 10-20). They compare the two along a number of parameters and relate them to the notion of Mode 2. In addition, the authors relate their work to literature about post-modernism and co-evolution.

Secondly, 'Re-thinking Science' extends the argument of Mode 2 beyond the boundaries of the science system. Expanding its meaning, the term Mode 2, here refers to a society consisting of 'transgressive' institutions. In a post-modern fashion, the book argues that currently a de-differentiation of the various societal spheres (state, market, culture) is taking place. These are increasingly fuzzy and blurring categories that overlap and interact. According to Nowotny et al. this development constitutes the background against which the shift towards Mode 2 knowledge production takes place (p. 29).

Thirdly, the authors make attempts to specify the nature of new scientific practices and discuss additional observations of contemporary scientific practice. They describe changes they perceive in various institutions involved in knowledge production: industrial and governmental research institutes, research

11 In this paper we will use the definitions of Mode 1 and Mode 2 as given in NPK and we will primarily analyse the reactions that this book has received.

councils and universities. In particular they introduce the concept of 'contextualized science' which basically means that 'society now "speaks back" to science' (p. 50). This refers to the demand for innovation, to new regulatory regimes, and to the multiplication of user-producer interfaces. Depending on the degree of importance, one can speak of weak, middle range, or strong contextualization. This development affects scientific activity not only on the organizational level, but also 'in its epistemological core' (p. 94). The authors claim that Mode 2 (or contextualized) research yields 'socially robust knowledge', which has a different epistemological status than Mode 1 science. Perhaps surprisingly, the participation of a wider range of non-scientific actors in the knowledge production process enhances its reliability.

2.3 Mode 2 and its alternatives

The Mode 2 diagnosis is popular, visible and contested, but not unique: it appears amongst competing approaches to study changes in the science system. The first step in our reconsideration is a comparison with a set of alternatives, which we have identified in a literature study[12] (see Table 2.2). Each gives an account of current changes in scientific knowledge production and / or the changing relationship between science and society. We will briefly introduce each of them and discuss the agreements and disagreements with the Mode 2 diagnosis. Please note that the order of appearance is chronological and does not reflect any judgement about their relative importance. We will discuss the approaches in terms of their claims about cognitive changes, organizational changes and science – non science relationships. The discussion is summarized in Table 2.3.

12 The bodies of literature addressed were selected based on their prominence (number of citations) and the degree of apparent similarity with NPK.

Table 2.2 Alternative diagnoses studied

Concept	Aim (descriptive or prescriptive)	Format	'Main' publication	Number of citations[a]
Finalization science	D / P	Articles	Böhme et al. (1983)	22
Strategic research / strategic science	D (P)	Diverse	Irvine and Martin (1984)	58
Post-normal science	P	Articles	Funtowicz and Ravetz (1993)	204
Innovation systems	D / P	Diverse	Edquist (1997)	298
Academic capitalism	D	Book	Slaughter and Leslie (1997)	315
Post-academic science	D	Book	Ziman (2000)	97
Triple Helix	D	Articles	Etzkowitz and Leydesdorff (2000)	175

a Scopus search, April 27th 2007

2.3.1 Finalization science

The concept of 'finalization science' (Böhme et al. 1983, Böhme et al. 1973) describes and explains the dynamics of science and its societal function. To some extent, it also contains a prescriptive message. In the 1970s, the German research group known as the 'Starnbergers' (Rip 1989), developed a research programme on science dynamics consisting of case studies of scientific disciplines. The programme has resulted in a number of journal articles, part of which have been published in German. Most accessible is an edited volume with contributions of the main proponents of the programme (Böhme et al. 1983). Based on the case studies, their main claim is that all disciplines follow a general development in which an explorative phase, a paradigmatic phase, and a post-paradigmatic phase can be distinguished. In the context of this paper, the last phase is most important. In this phase, 'finalization' may occur: theoretical development that is determined by external factors. When a discipline attains theoretical maturity, it becomes open to orientation in accordance with external objectives. Its further theoretical development then proceeds along the paths that these goals indicate.

According to the 'finalists', more and more disciplines reach this phase. This implies that the relation between science and society is changing. In this relationship, society is becoming an active rather than a passive partner, and it increasingly takes a guiding role.

As Weingart has indicated (1997), this observation is comparable to the Mode 2 thesis. Four important differences need to be mentioned, however. First, the 'finalization'-approach has a strong empirical basis. Second, it clearly differentiates between scientific disciplines: it studies various disciplines separately. The claim that the whole science system is undergoing change arises as an inductive conclusion from the observations of different disciplines. Third, distinct from the emergence of Mode 2 knowledge production, finalization of disciplines is related primarily to internal rather than external causes. Its driving force is the theoretical maturing development which facilitates social orientation. In contrast, in the Mode 2 thesis, global developments such as globalization of business and the complexity of policy issues figure as causal factors. Fourth, the Starnbergers are explicitly prescriptive when they speak of 'normative finalization'[13]. They do not merely report the increasing social orientation of science but also provide policy recommendations. In their writings, one finds a call for 'social natural science', science in which natural norms and social interests are coordinated. Given the possibility of social orientation of scientific research, they argue for setting restrictive conditions that the aims set by scientists must meet (Schäfer 1983).

2.3.2 Strategic research / strategic science

The notions of strategic research and strategic science appear in a variety of sources. They are mainly used for descriptive purposes, but are often translated into policy goals in a prescriptive way. The term 'strategic research' was coined in a policy study (Irvine & Martin 1984) and is defined as: 'basic research carried out with the expectation that it will produce a broad base of knowledge likely to form the background to the solution of recognized current or future practical problems'. A striking feature is the emphasis on basic rather than applied research. This distinguishes this diagnosis from Mode 2, in which the distinction between basic and applied science has disappeared. Strategic science, however, has internalized the pressure for relevance while maintaining the (academic) freedom to continuously move to the most promising line of research. Scientists do not operate in the 'context of application', but they do consider the relevance of their work as a legitimate condition to take into account. There remains a distance between the actual research and its eventual uptake in the form of solutions to societal problems or innovations that enhance economic growth.

Rip uses the notion of 'Strategic Science' to describe an upcoming regime (Rip 2004). This regime is characterized by a recontextualization of science in society. Due to the importance of science for innovation and for decision-making, there is more emphasis on strategic research: producing knowledge which combines relevance[14] with scientific excellence. It replaces the regime of 'Science, the endless frontier', in which resources have been available to basic science without requiring clearly articulated promises.

13 This element is particularly visible in the work by Wolf Schäfer (e.g. (Schäfer 1983)).

14 In this context, 'relevance' refers to application possibilities in either (industrial) innovations or in (governmental) decision making.

Rip regards the spread of 'centres for excellence and relevance' (Rip 2004) and also the commitment of (entrepreneurial) universities to both regionalism and academic excellence (Rip 2002a) as indicators for the new regime.

2.3.3 Post-normal science

'Post-normal science' is a prescriptive approach that is presented in a journal article (Funtowicz & Ravetz 1993), but it has led to the development of a research community working on the further development of the programme. The concept originates from policy relevant science fields and starts from an acknowledgement of the limitations of rational decision-making. Given the complexity of current issues in (environmental) policy, it argues for a reassessment of the appropriate role of scientific research. In environmental debates typically 'facts are uncertain, values in dispute, stakes high, and decisions urgent' (Funtowicz & Ravetz 1993). According to the authors, 'normal science' in the Kuhnian sense is not an adequate mode of knowledge production in this situation, as it assumes that problems can be divided into small-scale problems that can be handled without questioning the broader framework or paradigm. There is a need for a scientific practice which can cope with uncertainty, with value plurality and with the decision-stakes of the various stakeholders of the problem at hand. In addition it must have the capacity to support policy makers taking their time constraints into account. For this purpose the term 'post-normal science' has been invented.

The most striking characteristic of post-normal science is public participation. The solutions that proponents of this model offer generally boil down to engaging stakeholders in decision-making processes or in the quality assessment of scientific knowledge production. According to the post-normal science view, quality assurance of scientific input to policy processes should be performed by an 'extended peer community' (Funtowicz & Ravetz 1993). To this end, several frameworks have been developed that enable dealing with different types of uncertainty, both on the level of model parameters and assumptions (Van der Sluijs et al. 2005) and on the level of societal perspectives (Craye & Funtowicz 2005) and value diversity (Kloprogge & van der Sluijs 2006).

Post-normal science shares a number of characteristics with Mode 2 knowledge production but places slightly different accents. Common features of both approaches are the increased interaction across disciplinary and organizational boundaries, additional quality criteria and a greater reflexivity. However, there is a clear difference in scope. Because post-normal science is only relevant for policy-supporting research, it does not deal with the university-industry interactions. In post-normal science, corporations play a role only in as much they are a stakeholder of the policy problem at hand, not because of their potential role as a knowledge (co)producer. For the same reason, there is no consideration of product or process innovations, but only for policy innovations or system innovations. Because of its focus on the public function of research, post-normal science has stronger similarities with the more recent book (Nowotny et al. 2001) by some of the authors of NPK. It fits the ideal of contextualized research, yielding 'socially robust' knowledge.

Compared to Mode 2, post-normal science has a more programmatic character. It does not have a descriptive content in the sense that it *reports* the emergence of a new mode of research. Rather, in a prescriptive sense, it expresses a need for new modes of knowledge production and aims to contribute to its fulfilment by developing the required tools.

2.3.4 Innovation systems

Systems thinking in innovation studies emphasizes the importance of interactions and feedback mechanisms between all actors involved in innovation, including university researchers, industrial product developers, intermediary organizations and end-users. The concept of innovation systems is primarily applied as a heuristic framework, in order to describe and explain the complexity of innovation systems. In addition, it is used in a prescriptive sense, by arguing for a more systemic innovation policy (Smits & Kuhlmann 2004). In accordance with the variety of approaches, a diversity of publications is available on the topic including numerous journal publications and (edited) books. The innovation systems perspective is applied on various levels of aggregation: National Innovation Systems (Freeman 1997), Regional Innovation Systems (Cooke et al. 1997) and Technological Innovation Systems (Carlsson & Stankiewicz 1991). However, all approaches share a consideration of the interactive nature of successful innovation processes (Edquist 1997).

The innovation systems approaches share with NPK the emphasis on the non-linearity and heterogeneity of knowledge production. Both reject the linear model of innovation in which basic research is translated into applied research, which in turn may result in technological product development (and subsequent diffusion). In Mode 2, the distinction between basic and applied science does not exist; in innovation systems such a distinction is conceived to be ineffective. Moreover, the organizational diversity of Mode 2 corresponds to the network character of innovation systems. Collaborations between universities and industry, but in particular the role of intermediary research organizations, figure in both bodies of literature.

A distinctive feature of the systems approach in innovation studies is that it is merely a heuristic framework rather than a descriptive theory. Compared to the Mode 2 thesis, it hardly contains any descriptive claims. While NPK's authors argue that contemporary knowledge production is heterogeneous and non-linear by nature, innovation systems literature only argues it should be heterogeneous and non-linear in order to facilitate fruitful innovation processes.

2.3.5 Academic Capitalism

The book 'Academic Capitalism' (Slaughter & Leslie 1997) reports the observation of increasing market- and market-like activities at universities in a set of empirical case-studies[15]. The authors aim both to describe and to explain this phenomenon. With 'academic capitalism' they refer to two types of activities. First they point to the increasing (market-like) competition for external funding: grants and contracts, endowment funds, university-industry partnerships, institutional investment in spin-off companies, or student tuition and fees. Second they discern increasing market activities: for-profit activity, patenting or subsequent royalty and licensing agreements, spin-off companies, and university-industry partnerships having a profit component.

The authors explain this development by two factors. First increasing globalization enhances the pressure on industry to innovate and causes corporations to turn to universities for assistance. In the same time, the flow of public moneys to universities is receding. Together these factors make universities more willing to engage in 'capitalist' activities. Notably, both identified causes are external, in the sense that they originate outside the science system.

Slaughter and Leslie complement their empirical observations with a warning for the risks of the developments they describe. In all four countries (US, UK, Australia, Canada) they have studied, governments promoted academic capitalism as a means of stimulating economic growth. Except for Canada, they all succeeded in developing promoting policies. However, there is no clear indication for the success of market-activities, as only some universities in the United States manage to make money. Opposite of the potential benefits the authors identify substantial risks for researchers, universities and their managers. Market(-like) activities can lead to 'business failure', to product responsibility, failure to meet societal expectations (with regard to economic growth and employment) and neglect of students. For this reason, Slaughter and Leslie recommend governments to create incentives for universities to spend their money in the desired ways in order to avoid a decline in academic education.

'Academic Capitalism' partly confirms the claim of rising importance of Mode 2 knowledge production. The market(-like) activities described include (at least to some extent) the attributes 'context of application', 'organisational diversity' and 'novel modes of quality control'. The authors do not pay particular attention to transdisciplinarity. A curious empirical result is the observation that researchers are ambivalent with regard to 'altruism'. They hope that their research will benefit humankind, but this does not seem to be their first priority (p. 222). From their interviews Slaughter and Leslie have got the impression of researchers being pushed in the direction of academic capitalism, but they do everything

15 There is a large body of literature that deals with the increasing links with industry. However, a lot of studies in this category start from a firm's perspective and mainly deal with the potential benefits and costs for industry of collaborating with university researchers (Kaufmann & Tödtling 2001, Meeus et al. 2004, Meyer-Krahmer & Schmoch 1998). In the present context, however, we are mainly interested in the consequences of this development for scientific knowledge production.

they can not to become Mode 2 researchers. They do not show the intention of leaving university as they prefer to keep the advantages of being 'state-supported entrepreneurs' (p. 206).

2.3.6 Post-academic science

In Ziman's notion of post-academic science, he incorporates elements from several other diagnoses: Mode 2, Academic Capitalism and post-normal science. The notion is introduced in a single-author volume (Ziman 2000), which elaborates on ideas published in his equally successful earlier book (Ziman 1994). Ziman intends to describe and explain a set of developments in scientific knowledge production. To summarize, post-academic science refers to a 'radical, irreversible, worldwide transformation in the way science is organized, managed and performed' (p. 67). Post-academic science (or 'post-*industrial* science' science, as Ziman calls it as well) can be characterized by the following five (strongly connected) elements.

First, science has become a collective activity: researchers share instruments and co-write articles. Moreover, both the practical and fundamental problems that scientists are concerned with are transdisciplinary in nature, calling for collective effort. Second, the exponential growth of scientific activities has reached a financial ceiling. The resources available for research seem not to increase much more, creating a need for accountability and efficiency. Thirdly, but strongly related, there is a greater stress on the utility of knowledge being produced. The success of applying scientific knowledge into products or practical solutions in some fields has made industry, government and the public impatient with its diffusion rate in general. There is an increased pressure on scientists to deliver more obvious 'value for money'. Next, the emergence of science and technology policy has strengthened the competition for resources. In the resulting situation, competition for real money becomes more important than competition for scientific credibility. Research groups can be conceived as small business enterprises, their staff as 'technical consultants'. Finally, science has become 'industrialised': the links between academia and industry become closer and funding increasingly comes from contract research. This development contravenes the Mertonian norms of academic science. Due to the industrial orientation a new set of norms can be discerned, which Ziman labels as 'PLACE': '*Proprietary, Local, Authoritarian, Commisioned,* and *Expert*'[16].

Although his approach is primarily descriptive, Ziman is not neutral towards the development of post-academic science. In a recent paper (Ziman 2003) he draws attention to the 'non-instrumental roles of science', which are threatened in the post-academic era. If science is valued primarily as a mode of wealth creation, certain functions of knowledge production are overlooked. These include the creation

16 'It produces *proprietary* knowledge that is not necessarily made public. It is focussed on *local* technical problems rather than on general understanding. Industrial researchers act under managerial *authority* rather than as individual. Their research is *commissioned* to achieve practical goals, rather than undertaken in the pursuit of knowledge. They are employed as *expert* problem solvers, rather than for their personal creativity.' (Ziman, 2000, p. 78-79)

of critical scenarios and world pictures, the stimulation of rational attitudes, and the production of enlightened practitioners and independent experts. Ziman is convinced that post-academic science is here to stay; we cannot go back to the old academic model. However, he argues for a fuller consideration of the non-instrumental roles in the debate about the future of science.

The concept of post-academic science is quite similar to that of Mode 2 knowledge production. There are no real contradictions between the content of both notions, only some difference in emphasis. Indeed, Ziman refers to Mode 2 in a way that suggests he conceives it as a synonym of 'post-academic science' or at least for the manifestation of that which he calls 'post-industrial science' (p. 80). The most important difference between Mode 2 knowledge production and post-academic science is probably the scope of the two central notions. Whereas Mode 2 refers to a particular way of conducting and organising research that constitutes a limited but increasing part of the science system, post-academic science is a name for the whole science system in its new state. This difference results in a different relation between the traditional and the new mode of research. While NPK explicitly states that Mode 2 emerges 'next to' Mode 1 research and suggest a future in which both develop in co-evolution, Ziman speaks of post-academic science as a practice that replaces traditional academic research. 'Our exemplar is changing before our eyes into a new form – *post-academic science* (…)' (p. 60).

A similarity between Ziman and Gibbons et al. is the loose empirical foundation of their observations. In both cases, the authors themselves have not gathered any new data. In the same way as in NPK, Ziman only loosely refers to secondary data, although he does it a little more frequently.

2.3.7 Triple Helix

The Triple Helix model (Etzkowitz & Leydesdorff 2000, Etzkowitz & Leydesdorff 1998, Leydesdorff & Meyer 2006) is based on the assumption that industry, university and government are increasingly interdependent. This implies that these different institutional spheres have to be studied in co-evolution. The model can be seen as a heuristic forcing researchers to systematically take into account all three spheres when studying dynamics of knowledge production and innovation. Triple Helix does not have a uniform descriptive message like NPK, but it rather constitutes a research program that has yielded a variety of descriptive claims. Its body of literature consists mainly of special issues of scientific journals dedicated to the Triple Helix conference series.

The central insight that this approach has yielded is the observation of 'an overlay of reflexive communications' between universities, industries, and governmental agencies. According to Etzkowitz and Leydesdorff (2000), in most countries there is a tendency towards a knowledge infrastructure in which these three institutional spheres (academia, state and industry) overlap. In this configuration the spheres can take each other's forms and hybrid organizations emerge at the interfaces. The linear model of utilization of scientific knowledge is replaced by new organizational mechanisms that integrate market pull and technology push. Basic research is linked to utilization through series of intermediate processes such as government initiated programs that facilitate university-industry interaction. The rise of this configuration is mainly due to the enhanced role of knowledge in our economy and society, and to the decreasing role of the military.

The role of universities in this configuration is often referred to as its 'third mission'[17]. Making a contribution to economic growth is becoming a central task next to teaching and research[18]. Within the Triple Helix literature, research with this mission is referred to as 'entrepreneurial science' (Etzkowitz et al. 2000b, Etzkowitz 1998, Kleinman & Vallas 2001).

This new role of universities and its new relations with government and industry are roughly in agreement with the idea of Mode 2 science. Especially the context of application and organizational diversity are apparent. Etzkowitz and Leydesdorff also confirm transdisciplinarity with their observation that new disciplines (such as computer science or nanotechnology) arise 'through synthesis of practical and theoretical interests' (Etzkowitz & Leydesdorff 2000). As has been indicated in the previous section however they disagree with the view of Mode 1 as the original format of knowledge production. Moreover Etzkowitz and Leydesdorff prefer to speak of Mode 2 as an 'emerging' system emphasising historical dynamics. In their eyes the current knowledge infrastructure is characterized by mixes of Mode 1 and Mode 2 (p. 119).

2.3.8 Concluding remarks

Table 2.3 summarizes the main findings. The comparison shows that the individual elements of the Mode 2 diagnosis are not unique. All characteristics that it addresses return in one or more of the other approaches. Nearly all approaches pay attention to the changing research agenda and the increasing interaction between science and other societal actors. This suggests that these observations are correct, especially since they are supported by empirical evidence in for instance the 'Academic Capitalism' volume and the 'Triple Helix' corpus. Other claims are more distinctive, especially the ones dealing with methods, epistemology and values. The most striking result of the mutual comparison, however, is that it shows the exceptionally wide scope of the Mode 2 diagnosis. None of the alternatives deal with as many characteristics of science as NPK does. It is unclear, however, whether this is a strength or a weakness[19].

17 This point is elaborated in literature on the 'entrepreneurial university' (Etzkowitz 2003, Etzkowitz et al. 2000a). Although written by one of its main founders, it is not necessarily part of the Triple Helix corpus.

18 One can argue about the adequacy of the term 'third' mission as the authors do not seem to refer to a completely new task but to a reformulation of the second mission.

19 We answer this question in the concluding section

Table 2.3 The various diagnoses put emphasis on different characteristics of scientific knowledge production

Levels	Characteristics	NPK	Post-normal science	Triple Helix	Post-academic science	Academic Capitalism	Strategic Science / Research	Innovation systems	Finalization science
Cognitive	choice of research agenda (research content)	X	X	X	X	X	X	X	X
	methods (teamwork, transdisciplinarity)	X	X						
	epistemology (socially robust knowledge)	X[a]	X						
Organizational	map of disciplines (transdisciplinarity)	X		X					
	values / labor ethic of scientists (reflexivity)	X			X				
	norms of quality control (extended peers)	X	X		X		X		
External relations	interaction with other societal 'spheres' (industry, government)	X		X	X	X	X	X	X
	incorporation of non-scientific expertise (participation)		X						

[a]NPK is unclear on this point. The follow-up book 'Re-Thinking Science' (Nowotny et al. 2001), however, does claim that an epistemological transformation is taking place.

2.4 The reception of Mode 2

After the comparison with alternative approaches, we will now focus directly on the strength of the Mode 2 diagnosis and study its reception in scientific literature. 'The New Production of Knowledge' (Gibbons et al. 1994) has received over 1000 citations in scientific journals[20] and Figures 1 and 2 indicate that the number of references per year is still increasing. Table 2.4 presents a list of all journals in

20 Scopus search on January 18th 2007

which 10 or more references were found. As one can expect, journals in the area of science, technology and innovation studies are dominant, with a light emphasis on journals that are policy-oriented. Some exceptions in the higher rank deal with management ('British Journal of Management'), organization studies ('Organization'), and with policy and planning ('Futures').

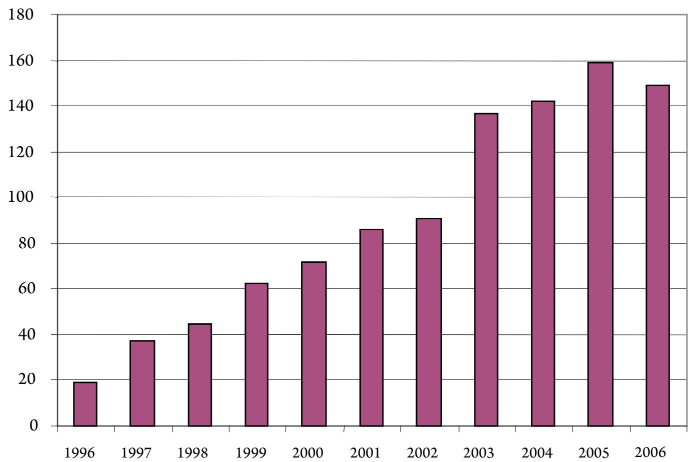

Figure 2.1 Number of citations of NPK found in Scopus

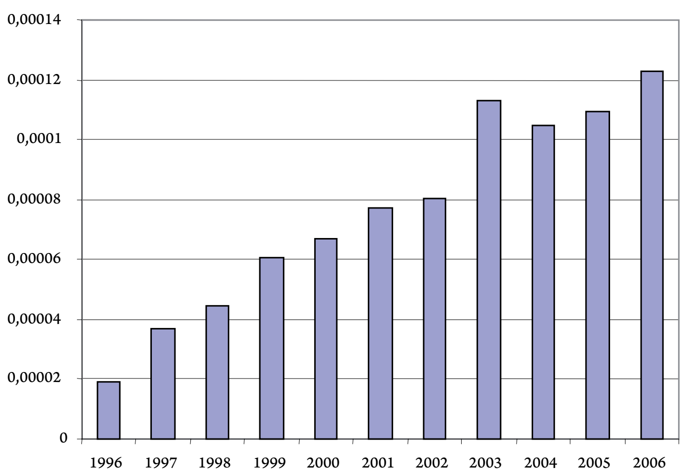

Figure 2.2 Number of citations of NPK found in Scopus, as a fraction of all articles containing the letter 'A', in order to correct for the growth of the number of journals that are included in the Scopus database.

Table 2.4 Scientific journals in which NPK was cited at least 10 times

Journal name	**Number of references**
Science and Public Policy	51
Research Policy	40
Scientometrics	38
Higher Education	36
Minerva	21
Research Evaluation	20
International Journal of Technology Management	19
British Journal of Management	18
Studies in Higher Education	15
Social Science Information	15
Technology Analysis and Strategic Management	15
Science Technology and Human Values	15
Futures	13
Social Studies of Science	12
Organization	12
Prometheus	12
R and D Management	12
Higher Education Policy	10

A closer look at the corpus that is represented in Figures 1 and 2 reveals that NPK's citations can be roughly divided into two sets. The first set of papers (we estimate about 80%) refers to NPK in the introduction or conclusion section, treating it as an accepted account of the current transformations. In these cases, the notion of Mode 2 serves to sketch the background for the research that is reported. It helps either to design a theoretical framework from which research questions are formulated or to discuss the implications of the findings. For example, Starkey and Madan (2001) use the Mode 2 notion as a theoretical framework to discuss current developments in management studies. Lee and Bozeman (2005) refer to the rise of Mode 2 in order to emphasize the importance of research collaborations. Lenhard, Lücking and Schwechheimer (2006) cite NPK to sketch the background of their discussion about the future of transdisciplinarity[21]. The content of these articles varies widely, but they have in common that they refer to NPK in an approving manner, without questioning the validity of its claims.

The second set (roughly 20%), however, does not take the legitimacy of the Mode 2 concept for granted, but puts its claims to the test. Papers belonging to this category generally dedicate more text to the issue of Mode 2. They do not cite NPK 'by accident', but use it as an essential theoretical starting point. The following section addresses a number of papers that belong to this second set.

A Scopus citation search[22] yielded a list of all scientific articles with a reference to NPK. Based on source title and article title, a subset of all papers that have been cited 20 times or more was selected for detailed study. This subset constituted the starting point of the literature study. Other papers and book chapters taken into account were all found by tracing references of this subset. In this way, the most important[23] contributions to the Mode 2 debate should be included (see table 2.5).

21 Although the authors do no criticize Gibbons et al., their discussion of transdisciplinarity in fact contradicts NPK's message. Their plea for 'late integration' rather than 'early integration' amounts to a preference for Mode 1 above Mode 2 knowledge production. The integration of disciplines early in the research project is the central characteristic of NPK's concept of transdisciplinarity.

22 Search entry: REF(gibbons AND "new production of knowledge")

23 In this context, two conditions for 'importance' can be distinguished: articles that give extensive reactions to NPK and that have influenced other scholars (to be measured by the number of citations). Note that our focus is on the debate directly linked to the Mode 2 concept. Literature about the changes addressed in NPK in fact is much broader, as not all research dealing with these developments necessarily cites NPK. We do not aim to cover all literature dealing with changes in contemporary science systems, but only papers explicitly reacting to the Mode 2 concept.

Table 2.5 Important reactions to NPK

Authors (year)	Title	Number of citations[b]
Godin (1998)	Writing performative history: The *new* new Atlantis?	21
Weingart (1997)	From "Finalization" to "Mode 2": Old wine in new bottles?	41
Hicks and Katz (1996)	Where is science going?	35
Godin and Gingras (2000)	The place of universities in the system of knowledge production	23
Hemlin and Rasmssen (2006)	The shift in academic quality control	0
Rip (2002b)	Science for the 21st century	3
Albert (2003)	Universities and the market economy: The differential impact on knowledge production in sociology and economics	11
Shinn (2002)	The Triple Helix and new production of knowledge: Prepackaged thinking on science and technology	19
Rip (2000)	Fashions, lock-ins and the heterogeneity of knowledge production	5
Etzkowitz and Leydesdorff (2000)	The dynamics of innovation: from National Systems and ''Mode 2'' to a Triple Helix of university-industry-government relations	175
Jansen (2002)	Mode 2 knowledge and institutional life: Taking Gibbons on a walk through a South African University	4
Jacob (2000)	'Mode 2' in context: the contract researcher, the university and the knowledge society	10
Pestre (2003)	Regimes of knowledge production in society: Towards a more political and social reading	7

b Scopus search, April 27th 2007

2.5 The main objections to the Mode 2 notion

The criticism of the claims of NPK found in scientific literature is very diverse. We have identified 7 recurring objections, which will be subsequently presented. In our evaluative discussion (5.8 and 6), we clustered them into three categories: NPK's descriptive or empirical validity (5.1-5.3), its theoretical and conceptual strength (5.4 and 5.5), and its political value (5.6 and 5.7). The first type of objection is the most common, but the other two appear regularly as well.

In the following, we will discuss the main criticisms to NPK. In some cases we regard it appropriate to comment on the validity of the objections posed. Is the objection indeed a serious problem to NPK? To our mind, this depends on correct citation of NPK, available empirical evidence and convincing arguments.

2.5.1 The descriptive validity of the various attributes of Mode 2

Table 2.6 lists the authors that comment on the five Mode 2 attributes, indicating the nature of their comments (positive or negative).

Table 2.6 Reactions to the individual attributes of Mode 2.

Mode 2 attributes	Godin	Weingart	Hicks and Katz	Godin and Gingras	Hemlin and Rasmussen
Context of application	-	-	+/-		
Transdisciplinary	-	-	+		
Heterogeneity	-		+/-	+/-	
Reflexivity / social accountability		-			
Novel quality control	-	-			+

Context of application

The assertion that research is increasingly conducted in the context of application receives relatively little protest. Weingart (1997) argues that the 'context of application' would lack stability, which means that it will always remain dependent on disciplinary practices. However, this does not contradict the Mode 2 theory as Gibbons and his co-authors also expect Mode 1 science to remain present.

According to Gibbons et al. Mode 2 knowledge is produced in contexts of application, going beyond the distinction between basic and applied research. Godin (1998) argues, however, that this distinction (which would disappear in Mode 2) in fact has never existed. Fundamental research has always been inspired by more applied knowledge and applied research has always shown interest in fundamental understanding of the relevant phenomena. This observation, however, does not imply that basic and applied research have never been separate domains. Godin's argument does not affect NPK's claim that the interactions between basic and applied research are intensifying.

Hicks and Katz (1996) test the claim that the locus of knowledge production shifts to the context of application Their bibliometric analysis, however, does not succeed to either confirm or reject this claim.

In conclusion, the 'context of application' remains a complicated concept and there is a lack of clarity with regard to the difference with 'applied' science. A possible solution for this problem can be found in typology of research modes of Stokes (1997). He breaks open the classic dichotomy of basic and applied research resulting in a quadrant-model, which treats the quest for fundamental understanding and the considerations of use as distinct variables (see Figure 2.3). In this framework, 'considerations of use' corresponds to 'context of application' and still leaves room for different degrees of being inspired by the quest for fundamental understanding.

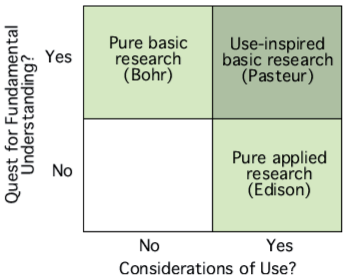

Figure 2.3 Stokes' model of scientific research (Stokes 1997)

Transdisciplinarity

The claim that science becomes increasingly transdisciplinary receives mixed reactions. The discussion of this topic is complicated by the lack of universal definitions of inter- and transdisciplinary research.

Godin (1998) criticizes the dichotomy between disciplinary research and interdisciplinary research. According to him, the development of disciplines with specializations and hybrid formations is typical of any scientific practice. Knowledge production never occurs in isolation; it always involves the employment of elements from other disciplines. Disciplines might acquire some degree of autonomy, but Godin assumes the same could happen to 'transdisciplinary' research. This argument would hold for interdisciplinary research. However, transdisciplinarity as proposed by Gibbons et al. implies more than only the cooperation of different disciplines. Additional conditions include the co-evolution of a common guiding framework and the diffusion of results during the research process. The assertion that this type of knowledge production is currently gaining importance is, in our mind, not sensitive to the criticism just described.

Weingart (1997) shares Godin's concern that the recombination of disciplines is not a new phenomenon. As defined in NPK, transdisciplinarity involves more than that, but according to Weingart, what the differences are 'remains vague and ambiguous' (p. 596). A more serious problem raised by Weingart is that the difference between the level of program funding and the actual research. Research programs may formulate interdisciplinary or even transdisciplinary problems, but the research carried out under their headings is often of a disciplinary or multi-disciplinary kind.

Hicks and Katz (1996) observe a growth in 'transdisciplinary' journals (although disciplinary research still accounts for the bulk of scientific output). However, they employ this term in different sense than Gibbons et al. According to Hicks and Katz, a journal counts as transdisciplinary when it cannot be classified as belonging to a single field or discipline. They do not take into account at all the epistemological and methodological dimensions of the definition of transdisciplinarity as used in NPK. As a result, the selection made by Hicks and Katz includes journals which Gibbons and his co-authors would call 'multidisciplinary' journals. Their outcomes do prove that interactions between the various disciplines are increasing, but they can not provide insight into the qualitative nature of these interactions and does not imply that they are 'transdisciplinary' in NPK's sense.

Heterogeneity

Most scholars seem to agree that the heterogeneity of knowledge production is increasing, but they disagree about the extent.

In a bibiliometrical study, Godin and Gingras (2000) show that the share of academic publications that include non-university contributions is increasing, in accordance with the Mode 2 claim of 'organisational diversity'. In spite of this diversification, however, the presence of universities in scientific papers is not diminishing at all. In Canada the share of papers including a university address has increased from 75% to 82% in the period 1980-1995. From this study it can be concluded that intersectoral collaboration is growing, but universities remain at the centre of knowledge production.

Weingart (1997), on the contrary, argues that the role of think-tanks and consulting firms is negligible in terms of manpower and budgets. This will probably vary for different disciplines; in management research, for instance, they will be prominent (Huff 2000). Weingart's remark that they remain dependent on academic research does not contradict the NPK claims.

In their bibliometric analysis, Hicks and Katz (1996) show that an increasing number of organizations house authors of journal articles. Between 1983 and 1991, the number of organizations in the UK participating in scientific publishing has increased in all sectors they studied (hospitals, industry, non-profit, universities, government and polytechnics), except for research councils[24]. However, within the various sectors, Hicks and Katz do not discern a uniform trend toward dispersion of the publication activity. Several sectors (e.g. industry, and hospital) even have become more concentrated. For this reason, their observation can not be seen as an indicator for the increasing heterogeneity of scientific knowledge production.

24 This observation may seem to contradict the findings of Godin and Gingras but does not necessarily imply a difference between Canada and the United Kingdom. The co-existence of the findings of the two studies can be explained by assuming that the share of papers with authors from several sectors has increased, or even more general, that the average number of authors on each paper has increased.

Reflexivity / social accountability

Reflexivity and social accountability receives less attention in the criticisms of the Mode 2 concept. Weingart, for instance, regards social accountability to be mainly applicable to policy-relevant knowledge production. In these disciplines he agrees that an institutionalization of reflexive mechanisms is discernable. Yet, in areas of knowledge lacking an immediate connection to social values and subjective risk perception ('high-energy physics, astronomy, and paleontology' (p.603)), he denies that there is either a need for or a perceivable rise of reflexivity. This assertion is not supported by empirical evidence, however. The issue of reflexivity seems to deserve further investigation.

Quality control

The novel types of quality control probably constitute the most controversial attribute of Mode 2 knowledge production.

In agreement with NPK, for instance, Sven Hemlin and Søren Barlebo Rasmussen (2006) argue that a shift is taking place from 'quality control' to 'quality monitoring'. Similar to Mode 1, however, the former remains important. The notion of quality monitoring shares some characteristics with Mode 2. It is subject to influences of industry and policy, it includes new 'peers' (users, consultants, lay persons) and a greater consideration of ethical and political issues. However, Hemlin and Rasmussen also add some elements that are not apparent in NPK: a shift from the assessment of individuals to organizations and a shift in time perspective from a retrospective judgment of research activities to an ongoing evaluation process during the research progression. The authors also relate the putative shift to organizational theory and argue for a further study on organizational learning in scientific institutes. However, they do not provide much empirical evidence for their claims, apart from a couple of illustrative examples.

Weingart and Godin are sceptical, however. Godin (2000) argues that the scientific criteria are still the most important. Referring to the personal experience of his readers, he states that the success of attracting research funds being dependent on extra-scientific criteria relating to social priorities, relevance and accountability 'is still a rhetoric rather than a fact' (p. 478). In our opinion this assertion does not do right to the diversity of contemporary funding systems. Probably still a lot of funding allocation is still ruled by scientific considerations. Nevertheless, today a significant share of funding in many western countries[25] depends on societal priorities.

Similar to Godin, Weingart (1997) claims that in industrial research marketability and cost effectiveness have always been present, while in academic quality assessment they are still of minor importance. Even in policy-relevant science that is carried out in the context of application, the scientific standards assessed by 'peer review' remain the most significant measure of quality. However, in contrary to Wein-

25 For instance the European Framework Programmes and national innovation policy instruments. Examples in the Netherlands are 'Technologiestichting STW', 'Technologische Topinstituten' and 'Innovatiegerichte Onderzoeksprogramma's'.

gart's suggestion on 'ample freedom for (…) researchers and long-term perspectives' (p. 603) at corporations such as IBM, multinationals today seem to be increasingly reluctant to engage in fundamental research programs. Due to globalized competition, industrial corporations have been forced to cut their budgets and have chosen to cut the costs of long term research activities (De Wit et al. 2007).

In conclusion, the importance of additional quality criteria at universities is contested and remains a question open for empirical investigation.

2.5.2 Generality of Mode 2 notion

In addition to the detailed comments on the individual Mode 2 attributes, scholars point to limitations in the empirical validity of NPK's claims on a more generic level. In this respect, two major problems can be discerned: the generality of the argument *per se* and its historical perspective.

Gibbons et al. argue that the rise of Mode 2 affects the whole science system: 'Mode 2 is spreading across the entire landscape of science and technology' (NPK, p. 22). 'These changes appear in the natural and social sciences but also in the humanities' (NPK, p. 3).

Weingart (1997) contests these claims. He argues 'that the features of "Mode 2" (…) are limited to a fairly small sector of the entire science system' (p. 608). He claims that some Mode 2 attributes (context of application, transdisciplinarity) make sense only for science which is close to policy-making such as environmental research. The subset of academic research from which he believes Gibbons et al. draw their evidence, represents only a fraction of the entire science and technology system. Features like 'uncertainty of knowledge, complexity of subject matter, policy orientation and value-ladenness' (p. 600), which characterize the sector of technology assessment, risk research, and environmental and climate research, cannot be generalized. Therefore, Weingart sees no reason to believe Mode 2 will extend to all other areas of science.

Godin (2000) rejects the generality of NPK's approach, too. In particular, he argues that 'the social sciences, as well as the humanities, have always been of Mode 2, much more than has been the case for the natural and physical sciences' (p. 472).

Albert (2003), however, has shown that there is no observable trend towards Mode 2 in the sociology and economics departments of two Canadian universities. Based on interviews with scientists and a study of their publications, he states that there is no tendency towards problem-oriented research, but rather a predominance of Mode 1 knowledge production. Albert concludes that his findings demonstrate that academic research can not be seen as a homologous unit. He argues that one should take into account the heterogeneity of scientific disciplines and of the various research 'regimes'.

Critics also mention the neglect of national contexts. Shinn (2002) claims NPK fails to 'recognize that the university, business and government all function in a national setting' (p. 610). He argues that scientific disciplines and specialties operate differently in different national institutions. In spite of current globalization, the national component of the organization and work of science is still apparent. Referring to the literature on national systems of innovation, Shinn states that, even in Europe, national science policies are still of great importance.

The claim of NPK of a general move to Mode 2, therefore, is denied by several authors on different and sometimes contradicting grounds. This implies in any case that the specificity of disciplinary developments needs be taken into account much stronger.

2.5.3 The long-term historical perspective

NPK's second generic limitation indicated in literature concerns its historical perspective. Referring to historical studies of sciences, several scholars (Etzkowitz & Leydesdorff 2000, Rip 2000) claim that at least some of the attributes of Mode 2 knowledge production have always been present in modern science.

Rip (2002b) rejects the view of Mode 1 as the original type of research. We should rather see it as historically located: it emerged during the course of the19th century and became locked in during the 1950s and 1960s. Features of Mode 2 such as heterogeneity and transdisciplinarity are not new; they were already present in the 'Renaissance melting pot' (Rip 2000) before the birth of modern science.

Etzkowitz and Leydesdorff (2000) also contest the newness of Mode 2 and use similar arguments. Referring to historical studies of science they claim that it is not Mode 1 but Mode 2 which is the original format of science, as, in the 17th century, research focused on practical problems. 'Mode 2 represents the material base of science, how it actually operates. Mode 1 is a construct, built upon that base in order to justify autonomy for science, especially in an earlier era when it was still a fragile institution and needed all the help it could get' (p. 116).

Similarly, Pestre (2003) argues that elements of Mode 2 have always existed in modern science. Knowledge producers have never isolated themselves in an ivory tower, but have always paid attention to the interests of states and economic elites relating to science. Moreover, 'science has always directly contributed to, and has been a major resource for, changes in social ideologies' (p. 250).

2.5.4 The coherence of the concept

The Mode 2-authors describe Mode 2 as a stable entity, with a specified set of characteristics. This implies that the notion of Mode 2 is coherent in the sense that the various attributes mutually correlate. 'These attributes, while not present in every instance of Mode 2, do when they appear together have a coherence which gives recognisable cognitive and organisational stability to the mode of knowledge production', Gibbons et al. declare (NPK, p. 8). Critics seriously question this assumption, and the criticisms discussed above illustrate this. If the evidence for the diverse attributes of Mode 2 varies, and some receive more assent than others, one can wonder about their mutual relations. Possibly the claim of the NPK authors about the rise of Mode 2 should be divided into five different claims about five distinct trends in contemporary science.

According to Rip (2002b), the separate features that Gibbons et al. describe 'are clearly visible, but one might question their overall thesis that these add up to a new mode of knowledge production' (p. 104-105). He doubts whether the features together have enough stability to make it appropriate to speak of a new research mode.

Godin (1998) confirms two of those claims, but he rejects the other three. He agrees that the heterogeneity of the science system has increased and that researchers are more socially accountable. But in his view both the 'context of application' and 'transdisciplinarity' have always existed, while novel criteria of quality control are not yet apparent. Similarly, Weingart (1997) considers all attributes to be present in policy-relevant science, except for novel quality criteria.

2.5.5 Theoretical underpinning

Shinn (2002) discusses a second conceptual problem. He is concerned about the lack of theoretical underpinning of NPK's sociological framework. In Shinn's reading, 'anti-differentiationism' is the central feature of NPK's approach, as it blurs the boundaries between academic, technical, industrial, political and sociological institutions. However, this central idea 'is never buttressed with sociological theory, concepts or models' but it 'stands as a free-floating, unintegrated component' (p. 604). NPK does not account for 'how differentiations have operated in the past, how and why they would have eroded, and what their putative demise implies for sociological theory' (p. 611).

This criticism is not completely fair. The fact that NPK does not talk about the past indeed is a severe limitation that calls for further study. However, although its treatment of the mechanisms may not be sociologically sound, it does provide an account of the why and how of the erosion of differentiations. It refers to causal forces in the supply and in the need of knowledge and it offers an explanation of the putatively increasing interactions across disciplinary and institutional boundaries. Explicit relations with sociological theory are indeed absent in NPK, but these receive some attention in the follow-up book Re-thinking Science.

2.5.6 Mode 2: wishful thinking?

Several authors complain about the uncritical blend of descriptive and normative content in NPK. According to Godin (1998), the talk of Mode 2 is more a political ideology than a descriptive theory. He associates the message with a 'polarized rhetoric', which 'denounces many of the characteristics of contemporary research and training in the name of social and political *desiderata* which are themselves in exact opposition to characteristics of traditional academic research' (p. 479). The lack of empirical foundations makes this a dangerous situation from a policy standpoint as it can easily raise the impression that the current research system *needs* to be replaced. Godin warns that some readers of NPK may conclude that the old system and the old academics are wrong and that a new type of research would be better than traditional academic research.

The comments made by Shinn (2002) are similar: 'Instead of theory or data, the New Production of Knowledge – both book and concept – seems tinged with political commitment' (p. 604). Shinn perceives the authors as being in favour of a new cognitive and social order. In his reading, they aim to support Mode 2 by persuading others to believe in its importance and desirability.

This relates to what Weingart (1997) calls 'performative discourse'. He also has the impression that the idea of Mode 2 is part of a normative program rather than an empirical analysis (p. 608), but in this remark he mistakenly fails to distinguish between Mode 2 and post-normal science. In fact, the latter indeed contains strong normative elements, but – as we have argued in Section 2.4 – this is exactly the main difference from the former.

Some commentators have indeed treated NPK as a prescriptive rather than a descriptive theory. For instance, the criticism uttered by Jansen (2002) and Jacob (2000) is more concerned with the problems of bringing Mode 2 into practice than with the question whether the change is actually taking place. In contrast to the previous critics, however, these do not contest the fact that NPK may bear a normative dimension, but question the validity of the normative content itself.

2.5.7 Lack of future outlook

A minor comment to be mentioned here is expressed by Weingart (1997), who accuses Gibbons et al. of not being clear with regard to the persistence of Mode 1 knowledge production (p. 593). NPK indeed is somewhat ambivalent in its future outlook. On the one hand, the authors acknowledge that the disciplinary forms of cognitive and social organization continue to be prerequisite of identity, as happens during education and training. On the other hand, they expect that eventually 'Mode 1 will become incorporated within the larger system which we have called Mode 2' (p. 154), but this statement does not receive any explication. What Gibbons et al. exactly expect to remain of Mode 1 research, therefore, remains vague.

2.5.8 Concluding remarks

Clearly, the empirical validity of the Mode 2 claims is limited. On a generic level, critics convincingly indicate two major problems. First, the NPK authors disregard the diversity of science and second, their historical view is mistaken. Moreover, some of the Mode 2 attributes are heavily disputed. In particular empirical evidence to show the rise of reflexivity, transdisciplinarity, and new modes of quality control is lacking.

Concerning NPK's conceptual strength, one can argue that more links to sociological theory are required. We consider the problem of a lack of coherence more serious. Given the fact that the different attributes of Mode 2 receive assent to varying degrees, there seems no compelling reason to tie them together under a common heading. Whether Mode 2 has 'recognisable cognitive and organisational stability' is highly questionable.

In our opinion, the comments regarding the normative message of NPK do some injustice the content of the book[26]. A careful reading reveals that the authors do not have explicit normative intentions. On the first page, Gibbons et al. already mention that their intentions are descriptive rather than normative: 'No judgement is made as to the value of these trends – that is, whether they are good and to be encouraged, or bad and resisted …'. The remainder of this sentence, however, can explain some of the confusion: '… - but it does appear that they occur most frequently in those areas which currently define the frontier and among those who are regarded as leaders in their various fields.' Strictly speaking, this sentence does not qualify Mode 2 as better than Mode 1, but it does hint in this direction. So there is some rhetoric, and as rhetorics about science can lead to social reality (van Lente & Rip 1998), the critics have right to complain. This means, thus, that the NPK authors could have been a bit more careful in distancing themselves from normative claims, that is, if that was their intention.[27] In addition, more clarity with regard to the future of Mode 1 knowledge production is desirable.

26 Neither Godin, Shinn nor Weingart support their objections with citations from the book.

27 Note, however, that even when the authors did not have any normative ambitions, the notion of Mode 2 could be taken up by others as a direction to follow. Texts have a life on their own, beyond the reach of authors, and may lead to self propelling dynamics.

2.6 Towards a research agenda

Our comparison with alternative diagnoses of the dynamics of contemporary science systems shows the particularly wide scope of the concept of Mode 2 knowledge production. The diagnosis expressed in NPK (Gibbons et al. 1994) includes statements about changes on the cognitive, organizational and the societal level. Whatever one may think of the adequacy of its analysis, this must be regarded as an accomplishment. Thanks to its breadth, NPK has created a forum to discuss a wide range of putative trends.

There are certainly big differences in aims and scope between the various diagnoses that we have addressed in Section 2.4. An important difference is their balance between descriptive and normative content. While the 'post-normal' science literature shows a clear normative orientation, others primarily limit themselves to reporting observations. Another difference is between heterogeneous research programmes (such as Triple Helix and innovation systems) and well-defined analyses (such as NPK and Academic Capitalism).

However, as displayed in Table 2.3, the content of the various accounts show strong similarities. The claim that the content of scientific research agenda is currently changing recurs in all diagnoses: all address a turn towards more relevant research, research that (sooner or later) may lead to applications in the form of innovations or policy. Furthermore, all approaches point to more interactive relationships between science, industry and government. In conclusion, at least two characteristics of knowledge production that NPK claims to be undergoing change find strong resonance in the alternative diagnoses. In other words, there is some degree of consensus on these points.

The other characteristics that are addressed in the Mode 2 diagnosis are much more contested. The conviction that the shift towards more relevant knowledge production also involves a change in the research methods is hardly shared by other diagnoses. The same holds for the claims that the disciplinary map is undergoing profound changes and that there is a rise of novel modes of quality control. To conclude from this comparison then, that these three claims are wrong, would be too quick. It does point, however, to the need for empirical evidence for the putative changes.

The Mode 2 claims have received mixed reactions: hundreds of papers cite NPK affirmatively and policy makers use the arguments, but there is also serious criticism. An analysis of the receptions of NPK in the literature has yielded a list of seven objections, which we divided into three categories: the empirical validity, the conceptual strength, and the political value of NPK.

Empirical validity

1 There is a lack of empirical evidence for the rising importance of the attributes of Mode 2 (Weingart 1997, Godin 1998, Hicks & Katz 1996).

2 The long-term historical perspective is incorrect: the view of Mode 1 as the original type of knowledge production is contested (Etzkowitz & Leydesdorff 2000, Rip 2000, Pestre 2003).
3 The universality of the claims is not justified: in contrast with the generality of NPK, scholars expect the dynamics to be different in different national contexts and in different scientific disciplines (Shinn 2002, Tuunainen 2005, Albert 2003).

Conceptual strength

4 The necessary coherence of the concept is questionable: there might be a lot of multidisciplinary, application oriented research that does not show organizational diversity or novel types of quality control (Rip 2002b).
5 The claims lack a theoretical underpinning and references to sociological theory (Shinn 2002).

Political value

6 The authors seem to implicitly support the observed trends (Weingart 1997, Godin 1998, Shinn 2002).
7 The book lacks a proper future outlook (Weingart 1997).

These seven objections point to problems with the content as well as to problems with the form of NPK. The last three accusations, for instance, merely deal with the form that NPK's authors have chosen to present their message. In fact, they indicate that NPK does not always meet the standards that a scientific reader expects. Although many critics have treated it as a scientific theory, we rather suggest conceiving it as a manifesto[28], in which the authors are more concerned with getting the message across than with building sociological theories or with carefully distinguishing their observations from their opinions.

However, this observation does not necessarily affect the validity of the descriptive and conceptual content of NPK. Even the most carelessly written manifesto can still point in the right direction. And, in the case of NPK, even if both complaints about its political value were correct, the concept of Mode 2 can still be useful: they affect the appreciation of NPK as a book, but not necessarily as a descriptive project. The same holds for objection 5. Yes, the lack of theoretical underpinning definitely constitutes a limitation of NPK. Nevertheless, it does not automatically have implications for the accuracy of its diagnosis. The fact that the Mode 2 concept was introduced without references to sociological theory does not imply that it cannot be related to theory. For the concept to be viable, however, it would be required to make these references later to avoid it becoming a theoretical 'island'.

The first 4 objections are more severe threats, especially the noted lack of coherence. Probably least threatening is the mistaken historical perspective, as this weakness can be corrected. In this respect, NPK is simply wrong, as it gives a too linear account of the historical dynamics of scientific practice. The

28 Note that the volume was originally written for an audience of policy makers rather than scientists.

suggestion that a traditional disciplinary mode of research is gradually giving way to a more interactive mode is not historically correct. For example, intimate interactions between science, invention and entrepreneurship were already important in the British industrial revolution (Freeman 1997). Another example is the well-known steep increase in prestige of and available funding for basic research in disciplines such as chemistry and physics just after WWII provides another illustration. Inspired by the presidential advice by Vannevar Bush (1945), western economies devoted large amounts of money to basic research, as conducted in universities[29]. This points to a strong increase - or even stronger: a 'lock-in' (Rip 2000) - of Mode 1 at the expense of research with Mode 2 characteristics. We should not regard the changing modes of research as a one-dimensional development. Yes, it is conceivable that currently Mode 2 knowledge production is gaining importance in comparison with Mode 1. But to view of Mode 1 as the 'traditional' mode, and Mode 2 as the mode which introduces radically new characteristics to scientific practice, is incorrect. As has been suggested by Martin (Martin 2003), it may be more appropriate to speak of '*shifts in the balance* of Mode 1 and Mode 2 over time'. Research is needed, then, to specify the historical contingencies in the Mode 2 concept.

Problem 1 indicates a serious weakness of NPK: its lack of empirical evidence. We concluded that some claims are readily shared amongst alternative approaches. Of the five attributes of Mode 2, the 'context of application' and 'heterogeneity' receive assent both in the alternative diagnoses and in the direct reactions to NPK in scientific literature. For the other three (transdisciplinarity, reflexivity, and novel modes of quality control), however, neither confirming nor falsifying evidence is available. Further empirical research is required to decide whether NPK's claims about these points are appropriate.

The idea of transdisciplinarity is contested. In their reactions to NPK, several authors have addressed the issue of interdisciplinarity with theoretical comments (Godin 1998) or empirical investigation (Hicks & Katz 1996). However, the question of NPK's concept of transdisciplinarity is still open, because this includes additional features when compared to the concept of interdisciplinary research. Is the integration of disciplinary research elements as dynamic as Gibbons et al. argue? Numerous publications are available on the issue of transdisciplinarity (Lenhard et al. 2006, Pohl 2005, Després et al. 2004), but these do not demonstrate the actual diffusion of this mode of research[30]. A question to address in an empirical study is how often the research outcomes are indeed communicated *during* the process of knowledge production.

29 Ironically, the original report did emphasize the importance of interconnections between basic research and other parts of innovation processes, too. However, the report has generally been interpreted as a propaganda document advertising the importance of funding basic research and viewing applied research as 'second rate' (Shapley & Roy 1985).

30 It must be noted that various definitions of transdisciplinarity are available, some in disagreement with NPK (Lawrence & Després 2004). For some (Regeer & Bunders 2003), the distinctive characteristic of transdisciplinary research is the inclusion of knowledge of non-scientists. Strictly speaking, however, this feature is more related to what Gibbons et al. call 'heterogeneity' than to their concept of transdisciplinarity.

The claim of NPK that reflexivity and social accountability tend to increase also merits more research. Slaughter and Leslie (1997), for instance, report that the ambition to enhance human welfare does not seem to be a first priority for scientific researchers. It remains to be seen to what extent the (potential) relevance of their work influences the choices researchers make about problem choice, research design and methods. This raises questions about the reflexivity reported by Gibbons et al. Can one discern an increased awareness of possible societal effects on the actual laboratory floor or is it only visible during interactions of researchers with their societal 'stakeholders'? Following Weingart's criticism (Weingart 1997), one can wonder whether the trend of increased reflexivity is limited to policy-relevant sciences. There are many other fields that appear to have a stronger orientation towards commercial applications, but it is uncertain whether this makes the scientists involved more reflexive.

About the possible new modes of quality control the evidence is mixed. Some studies confirm the change (Hemlin & Rasmussen 2006) but others (Weingart 1997, Godin 1998) reject it. In Swedish funding agencies for technical research, applicability has become an important criterion (Benner & Sandstrom 2000), but the impact of this change is still uncertain[31]. After a comparison of funding practices in Sweden, the UK, Norway, Canada and the USA, Benner and Sandstrom conclude that, although new criteria such as utility and demands from 'customers' have been added, research councils preserve their core orientation: the collegial control and evaluation of research (Benner & Sandstrom 2000). In the Netherlands, societal relevance is a structural element of the evaluation procedure of research group performances[32]. However, it is unclear how important this specific criterion is and how it should be measured. Recently a number assessment tools have been developed and tested which address criteria for societal relevance. Examples are the 'societal quality research profile' (SQRP) in health research (Spaapen 1995) and the 'research embedment and performance profile' (REPP) of agricultural sciences (Wamelink & Spaapen 1999) and pharmaceutical research (Dijstelbloem et al. 2002). Novel modes of quality control also figure prominently in post-normal science literature. In this context, scholars have developed novel assessment systems which contain additional criteria in which non-academics have an important place (Van der Sluijs et al. 2005). It is unclear, however, on what scale this type of evaluations has been adopted to date. Quality control is a broad phenomenon that comprises diverse practices. In the NPK definition, quality control is the set of procedures and criteria that constitute the 'selection mechanism of problems, methods, people and results' (p.32). This implies that it includes the assessment of research proposals applying for funding, manuscripts for publication in scientific journals, applications for conference contributions, applications for academic positions, and performances of research groups, programmes and projects. The question, then, is to what extent the novel criteria count in all these practices. It is conceivable that relevance or applicability is of more decisive importance as a cri-

31 The new criteria employed by the funding agencies have aroused a debate including public letters signed by hundreds of university professors (Benner & Sandstrom 2000).

32 VSNU, NWO & KNAW 2003, Standard Evaluation Protocol 2003-2009 for Public Research Organisations.

terion for attributing funding than it is for assessing candidates for academic positions. To put it simply, the issue at stake is: What rewards do researchers receive for conducting relevant research?

The disregard of the diversity of scientific practice constitutes another weakness in the Mode 2 diagnosis. NPK raises the impression of a dichotomy of two research modes. Contemporary philosophy and sociology of science, however, emphasize the heterogeneity of scientific practices (Stengers 1997). Scientific research is carried out in an endless variety of ways. Modern science is a 'patchwork of very different activities, joined together under an umbrella label, SCIENCE' (Rip 1997). It is improbable that they can all be classified as either Mode 1 or Mode 2 knowledge production. Probably it is much more valid to speak of Mode 1 and Mode 2 as the extremes of a continuum than of them as two mutually exclusive categories. In this way Mode 1 and Mode 2 are ideal types, rather than really existing phenomena[33] and this raises the possibility to position existing practices of knowledge production on the continuum. It seems, however, that currently the knowledge is lacking for systematically positioning the different disciplines and subfields. Although several scholars argue against the generality of NPK, they only offer rough estimations of the differences between the disciplines.

An important lesson of our review is that in investigating changes in contemporary science systems, one should take into account the diversity of science. Due to the heterogeneity of scientific practice, the emergence of new modes of knowledge production will not have the same impact in the whole science system. Its importance may vary in national contexts (Shinn 2002) and in scientific disciplines (Albert 2003). Disciplinary characteristics that influence the shifts in balance between different modes of knowledge production need not be limited to the content of their inquiries but include features of social organization (Whitley 2000) too. Further research must show how visible the various Mode 2 attributes are in different disciplines and in different countries.

The problem of the lack of coherence is probably the largest threat to the Mode 2 concept. The disagreement about the five attributes of Mode 2 and their relative importance shows, in the end, that there is no compelling reason why they should operate together[34]. It seems more appropriate to regard the individual attributes as separate trends than as characteristics of a general development. Literature suggests that there may be a lot of research with one or more of the features, but that the amount of work assembling all five is marginal. For this reason, we propose to untie the wrapping around the Mode 2 concept. The individual trends that it addresses definitely deserve further research, but this should be conducted separately, ignoring the common heading of Mode 2.

33 Muller (2000) as cited in Tuunainen (2005: p. 282): NPK over-dichotomizes the evolution of science 'presenting it as two discrete ideal types that probably never exist in their pure form in the real world'.

34 This problem is also mentioned by Steven Yearley (2005).

To conclude

NPK has been successful as a manifesto. With its broad scope and evocative claims it has raised considerable attention in the area of science policy. It identifies a number of trends which still deserve further consideration. A review of alternative accounts and criticisms shows that the Mode 2 diagnosis of contemporary dynamics of scientific practice contains some adequate claims, and that some claims seem doubtful (the rise of transdisciplinarity, reflexivity and novel modes of quality control). Moreover, the generality of the arguments, the linear historical perspective and the necessary coherence of the original Mode 2 arguments are all problematical. The next step we suggest in rethinking new knowledge production is addressing the following three empirical questions:

1. Do transdisciplinary research activities, with a dynamic integration of theoretical and practical components from various disciplines, constitute a substantial part of contemporary science systems?

2. Are university scientists in general increasingly reflexive, in the sense that they are aware of the potential societal effects of their research and take these into account in their choice of research objects, methods and approaches?

3. Do new criteria, relating to the societal relevance of research results, currently count significantly in all types of scientific quality control, not only in funding allocation, but also in retrospective evaluations of individuals, projects or organizations?

To scholars addressing these three questions we strongly recommend taking into account the heterogeneity of science, paying attention to the differences between scientific fields and national contexts.

The conclusion of this paper is that the viability of an aggregate Mode 2 claim that is constituted by five attributes is limited. Our review shows that it is time to disconnect the five major constitutive claims and to investigate them separately.

Chapter 3.

In search of relevance: The changing contract between science and society[35]

Abstract
This paper reflects on the relevance of academic science. Relevance plays a central role in what we define as the 'contract' between (academic) science and society. The manifestations of relevance in the daily practice of academic research can be studied using the credibility cycle. Together, the science-society contract and the credibility cycle enable a systematic analysis of relevance in scientific disciplines. This is illustrated with a case study of academic chemistry in the Netherlands. We conclude that science's search for relevance is not new, but that its meaning changes together with changing ideas about the potential benefits of scientific research.

3.1 Introduction

The relevance of academic research is a challenge for policy makers and science studies. In various bodies of literature, it is claimed that the relationship between academic science and society is changing. Approaches like Post-Academic Science (Ziman 2000), Mode 2 knowledge production (Gibbons et al. 1994) and the Triple Helix of university-government-industry relations (Leydesdorff & Meyer 2006) all refer to an increasing orientation towards the production of 'relevant' knowledge (see Chapter 2). They observe an enhanced aspiration of academic research to contribute to the solution of societal problems and to support innovations and economic growth. This development expresses a profound change in the relationship between science, the state, the market and civil society. Some argue that interactions

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between traditional institutions are intensifying (Etzkowitz & Leydesdorff 2000), to such an extent that the boundaries have become blurred (Nowotny et al. 2001). Due to the limits on resources available, governments can no longer afford unconditional support for basic science (Ziman 1994) and demand value for money, urging university scientists to demonstrate that they are effectively fulfilling their tasks. In the meanwhile, knowledge is increasingly seen as a valuable resource to ensure economic competitiveness, but global competition has forced industry to cut budgets on research with a long-term horizon (De Wit et al. 2007). They are now dependent on strategies like Open Innovation (Chesbrough 2003), in which universities are seen as a supplier of strategic knowledge.

It is mistaken to consider the stress on relevance as a completely new phenomenon (Rip 1997). Since the emergence of modern science, expected benefits have played a role in research funding (Martin 2003). In the course of centuries, there have been several periods in which knowledge production was strongly connected to contexts of application (Pestre 2003). Intimate interactions between science, invention and entrepreneurship were, for example, already important in the British industrial revolution (Freeman 1997). Science is not an autarkic system; it has always depended on the provision of resources from the government or from other wealthy individuals or organizations so it may always have been in search of relevance. Nevertheless, the question remains when and how science can be relevant. Is research relevant when it yields knowledge about an urgent problem such as global warming? Is it relevant when societal stakeholders are involved? Is it relevant when industrial companies are willing to pay for it? Or is it relevant by definition, thanks to the cultural value of scientific activities? How is relevance defined and measured? When and how does the stress on relevance affect scientific practices?

Any attempt to address these questions should take into account that the notion of a 'singular science', one model of doing and organizing science, like Mode 1 knowledge production, is historically incorrect (Pickstone 2007). The way science is conducted at universities and the way it is influenced by outside forces and how it reacts to societal developments has changed over time. In a broad-brush narrative, Mode 1 may have shown an increase - or even a 'lock-in' (Rip 2000) - at the expense of research with Mode 2 features after WWII. The general conviction that basic research should be cherished like a 'diva' and separated from applied research and technology development, however, is specific for that time rather than a universal starting point for funding science and technology (Shapley & Roy 1985). The issue of relevance clearly is a complicated one. It seems that no straightforward answer is possible to the questions above, and there is not even a common definition of societal relevance.

The aim of this paper is to contribute to develop a heuristic framework that can contribute to the understanding of the various meanings relevance can get and the role these play in academic research practices[36]. We will start with the basic notion that relevance refers to the expected value scientific

36 This paper deals only with academic science, so research conducted at universities, not at public research institutes or in industrial laboratories. For the sake of readability we will not consistently use the predicate 'academic' throughout the paper. Everything we claim about science, scientists, research or researchers, however, is limited to university science and university scientists.

research will have for society. What does this added value mean more precisely? We will investigate how definitions of what relevance entails have shifted and may continue to shift. Our argument will proceed as follows. In the next section we will discuss the notion that the relationship between science and society can be conceived as a contract which defines two parties and their privileges and obligations. We build on existing literature by using the idea of a contract as a heuristic device, and enrich it by suggesting a basic composition of the contract between science and society. Next, we show how an analysis of the 'credibility cycle' (Latour & Woolgar 1986) can help to explore the meaning and position of relevance in actual research practices. Together, our notion of the contract and the credibility cycle constitute a heuristic framework to study 'relevance' in specific disciplines. In the last section we will illustrate and substantiate the framework in a case study of chemistry in the Netherlands. Our first intention with developing and presenting this framework is to enhance the theoretical understanding of the relevance of science. Second, we hope to contribute to the process of gathering empirical insights into the changing relationship between academic science and society.

3.2 A contract between science and society

Academic science is not an isolated enterprise. To keep its position in society requires legitimacy. Practices of science have changed over the course of centuries, but science has always depended on societal support in the form of resources and legitimacy. In the development of changing identities of science, different visions of science and connected legitimations can be discerned. Early modern science could be legitimatized by the claim that it was neutral in the sense that it would not interfere with religious matters and it would not disturb social order. In the 18th and 19th centuries, science's legitimacy became dependent on the visions of science as a 'model of rationality' and, later, as a 'reservoir of innovations' (Rip 1982, van den Daele 1978). The large-scale institutionalization of modern science in the twentieth century has made these latter two legitimations even more important. The substantial investments societies make in science today are only regarded as legitimate thanks to the great promises of modern science in terms of economic competitiveness, cultural enrichment or social progress. In other words, the 'relevance' of science has become crucial for its public support. Relevance can refer to practical applications of research outcomes, but also to other ways in which science may serve society, like the socio-cultural value of improved understanding of the world or the provision of a breeding ground for high quality education.

Numerous writings about changing science systems and concomitant legitimations employ the idea of a (tacit) contract. As Guston and Kenniston (1994b) argue the contract is a useful metaphor because:

- it 'implies two distinct parties, each with different interests, who come together to reach a formal agreement on some common goal';
- a contract is negotiated, 'arrived at through a series of exchanges in which each party tries to secure the most advantageous terms';
- a contract 'suggests the possibility of conflict – or at least disparity of interests'; and
- 'contracts can be renegotiated if conditions change for either party' (p. 5).

Baldursson (1995), Elzinga (1997), Martin (2003), Jasanoff (2005) and Rip (2007) use the notion of a contract as a heuristic for studying the changing relation between science and society. Although the approach, scope and size of their studies differ, their diagnoses are roughly similar. They start with the 'Endless frontier' contract, which refers to Vannevar Bush's report 'Science, the Endless Frontier' (Bush 1945). It holds a clear distinction between basic research, which is self-regulated and should not be disturbed by outside steering and applied research which is subject to immediate questions about relevance and external steering. This division is institutionalized by two categories of funding agencies: basic research councils headed by scientists and sectoral funding agencies mandated by different ministries. For several reasons, the 'Endless Frontier' contract is currently under attack. One of the factors is science's own success in sectors like healthcare and agriculture, which prompts a wish to co-determine its directions (Baldursson 1995). Society demands 'strategic research' (Irvine & Martin 1984, Rip 2004) or 'targeted research', new categories of basic research that combine internal scientific quality with external societal relevance (Elzinga 1997). Science-industry relationships intensify, science policy becomes strategic and funding arrangements have an increasing mission orientation (Etzkowitz & Leydesdorff 2000, Gibbons et al. 1994, Ziman 1994). A new contract seems to be emerging in which science's autonomy is increasingly constrained.

Other authors use the contract notion in a more normative way. Vavakova (Vavakova 1998), for instance, argues that to speak of a new contract is dangerous, because it tends to reduce the societal usefulness of science to purely economic value. In her reading, arguments for a new contract conceive research in a too utilitarian way. A new contract in which science is more closely linked to the private sector would threaten the availability of knowledge as a public good. In contrast, Gibbons (Gibbons 1999) argues that the prevailing contract which 'was set up to sustain the production of reliable knowledge', is not adequate anymore. Given the changes Gibbons observes in science and society (increasing societal complexity, convergence of university and industrial research, rise of Mode 2), a new contract is needed to ensure the production of 'socially robust knowledge' of which the validity is determined by an extended group of experts, including lay 'experts'. This knowledge is less likely to be contested than knowledge which is merely 'reliable' in an exclusively scientific way. For Gibbons, entering into the new contract involves embracing the rise of Mode 2 and contextualized knowledge production: '(...) science must leave the ivory tower and enter the agora' (Gibbons 1999).

In this paper, we follow the scholars who use the contract as a heuristic but we enrich their approach by a more specific conception of the *content* of the contract. Here we continue the route taken by Guston (Guston 2000) who defines 'the social contract for science' as follows: 'The political community agrees to provide resources to the scientific community and to allow the scientific community to retain its decision-making mechanisms and in return expects forthcoming but unspecified technological benefits' (p. 62). That is, science can take care of its own integrity (by self-regulation) and is able to produce benefits for society (productivity).[37]

Because the contract concerns the delegation of a particular task, Principal-Agent Theory (van der Meulen 1998, Braun 2003, Guston 2000) will be helpful. This approach assumes that policy-makers ask scientists to do something for them that they cannot do themselves, because they lack the capabilities or the knowledge the scientists have. In this view of relationships between policy-makers and scientists, policy-makers have four fundamental problems:

- getting scientists to do what politics want (problem of responsiveness);
- being sure that they choose the best scientists (problem of adverse selection);
- being sure that scientists do their best to solve the problems and tasks delegated to them (moral hazard); and
- knowing what to do (decision-making and priority-setting problem) (Braun 2003).

Science policy, in all its different, often complex, forms, can be understood as attempts to solve these four problems of delegation. Indeed, the evolution of science policy in the recent few decades can be described as a sequence of different models of delegation: blind delegation, delegation by incentives, austerity, contract delegation, and delegation to networks (Braun 2003, Potì & Reale 2007).

Principal-Agent Theory starts its analysis at the principal's side and tends to overemphasize the government's power in structuring the relationship and to ignore the perspective of the agent (Mor-

37 For the sake of clarity let us indicate two differences between Guston's approach and ours. First, in our view science has a contract with society in the broader sense rather than with the government only. Guston is mainly concerned with the government as a stakeholder in science. With his work on the 'social contract for science' he analyzes the changing ways in which the US government deals with scientific research. We, however, aspire to capture the societal environment in a broader sense, taking into account all actors that have an interest in scientific research. Besides governmental authorities, these are all other organizations and individuals that give financial support to academic research and all that can be expected to benefit from its outcomes. In fact, one may argue, all actors in society have a stake in science, but in varying degrees.
A second difference concerns the durability of the contract. In Guston's work, 'the social contract for science' is a label for a specific historical period roughly from 1945 until 1980. Guston uses the notion of the contract to describe and explain the particular relationship that existed between science and the government in the USA in these years, and to contrast it with the configurations afterwards and before. In our approach, however, the meaning of the contract is not limited to a specific point in time. We argue that the idea of a contract can always be applied to the science-society relationship, regardless of the historical setting. Of course the specific content of the contract can change, but its general structure will remain the same.

ris 2003). Yet, scientists also shape the institutions governing the task delegation. Another limitation is that Principal-Agent Theory has difficulties with dealing with multiple principals (Shove 2003) The framework is useful to describe the relationship between science and the government, but it can not easily accommodate the influence of other principals like industry or NGOs. In order to overcome these limitations, we will also draw from other theories, in particular the credibility cycle (Latour & Woolgar 1986, Rip 1988) and resource dependency theory (Oliver 1991, Pfeffer & Salancik 1978).

To conclude, the notion of a contract is a powerful metaphor and it can serve as a heuristic in order to enhance our understanding of relevance and of the changing science-society relationship. Existing literature offers several useful starting points, but some further elaboration is necessary.

3.3 A framework to examine the relationship between academic science and society

A contract expresses the positions and the mutual relationships of the actors involved. In other words, it draws on and expresses a moral universe, as in the case of a relation between a mother and her child, where norms and values involved of care and protection are involved. Although the norms exist and exert force, they are not fully recorded. So, explicit and implicit norms together shape a moral universe which indicates how mothers and children relate to each other. In the same vein, the contract between science and society expresses implicit and explicit agreements between science and societal parties.

The contract between academic science and society deals with the delegation of a particular task. Multiple principals ask scientific agents to do a job they cannot do themselves. From the scientists' perspective, the contract can be regarded in terms of resource dependence (Leišytė et al. 2008, Oliver 1991, Pfeffer & Salancik 1978). Scientists are dependent on their societal environment for their survival, so they cannot easily break the contract with society. If their environment develops new demands, scientists can choose between several strategies. They can shift their activities towards the new societal needs, in accordance with the 'compliance strategy'. Sometimes they manage, however, to pursue a 'symbolic compliance strategy', by creating an image which corresponds well with societal demands, without changing anything in the actual work. Alternatively they can try to negotiate with their environment about the content of the contract, according to the 'manipulation strategy' (Leišytė et al. 2008).

In this paper we propose to use the notion of a contract as a heuristic to study the changing relationship between academic science and society. Just like the moral universe of the example above, this contract cannot be found at a particular place. Nevertheless, its history can be told using various data sources like written utterances of different actors, actual policy measures, funding patterns and interviews with key actors. Assuming the existence of a contract can help to organize these data into a coherent history of relevance. To this end, we suggest to assume the contract to contain three main components. In our conception it addresses three basic questions: what is the nature of the delegated task, why is it delegated and how is it arranged? In other words, the contract arranges what science should do, why it should do this, and what are the appropriate conditions for science to fulfill its delegated task (see Figure 3.1). The answers to these questions, of course, are the result of negotiations between at least two parties. In the case of science and society there are more: neither science nor society can be seen as one actor. Moreover, science is a subsystem of society rather than external to it. Thus, the contract is an agreement among all actors that have a stake in academic research, including researchers doing the actual work, university managers, policy makers and research councils facilitating it, and companies, NGOs, authorities and any other actors who are using the knowledge developed by science. The assumption of a contract does not imply that there is no place for disagreements. The parties involved of course have diverse interests. Still at each point in time there are a number of rather fundamental notions which the actors do not call into question, but they refer to them as a shared vision.

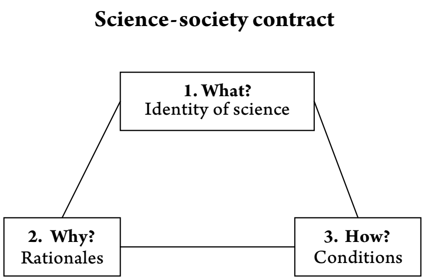

Figure 3.1 Suggested composition of contract between academic science and society

The identity of science

Academic science is concerned with conducting research in order to produce relevant outcomes. Science's task is to produce knowledge and to deliver it in the form of communication (publications, presentations) or artefacts. More specifically, the contract can describe what type of research should be carried out. What is the balance between basic and applied research, or is science supposed to conduct strategic research (Irvine & Martin 1984), or to work in Pasteur's Quadrant (Stokes 1997)? This question relates to the kind of knowledge science is expected to deliver, or in other words, what knowledge is considered 'relevant'. Societal actors may want science to offer expertise that contributes to the solution of complex policy issues, to deliver insights in metaphysical puzzles or to produce strategic knowledge which enhances its economic competitiveness. The contract can have (and, indeed, has had) various concepts of relevance and these can also be specified to varying degrees. However, one can probably assume that relevance always has a place in the identity of science, as society will demand some sort of return for its investments.

Rationales for funding science

In the proposed conception, the contract also describes why academic science deserves support. Many different (combinations of) arguments are possible, but most relate to concepts of the societal relevance of science. The expected benefits of science can take various forms, such as 'new or improved weapons, more accurate nautical almanacs (based on improved astronomical observations), better medical care and agriculture, improved engines, new chemicals and materials, new energy sources, new electrical devices and so on' (Martin 2003 p. 19). Without claiming completeness, we mention four generic rationales which are prominent in science studies literature. One possibility is to regard science as a cultural good, if one believes that it has an intrinsic value or 'axiomatic relevance' (Spiegel-Rösing 1980). Or academic research may be seen as inevitable to sustain a system of higher education (Martin 2003). Third, one may fear that academic research is needed because the market will invest insufficiently in basic research (Guston & Kenniston 1994b). Finally academic science yields knowledge of general interest (like expertise which can support governmental decision making or strategic knowledge that may lead to medical innovations) (Funtowicz & Ravetz 1993, Gibbons 1999, Böhme et al. 1983).

Conditions

The third element of the contract we propose contains agreements about the conditions under which university researchers work. 'To create new knowledge, special procedures, norms, rewarding mechanisms, and institutionalizations are put into place characterising scientific activities and distinguishing them from other professional activities in society' (Braun 2003). Society delegates research to science by 'a set of specific grants and contracts, each with its own terms and conditions' (Guston & Kenniston 1994b p. 8). For the government, the challenge is 'how to make sure that good and useful knowledge is produced, as well as how to assure that the investments in science do not go with unproductive pressures from the government to produce applicable knowledge' (van der Meulen 1998 p. 398), and the same goes for other stakeholders of science. Science's organization is attuned to its identity. A traditional feature of the way academic science is currently organized is the nested hierarchy of research groups in faculties. This may change, however, with the rise of inter-university 'Centres of Excellence' and other novel forms of organization (Rip 2004). Another agreement concerns the way money is distributed to scientific researchers, in which intermediary organizations have a central role (Lepori et al. 2007, van der Meulen & Rip 1998). This is not the right place for a complete description of all agreements concerning science's organization. Here, we just want to emphasize that the features of the organization of science in principle aim at facilitating the fulfilment of science's task. There can also be specific instruments ('organizational devices') which aim to enhance the quality and / or relevance of research, like earmarked funding (Benner & Sandstrom 2000, Potì & Reale 2007) and performance assessments (Geuna & Martin 2003, van der Meulen & Rip 2000)[38].

As the descriptions of the three elements of the contract indicate, their precise content can change and is subject to negotiation among all parties involved. We assume, however, that the general composition of the contract is of all times, as these three central questions are always at stake.

Credibility cycle

The next step in our investigation is to consider how 'relevance' manifests itself in the daily practice of academic research. Given the basic ingredients of the science-society contract (Fig. 2) it is now pertinent to ask how this may interfere in the way science operates. This undertaking can build on the sociology of science. A central finding in the sociology of science is that scientists are not primarily driven by financial incentives and rewards, but rather by a desire for recognition and reputation. In science, 'the need to acquire positive reputations from other scientists is a crucial factor controlling what tasks are carried out and how, and how they are evaluated' (Whitley 2000). Reputations play a central role in the organization of scientific work. Scientific activities are carried out to convince fellow researchers of the importance and significance of the results and hence enhancing one's own reputations. A scientist's reputation in his field is a key factor in acquiring jobs and resources. For this reason, his position in his employment organization (university) also depends on his reputation.

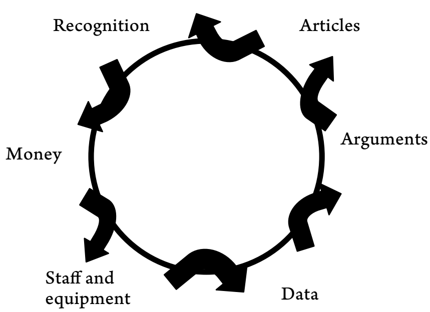

Figure 3.2 The credibility cycle, adapted from Latour and Woolgar (1986)

The concept of the 'credibility cycle' (Latour & Woolgar 1986) has been introduced to explain how struggles for reputation steer the behavior of individual scientists or research groups. Similar to Whitley's notion of reputation, credibility refers to the ability 'actually to do science' (Latour & Woolgar 1986 p. 198). This notion is broader than only recognition or reward for scientific achievements. It is of a more generic nature and can take various forms such as money, data or articles. Scientific behavior, then, can

38 Most of these devices are often referred to as 'practices of quality control' (e.g. Hemlin and Rasumussen (2006)). But in the present context, this notion is a little confusing, because these devices do not only control quality, but also relevance.

be described as a cycle of conversions of different types of credibility. Scientists invest time and money to acquire data that can support arguments. These are written down in articles, which may yield recognition from colleagues. Based on this, scientists may receive new funding, from which they buy new equipment (or hire staff) which will help to gather data again, etc. Conceived in this way, the research process can be depicted as a cycle in which conversions take place between money, staff and equipment, data, arguments, papers, recognition, and so on (see Figure 3.2). In this quasi-economic model, researchers are like investors of capital who spend credibility with the intention to earn it back after a complete cycle, with interest[39].

It is important for our purposes to note that at each point in the cycle the scientific actor meets specific institutions which facilitate or constrain the conversions he is making – a point not well elaborated in the original version of Latour and Woolgar. The scientist can not make the conversions independently. In each step there are formal or informal structures which influence the transformation of one form of credibility in another. For example, peer review procedures govern the conversion of arguments into articles. The criteria used by peers to assess the quality of manuscripts determine what type of arguments can be converted into publications. In the next conversion, recognition is only awarded to articles which have been accepted in prestigious journals, or – increasingly – which are cited by other scholars. In a similar fashion, all other conversions in the cycle are also governed by institutions of various kinds, such as university organizations, funding bodies and quality assessments. The institutions determine the exchange rate, so to speak, of one form of credibility into the next. For the researcher, the institutional environment is instrumental in building credibility.

Two issues deserve attention here to clarify how we understand the credibility cycle. First, although scientific researchers are the main actors in this model, it does not deal with science only. Knorr-Cetina (1982) has warned for the danger of internalism when using quasi-economic models of science such as the credibility cycle. One should not conceive scientists as if 'they were isolated in a self-contained, quasi-independent system' (Knorr-Cetina 1982 p. 109). Scientists' activities go beyond the boundaries of their specialist community. Recognition is received from peers, but also from scientific colleagues in other disciplines. As an extreme example, Nobel-prizes usually are awarded to research with an impact extending towards other scientific disciplines or even towards products or solutions in society. What is more, non-scientific actors play crucial roles in the acquisition of money. The availability of funding for particular scientific projects is a product of interactions between scientists, societal actors (in industry, government, or else), and often intermediary bodies like funding agencies. These interactions, however, can be described with the credibility cycle, if it is not applied in a narrow sense (Elzinga 1997, Wouters 1999, Leišytė 2007, Rip 1994). We follow Leišytė in assuming that credibility is achieved not only within a researcher's specialist community, but in 'several arenas that interact with each other, such as

39 This does not imply that the goal of scientists is only gaining credibility and not gaining knowledge. Rather, in order to take part in the research process (to gain knowledge) it is necessary to earn, retain and convert credibility.

financial backers, the scientific community, regulatory authorities, and professional and consumer marketing' (Leišytė 2007, p. 48).

Second, we assume that the precise composition of the credibility cycle can be subject to change. We consider it a valuable model for scientific practice. Its precise composition, however, is a historical contingency rather than a necessity. What conversions are part of the cycle, which ones are most important and what are the guiding norms and values may change over time. Although Latour and Woolgar (1986) have introduced the cycle as a timeless, generic model for scientific activities, several scholars have already shown that it is useful starting point for describing changes in the science system. Guston (2000), for instance, uses the credibility cycle to explain the relationship between scientists and the government in the USA during the period of the 'social contract for science', which he positions between 1945 and 1980. Packer and Webster (1996) argue that the cycle needs to be extended in order to accommodate patenting activities, which are of increasing importance for academic scientists. Rip (1988, 1994) shows that the credibility cycle can help to understand how the role of research councils has developed over the years. His analysis indicates the rise of a pressure for relevance and related criteria in the assessment of research proposals. As a result, scientists are now also involved in struggles for relevance and for legitimity, on top of struggles for facticity and for fundability (Rip 1988). In this paper we focus on the struggle for relevance. We use the credibility cycle in a more specific way than Rip, in a step-by-step analysis.

As a model for scientific activity, the credibility cycle can be used for studying the changing relationship between science and society. The central question, then, is: how is relevance manifested in each conversion of credibility? Are there significant changes in the composition of the cycle?

A combined model

Above we have explained how the notion of a contract and the credibility cycle both can help to understand what relevance is and how it figures in academic research practices. But how do both elements relate?

The credibility describes the activities of university researchers as individuals or as organized in groups or research institutes. In this way, it puts one actor in central focus. The conversions of the cycle may seem primarily processes that are internal to science, conducted by the scientists themselves. However, the institutions governing each step are co-constructed by society (to varying degrees). For instance, the possibilities to convert recognition into money depend on the formal structures of university funding and research councils. In turn, the criteria used in the selection of research projects to fund are strongly determined by notions of relevance as expressed in the science-society contract. Similarly, the production of scientific papers is in the first place ruled by peer review processes. But how scientists act is also strongly influenced by university policy and national quality evaluations. If the allocation of funding is increasingly based on the number of previous publications of the applicants, because this is conceived as an indicator of quality, this will stimulate scientists to develop strategies to increase their publication output (Weingart 2005).

In the terminology of Rip (1988), scientific activity takes place at level of 'scientific entrepreneurs mobilizing resources', but the intermediary level 'organized field of scientific institutions' also give shape to the credibility cycle. Some of the credibility conversions are ruled by institutions on the intermediary level, such as research councils or disciplinary associations. These institutions constrain actions and interactions of scientists, but also enable them by providing legitimacy and opportunities. The science-society contract can be located on the macro level of the 'general social context' (Rip 1988) in the sense that it is of a general kind and all stakeholders of science take part in it.

The science-society contract constitutes a platform for all actors in the research system which provides them with arguments, legitimacy and other resources. The particular specification of 'relevance of science' in the operative contract precipitates into the intermediary level organizations and therefore shapes the credibility cycle. The conditions which are generally believed to be needed for science to optimally fulfill its task are translated into institutions such as university organizations and research evaluations.

From the scientist's perspective, the conversions of the credibility cycle may appear as exchanges of different currencies. All forms of credibility constitute pools of resources that can be transformed into one another. The exchange rate of these conversions, however, is determined by the contract. For instance, the criteria ruling the selection of manuscripts for publication, the selection of candidates for academic positions, or the selection of research proposals for funding depend on the content of the contract between science and society. The agreements about the conditions probably have direct influence on credibility conversions, while the identity of science and the rationales for public support will indirectly contribute to the institutions steering the conversions of credibility.

In their turn, developments in scientific practice also contribute to shifts in the content of the science-society contract. As actors refer to concepts of relevance in their daily work, they reinforce the contract. In giving credit to particular research contributions or in deciding how to spend money, scientists refer to shared views on what science is and what it should or should not contribute to society. For instance, technological expectations that go together with research outcomes influence ideas about potential benefits of a particular field for society (van Lente 1993, van Lente & Rip 1998). Scientific research yields insights both in problems to be solved (e.g. climate change) and in possible solutions (e.g. metal hydrides for hydrogen storage on board of vehicles) (Bakker et al. 2008). These promises can give rise to dedicated research programmes, schools or institutes on the meso-level that subsequently influence the negotiations about the science-society contract. Expectations about how particular science and technology can solve societal problems shape concepts of the societal relevance of science in general, too. Particular expectations may deal with specific domains, but in the end the overall view on the value of science will be affected by the expectations in all fields together.

To conclude, the contract enables and constrains scientists and others in their activities. It expresses the agreements among all actors and determines the rules and regulations of the institutions which give shape to the credibility cycle. In this way, it makes particular conversions of credibility possible, but complicates others. There is no uni-directional causality between the contract and the various credibility cycles. The contract influences academic practice, and developments in practice, in their turn, influence the agreements in the contract. Because of these interactions, we believe that a study of the relevance of science requires analyzing the contract as well as scientific practice. In the following section we will illustrate this in an empirical case study.

3.4 Struggles for relevance in academic chemistry

We now go one step further, and investigate how concepts of relevance and the struggles around it are visible in a case study. As a unit of analysis, the scientific discipline seems appropriate. In this section we narrow down the focus from science in general to a case study of academic chemistry in the Netherlands[40]. Obviously the empirical outcomes of this case study do not represent science in general. The meaning of relevance can be different in each field of research. An interesting aspect of chemistry is the intensity which the relationships between universities and industry traditionally have. For this reason, one can expect the relevance of chemistry to be strongly connected with industrial applications and the relationship between science and society to be relatively stable. As such it seems a 'hard case' for studying changing notions of relevance and an interesting test for our framework. Given the heterogeneity of science, this case can obviously not be taken as representative of the whole science system. In some other fields, more radical transformations may occur, especially if considerations of economic or commercial relevance are traditionally absent. Our analysis starts with the post-war situation, previously referred to as the era of 'Science, the endless frontier'. First we will describe the changes in the identity, the rationales and the conditions that have regulated academic chemistry research in the Netherlands. Then we will address the changing credibility cycle.

Identity

In the first postwar decades, some professors have strong ties with chemical companies, but the main task of academic chemistry is to conduct basic research not directly aiming at industrial applications. The relevance of academic research is often defined in terms of a contribution to chemical industry, but more in terms of skilful and knowledgable R&D staff than in terms of useful knowledge.. Knowledge

40 This 10 month case study is based on document analysis and in-depth interviews with scientists, policy-makers and other societal stakeholders. Here we summarize our findings, which requires simplifying a complex history involving various actors with different stakes and interests. A more elaborated analysis of this case will be published separately.

transfer to industry takes place through the people who move to industry after their (doctoral) graduation, by consultancy services and by industrially funded research assistants (Homburg 2003).The usefulness of academic chemistry is not called into question, given the importance of chemical industry for the Dutch economy and the linear model-based belief that basic research will ultimately lead to innovation and economic growth. In the 1970s, when "science policy" is introduced, the government demands academic research to more effectively address societal needs[41]. The minister of science policy emphasizes that chemistry should produce knowledge which is useful not only in chemical industry, but also in domains like agriculture and healthcare. In the 1980s, when innovation emerges as a central topic for policy making, chemistry's relevance is broadened to include delivering applicable knowledge, too. The main activity remains basic research, but this is justified merely as a necessary condition for the existence of good applicable research. During the 1990s the idea of 'strategic research' emerges which blurs the boundary between basic and applied research. Chemistry's task then becomes producing 'strategic knowledge' which combines aspects of basic and applied research. Strategic knowledge can be defined as fundamental insights in domains of high relevance for economy or society (Irvine & Martin 1984, Rip 2004). The domain of 'sustainability', which gains importance since its introduction in 1987, becomes a major strategic field for chemistry[42]. This notion combines the aspirations to contribute to economical growth and, at the same, to solve environmental issues. The strategic identity of academic chemistry endures in the new millennium as it is compatible with the most recent innovation concepts, in which the university is seen as a supplier of basic knowledge which can be valorized by other actors in the innovation system.

Rationales

The most dominant rationales for funding academic chemistry of the 1950s and 1960s seem to be its necessity for the training of new R&D-workers and (less importantly) the cultural value of basic research (see Table 3.1). Chemical industry, with which academic chemistry always had strong bonds (Homburg & Palm 2004a), has a strong interest in highly educated researchers. Although some university researchers also provide industry with technical advice, in the post-war period, industry tends to regard universities primarily as an educational system to supply a steady stream of highly skilled researchers (Homburg 2003). The research council SON, founded in 1956, provides support to basic chemical research with the training of young scientists and the stimulation of cooperation as its primary aims. In the 1970s, when governmental science policy is born, the need for chemical research is framed in more general terms, going beyond its value for chemical industry. In justifications for its public support the po-

41 In the first policy paper (1974) by the first Dutch minister of Science Policy, Mr. Trip, the primary mission of science policy is defined as enhancing the agreements of research agenda's with societal demands.

42 Foresight studies by OCV (OCV 1995) and by KNCV and VNCI (KNCV & VNCI 1994) both pay significantly more attention to environmental issues than their predecessors from the 1980s.

tential to contribute to problem-solving in other sectors gains visibility[43]. This broadening is a response to the negative image of the chemical sector, due to the rising awareness of environmental problems. The budget cuts on basic research in industry in the 1980s increase the importance of the rationale for supporting academic chemistry relating to its potential contribution to technological innovations[44]. Chemical industry does not only desire to benefit from academic research in terms of educated staff, but also in terms of applications of the knowledge produced[45] (De Wit et al. 2007, Van Helvoort 2005). In the 1990s the notion of sustainable development becomes increasingly significant in rationales for funding (chemical) research[46]. This umbrella concept connects several previous rationales because it refers to growth in economic and environmental dimensions[47] (Rip 1997, van Lente & van Til 2008). Around the turn of the century, the funding of university research is increasingly framed as support for the national innovation system. Maintaining a "healthy innovation system" becomes a central goal of economic policy, which implies that universities deserve support thanks to their central position in this system. In this perspective, however, support for university researchers is accompanied with the expectation that they actively interact with other actors in the innovation system, and contribute to the process of "valorization", by writing patents or by founding spin-off companies.

Conditions

During the 1950s and 1960s chemical scientists have a high degree of autonomy. The most important types of funding (university funding and research council SON) are distributed without any conditions attached, based on considerations of academic quality. Industry also pays for research assistants and consultancy service, but only to a minority of all professors (Homburg 2003). In the 1970s scientists are increasingly held accountable for their work. Although they are not yet affected by formal policy measures, chemical researchers are under pressure to put more effort in explaining their research priorities to policy makers and funding agencies. From 1980 onwards, however, a substantial change occurs in the

43 E.g. Nota Wetenschapsbeleid 1974.

44 The first foresight study (1980) of chemistry, commissioned by the minister of science policy concludes that academic chemistry should define its research goals more sharply and that the contacts with industry deserve intensification.

45 The interest of chemical industry in academic research is also visible in an advisory report by VNCI (association of chemical industry) and KNCV (professional organization of chemists) that argues for increasing industrial steering of academic research and suggests a set of six 'priority fields' as a guideline in the conditional funding process.

46 'For all new scientific and industrial activities on the field of chemistry the **strives for sustainability and the minimalisation of environmental pressure** have become important boundary conditions.' (original emphasis) (OCV 1995, Chemie in Perspectief: een verkenning van vraag en aanbod in het chemisch onderzoek, Overlegcommissie Verkenningen, Amsterdam). NWO's Strategy Note on Chemistry for 2002-2005 is even titled: 'Chemistry, Sustainable and Interwoven' (NWO CW 2001, Chemische Wetenschappen Strategienota 2002-2005: Chemie, Duurzaam en Verweven, Nederlandse Organisatie voor Wetenschappelijk Onderzoek, Den Haag).

47 The 1995 foresight study on academic chemistry, for example, writes about sustainability: 'Much of the research on resources (...) is not only motivated by economic considerations but serves at the same time important ecological goals.' (OCV 1995, Chemie in Perspectief: een verkenning van vraag en aanbod in het chemisch onderzoek, Overlegcommissie Verkenningen, Amsterdam).

funding of chemical science, due to two interrelated developments. First, faced with the need to restrict the growth of science funding (Ziman 1994), the ministry of Science, Culture and Education starts to supply part of the first money stream on a conditional base (Blume & Spaapen 1988). Moreover, informed by foresight studies[48], an increasing share of the second money stream is dedicated to application oriented research[49]. Second, a growing part of all funding stems from industrial corporations[50] and the ministry of Economic Affairs starts to implement a number of innovation programmes in specific fields like Carbohydrates and Catalysis[51]. These developments continue in the 1990s. The reorganized research council NWO continues to broaden its mission beyond basic research and increasingly works with 'priority programmes' and 'fields of attention' that are chosen partly thanks to their societal relevance[52]. Another significant event in the 1990s is the institutionalization of performance evaluations. Governmental policy makers, research councils and university managers who have become more selective in supporting particular activities, develop a need for transparency with regard to research outcomes. In 1996 the first nation-wide quality assessment of chemical science is conducted[53], the second in 2002[54]. Due to the lack of a strict protocol, the evaluators can choose themselves to what extent they take into account considerations of societal relevance as for instance the economic value or technological applications of the produced knowledge (van der Meulen 2008). In practice, they turn out to generally ignore this type of criteria and focus strongly on traditional scientific norms[55]. After 2000 chemistry faces a further diversification of policy instruments. Thanks to their continued growth, the European Framework Programmes become a substantial source of income for academic chemists. Moreover, there is a rise of consortia-based funding, large sums of governmental money supplied to collaborative programs of university scientists which are monitored by (industrial) user committees and which explicitly aim at enhancing the interactions with industry[56].

48 Chemie, nu en straks: een verkenning van het door de overheid gefinancierde chemisch onderzoek in Nederland, 1980, Staatsuitgeverij, Den Haag; Over Leven: Betekenis van de Biochemie in Nederland, 1982, Staatsuitgeverij, Den Haag

49 SON, the major chemical research council, starts a program for applied chemical research in 1980, together with the new technology foundation STW. Moreover, SON's mother organisation ZWO ('Organization for Pure Scientific Research') is reorganized in 1988 into NWO ('Dutch Organization for Scientific Research'), in which there is more room for funding application oriented research.

50 During the 1980s, the number of temporarily paid chemical researchers funded by the 'third money stream' rises from about 70 to 350 (ACC-evaluatiecommissie 1991, Evaluatie van de universitaire chemie in de jaren '80, ACC/KNAW, Amsterdam).

51 The 'Innovation Oriented Programs' (IOPs), for example 'Membranes' (1983), 'Carbohydrates' (1985) and 'Catalysis' (1989).

52 SON Jaarverslag 1991, 1993, 1995.

53 VSNU 1996, Quality Assessment of Research: Chemistry: past performances and future perspectives.

54 VSNU 2002, Assessment of Research Quality: Chemistry and Chemical Engeneering.

55 This may change, as in the meanwhile a protocol is available (VSNU et al. 2003) and there are attempts to develop more holistic methods that include societal relevance (Spaapen et al. 2007)

56 E.g. the ACTS-program (Advanced Chemical Technologies for Sustainability) of NWO, the Technological Top Institutes funded by the ministry of Economic Affairs and the BSIK-programs (Besluit Subsidies Investeringen Kennisinfrastructuur).

Table 3.1 Overview of the changing contract for academic chemistry. + signs indicate that these elements complement existing elements rather than replacing them

	Summary of identity	Most dominant rationales	Most important conditions
50s and 60s	basic research	education cultural value	autonomy unconditional funding SON communities
70s	+ useful knowledge	+ problem solving potential	+ social accountability
80s	applicable knowledge	technological innovation	+ conditional funding + application oriented funding (STW, IOP, contract research) + foresight + scarcity of resources reorganization NWO
90s	strategic knowledge	+sustainable development	further prioritization + performance assessments
2000+	strategic knowledge	+ innovation system	+ consortia (ACTS, TTI, bsik) + European FPs

To summarize, there have always been bonds between academic chemistry and industry but the type of interaction has changed. The meaning of relevance has changed in the course of years. Initially education and cultural value ruled its definition; later serving society and the environment; in the 1980s innovation became dominant; since the 1990s specified in terms of sustainability. Related, the emphasis in the rationales for funding chemical research have shifted from its function to support higher education and its cultural value to the notion that basic research is needed to sustain the innovativeness of industry since global markets fail to stimulate private sector basic research. An additional rationale that has evolved over the years is the need of chemical expertise for governmental decision making about regulations of emissions. The conditions specified in the contract have become increasingly complex. Chemists receive less unconditional support. Still the ministry of science provides a certain share of funding without specifying how it should be spent, but the degrees of freedom in spending this 'basic funding' is decreasing too[57]. Moreover, for a fruitful career, scientists depend on the acquisition of additional funding, from NWO, European Framework Programmes or from private companies. Each of these sources has specified targets and requires from researchers to define ex ante the societal significance of the research they propose. Moreover, a couple of new devices are in place to stimulate the production of good and relevant knowledge: performance assessments and foresight activities.

57 Due to university management like the current 'Focus en Massa' policy (e.g. Universiteit Utrecht, 2005, Strategisch Plan 2005-2009, Utrecht) and due the requirement of 'matching' externally funded research projects with block-grant support (AWT 2004).

Credibility cycle

Changes in the identity, rationale and conditions of academic chemistry will have an impact on scientific practice, which can be analysed in terms of the credibility cycle. The institutions around each conversion in the cycle are influenced by changes in the contract. Some conversions seem solely ruled by the scientific community, but in other cases external parties deliberately interfere. In our case study we followed a number of 'organizational devices' that have been designed to enhance a particular form of relevance of scientific research. In the case study, we identified five types of these devices:

- earmarked funding;
- foresight activities (e.g. Verkenningscommissies, Sectorraden);
- internal (scientific) procedures of quality control (peer review of scientific papers, selection of candidates for academic positions, citation practices);
- university management, (e.g. 'focus and mass'-policy, promotion criteria); and
- performance assessments (visitaties)

In the following we will discuss how these organizational devices interfere with particular credibility conversions (see Figure 3.3) and, thus, how 'relevance' has been expressed in the practices of Dutch academic chemistry research.

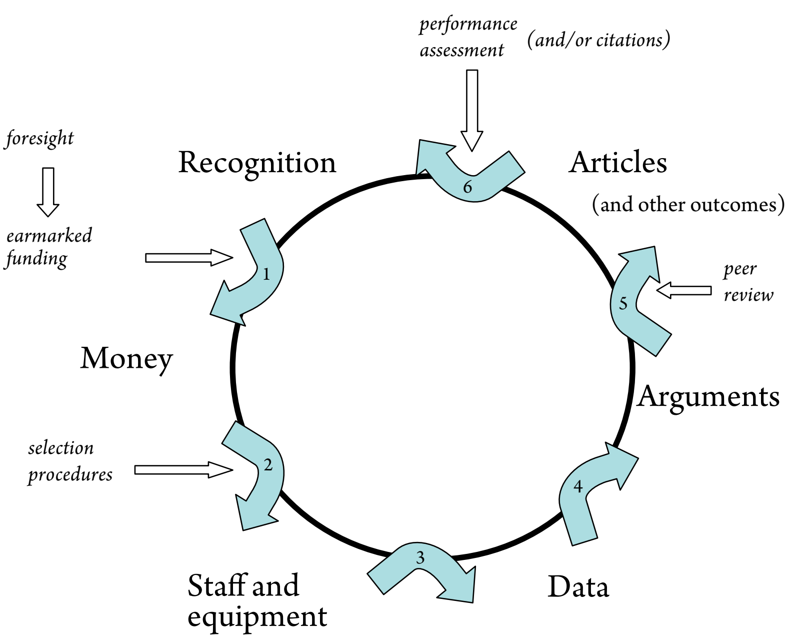

Figure 3.3 The credibility cycle, adapted from Latour and Woolgar (1986). Points at which organizational devices connect to the cycle are shown.

1. First, the rise of earmarked funding interferes with the conversion from recognition to money. The criteria connected to particular funds determine the possibilities for scientists to obtain money. In this way they co-shape the prevailing norms that govern this specific conversion. The availability of a number of strategic programmes for chemical research in the Netherlands implies that scientists who are recognized experts in particular fields (like catalysis or nano-science) have an advantage in acquiring money. Also the ability to promise practical applications is increasingly valued in this conversion. Moreover, earmarked funding is indirectly influenced by foresight activities. From a principal-agent perspective, foresight studies can be seen as an attempt of a principal to overcome delegation problems, since the studies seek to make the world of the agents more transparent to the principal (van der Meulen 1992). The foresight studies on chemistry[58] in the Netherlands have had a significant influence on the available funding for particular sub-fields[59].

2. The procedures for selecting candidates for academic positions influence the conversion of money into staff (and equipment). In these procedures normally a combination of various criteria is taken into account. Interviews with scientists indicate that recently, the publication and citation records have gained importance here. The most recent trend is that a candidate's proven abilities in funding acquisition are rewarded. Obviously, these developments co-determine how money can be transformed into staff.

3. The conversion of staff and equipment into data takes place relatively autonomously inside laboratories and other research locations. Governing norms seem to be primarily of scientific nature. However, the choice of *what* data to produce can strongly depend on agreements with funding sources and – less explicitly – on societal issues calling for particular scientific knowledge. The frequency of interaction with potential users during the research process varies across funding sources, from about two to six times a year. However, in interviews academic researchers speak about attuning their activities to user needs as a boundary condition rather than a primary goal.

4. The conversion of data into arguments depends on norms about the characteristics data should possess before they can support arguments: reliability, validity. Traditionally, there is little societal interference here. Proponents of post-normal science (Funtowicz & Ravetz 1993) claim this to change, the public becoming increasingly involved in quality assessments of scientific results in fields of social importance, but we have not observed this in our case study. Some interviewees, however, indicate that

58 Chemie, nu en straks: een verkenning van het door de overheid gefinancierde chemisch onderzoek in Nederland, 1980, Staatsuitgeverij, Den Haag; OCV 1995, Chemie in Perspectief: een verkenning van vraag en aanbod in het chemisch onderzoek, Overlegcommissie Verkenningen, Amsterdam.

59 For example, SON's decision to start funding application oriented research is partly based on the outcomes of the 1980 foresight study (SON Jaarverslag 1980).

sponsoring organizations sometimes attempt to influence the formulation of arguments because they have a strong interest in particular outcomes, for example regarding the safety of chemicals.

5. The conversion of arguments into articles is strongly ruled by peer review. All scientific journals employ this practice of quality control. Peers judge the quality of papers in academic terms, without considering their content's societal relevance. A significant modification of this conversion concerns the rising interest of research sponsors, particularly in industrial fields like catalysis, to patent research outcomes. For this reason industrial funding often implies a delay of 1-3 months for publications, due to the scientists' contractual duty to give their industrial partners some time to explore the patentability of their findings. We also found that chemical researchers are actively involved in writing patents, especially in the field of catalysis.

6. Obtaining recognition based on one's articles and patents traditionally depends on peer judgment of innovativeness and contribution. However, in the 1980s and 1990s the government and the union of universities have installed performance assessments that formalize the attribution of recognition (van der Meulen 2008). Bibliometric analyses combined with qualitative judgments from peers (and sometimes also of 'knowledge-users') yield scores for the performance of research groups, programmes and individuals. Interview data suggest that these scores increasingly contribute to an individual's scientific recognition. Although they do not play a dominant role yet, the intended involvement of 'users' and including the 'societal relevance' as a significant criterion in these assessments (Spaapen et al. 2007) may considerably modify the conversion of articles into recognition.

Note, however, the paradox here. Performance assessments, including bibliometric analyses, have increased the 'publish or perish' norm (Weingart 2005, Wouters 1999). Although they were started to enhance the societal accountability of scientists (van der Meulen & Rip 2000), they have increased the need for peer recognition rather than the need for societal justification. The development of bibliometric evaluation tools has added a quantitative dimension to the conversion from articles to recognition. Originally a means of communication, publications have become an end in itself.

The new mosaic of funding options, partly influenced by foresight activities, cause that only a happy few manage to get money without promising some sort of practical applications. In the contract of the 1950s and 1960s, scientists had many options for acquiring resources based solely on considerations of originality or scientific relevance, but this has become a rarity nowadays. One effect of the increasing complexity of funding options is that acquisition activities today consume a major share of senior scientists' time. This development has also influenced the procedures for selecting candidates for academic positions. Scientific publication records still seem to be the major criterion here, but increasingly the candidate's estimated fundraising abilities[60] are rewarded, too. Together the changed conditions under which scientists work have strengthened the mutual competition. The pressure to publish and to be cited has intensified[61].

3.5 Conclusion

The framework we have developed in this paper enriches the understanding of how scientific practice relates to external pressures and how internal developments influence science's relation with society. Today, relevance is at the core of the relationship between academic science and society. The legitimacy of public support for university research depends on its perceived potential benefits, ranging from contributions to culture or education to specific insights or products with economic value. The idea of a contract as a negotiated set of rights and obligations to understand this relationship turns out to be helpful. After making assumptions about the composition of this contract it can serve as a heuristic. Continuing on the route taken by Guston (2000), in this paper we have attained this higher degree of specificity, by conceptualizing the general content of a science-society contract in terms of 'identity', 'rationales' and 'conditions'. Moreover, we have created a link with the credibility cycle (Latour & Woolgar 1986), turning it into a tool to study the role and position of relevance in academic practices.

The 'relation between science and society' and 'scientific practice' both are vague concepts trying to grasp complex pieces of reality. The strength of our approach is that we have developed a model in which both are reduced to something that does justice to reality and at the same time allows for systematic analysis. Our notion of the contract clearly articulates the relation between science and society in terms of a limited number of variables. Similarly, the credibility cycle may not give an exhaustive overview of all activities involved in scientific research but it does describe the general pattern in which all activities have a position. Assuming that all scientists' actions aim to contribute to the conversion of credibility, the form of this cycle becomes a crucial factor determining scientific practice. Our framework does not imply any form of determinism. The cycle is not a result of only the contract, but of social interactions within and outside science. In the same way, the credibility cycle is not a dictating norm system, but rather a particular pattern of activities that are necessary for each researcher to produce credible information. 'Norms, the socialisation process, deviance, and reward are the consequences of social activities rather than its causes' (Latour & Woolgar 1986, p. 205).

60 Important indicators for the assessment of a candidate's fundraising ability are his / her past funding acquisitions and networking skills (evidence from interviews with scientists).

61 This is confirmed in our interviews with chemical researchers: 'Without it I do not survive' (full professor in biochemistry) and 'I think it is the only way to show what you have done. And I think that if something is not publishable in a scientific journal, it is not worth much' (PhD-student in biochemistry).

We believe to have demonstrated the usefulness of our framework in a case study of academic chemistry in the Netherlands. The framework helped us to identify the ways in which concepts of relevance change and how they influence scientific practice. In particular, we have distinguished various 'organizational devices', which aim to enhance the relevance of scientific research, the most influential ones being earmarked funding and performance assessments. An important finding in our case study is that the effects of the changes in the credibility cycle are partially in contradiction, creating a paradox around relevance. The pressure for strategic knowledge is most visible at the conversion from recognition to money, but at most other conversions it is of little importance. On the contrary, in the other conversions, the importance of academic criteria of success has increased further due to the rise of bibliometric quality criteria. In some subfields of chemistry this creates a tension between different parts of the cycle. This tension is absent in catalysis, where the applicability of basic research is easily demonstrable. But in other fields, like biochemistry, there is a difference between research which is excellent according to scientific peers and research which is considered useful by societal actors. This implies that the criteria guiding the selection of research proposals contradict the criteria guiding the selection of manuscripts for publication or the attribution of recognition. Scientists are then rewarded for making beautiful promises about the (possible) relevance of their research, but not for realizing these promises.

Possible next steps are to elaborate this case and to extend the approach to different scientific disciplines and to other national contexts. The way our framework helped us to observe changes in the relationship between academic chemistry and society suggests that it will work for other cases, too. Most other disciplines do not traditionally have such a strong relationship with one industrial sector, so they may show even stronger developments.

It is often assumed that the pressure for relevance has increased during the past few decades. However, it seems more fruitful to conclude that relevance has got different meanings and different forms in which it is expressed and expected. These forms can also be in contradiction, as our case study has shown. The meaning of relevance changes together with changing ideas about the potential benefits of scientific research. Here we have introduced a framework that facilitates a systematic empirical study of changing concepts of relevance as products of the interaction between science and society.

At least two implications for public policy follow from this paper. First, an enhanced understanding of the changing societal position of university research is crucial for making sound policy in the field of science and innovation. This is often based on popular and unarticulated notions of societal relevance, without a clear understanding of what these entail. This paper shows that it is important to be careful with the term 'relevance', as its meaning is variable in time and place. The notion sometimes even is an empty shell, as actors use it to their own benefit. Second, our brief case study of academic chemistry indicates that the effect of public policy is limited when it only intervenes at one position of the credibility cycle. Policy aiming to enhance the correspondence between academic research agendas and societal needs would be more effective if a more systemic approach is taken which includes a combination of instruments directed at various conversions of credibility.

Chapter 4.

Practical applications as a source of credibility: A comparison of three fields of Dutch academic chemistry[62]

Abstract

In many western science systems, funding structures increasingly stimulate academic research to contribute to practical applications, but at the same time the rise of bibliometric performance assessments has strengthened the pressure on academics to conduct excellent basic research that can be published in scholarly literature. We analyze the interplay between these two developments in a set of three case studies of fields of chemistry in the Netherlands. First we describe how the conditions under which academic chemists work have changed since 1975. Second we investigate whether practical applications have become a source of credibility for individual researchers. Indeed, this turns out to be the case in catalysis, where connecting with industrial applications helps in many steps of the credibility cycle. Practical applications yield much less credibility in environmental chemistry, where application oriented research agendas help to acquire funding, but not to publish prestigious papers or to earn peer recognition. In biochemistry practical applications hardly help in gaining credibility, as this field is still strongly oriented at fundamental questions. The differences between the fields can be explained by the presence or absence of powerful upstream end-users, who can afford to invest in academic research that promises long term benefits.

62 This chapter was co-authored with Harro van Lente and has been submitted for publication.

4.1 Introduction

Under labels such as 'entrepreneurial science' (Etzkowitz 1998), Post-Academic Science (Ziman 2000), and Mode 2 knowledge production (Nowotny et al. 2001, Gibbons et al. 1994), influential scholars have reported an increasing intertwinement of university research with practical applications. However, these diagnoses have been criticized for their theoretical shortcomings and for a lack of empirical support (Pestre 2003 , see also Chapter 2). Our starting point in this paper is that two major developments can be discerned in the governance of academic research, which may be (partly) in contradiction. First, the pressure on academic research has grown to contribute to practical applications of the knowledge it produces. Public support for university research has shifted from block-grant support to earmarked funding for specific projects and programs (Morris 2000, Lepori et al. 2007). University researchers are increasingly stimulated to engage in for-profit activities, patenting and subsequent royalty and licensing agreements, spin-off companies and university-industry partnerships (Slaughter & Leslie 1997, Geuna & Nesta 2006). At the same time, however, the rise of quantitative performance evaluations has increased the need for scientific accountability, which could enhance the pressure for publications in academic journals (Hicks 2009, Wouters 1997). While it seems of crucial importance for the future of academic science, the interplay between these two developments has received little attention so far. The current paper aims to fill this gap. We explore how the shifts in funding and the rise of evaluations have taken place in a specific discipline and analyze how these developments shape the research practices of individual researchers, in particular their orientation towards practical applications.

The central question of this paper is: have the changes in the science-society relationship made practical applications into a source of credibility for academic scientists in three fields of chemistry? Our analysis consists of two major steps. First we give a detailed analysis of the changing relationship between Dutch academic chemistry and society using the framework of a *science-society contract*. Second we systematically analyze the role of practical applications in the research practices of three fields of chemistry.

We have chosen for a comparative case study set-up, to be able to adequately deal with the heterogeneity of science. Labels like entrepreneurial science or 'Mode 2' tend to obscure the diversity of science, treating it as a monolithic system moving from one state to the other (Heimeriks et al. 2008, see also Chapter 2 of this thesis). However, changes in the governance of science will have different implications for different research fields, due to their cognitive and organizational differences (Bonaccorsi 2008, Whitley 2000, Albert 2003). Moreover, national science policies, the presence of particular industrial sectors and cultural variation considerably influence the dynamics of science-society relationships (Rip & van der Meulen 1996, Shinn 2002).

4.2 Theoretical framework

Our theoretical starting point is that scientists and their organizations are no isolated entities, but they interact with their environments to achieve their objectives. They depend on their environments for critical resources like funding and legitimacy (Leišytė et al. 2008, Pfeffer & Salancik 1978). From this perspective, on the *macro-level* the relationship of academic research with society can be seen as a 'contract' (Martin 2003, Elzinga 1997, Guston & Kenniston 1994a). In this paper, we build on the concept of the science-society contract developed in Chapter 3 (Hessels et al. 2009). This contract is not a physical entity, but a representation of the moral positions that encompasses all implicit and explicit agreements between academic science and governmental departments, NGOs, firms and other societal parties, specifying what science should do (identity), why it should do this (rationale), and the appropriate conditions for science to function well (conditions), see Figure 4.1. According to this contractual perspective, the very *identity* of science is connected to the provision of a valuable public good: relevant research outcomes. Science's task is to produce knowledge and to deliver it in forms like papers, patents, artifacts or educated people. The precise type of expected knowledge (basic, applied) and the degree to which science should be involved with practical applications vary over time and across disciplines. The contract, that is, the set of implicit and explicit agreements, also describes why science deserves support. Academic research is often regarded as a necessary stipulation for sustaining a system of higher education, commercial product development, and informing complex decisions and innovation. The third element of the contract, which will receive most attention in this paper, contains agreements about the *conditions* under which scientists work, including expectations regarding the social structure of the research community, allocation of research funds, and incentives for producing more socially relevant knowledge. The changing science-society contract of Dutch chemistry will be analyzed in Section 4.4.

The resource dependency of *individual* researchers, on the other hand, can be expressed by the 'credibility cycle' (Latour & Woolgar 1986). This model explains how struggles for reputation steer the behavior of individual scientists. Its starting assumption, underpinned by many sociological studies of science, is that a major motivation for a scientist's actions is the quest for credibility. Scientists invest time and money expecting to acquire data that can support arguments. These are written down in articles, which may yield recognition from colleagues. Based on this, scientists hope to be able to receive new funding, from which they buy new equipment (or hire staff) which will help to gather data again, etc. Conceived in this way, the research process can be depicted as a repetitive cycle in which conversions take place between money, data, prestige, credentials, problem areas, argument, papers, and so on. In Section 4.5 we analyze the role of practical applications in the credibility cycle of three fields of Dutch chemistry.

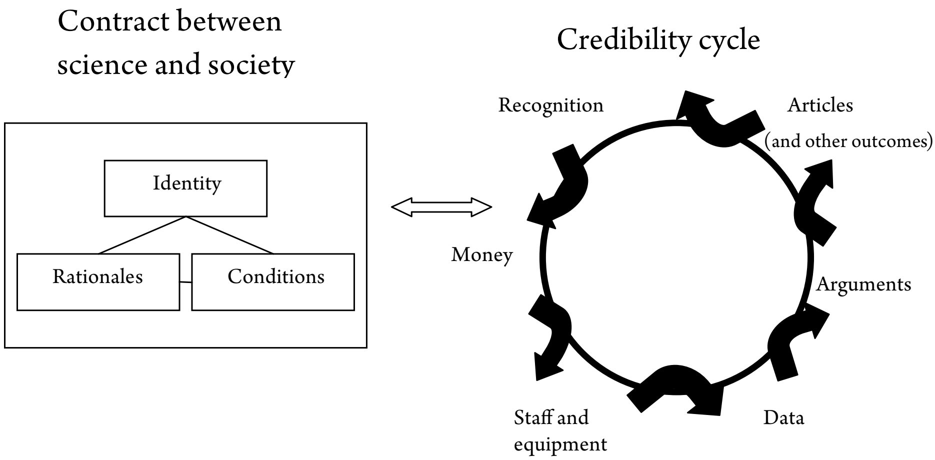

Figure 4.1 Our framework for studying the changing relevance of academic chemistry (based on Chapter 3 (Hessels et al. 2009))

4.3 Methods: a case study of chemistry in the Netherlands

For our exploration of the effects of institutional changes on academic research practices, we use a set of case studies of three chemical fields in The Netherlands[63]. Investigating the potential tension between pressures for academic publications and pressures for practical impact requires a discipline with both a strong publication tradition and possibilities to turn research outcomes into practical applications. Chemistry fulfills both conditions well. In Dutch chemistry there is a long tradition of relationships between university researchers and companies (Rip 1997, Homburg 2003). There is an exceptionally cooperative relationship between universities and industry in this field, also in the form of collective lobbying for public money (A07, R06). Moreover, chemistry has a strong academic tradition, and Dutch chemists have an excellent reputation for their scientific publications (Moed & Hesselink 1996).

Still, connections with industry are not of the same intensity across all chemical fields. Because we aim to explore the diversity of science, our case studies deal with three fields with different relationships to industry and other societal stakeholders. Biochemistry is a relatively fundamental field and has relatively few interactions with societal organizations. Its main applications are in the medical domain and are mediated by medical researchers. Environmental chemistry contributes directly and indirectly to environmental policy. It also delivers knowledge and tools to industry and non-governmental organizations related to the risk assessment of industrial chemicals. Catalysis is strongly connected to chemical

63 These case studies have also been used as a brief empirical illustration in an earlier, more theoretical paper (Hessels et al., 2009). The current paper presents a more detailed analysis of the material, focusing in particular on the differences between the three fields.

industry. Its knowledge can help firms to enhance the efficiency of their production processes and to decrease their environmental impact.

Our analysis of the changing science-society contract of Dutch chemistry (Section 4) is based on the documents listed in Appendix A in combination with interviews with four scholarly experts[64] on Dutch chemistry, R&D officers of five chemical companies, and representatives of research council NWO, the association of Dutch chemical industry (VNCI), the Dutch chemical association (KNCV) and the department of science policy of the Ministry of Education, Culture and Science. The documents were collected based on prior knowledge of the authors, tips from interviewees, and the 'snowball method'. The selection includes governmental policy documents, reports and strategic plans of research councils, foresight studies, evaluations and other important publications about Dutch academic chemistry. The findings from these documents were triangulated in interviews with the experts and stakeholders mentioned above. In this paper we will refer to these documents using the abbreviations presented in the Appendix. The contract analysis is delimited to the period of 1975 until about 2005. The starting year of 1975 marks the beginning of governmental science policy in the Netherlands (M74), which is generally regarded as a landmark event in the growing societal demand for application oriented research.

For the credibility cycle analysis (section 4.5) we carried out semi-structured in-depth interviews with 20 academic researchers in catalysis, environmental chemistry and biochemistry. The respondents' ranks ranged from PhD-student to full professor and they were employed at five different universities in the Netherlands (see Table 4.1). We asked them questions about their current and past research activities, their personal motivation, and their experiences and strategies concerning funding acquisition, publishing, scientific reputation, and performance evaluations. Using NVivo (qualitative analysis software), the interview transcripts were coded in accordance with the different steps of the credibility cycle. In the interview analysis special attention was given to differences among the three scientific fields.

Table 4.1 Distribution of respondents over fields, universities and academic ranks

Catalysis (9) Biochemistry (6) Environmental chemistry (5)	University of Amsterdam (8) Utrecht University (6) VU University Amsterdam (3) Radboud University Nijmegen (1) Eindhoven University of Technology (1) Leiden University (1)	Full professor (6) Retired full professor (5) Associate professor (5) PhD-student (3) Post-doc researcher (1)

64 Scholars in the field of Science, Technology and Innovation Studies with expertise on chemistry: prof. dr. Ernst Homburg (Maastricht University), dr. Barend van der Meulen (University of Twente and Rathenau Institute), prof. dr. Arie Rip (University of Twente), prof. dr. Jan de Wit (Radboud University Nijmegen).

4.4 The changing societal contract of Dutch academic chemistry, 1975-2005

In the following we will empirically address our question about practical applications as a source of credibility in two parts: a historical analysis of the changing science-society contract and a sociological analysis of the position of practical applications in today's credibility cycle. This section will describe the way the content of the science-society contract has changed for chemistry. The main attention will go to the conditions, as this part of the contract is most directly connected to the resources of research practices. The conditions (as precipitated in the contract) concern the institutional environment of academic research, which shapes the conversions of the credibility cycle. Our analysis covers the period 1975-2005, but we will begin with a sketch of the situation in the 1950s and 1960s.

During the last decades the identity of academic chemistry has changed from basic research to the production of strategic knowledge. Although some professors already had strong ties with chemical companies in the 1950s and 1960s (Homburg & Palm 2004b), in these years direct contributions to practical applications were not regarded as the main task of academic chemists. Since the introduction of science policy in the 1970s and innovation policy in the 1980s, however, the government has increasingly expected academic chemistry to address societal needs and to produce applicable knowledge as well[65]. During the 1990s the idea of 'strategic research' has won ground, which concerns the development of fundamental insights in domains of high relevance for economy or society (Irvine & Martin 1984) The strategic identity of academic chemistry endures in the new millennium as it is compatible with the most recent innovation concepts, in which the university is seen as a supplier of basic knowledge which can be valorized by other actors in the innovation system.

In the rationales for funding academic chemistry the emphasis has shifted from education and cultural value to the need for innovation and sustainability. In the first postwar decades the two dominant rationales were the necessity of chemical research for the training of new R&D-workers and (less importantly) the cultural value of basic research (Homburg 2003, Hutter 2004). In the 1970s the wake of environmental awareness and the start of science policy together caused a shift in the rationales from industry's need of educated workers to society's need of chemical expertise in the wider sense (Rip & Boeker 1975). The budget cuts on basic research in industry in the 1980s increased the importance of the rationale for supporting academic chemistry related to its potential contribution to technological innovations[66] (De Wit et al. 2007, Van Helvoort 2005). In the 1990s the notion of sustainable develop-

65 In the first policy paper (M74), the primary mission of science policy is defined as enhancing the agreements of research agenda's with societal demands.

66 The first foresight study of chemistry (VS80), commissioned by the minister of science policy concludes that academic chemistry should define its research goals more sharply and that the contacts with industry deserve intensification.

ment became increasingly significant in rationales for funding (chemical) research[67]. Around the turn of the century, the funding of university research was increasingly framed as support for the national innovation system. In this perspective support for university researchers is accompanied with the expectation that they actively interact with other actors in the innovation system, and contribute to the process of "valorization", by writing patents or by starting spin-off companies.

Regarding the institutional conditions, during the 1950s and 1960s chemical scientists had a high degree of autonomy. The most important types of funding (the so-called first and second money stream) were distributed without any conditions attached, based on considerations of academic quality and reputation. The first money stream was direct funding from the government to universities. The second money stream was supplied by SON, the Dutch research council for chemical research, founded in 1956, whose missions were to stimulate and coordinate basic chemical research[68]. SON's resources were distributed by 'working communities', thematic research networks, based on considerations of innovativeness but with little steering power (Hutter 2004). As a third stream of funding a small proportion of all professors engaged in contract research for industry (Homburg 2003).

In the 1970s scientists were increasingly held accountable for their work. Although they were not yet directly affected by policy measures, chemical researchers needed to put more effort in explaining to society what they were doing. Governmental science policy explicitly aimed at enhancing the agreement of research agenda's with societal demands (M74). Initially this was attempted by simply facilitating the interactions of scientists with societal actors, but in subsequent decades substantial shares of science's unconditional support were transformed into earmarked funding[69].

From 1980 onwards a considerable change occurred in the funding of chemical science, shifting the emphasis towards applicability (van der Meulen & Rip 1998). This change was due to two intertwined developments. First, the government was faced with the need for budget cuts on science, entering into the era of 'steady state' funding (Ziman 1994). Faced with the need for budget cuts on scientific research, the chemical community proactively took several initiatives to advice about task division and priorities. A committee of the chemical section of the Dutch royal academy, installed in 1979, gave input to the governmental generic report on Task Division and Concentration in 1983. In 1983, science policy minister Deetman started the implementation of 'conditional funding', a system in which part of the first money stream was subject to selection based on criteria of scientific quality and societal significance (M84). The implementation of conditional funding was an occasion to both cut budgets and to increase

67 'For all new scientific and industrial activities on the field of chemistry the **strives for sustainability and the minimalisation of environmental pressure** have become important boundary conditions.' (original emphasis)(O95, p. 2). NWO's Strategy Note on Chemistry for 2002-2005 is even titled: 'Chemistry, Sustainable and Interwoven' (CW01)

68 The aims of SON are 'the enhancement of fundamental research at universities, colleges and other institutes in the area of chemistry in the broadest sense and the development of cooperation among researchers who carry out such scientific research.' (Hutter 2004).

69 By various policy instruments such as 'priority fields', National Research Programs' and 'conditional funding'.

governmental steering of research directions. Deetman explicitly mentioned that the connection between chemical research and societal needs should be improved (M83, M84). In contrast to other fields, in the assessment of chemical research proposals, 'social relevance' was used as an important criterion (Blume & Spaapen 1988). Moreover, informed by foresight studies (K82, VS80), an increasing share of the second money stream was dedicated to application oriented research. SON started a program for applied chemical research in 1980, together with the new technology foundation STW, the share of which in SON's total budget grew steadily to approximately 20% in 1995 (S95). In 1988 SON's mother organization ZWO was drastically reorganized into the new NWO, which resulted in increased funding for application oriented research (van der Meulen & Rip 1998, Kersten 1996).

Second, and most significantly, the third money stream, which was often application-oriented, grew fivefold (see Figure 4.2). During the 1980s chemical companies became more willing to sponsor academic research, due to their budget cuts on in-house basic research which made them more dependent on basic research conducted elsewhere (De Wit et al. 2007, Van Helvoort 2005) (K84). Also the ministry of Economic Affairs entered the scene and started the IOP-programs[70] to fund university research, in order to enhance the innovation capacity of the Netherlands.

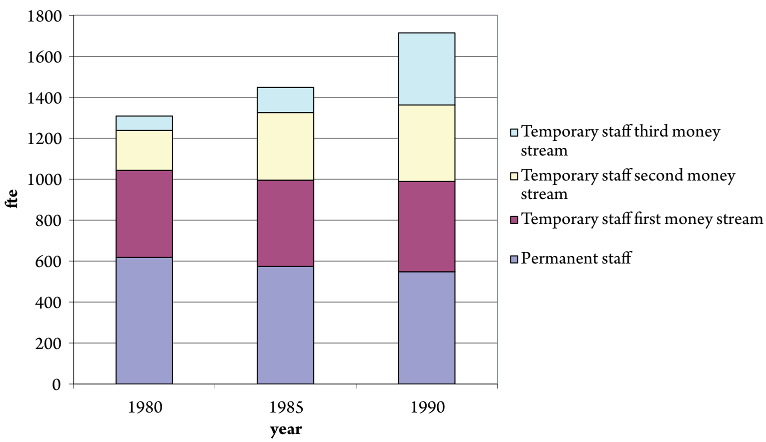

Figure 4.2 Growing importance of third money stream in the funding of chemical research at Dutch universities. Numbers represent temporary paid fte's in each money stream (AC91)

These developments continued in the 1990s. The second money stream continued to broaden its mission beyond basic research. The minister of science policy adopted the recommendation from the 1995 foresight study (O95) to strive for increasing the share of industrially steered chemical research from 50% to 75% (M97). The reorganization of NWO in 1998 implied the end of SON as a separate foundation, which continued as the area Chemical Sciences within NWO.

70 'Innovatie gerichte onderzoeksprogramma's' (Innovation oriented research programs), for example 'Membranes' (1983), 'Carbohydrates' (1985) and 'Catalysis' (1989).

Another significant change in the 1990s was the institutionalization of performance evaluations. Governmental policy makers, research councils and university managers who had all gained steering power in the funding allocation, developed a need for transparency regarding research outcomes. In 1996 the first nation-wide quality assessment of chemical science was conducted (V96), the second in 2002 (V02). As an input to these evaluations, the research groups had to carry out a self-assessment, dealing with the quality and impact of their work but also the societal value of their output. Due to the lack of guidelines for this issue, each group had its own style in presenting actual or potential applications of the knowledge it produced (van der Meulen & Rip 2000). As a result, the self-reported relevance seemed more a product of the group's creativity than of its actual relationships with societal stakeholders or contexts of application[71]. Also in the final examination by the 'Visitatiecommissie', the evaluators were free to choose to what extent they take into account societal relevance or applications, due to the lack of a strict protocol (van der Meulen & Rip 2000, van der Meulen 2008). In practice, they turned out to generally ignore this criterion and focus strongly on traditional scientific quality indicators, like the number of publications in high quality scientific journals. Relevance was one of the four major criteria used (the others being quality, productivity and viability), but this was mainly conceived as 'scientific relevance'. The societal or economic impact of the research was only assessed if this suits the group's (self-defined) mission[72].

After 2000 chemistry faced a further diversification of funding sources. Thanks to their continued growth, the European Framework Programmes have become a substantial source of income for academic chemists. Moreover, consortia-based funding emerged, large sums of governmental money supplied to collaborative programs of university scientists which are monitored by (industrial) user committees. Significant examples are the NanoNed program (2004) and the TTI 'Dutch Polymer Institute' (1997). Moreover, in 2002, the ACTS program was founded, for 'Advanced Chemical Technologies for Sustainability'[73]. This program is funded by several ministries and chemical companies, but it is carried out by NWO. Today its volume is about half of all second money stream funding available for chemical research in the Netherlands: in 2005, the program amounts to 11.3 Million Euros, compared with 14.5 Million for NWO-CW (CW06) and it anticipates further growth (A07).

71 E.g. the self-evaluation of chemical research in Utrecht (U01). Some groups refer to specific societal contributions (like more effective drugs or cleaner industrial processes) and actual interactions with medical practitioners or industrial researchers; others claim to support a more applied scientific research field, such as biology, which in turn supports medical or pharmaceutical practice.

72 This may change in the near future, as there are attempts to develop more holistic evaluation methods that include societal relevance (Spaapen et al. 2007)

73 The program was initially defined narrower as 'Advanced Catalytic Technologies for Sustainability', but soon the program was widened to a generic program for chemical technology.

Summary

Table 4.2 provides an overview of the changes in the science-society contract of academic chemistry, as has been discussed in this section. Four important changes can be identified in the conditions under which chemists work. First, the funding now available for university research provides room for considerable efforts in application oriented domains, while in the first postwar decades, there was general consensus that universities should restrict themselves to 'pure science'. Second, the interactions with industry have become more intensive. Because the main products of university research are not anymore people (only), but also knowledge, industrial steering of the content of research has become justified. Since the rise of innovation policy, industrial representatives have a say in the design of most major chemical research programs. Third, universities attempt to play an active role in the valorization of research outcomes. Merely providing knowledge is not considered sufficient anymore. The national government actively stimulates academic patenting and the creation of spin-off companies. Although there is little proof of actual success, all Dutch universities provide facilities to support researchers in translating their knowledge into commercial activities. Fourth, systematic performance evaluations have become a powerful institution governing academic research. Every research group is subject to regular assessments, which tend to focus most strongly on bibliometric quality indicators.

Table 4.2 Overview of the changing contract for academic chemistry (copy of Table 3.1). + signs indicate that these elements complement rather than replace existing elements[a].

	Summary of identity	Most dominant rationales	Most important conditions
1950s and 1960s	Basic research	Education Cultural value	Autonomy Unconditional funding SON communities
1970s	+ Useful knowledge	+ Problem solving potential	+ Social accountability
1980s	Applicable knowledge	Technological innovation	+ Conditional funding + Application oriented funding (STW, IOP, contract research) + Foresight + Scarcity of resources Reorganization NWO
1990s	Strategic knowledge	+ Sustainable development	Further prioritization + Performance assessments
2000+	Strategic knowledge	+ Innovation system	+ Consortia (ACTS, TTI, BSIK) + European FPs

[a] ACTS: Advanced Chemical Technologies for Sustainability; BSIK: Besluit Subsidies Investeringen Kennisinfrastructuur Programs; IOP: Innovation Oriented Program; NWO: Dutch organization for Scientific Research; TTI: Technological Top Institutes; SON: Chemical Research Netherlands; STW: Technology Foundation

4.5 Academic research and practical applications: a credibility cycle analysis

How do the changes in the contract play out in the daily practice of academic researchers? Have contributions to practical applications become rewarding in terms of credibility? In this section we will closely analyze the six major steps of the credibility cycle, with special attention for the differences across the three fields of chemistry we have investigated.

4.5.1 From recognition to money

Section 4 has shown that the palette of available funding sources has changed dramatically.
The three fields we have studied use a variety of funding sources (see Table 4.3). Do (promised) practical applications help them to acquire funding?

Table 4.3 Overview of the most significant funding sources in the three sub-disciplines[a]

	Biochemistry	Catalysis	Environmental chemistry
funding sources	NWO EU FPs	NWO, STW EU FPs industry consortia entrepreneurship	NWO, STW EU FPs government industry NGOs

[a] Based on our interviews.

In catalysis, promising a contribution to practical applications is a requirement for most types of funding. The procedures for acquiring funding vary. To get money from an individual firm, very short descriptions can suffice to convince of the quality and relevance of the proposed project. Research councils and hybrid consortia, however, demand extensive proposals addressing a number of predefined issues like innovativeness, scientific relevance, societal impact, research methods, expected outcomes and deliverables. Except for NWO and the 'National Research School Combination Catalysis', all funding sources require that their research is both relevant for industry and excellent in scientific terms. Several of our interviewees in catalysis have founded small firms based on patented inventions. Currently these are still too young to make profit, but in the future they may serve as sources of research funding. In case such a company is acquired by a larger firm, a significant sum will flow to the research department, to be spent freely on research activities. The increased industrial influence on the research agenda generates incentives to pay closer attention to possible practical applications, but it does not imply a shift from basic to applied research. Because catalysis is a technology to make chemical processes more efficient, it carries the potential of making chemical industry more sustainable. Thanks to this promise and thanks to the good reputation of Dutch catalysis, both public and private funding sources are willing to support research on fundamental questions:

> *'And it is of course the case, that they seldom really let us do a research project in order to get that specific catalyst after four year for sure. They are rather interested in having you work in a particular area of research, of which we see together: this is promising. And then the innovations come automatically and if they really want to apply it, they pick it up themselves.' (full professor, catalysis)*

For environmental researchers, however, there are significant differences among the various possible sources of funding. The national research council NWO and the European Framework Programmes (FPs), on the one hand, strongly focus on academic quality and reputation. Governmental bodies, industry and NGOs, on the other hand, have specific questions that can be answered by applied research. These funders tend to look for the researcher who can answer them with the best price-quality ratio, creating a competition between academic researchers and (semi-)commercial research institutes. The projects for industry and other application oriented funding sources are often pretty short. They can threaten the continuity in research activities, they involve little basic research and they are not suitable for researchers to get a PhD-degree. However, environmental chemists need to do them to remain financially healthy, and they pay better than research councils[74].

In biochemistry, scientists only use funding from sources oriented to basic research. The various grants and programs of NWO are most significant; next to that the European FPs are gaining importance. Acquiring money from industry seems hardly possible because *'the time horizon of companies, also of big companies, has become very limited'*[75]*. Academic quality of research proposals and of research groups are still the most important criteria to get money from NWO and FPs. The rise of thematic priority programs does not seem to have serious consequences. In many programs referring to potential practical applications enhances the chances of success, but this can usually be dealt with by rather loose and unrestrictive statements. One respondent explained that he simply looks around what thematic programs are available and then think up a link with his own competences and existing research plans:*

> *'You try to make your expertise fit, we have become pretty good at that. You write it down in such a way that it fits the program.' (full professor, biochemistry)*[76]

74 Researchers aim for a diverse range of funding sources, in order not to be dependent on one client (interview 11).

75 Full professor, biochemistry.

76 This reasoning is in line with Morris' observation that biologists tend to adapt their proposals to fit the priorities and initiatives of funding bodies (Morris 2000, p. 433).

Overall, the changes in the funding structures do not have strong implications for the research directions biochemical scientists choose.

Another way of turning practical applications into money is consultancy. In catalytic and in environmental chemistry it is common to conduct small consultancy projects for public-sector or private organizations beside the (bigger) research projects. Consultancy is not common in biochemistry.

It must also be noted that the scores on official performance evaluations increasingly contribute to the funding available to a research group. University managers take them into account when faced with the need for budget cuts[77]. Also in the review process of NWO proposals the scores are used as a quality indicator. Groups with a good score will advertise it when attracting contract research as well.

To conclude, in all fields an increasing share of all funding demands researchers to articulate possible practical applications in industry or society of the proposed project. In catalysis and environmental chemistry, however, the promises about practical applications tend to be much more explicit and specific than in biochemistry.

4.5.2 From money to equipment and staff

Once a scientist has acquired research funding, he or she can use it to buy equipment or to hire one or more people to carry out the work. What criteria are used in the selection of candidates?[78] Do (realized or promised) practical applications play a role? Asked what characteristics they look at when selecting candidates for academic positions, senior researchers mention research quality, abilities to attract funding and management and collaboration skills. Publication lists stand out as the most important quality indicator. This is confirmed by all respondents, both juniors and seniors, for example:

> *'When we hire new staff, their publication list is the most important criterion, possibly in combination with a Hirsch-index or something similar. The same goes for contract extension and for promotion to associate or full professor.' (full professor, environmental chemistry)*
> *'You've got to publish a lot. Imagine I have a post-doc position and there are 60 candidates. First I look at the people who publish 10 articles a year. Why? Because they will also publish 10 articles for me.' (associate professor, catalysis)*

PhD-students do not believe that practical applications of their research will enhance their prospects for an academic career. However, in catalysis some professors report that they do look at the number of patents a candidate has developed, or the amount of interest that industry shows for his research (in

77 This is the experience of most senior researchers we have interviewed.
78 Purchasing equipment has not been investigated in this study.

terms of industrial funding). But practical applications are not crucial for every scientist. As one professor claims, it is simply important that someone is part of an international community:

> *'(...) this international community can consist of pure scientists, but it can also consist of a mix of pure scientists and people from industry. And this balance, for some it lies all the way to the fundamental scientists and on the other hand it can also lie in the direction of applications. And I think both are just as valuable.' (retired professor, catalysis)*

Another quality taken into account is the candidate's proven acquisition skills:

> *'Today it is important that you have acquired a European project once, or at least paid a significant contribution to it. That you show that you can do that as well. But I would say that publications are really number 1 and this is a good number 2.' (full professor, environmental chemistry)*

For this reason, in catalysis researchers at various levels expect that contacts with industry will be valuable in academic job applications.

To conclude, in the selection of candidates for academic positions, academic criteria rule. In catalysis, career perspectives may be slightly enhanced by industrially relevant work or commercial activities, but one's patents are still far less helpful than one's publications and citations.

4.5.3 From equipment and staff to data

What is the role of practical applications in the production of data?

In general, the collection of data takes place within the boundaries of projects that have been defined in proposals or contracts. In practice, however, such descriptions function only as rough guidelines for the actual research. On the laboratory floor, the boundaries between projects can be pretty fuzzy.

> *'To the outside there is always a very clear line of demarcation. But within the group that demarcation is much vaguer.' (full professor, catalysis)*

Not all projects offer the same degree of operational autonomy. A personal grant from NWO, for example, is qualified as 'reasonably free money'[79], and is therefore highly appreciated. The same goes for university funding. Money from industry or other third parties typically involve more communication

79 Full professor, catalysis.

with the funding source, but the degree to which this decrease the flexibility for the researchers to deviate from the original plans varies across fields.

In catalysis, firms providing (co-)funding obviously aspire to benefit from it, but they do not steer the experimental work in detail. In principle, they do not predefine all details of the research to be conducted, but only the type of system, class of compounds, or type of reactions to be studied. Companies hope to benefit from obtaining more background knowledge in the field they are working in, which can serve as a source of inspiration for more applied innovation projects conducted in-house. Industrially (co-)funded projects usually have a supervising committee which receives an update about the progress about three to four times a year and which can suggest particular directions, but these are only followed if this does not hinder the academic development of the PhD-student involved. According to our respondents few disagreements on this point occur. In cases in which companies steer a project in specific directions this often has little implications for the academic question that is addressed. The same catalytic mechanism can be studied using different substances.

The situation is different in environmental chemistry. Here the projects for industry, the government or NGOs tend to be short and serve specific goals. In this domain specific actors often have a strong stake in particular outcomes, which can complicate the collection of data[80]. Researchers have an interest in having the assent of all organizations involved (government, industry and interest groups), because this increases the impact of their outcomes. However, this may challenge their independence, as some parties may try to influence the outcomes to their own benefit.

Most biochemists we have interviewed do not have any contacts with possible users of their knowledge. One professor regularly meets with medical researchers in the context of a research project with medical relevance, but the others do not report any interaction outside their own field influencing their work.

To conclude, during data collection biochemists are not concerned with practical applications at all, but researchers in catalysis and environmental chemistry tend to interact frequently with industry or other users that (co-)fund the research. In environmental chemistry such interactions sometimes disturb the data collection; in catalysis this happens less, and they are often perceived as a source of inspiration and motivation.

4.5.4 From data to arguments

Although the conversion of data into arguments is relatively straightforward in chemistry, it is still an active step with significant degrees of freedom. To what extent do practical applications influence this

80 An associate professor told us that in a project about the risks of a particular class of compounds his work was complicated, because of the sensitivity of the required information.

process? For academic chemists the main consideration in this step seems to contribute to scientific debates. Researchers use their data to construct claims that fit in a particular scientific discourse in which they are participating. The arguments they develop are their tool for positioning themselves within a particular research community (Latour & Woolgar 1986).

In catalysis we found no evidence of the influence of practical applications on the arguments researchers produce, apart from an emphasis on either environmental or economic benefits. In the other two fields, however, the funding source of the research does influence the production of arguments. In biochemistry arising practical applications can steer the arguments in a particular direction. The results of biochemical experiments paid by a patient organization need not be of a different kind than the ones from experiments funded by NWO, but the former are more likely to be converted into medical arguments while the latter may be used only to contribute to more fundamental biochemical debates[81]. In the case of contract research, the sponsor (or 'client') may also influence the (types of) arguments produced. The environmental chemists in our sample report that they sometimes have difficulties defending their academic 'objectivity' against unwanted interference of the companies or environmental agencies that have a stake in the research. Industry often hopes the data are turned into good news about the safety of chemical substances.

> *'Yes, then it is difficult to act. Also when deriving conclusions.' (associate professor, environmental chemistry)*

But researchers try to keep their independence.

> *'And maybe we should not be concerned too much with what the client wants.'(full professor, environmental chemistry)*

To conclude, in most cases researchers are relatively autonomous in developing their data into arguments, but in applied research projects the funding party sometimes succeeds to influence the conversion of data to arguments, thereby harming the objective position of academic scientists.

4.5.5 From arguments to articles (and other outcomes)

Publishing in scientific journals is of vital importance in all three fields of chemistry. Many scientists try to get their work published in the best journals possible, which is often defined as the one with the highest impact factor.

81 This is visible in the research of a full professor in biochemistry.

'Yes, it is important, for two reasons. First of course one wants to make ones findings publically known. This is a way to receive recognition of your peers. Second it is also dire necessity, in order to secure the continuity of funding. Because if you do not have publications... it is the way for the outside world to assess you.' (associate professor, biochemistry)

'I think it is the only way to show what you have done. And I think that if something is not publishable in a scientific journal, it is not worth much.' (PhD-student, biochemistry)

In principle, application oriented research can also be published. Both in environmental chemistry and in catalysis, scientists publish the results of research issued by industry or other users in prestigious journals, too. However, this is not always as easy as with research funded by the first or second money stream. Companies sponsoring catalytic research are protective with respect to commercially relevant outcomes. Research contracts usually specify a period in which a company has exclusive access to the results to explore the feasibility of the developed technology and to consider applying for a patent before the academics are allowed to make them public. This hardly ever leads to complete bans on publications, but it does complicate early stage communication such as poster-presentations. There are also exceptional cases in which a research project is completely secret and no publications are allowed at all.

In environmental chemistry, the small size of many assigned projects complicates the publication of academic papers.

'Yes, then you almost always face the situation that it is just too little to make a good scientific publication about it.' (associate professor, environmental chemistry)

Moreover, writing a paper beside the project report requires extra work which can not be accommodated within the projects themselves.

In their evaluation of manuscripts, journal editors and reviewers hardly assess the (possible) practical applications of the research. In all three fields, the main criteria in the selection of papers are the novelty, accuracy and the scientific relevance of the research. In environmental chemistry, it may help if one manages to link one's research to an important societal issue like global warming.

'Climate change is of course very hot. So in the piece we are currently working on, we try to steer it a bit in that direction. So that the result is useful. And sustainability. That you try to associate with the fashion terms. [...] On the work itself it does not really have an influence. But it does on your introduction, how you stage your story, sketch the framework, there you includes a couple of words.' (PhD-student, environmental chemistry)

In other fields, it is matter of personal style whether one refers to the societal context or the possible applications of the research. Both in biochemistry and in catalysis some researchers make an effort to do this, but this does not particularly help them to get their papers published.

Although scientific publications are the most important type of research outcomes, chemists deliver other products, too. In environmental chemistry it is common to write scientific reports for the organizations commissioning the research. Catalytic chemists are frequently involved in patent applications. Some senior researchers have contributed to tens of patents. PhD research commissioned by industry often leads to patents, for which the companies sometimes even pay a bonus. In other cases academics write patents themselves and start a company to make a profit of it, that partly be used as research funding. Although less common, this phenomenon starts occurring in biochemistry, too[82].

In conclusion, journal publications remain the most important form of output in academic chemistry, but under the new science-society contract researchers also produce patents and write scientific reports. In environmental chemistry application oriented research is more difficult to publish due to the small project size. In catalysis practical applications only create a delay, but do not inhibit the eventual publishing of results. Anyway, practical applications never help to get one's work published as the selection of journal manuscripts is based solely on academic criteria.

4.5.6 From articles (and other outcomes) to recognition

Do practical applications contribute to an academic reputation? One can distinguish a formal and an informal component of recognition. The formal component is the result of official research group evaluations and individual performance interviews. Someone's informal recognition is based on the assessment of colleagues of one's qualities, expressed in conference contributions, (informal) discussions, and publications. Both types of recognition mutually influence each other. A good score on official evaluations will be known by one's colleagues and taken into account in their informal recognition of one's work. In turn, informal recognition also contributes to the score on formal performance evaluations.

Within one's own small subfield one can earn recognition for the content of one's papers and lectures. These are valued for their innovative content and for being published in prestigious journals. Beyond one's own specialty one's reputation is more based on quantitative indicators like publication and citation scores and formal performance evaluations.

> *'[...] in practice we measure impact-factors. And these we add up. Calculations are being done up to three digits.' (full professor, biochemistry)*

82 The interviewee involved (associate professor in biochemistry) claims that he was stimulated by NWO policy.

Since research grants have become scarce, the amount of funding attracted has also become a contributing factor to one's reputation. In environmental chemistry especially the more academic grants, like the ones from research council NWO are appreciated, as they are generally regarded more prestigious than funds from the 'third money stream'.

Practical applications of academic chemistry contribute little to recognition. Some catalytic researchers have respect for contributions to industrial innovations, especially in engineering subfields. However, these contributions often remain invisible to academic colleagues. The number of patents scientists hold does not contribute much to their academic reputation; they can even have a negative effect. One professor argued that his long list of patents tends to distract people from his academic success and makes them forget that he also has an impressive list of scientific publications. In biochemistry societal contributions do not play any role in getting academic recognition. Promises about applications (most often in the medical domain) can contribute only indirectly to one's reputation if they help to start big research programs or consortia. Practical applications of environmental research also contribute little to one's academic reputation[83].

Beside the issues discussed so far, some other aspects that may contribute to informal recognition are management skills, collaborations with well-respected scientists, educational work, and presentation skills. However, our interviews indicate that these are all of far less importance than journal publications.

With regard to formal recognition, all scientists have a performance interview with their direct boss once a year. Practical applications receive very little attention[84]. Research productivity is the most important issue on the agenda. In environmental chemistry, also non-academic output, the so-called 'grey' publications, is taken into account. In catalysis and biochemistry these are not regarded as output. For senior researchers contributions to education, funding acquisition and other management tasks are also discussed.

The scores on performance assessments both count for internal university policy and for the acquisition of additional funding in the second and third money stream. In the official protocol one out of the four main criteria is 'relevance' (V03). In practice, however, the evaluation committee has the freedom to choose its own interpretation of this criterion. In biochemistry, the committees typically define it as *scientific* relevance, because this is considered most appropriate in a field of basic research. In catalysis and environmental chemistry, however, the reports do not clearly define their concept of relevance and do not express the extent to which it concerns societal relevance as well (V96, U01, V02).

83 Two out of five respondents perceive them not to contribute at all. The three others claim that applications can make a contribution to one's reputation, but this is complementary to one's scientific impact. 'Everything with climate change of course is an example. If you find important new things there, you will receive many invitations to tell about it somewhere, both at scientific and at more societally organized conferences. But, on the other hand, you are invited just as often for scientific conferences if you simply have produced sound research and you have shown that you can give a nice talk about it.' (Associate professor, environmental chemistry).

84 One professor reports that his boss appreciates his publications in popular media and some of his additional functions because they contribute to the visibility and the impact of his institute.

To conclude, in none of the three fields practical applications significantly contribute to recognition. Recognition is mainly based on academic publications. Beside informal recognition, formal processes like performance interviews and performance assessment also contribute to one's reputation, but all focus on the same quality indicators: publications and citations.

4.6. Conclusion: the importance of stakeholders

Our analysis has shown that the relationship between academic chemistry and society has undergone some major changes. Under the current societal contract, academic chemists are expected to deliver strategic knowledge and to participate actively in the valorization of research outcomes. Due to the changing demands of both public and from private funding sources, academic researchers have lost a considerable degree of autonomy in choosing research agendas. At the same time, however, they are subjected to practices of quality control which hardly reward applicable knowledge, spin-offs or patents, but mainly publications in academic journals. In the performance assessments of 1996 and 2002 the societal dimensions of academic chemistry receive little attention. Although installed to increase the social accountability of scientists, evaluations merely enhance the need for peer recognition. Bibliometric quality indicators strengthen the pressure to publish in scientific journals and enhance the 'publish or perish' norm (Weingart 2005, Wouters 1997). As a result there is a potential contradiction between research agendas that are fruitful with regard to funding acquisition and research agendas that promise to yield peer recognition and high scores on formal evaluations.

Have practical applications become a source of credibility? We found considerable differences among the three fields under study. In catalysis, practical applications constitute a rich source of credibility. Promising a contribution to industrial processes is a necessary requirement for acquiring research funding. The intensive interactions with firms during the research process stimulate rather than inhibit data collection and publications. Moreover, commercially viable outcomes can be turned into new research funding by selling patents or exploiting them in a spin-off firm. In biochemistry, practical applications do not help a lot in gaining credibility. The available funding sources provide incentives to articulate possible practical applications, but this has a modest effect, as subtle cosmetic adaptations of existing research plans usually suffice. Due to the rise of bibliometric performance evaluations, biochemists experience a much stronger pressure to publish than to contribute to practical applications. For environmental chemists, practical applications have a positive effect on some parts of the credibility cycle, but a negative on other parts. The funding structure provides strong incentives for application-oriented research and to contribute even more directly to practical solutions than before. However, relatively short, application-oriented projects are not most fruitful in terms of scientific publications, evaluation scores and academic recognition.

A partial explanation of the different effects of the changing contract on these three fields, can be found in their socio-organizational characteristics. In environmental chemistry the task uncertainty (Whitley 2000) is probably higher than in the other two fields. Catalysis and biochemistry have developed a convergent research agenda, in which there is considerable agreement about problem definitions and theoretical goals. In the young field of environmental chemistry, however, there are less standardized procedures and less certainty about intellectual priorities. The boundaries with neighboring fields of environmental science (toxicology, soil science, ecology) are weak. In spite of the small size of environmental chemistry, its research activities are very diverse. In the terminology of Becher and Trowler (2001), environmental chemistry can be characterized as a 'rural' field, with a low people-to-problems-ratio and with no sharply demarcated or delineated problems. Catalysis and biochemistry are more 'urban', in the sense that there are many researchers working on a narrow area of study, and there is strong mutual competition for priority of discoveries. The weaker competition and higher task uncertainty make it more difficult for environmental chemists to publish in prestigious journals. Moreover, they make this field more sensitive to external steering of its research agenda.

However, the difference between biochemistry and catalysis can not easily be explained by their social organization. Both fields have high mutual dependence and low task uncertainty (Whitley 2000), they are both urban, and composed of 'tightly knit' communities (Becher & Trowler 2001). The crucial difference between the two seems to be the type of stakeholders they have outside university. Catalysis, on the one hand, has a strong relationship with a homogeneous set of 'upstream end-users' (Lyall et al. 2004), namely chemical firms (see Table 4.4). Due to the high investments these firms make in their industrial facilities, they have a long term perspective. They make enough economic profit to be able to make substantial investments in relatively fundamental research. Moreover the relationship between chemical industry and academic catalysis is characterized by high cognitive and social proximity (Tijssen & Korevaar 1997), which facilitates the knowledge transfer and alignment of research activities.

Biochemistry, on the other hand, hardly has any 'upstream end-users' (Lyall et al. 2004). As its main applications are in the medical domain, its main stakeholders are health care practitioners and patients. But these both figure more as 'downstream end-users', as they do not directly interact with biochemical researchers and they do not have any formal channels to influence academic research activities. Biochemical researchers only interact with 'collaborators' and 'intermediaries' that represent the stakes of these downstream users. These categories of stakeholders, however, provide little funding for academic biochemistry, compared to the more fundamental research councils. The lack of upstream users explains that practical applications do not form an important source of credibility for academic biochemists.

Environmental chemistry has a heterogeneous set of upstream end-users. Environmental policy makers, firms and environmental NGOs all have a stake in this research, and all provide a share of the research funding. However, the time horizon of these users is relatively short. The knowledge needs of firms and NGOs with respect to environmental chemistry are usually related to short term problems, dealing with the regulation of specific chemicals. Policy makers typically have a longer term perspective, as they invest in generic models for the regulation of different classes of chemical compounds. Still, their time horizon is much shorter than that of the companies taking an interest in catalytic research. Although environmental policy itself could benefit from a perspective up to several decades, in practice the time horizon of policy makers is often limited by election cycles.

Table 4.4 The different categories of end-users of each field

	Catalysis	Biochemistry	Environmental chemistry
Upstream end-users	Industry	-	Policy makers Industry NGOs
Collaborators	(Other catalytic chemists)	Medical researchers	Other (more applied) environmental scientists
Intermediaries	Research councils	Research councils Medical charities Patient organizations	Research councils
Downstream end-users	Industry	Health care practitioners Patients	Policy makers Industry NGOs

To conclude, in this paper we have explored the interplay between shifting funding sources and the rise of performance evaluations in the case of Dutch chemistry. The degree to which these developments stimulate scientists to contribute to practical applications turned out to strongly differ across fields. In a field with powerful upstream end-users who can afford investments in fundamental research, contributions to practical applications have become a source of credibility, but in fields lacking such stakeholders, researchers can hardly earn credibility by involvement with practical applications.

Appendix A (chapter 4). Documents studied

Abbreviation	Publisher / Author[a]	Year	Title	City
S73-S95	SON	1973-1995	Jaarverslag	Den Haag
M74	Ministry of Science and Education	1974	Nota Wetenschapsbeleid	Den Haag
M76-M97	Ministry of Science and Education	1976-1997	Wetenschapsbudget	Den Haag
AR79	Academische Raad	1979	Beleidsnota Universitair Onderzoek	Den Haag
K80	KNCV	1980	Tien Researchdoelen	
VS80	Verkenningscommissie Scheikunde	1980	Chemie, nu en straks: een verkenning van het door de overheid gefinancierde chemisch onderzoek in Nederland	Den Haag
K84	KNCV & VNCI	1984	Toekomstig Chemisch Onderzoek: Een uitwerking van het rapport Wagner I voor de Chemie	
AC91	ACC-evaluatiecommissie	1991	Evaluatie van de universitaire chemie in de jaren '80	Amsterdam
S91, S93, S94, S96	SON	1991, 1993, 1994, 1996	Meerjarenplan	Den Haag
K94	KNCV & VNCI	1994	Toekomstig chemisch onderzoek: Universitair fundament voor industriële meerwaarde	
O95	OCV	1995	Chemie in Perspectief: een verkenning van vraag en aanbod in het chemisch onderzoek	Amsterdam
V96	VSNU	1996	Quality Assessment of Research: Chemistry: past performances and future perspectives	
M00	Ministry of Education, Culture and Science	2000	Wie oogsten wil, moet zaaien: Wetenschapsbudget 2000	Den Haag
N00	NVBMB	2000	Verder met Biochemie en Moleculaire Biologie: Beleid voor een Vitale Wetenschap,	Nijmegen
U01	Chemistry - Utrecht University	2001	Assessment of research quality	Utrecht

CW01	NWO-CW	2001	Strategienota 2002-2005: Chemie, Duurzaam en Verweven	Den Haag
V02	VSNU	2002	Assessment of Research Quality: Chemistry and Chemical Engeneering.	
VI03	VNCI	2003	Vijfentachtig jaar VNCI in vogelvlucht	Leidschendam
V03	VSNU, NWO and KNAW	2003	Standard Evaluation Protocol 2003-2009 for Public Research Organisations.	
CW03-CW05	NWO-CW	2003-2005	CW-Jaarverslag	Den Haag
M04	Ministry of Education, Culture and Science	2004	Focus op Excellentie en meer waarde: Wetenschapsbudget 2004	Den Haag
N04	VNO-NCW, VSNU & NFU	2004	Beschermde kennis is bruikbare kennis: Innovation Charter bedrijfsleven en kennisinstellingen.	
V05	VSNU	2005	Onderzoek van Waarde: Activiteiten van Universiteiten gericht op Kennisvalorisatie,	Den Haag
CW06	NWO-CW and ACTS	2006	Chemie@NWO: Naar een Environment of Excellence; Strategische koers 2007-2010	Den Haag
R06	Regiegroep Chemie	2006	Businessplan: Sleutelgebied Chemie zorgt voor groei,	Leidschendam
A07	ACTS	2007	ACTS Means Business: Second Phase ACTS Plan 2007-2011	Den Haag
M07	Ministry of Education, Culture and Science	2007	Voortgangsrapportage Wetenschapsbeleid	Den Haag

[a] ACC: Academische Commissie voor de Chemie (The committee for chemistry of the KNAW)
ACTS: Advanced Chemical Technologies for Sustainability (a research program)
CW: Chemische Wetenschappen (division for chemical sciences)
KNAW: Koninklijke Nederlandse Akademie voor de Wetenschappen (royal Dutch academy)
KNCV: Koninklijke Nederlandse Chemische Vereniging (royal Dutch chemical association)
NCBMB: Nederlandse Vereniging voor Biochemie en Moleculaire Biologie (Dutch association for biochemistry and molecular biology)
NFU: Nederlandse Federatie van Universitair Medische Centra (federation of Dutch academic medical centres)
NWO: Nederlandse organisatie voor Wetenschappelijk Onderzoek (Dutch organization for scientific research)
OCV: Overlegcommissie Verkenningen (committee for foresight studies)
SON: Scheikundig Onderzoek Nederland (research council for chemistry in the Netherlands)
VNCI: Vereniging voor Nederlandse Chemische Industrie (association of the Dutch chemical industry)
VNO-NCW: Verbond van Nederlandse Ondernemingen en het Nederlands Christelijk Werkgeversverbond (Dutch employers' association)
VSNU: Vereniging voor Samenwerkende Nederlandse Universiteiten (association of Dutch universities)

Chapter 5.

Multidisciplinary collaborations in toxicology and paleo-ecology: Equal means to different ends[85]

Abstract

In this chapter we describe and explain how the relationship between biology and society has changed in the Netherlands over the past 30 years, and how this has provided pressures and incentives for multidisciplinarity in two different fields. In paleo-ecology we observed a rise in multidisciplinary research collaborations as a response to outside pressures to do more 'relevant' research, particularly in the area of climate change. In toxicology we observed the opposite: multidisciplinarity is used to strengthen the fundamental research on physiological mechanisms of toxicity. We conclude that multidisciplinary research collaborations can serve various ends, ranging from enhancing the practical value of a fundamental field to strengthening the fundamental basis of an applied field.

5.1 Introduction

Across the whole science system the importance of collaborations between researchers with different disciplinary backgrounds is growing (Hicks & Katz 1996, Morillo et al. 2003, Hackett 2005). This development is regarded as one of the most profound changes currently affecting academic research systems (Gibbons et al. 1994 , see also Chapter 2). The rise of multidisciplinarity is often considered beneficial because it opens up new fundamental research lines, because it may help science to more adequately address society's knowledge demands and because it may enhance the economic impact of science. For these reasons, increasing interactions between scientific disciplines are a key feature in influential

85 Forthcoming as: Hessels, Laurens K., Stefan de Jong & Harro van Lente. 2010. 'Multidisciplinary collaborations in toxicology and paleo-ecology: Equal means to different ends', in J.N. Parker, N. Vermeulen & B. Penders (eds), Collaboration in the New Life Sciences, Ashgate, 37-62.

visions on the future of academic research like Mode 2 knowledge production, Post-Normal Science and the Third Generation University (Nowotny et al. 2001, Funtowicz & Ravetz 1993, Wissema 2009). However, the factors supporting the rise of multidisciplinarity are poorly understood. This chapter aims to alleviate this by a detailed analysis of case studies of two life sciences, namely toxicology and paleo-ecology.

One can distinguish between factors which are internal to the disciplines involved, and factors which arise externally to them. A well-known example of an internal factor that incites multidisciplinary collaboration is the incorporation of molecular approaches in the life sciences, which has enhanced the need to combine expertises and skills from different disciplines (Cassidy & Radda 2005). Next to internal scientific dynamics, external changes in the societal context of academic research also stimulate multidisciplinary interactions. According to Gibbons et al. (1994), for instance, both public and private actors demand the production of more 'relevant' knowledge, which may require multidisciplinary approaches. Others point to policy instruments on the national and international level that directly stimulate inter- and multidisciplinary research (Lepori et al. 2007, Bruce et al. 2004).

In this chapter we focus on these external factors in particular. The question we address, thus, is how multidisciplinary collaborations in academic biological research have been affected by societal changes. We are not the first to raise such questions. However, earlier accounts of the influence of contextual dynamics on the rise of multidisciplinary collaborations are often of a generic nature, paying little attention to the different ways in which specific scientific fields are affected (e.g. Gibbons et al. 1994). The current chapter works to overcome this limitation by comparing two biological fields in their specific societal context. We empirically limited our analysis to the Netherlands, and to the period between 1975 and 2005. We found interesting differences between these two fields *within* biology. This suggests that any generalized claim about the changing relationship between biology and society is severely vulnerable. In the following section (5.2) we present the theoretical framework that guides our analysis, introduce multidisciplinary research collaborations and briefly review literature on the topic. A key point is the notion of a contract between science and society to understand the changing societal context of the Dutch life sciences. Section 5.3 discusses the methods and case-selection. Section 5.4 presents our empirical analysis of the changing 'contract' between biology and society; Section 5.5 addresses the rise of multidisciplinarity in toxicology and paleo-ecology. We conclude with a comparison of the outcomes of our two case studies and a general reflection on societal influences on multidisciplinary research collaborations (section 5.6).

5.2 Theoretical framework

A scientific collaboration can be defined as 'the work of teams of scientists with shared goals, such as formulating or testing particular empirical hypotheses, and with shared products, such as co-authored papers' (Griesemer & Gerson 1993). *Multidisciplinary* research collaborations involve individuals representing different scientific disciplines. Multidisciplinarity belongs to the same family of concepts as inter- and trans-disciplinarity. In multidisciplinary collaborations, each discipline remains autonomous, while the participating disciplines each contribute to the solution of a common problem. The collaboration does not change existing disciplinary structures (Jantsch 1972). Interdisciplinarity implies closer interaction than multidisciplinarity. Interdisciplinary collaborators explicitly formulate a 'discipline-transcending terminology or a common methodology' (Gibbons et al. 1994). Transdisciplinary research goes even further; it requires a common theoretical understanding and is accompanied by interpenetration of disciplinary epistemologies (Lawrence & Després 2004).

This definition of multidisciplinarity calls for clarification in two directions. First, it makes multidisciplinarity a relative concept, depending on one's definition of a discipline. In this paper we speak of biology (or the life sciences) as a *discipline* and of paleo-ecology and toxicology as *fields*. We regard interactions within fields, which may include various specialties, as regular collaborations, but speak of interactions among different fields or different disciplines as multidisciplinary collaborations. A second aspect that requires clarification is how scientists relate to their discipline or field. In this chapter, one's disciplinary background refers to one's current work rather than to one's education. Education strongly influences the direction of one's research, but still researchers can switch and develop careers in other fields than those in which they were trained. An indicator to consider for a researcher's field could be his or her current affiliation. However, this can be deceptive as well. Bourke and Butler (Bourke & Butler 1998) have shown that a significant proportion of publications within a discipline originate from authors affiliated to institutes of a different disciplinary classification. Therefore, we consider a researcher's own *perception* of the field to which he or she belongs as the most reliable indicator.

Although interactions between scientific disciplines have occurred for centuries,[86] the relative occurrence of multidisciplinary research collaborations has recently grown in all fields of science. The number of journals that cannot be assigned to a single scientific field grows faster than the number of single-field journals (Hicks & Katz 1996). Moreover, since 1980 the number of papers that use the term 'interdisciplinarity' or 'multidisciplinarity'[87] has grown exponentially (Braun & Schubert 2003). Finally, papers increasingly cite publications in journals outside their own field (Porter & Rafols 2009).

86 In a sense, biologists were already involved in multidisciplinary collaborations in the 17th century when they joined expeditions into the unknown world in order to collect new species (Magner 1994).

87 The set of these papers contains both multi- and interdisciplinary studies and studies about the (desired) rise of multidisciplinarity and interdisciplinarity.

The appeal of multidisciplinary collaborations has increased against the background of the increasing specialization of sciences in the 1950s and 1960s (Klein 1990). In the life sciences pleas for multidisciplinarity are often based on the conviction that the most pressing scientific and societal questions can only be solved by combining knowledge and techniques from different disciplines. The Human Genome Initiative, for example, was set up as a multidisciplinary research effort, bringing together technologies from biology, computing, material science, instrumentation, robotics, physics and chemistry, in order to create tools for the analysis and understanding of human genes (Tinoco 1987). The benefits expected of multidisciplinary collaborations in the life sciences range from improved management of fishery resources (Thakur et al. 2008) to the improvement of human health (Penders et al. 2009).

Previous research has provided insight into why scientists choose to collaborate, and why collaborations are of growing importance. The most important reasons for scientists to collaborate are access to expertise, improving access to funds, obtaining prestige or visibility, learning tacit knowledge about a technique, pooling knowledge for tackling large and complex problems, enhancing productivity, mentoring junior researchers and fun or pleasure (Katz & Martin 1997, Melin 2000, Bozeman & Corley 2004). The growing frequency of collaborations over the past few decades is also explained in part by increasingly complex and costly instrumentation, decreasing costs of travel and communication, the importance of social interaction for the cognitive advance, increasing specialization, the growing importance of interdisciplinary fields and political factors such as European integration (Katz & Martin 1997, Laudel 2001). More recent studies also point to the stimulating influence of funding arrangements on collaborations (Vermeulen 2009, Shrum et al. 2007).

In general one could argue that the rise of multidisciplinary biology may be caused by both internal and external factors. Internal factors are social, cognitive or technical developments within biology. To illustrate, the development of new research systems, such as molecular visualization techniques, created the need to bring together expertise of different scientific disciplines. Biology and medicine provide the research questions, but physicists and engineers develop the hardware, mathematicians and computing scientists deliver software and analytical tools, and chemists and biologist develop contrast agents (Cassidy & Radda 2005, Loging et al. 2007).

Besides factors in science itself, societal developments can also stimulate multidisciplinary collaborations in a number of ways. First, research councils and other funding sources nowadays exert a greater pressure on academic science to deliver ‘relevant’ knowledge, knowledge that is practically useful, either in the public or private sphere (see Chapter 3). For instance, the rise of biotechnology has created a new context in which the outcomes of academic research can be turned into commercial applications, and urgent social problems such as climate change require new insights from the life sciences. In these cases traditional disciplinary research may not suffice. Second, governmental science and innovation policy may directly aim to directly stimulate multi- and interdisciplinary research. Since the 1970s, research councils have developed funding instruments to stimulate multidisciplinary collaborations,[88] both to address complex societal issues and enhance fundamental understanding. Moreover, the European Framework Programmes supply funding for collaborative projects and programs that often include multiple disciplines (Bruce et al. 2004). Third, the rise of the World Wide Web and related communication technologies facilitate more diverse and distributed forms of collaboration (Vasileiadou 2009). The Center for the Development of a Virtual Tumor, for example, successfully brings together researchers from all over the globe to collaborate on computational and mathematical cancer modelling (Sagotsky et al. 2008). These external factors are the focus of this chapter.

To examine the changing societal context under which the academic life sciences operates and its influence on multidisciplinary collaborations, we employ the sociological notion of a 'contract' between science and society (Martin 2003, Elzinga 1997, Guston 2000 , see also Chapter 3). This contract is not a physical entity, but a representation of the moral positions that encompasses all implicit and explicit agreements between academic science and governmental departments, NGOs, firms and other societal parties,[89] specifying what science should do (identity), why it should do this (rationale), and the appropriate conditions for science to function well (conditions), see Figure 5.1.

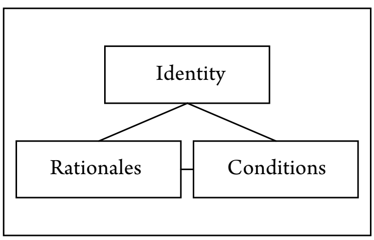

Figure 5.1 The contract between science and society (adapted from Figure 3.1)

According to this contractual perspective, the *identity* of science is connected to the provision of a valuable public good: relevant research outcomes. Science's task is to produce knowledge and to deliver it in forms like papers, patents, artefacts or educated people. The precise type of knowledge (basic, applied) which is expected and the method of deliverance specified by the contract vary over time and across disciplines. The contract, that is, the set of implicit and explicit agreements, also describes *why* science deserves support. Academic research is often regarded as a necessary condition for sustaining a system of higher education, commercial product development and informing complex decisions and innovation. The third element of the contract contains agreements about the *conditions* under which scientists work, including expectations regarding the social structure of the research community, allocation of research funds and incentives for producing more socially relevant knowledge.

88 E.g. The Dutch research council NWO (see report N97 in Appendix A).

89 The concept of the science-society contract is further elaborated in (Hessels et al. 2009).

Although one may speak about a contract between 'science' and society in general, the notion is also suitable for analysis of specific scientific disciplines, as each discipline promises different types of applications and is linked to different societal stakeholders. Moreover, some disciplines are more application oriented than others, or interact more closely with stakeholders. Applied disciplines (like biotechnology and medicine) may have a stronger multidisciplinary nature than basic disciplines (such as mathematics and physics) because solving practical problems requires knowledge from several fields. However, cutting edge fundamental research also often occurs at the borders of two or more fields, so multidisciplinary collaborations may be just as important in disciplines working mainly to produce basic knowledge.

5.3 Research Approach and Methods

Case selection

In this chapter we analyse the external factors that incite multidisciplinarity in biology. We take the idea of a contract between science and society, or between science and a discipline, as a starting point and raise the question whether and how a change in contract will affect fields of biology differently. To maximize comparative explanatory power, we have selected two biological fields that differ substantially in terms of the orientation of their research agenda. Traditionally paleo-ecology has a more fundamental research agenda than toxicology. In the quadrant model of Stokes (1997), toxicology traditionally fits in the 'Edison Quadrant', while paleo-ecology fits in Bohr's quadrant (see Figure 5.2).

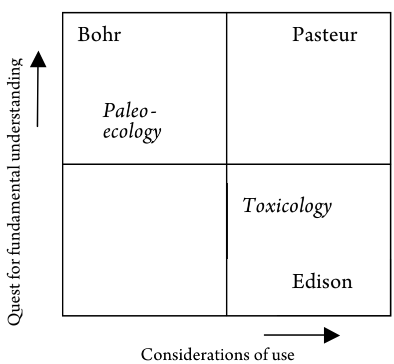

Figure 5.2 Positioning of paleo-ecology and toxicology in relation to Pasteur's quadrant (based on Stokes (1997))

Toxicology is a typical example of a field that is inspired by considerations of use. It studies the (adverse) effects of compounds on organisms. It contributes to the risk assessment of chemical substances, and also informs medicine and pharmacy about side effects of drugs (Hayes 2007). Traditionally, toxicological research questions are related to practical issues and are not strongly inspired by a quest for fundamental understanding, but within toxicology there is increasing interest in the study of the underlying mechanisms of toxicity.

On the other hand, paleo-ecology, that is, the study of ancient ecosystems and environments, is inspired by a quest for fundamental understanding. In contrast to toxicology, paleo-ecology has historically contributed little in the way of practical applications, instead providing fundamental insights into biological evolution and taxonomy. Although our analysis in this paper will show that this has recently changed, considerations of use traditionally did not strongly influence the paleo-ecological research agenda.

Methods

Data for this study are drawn from in-depth interviews and documentary analysis. We conducted 18 semi-structured interviews, each lasting about one hour. Respondents included 15 academic researchers of various ranks, working at three different Dutch universities (Table 5.1). Questions were asked about their (collaborative) research activities, personal motivations, funding sources, and performance evaluations.

Table 5.1 Distribution of respondents over fields, universities and academic ranks

Paleo-ecology (8)	Utrecht University (10)	Full professor (4)
Toxicology (7)	University of Amsterdam (3)	Retired extraordinary professor (1)
	Wageningen University (2)	Associate professor (3)
		Assistant professor (2)
		Post-doc researcher (1)
		PhD-student (4)

In order to triangulate and complement our findings in documents about the allocation of public funding and their impact on multidisciplinary collaborations we have also spoken with key-representatives of three prominent biological research councils in the Netherlands: BION, NWO-ALW and ZonMw. BION (Dutch Foundation for Biological Research) was founded in 1970. It has funded biological research until 1994, when it was reorganized into the Foundation for Life Sciences (SLW). In 1998 SLW became part of the department of Earth and Life Sciences (ALW) of the new Dutch Organization for Scientific Research (NWO). ZonMw is the Dutch research council for medical and health research and exists since 2001. All interviews were recorded and transcribed. The interviews with academic researchers were analyzed using Nvivo 8, a software package for qualitative analysis. A previous draft of this paper has been distributed among all interviewees in order to validate our findings.

Appendix B lists the 82 documents studied. The main purpose of these documents is to provide insights in the changing contract between biology and society. Documents were selected in order to represent the positions of the most important parties that take part in this 'contract'. Academic researchers are represented by the royal academy (KNAW) and by various foresight committees; the government by policy reports of the ministry of science and education and the ministry of economic affairs. Furthermore, intermediary organizations like research councils and the association of Dutch universities (VSNU) are represented by annual reports and other documents. The documents were screened for statements about the three elements of the contract: the identity of biology, rationales for supporting it and the conditions under which it should function.[90] Besides, a sub-set of documents provided factual data about the institutional conditions of academic biology, mainly concerning shifts in research funding and the development of performance evaluations. Based on the collection of these statements and data a history of the biology-society contract was written. Our documentary analysis begins in 1975 because this year roughly marks the birth of Dutch governmental science policy (W74[91]).

5.4 A changing contract between biology and society

Science does not proceed in isolation. In contrast, it is embedded in a wider set of institutions, privileges and expectations. The fundamental ideas about what science is (identity), why it should be supported (rationales) and how this should be arranged (conditions) are the three fundamental ingredients of what we have labelled as the contract between science and society. The contract is historically situated and will be different for different disciplines. In this section we will trace the major changes of the contract between biology and society since the 1970s, when science policy was introduced in many western countries. In our analysis we will follow the three lines of the contract: the identity of academic biology, the rationales to support it in the first place and the conditions to do so. In the next section we will inquire how paleo-ecology and toxicology have responded to these general changes.

Identity: from basic to strategic

In reports and documents of the 1970s academic biology is mainly positioned as a field of basic research, not directly connected to any practical problems or needs. The first research assessment of biology (R80) stated that most research can be qualified as purely scientific, not explicitly designed to lead to application. In this period research council BION (Stichting Biologisch Onderzoek Nederland) only funded 'purely-scientific' research which addressed questions of 'purely fundamental nature, not inspired by any immediate societal issue' (B75: 3). During the 1980s, however, biology, like other sciences, became increasingly defined in relation to its contribution to society and the economy. The relationship with practical applications became stronger and especially the commercial success of biotechnology firms underpinned the general promise of the life sciences. In the discourse of the policy documents, strategic plans and evaluations biology has shifted from a 'basic' discipline to a project with huge potential

90 This analysis was conducted manually, without the use of a formal coding scheme.

91 In the following we will use abbreviations such as W74 to refer to the documents listed in Appendix A.

economic impact. In 1980 BION started to fund applied research for the first time.[92] Moreover, BION distributed an increasing share of its money in programs dedicated to five 'priority themes' that relate to a societal need or to expected economic profit: molecular cell biology and genetics, biotechnology, gerontology, soil biology and toxicology (B86). Foresight studies and evaluation reports of the 1980s and 1990s increasingly expressed the value of biology in terms of its contribution to society and the economy. The 1985 foresight study by the Dutch Royal Academy of the Sciences argued: 'Also because of biology, current prosperity and current wellbeing of humankind has been achieved and will be maintained' (R85: 211). And a performance evaluation of the life sciences in 1994 stated:

> *'Biology, in addition to its own scientific raison d'être, is a basic, - if not the basic - discipline underlying medicine, agricultural production and environmental sciences, all areas of knowledge essential to the survival of mankind. Therefore, biology is likely to become the central discipline among the natural science in the next century' (V9: preface).*

By now, it has almost become a cliché to coin the 21st century as the 'century of biology'. This change in identity has affected the way biology is stimulated and assessed. At the end of the 1990s, for instance, 'collaborations with industry' were introduced as an indicator for the strength of biological research in assessment reports (V99).

A second shift in the shared understanding what 'biology' is and does, relates to the rise of multidisciplinary linkages. In the 1970s biology consisted of a collection of semi-autonomous units. The Dutch research council for biology BION, for instance, was divided into sub-disciplinary 'Working Communities', such as Functional Morphology or Neurobiology. Interactions occurred, of course, but were not seen as valuable per se. Over the years, however, linkages between fields were increasingly seen as a goal in itself, as something to be cherished and supported. During the 1980s BION made agreements to foster such linkages with the research councils for agricultural science and environmental science. In 1988, it even reorganized its divisions in a multidisciplinary way (B90). The increasing interest in multidisciplinary life sciences was broader than BION only. In the 1990s the Dutch Royal Academy of the Sciences (KNAW) noted in biology a growing exchange of knowledge between the different levels of aggregation – from cell to ecosystem (VB97). The 1999 research assessment committee stressed that it 'was very pleased to notice a growing integration of the knowledge acquired in different areas of biology' (V99). The new research council NWO regarded the rise of multidisciplinary research as an important development of the preceding decades (N95). Around the turn of the millennium NWO's department for Earth and Life Sciences reported a growing willingness of others to cooperate, including NWO partners and external partners who believe in the application potential of the produced knowledge (A98). A

92 In cooperation with the new Technology Foundation (STW) (B80)

KNAW foresight committee even considered biology 'the nourishing core of a new kind of multidisciplinary research on living matter' (C0: 11), offering inspiration to other sciences.

To conclude, in the past 30 years, the identity of academic biology has undergone two major changes: first, it has lost it appeal of basic inquiry and has become connected with economic benefits; second, biological research is increasingly seen as multidisciplinary research.

Rationales: from education to application

The element 'rationale' in the contract between society and biology refers to the question why society should fund academic biology in the first place. Traditionally, the (implicit) answer to this question was: education. Across the whole period we have studied, both researchers and policy makers have argued that cutting-edge biological research is required to sustain high quality education in biology, both at high school and higher levels (e.g. R83, W84, V99, C01). Gradually, we noticed a shift to another rationale: potential practical applications. The contributions that the life sciences can make to biotechnology, agriculture and public policy are increasingly used to justify investments in academic biology. Already in the 1980s, such justifications were used when biologists claimed that their knowledge would lead to improvement of food supply and health care by providing industry with a broad range of improved enzymes (B85). Furthermore, it was argued that biology provided other sciences, like biotechnology, agricultural science and medicine, with fundamental knowledge (Rip & Nederhof 1985). The 1985 foresight study by KNAW, written against the backdrop of imminent budget cuts, used this argument to emphasize the importance of biology for human welfare:

> *The quality of human life is at stake in medical research, while the quality of the environment can be improved by restoring the disturbed balance in nature using the results of environmental research. In this way biological research, the foundation supporting all this research, is a source of wellbeing and prosperity for human life and for the appreciation and the conservation of the quality of life of plant and animal on planet earth. (R85: 209)*

The 2001 foresight study explicitly referred to the societal impact of biology as an argument for increasing governmental funding: 'Biology as a core discipline and the growing position of the biological sciences in society are not yet acknowledged enough in terms of financial means' (C01: 41).

Conditions: from mono- to multidisciplinary funding

As in other scientific fields, the circumstances under which biologists work have changed significantly over the past few decades. The mosaic of available funding has shifted, creating a stronger impetus for research with practical 'relevance', as discussed above. In addition, the funding allocation by research councils (BION and NWO) has changed from a rigid structure of monodisciplinary Working Communities to a flexible organization dominated by multidisciplinary research programs. Since the early 1970s research councils have stimulated multidisciplinary collaborations, in principle. Interdisciplinary cooperation between biology and biochemistry was one of BION's aims when founded in 1970. The realization of collaboration among different disciplinary Working Groups, Project Groups and Working Communities both within and outside BION has always been part of its explicit mission (B75). However, BION's money was initially allocated by disciplinary 'Working Communities', with few conditions attached and little formal oversight (Kersten 1996). Later BION used instruments like the 'Dwarsverbandcommissie' (cross-cutting committee) and the 'priority areas' in order to stimulate promising young interdisciplinary fields with the potential to develop into independent research areas. Yet, such measures did not break down disciplinary boundaries as individual researchers and groups tended to defend their own disciplinary interests.[93] In 1992, however, NWO, which had succeeded BION, announced that one of its long-term aims was building connections between biology and other sciences (e.g. the environmental sciences and behavioral sciences; N92). Since 1994 research programs, not restricted to disciplinary Working Communities, have become the dominant form of organising research funding.[94] The quality of proposals was no longer assessed in terms of individual projects but in terms of multidisciplinary programs in which the individual projects could fit.

The second shift in conditions is the establishment of an elaborate system of performance evaluations. In the Dutch university system research quality assessments have developed in the past 25 years from informal deliberations to formal procedures with a fixed protocol (van der Meulen 2008). After an 'open deliberation' conducted in 1978 (R80) and a foresight study in 1982 (R83), the Association of Dutch Universities (VSNU) organized systematic research assessments in 1992 and 1998 (V94, V99). In the course of years, increasingly specific criteria and indicators have been developed to guide the process. The most powerful indicators are by far the bibliometric indicators, such as the number of articles, their citations and the impact factors of the journals. The later evaluations also took into account the societal relevance and impact of the research but these aspects did only partly account for one of the four criteria. The main effect of the rise of quality assessment is an increased pressure to publish in scientific journals.

The third relevant change is the foundation of research schools at Dutch universities, which also contributed to the rise of multidisciplinary research in biology. It became important for researchers to be member of such schools, and these favored multidisciplinary research connections. A major task of the research schools is to provide training for PhD students and in the past decade PhD students in biology have been educated in the context of a broad research theme rather than a specific scientific field (C01).

93 Interview with former adjunct-director of BION

94 This change was implemented when BION was reorganized into the Foundation for Life Sciences (S94).

5.5 Two biological fields under pressure

While biology in general has faced some substantial changes, it remains an empirical question how this affects different biological fields. Are the responses to the urge for practical relevance the same? How do fields relate to institutional changes like shifts in available funding and the rise of performance evaluations? Do they benefit equally from the general promise of biology? In this section we will discuss the changes in the field of paleo-ecology and toxicology. In Section 5.6 we will compare and contrast the findings and relate them to our notion of contract and the analysis of multidisciplinarity.

Paleo-ecology

Paleo-ecology has traditionally had a descriptive orientation, aiming to enhance the fundamental understanding of ancient ecosystems and to contribute to biological taxonomy. It tends to be inspired by curiosity, or in the words of a retired professor, by 'fascination':

> *To find out what these plants looked like in the past, how evolution has been, what kinds of processes have taken place. That is fascinating. I always liked it. It remains fascinating. (retired professor)*

The traditional image of paleo-ecologists is one of patient researchers spending many hours looking through their microscopes. Compared to other life sciences, the work is relatively individual. Research collaborations within paleo-ecology were rare, and multidisciplinary collaborations even more so. The one area of applied research to which the field contributed was locating oil fields for the fossil fuel industry. This research was traditionally located outside the universities, but against the background of imminent budget cuts and reorganizations,[95] academic paleo-ecology started to conduct research projects for oil companies, which could serve as an additional funding source.

> *We simply saw good opportunities in it. Also because our field was a bit too... because it has always been a field in the twilight between geo and bio and always threatened to become the victim of the next reorganization, because we are simply standing in the periphery. So then it also was a means to keep people active here at university and also to maintain a bond. (full professor)*

The decreasing amount of university funding was an important driver for the move towards industry. Aiding fossil fuel companies did not require intensive collaborations with other disciplines. In Utrecht

95 In the early 1980s the ministry of science and education implemented a substantial austerity policy (the policy of 'Task Reallocation and Concentration', announced in W82) which has resulted in a 15% budget cut for universities, amounting to 285 Mfl. (about 128 Million Euros).

the contract research was organized in a foundation that was closely linked to the research laboratory for paleo-ecology, which even carried the same name.[96]

Significant shifts in the available funding have gradually decreased the possibilities for paleo-ecologists for conducting basic research. Apart from the decline of direct support from universities, the research council, as we discussed above, has also changed its policy and dedicated an increasing share of its funding to projects and programs that are 'application oriented' or in areas of 'strategic interest'. Against this background, a new area of potential applications emerged for paleo-ecology in the 1990s. Support for paleo-ecology was increasingly framed in relation to climate change. With the succession of different generations of environmental policy, the perspective has shifted to a global scale (Grin et al. 2003). In the 1970s and 1980s the emphasis was on mitigating effects of local environmental problems such as surface water pollution (e.g. W76, W87), but during the 1990s the emphasis shifted towards prevention, and a more global perspective emerged, with issues like global warming and biodiversity (W96). After the Rio de Janeiro treaty of 1992, there were increasing calls for knowledge about biodiversity. The topic of 'global change', including environmental problems, nature conservation and biodiversity, is often listed as topic of high policy relevance (C01). The government offered funding for optimising the conservation of collections and biodiversity data, strengthening taxonomic research and setting up a national herbarium (M95). Paleo-ecology received support for reconstructing paleo environments, which could be used in long term climate and biodiversity forecasting (W95, A00). The new field of applications added a sense of change to the research work:

> *The basis is still the same, this remains first. But then you will do something with it. And I think that that is a fundamental change. (retired professor)*

So, thanks to the issue of climate change, the potential contribution to policy making remains an important rationale for funding paleo-ecology. In two strategic reports (N01, N06) NWO mentions some paleo-ecological topics as important themes in life sciences: global change, biodiversity and the connection between the biosphere and the geosphere and the extinction of species. These topics are part of the NWO theme 'System Earth'. A recent foresight study claims that biogeology, of which paleo-ecology is a part, is of growing interest of policymakers (G03). This is also stressed by one of our interviewees:

> *In the 80s, it was generally believed that everything was known about fossils, so funding was hard to get. Nowadays, since there is renewed interest in fossils because of the climate change debate, it appeared there are big gaps in collections and it is recognized that these fossils are essential for recognition and analysis of fossil materials. (post-doc researcher)*

96 The foundation was simply called 'Laboratory for Paleo-botany and Palynology'.

The new appeal of paleo-ecology has enhanced the need for multidisciplinary collaborations: paleo-ecological research can make an important contribution to the understanding of climate change, but it cannot do so in isolation. Cooperation is required with other life sciences like microbiology and ecology, but also with geo-sciences.

> *We are not directly application oriented. So what we often do is, we contact people of whom we think: these could apply this. For example, climate results related to climate change, there we actually contact, as soon as we know how it is, climate modellers and they can then use it for the models and even test whether it is right as well. (full professor)*

The sense of contribution paved the way for paleo-ecologists to participate in unexpected academic endeavours, such as efforts to develop renewable energy solutions. An example is a multidisciplinary project studying the possibility to grow water ferns as a source of bio-energy:

> *We need an engineer who can calculate something like this, like what are the technical details. You have to dry this, you have to burn that, you have to do this and that. Of course that is not our core business. We can only say: look, in the past we already had the situation in which we say: a lot of carbon is stored, is it possible that we say this plant produces that much biomass, that we can obtain energy from it again? (full professor)*

The new status of paleo-ecology culminated in 2004 in the foundation of the Darwin Centre, a major research facility in biogeology (G03, A04). A KNAW foresight committee had argued that knowledge about biogeochemical processes is essential for understanding human-induced changes in System Earth. The Darwin Centre was founded with the explicit intentions to provide practically useful insights. Beside its academic mission, the Centre should act as a knowledge consultant to government authorities (G03). It appears to stimulate multidisciplinary paleo-ecological research by funding high-risk research and new combinations of research. According to the researchers involved, this has resulted in more multidisciplinary work and multidisciplinary staff. Multidisciplinary work is used to answer research questions from multiple perspectives, to do research more efficiently and to validate data with complementing techniques and methods: to increase reliability of studies. Within the Centre, PhD-students have multiple supervisors from various fields. The students regularly discuss their work in interdisciplinary meetings which stimulate them to switch between different disciplinary perspectives.[97] Although the Centre is not embodied in a physical building, it does make it easier to frequently interact:

97 Interview PhD-student in paleo-ecology: 'One is sitting there with a team of like... 10 people for sure... discussing often. "What have you done, lately? What can we improve upon it?" Of course we have a number of chemists in there and they say: "so, maybe you can use this chemical compound to do a particular method".'

> *Now I can also walk down here easily to organic geochemistry, what is obviously something very different from what we are doing here. ... Because yes, of course one gets results from something one is not an expert in oneself, and then it is very nice if one gets feedback from those who do know about it, I must say. (PhD-student)*

To conclude, in the field of paleo-ecology multidisciplinary collaborations have increased due to its relevance for a new, global agenda on climate change. To ensure sufficient income, academic paleo-ecology cannot rely on fundamental research agendas only. In the 1980s application oriented research for the fossil fuel industry provided extra income, but did not require multidisciplinary collaborations. The contribution to the research agenda of climate change, however, requires intensive cooperation with other fields.

Toxicology

In contrast to paleo-ecology, toxicology has had an applied orientation since its emergence, which is connected with the rise of environmental concerns in the 1970s. In its early years toxicology was strongly related to practical applications, such as risk assessment and risk management. Incidents with industrial toxic compounds and concerns about environmental problems in the late 1970s and the 1980s raised questions about the risks of synthetic chemicals. Academic toxicological research was expected to directly contribute to the development of environmental policies for regulating hazardous chemicals. The practical orientation is reflected in definitions of the field. In an influential handbook, for example, toxicology was defined as: 'the study of the effects of toxic substances occurring in both natural and manmade environments. The main task of environmental toxicologists is to assess objectively the risk resulting from the presence of such substances' (Duffus 1980), cited by Halffman (2003).

However, the relationship with regulatory practices was ambivalent. On the one hand, the practical value of toxicological knowledge provided resources but, on the other, hand it could hinder the development of more fundamental research lines. As in many fields of science there was a tension between application oriented toxicologists and those with a basic research orientation (Groenewegen 1988). This tension can be illustrated by the two dominant ways to divide the field. The first distinguished medical, environmental, industrial, pesticide and military toxicology, according to its application area. The second distinguished biochemical, neurological and other approaches, according to the methods and theories used (Groenewegen 1988).

The tension also appeared in the struggle of toxicologists to establish a strong position in the biological research council BION. In contrast to most other fields there has never been a dedicated 'Working Community'[98] for toxicology. In 1981 a special committee ('Dwarsverbandcommissie') was formed to

98 Working Communities decided upon funding and research strategy/priorities within a specific field of biology, like ethology, and plant ecology and vegetation sciences.

stimulate toxicological research (Groenewegen 1988). This was a bottom-up initiative from researchers arguing that more research was needed into the mechanisms of toxicity and that the new field of ecotoxicology deserved support. The name *Dwarsverbandcommissie*, which literally means connection-committee, indicates that toxicology was seen as a multidisciplinary field. It was also seen as an immature field, not yet deserving its own Working Group or Community. The committee was not directly linked to societal issues. Indirectly, however, the practical value of the field was important to justify the initiative.[99]

The most important funder of academic toxicology has always been the government. In the mid 1980s, the government funded 90% of all toxicological research in the Netherlands, and industry the remaining 10% (W84). In 1987, the government introduced the program Stimulation of Toxicological Research. It included a special budget for toxicology for four years. The goals of the program were to coordinate research investments and to increase efficiency (T88). The government also reserved a budget for organizing knowledge dissemination meetings (W90). The stimulation program focused on four themes, all connected with practical applications: labor and health, development of new test methods, eco-toxicology and toxicological compounds and chronic diseases in relation to nutritional habits

Gradually a more basic research orientation developed within toxicology, for several reasons. The shift was partly for practical reasons. Already at the end of the 1970s, toxicologists at Utrecht University turned their focus to the molecular or cellular mechanisms of toxicity. 'Neurotoxicology, biochemical toxicology and pathology were envisaged as providing fundamental support for environmental toxicology and veterinary pharmacology' (Groenewegen 1988 p. 135). The design of the more fundamental research was attuned to the problems in the practical studies, like the biochemical mechanisms of toxic effects. This fundamental research, however, could in turn lead to new research lines, further away from the original practical problems, developing into independent research projects with their own dynamics, completely detached from the original problem definition. This shift in research content was also visible in the research output. Groenewegen's analysis (1988) shows that between 1960 and 1980 Dutch reports and Dutch journal articles became less frequent and gave way to international publications. From 1983 onwards, simultaneous to the governmental program, research council BION ran a priority program with a more fundamental orientation (Halffman 2003). Priorities of this program were fundamental issues such as combination toxicology and processes of poisoning and detoxification.[100]

Second, the expansion of more fundamental research lines in toxicology was stimulated by the growing pressure for scientific publications, catalyzed by the rise of performance assessments. It was encouraged to devote more effort to fundamental research lines that might result in publications in high-impact scientific journals. Asked about the importance of publishing a lot:

99 Interview with former adjunct-director of BION

100 It was much smaller than the governmental program: only 200.000-500.000 Fl. per year. The program was managed cooperatively by the Dwarsverbandcommissie for toxicology of BION and the medical research council. There were several joint meetings with ministerial program and the two programs ended together in 1993 (Halffman 2003).

> *Yes, that is what we are judged on! So ... that's the visibility of the faculty, how productive one is, so to say. So it is pretty important. (assistant professor)*

Illustrative is the fact that PhD students are nowadays required to publish about four papers during their project. As in many other fields of natural science, PhD-theses are nowadays usually a collection of journal publications, rather than a monograph.[101] Moreover, contemporary toxicologists strive for journals with a high impact factor:

> *We actually have the demand that papers should be published in the top 30% of the field, but in fact, as an institute, and we are succeeding reasonably well, the greater share of the paper in the top 10%. ... When I am talking about toxicology, I almost always go for submission to the top 5 journals. (full professor)*

Adding a medical context to one's research can help to get into journals with higher impact factors:

> *So one has a nearly unlimited choice of journals. With different impact factors. Which all have a different focus. The higher the impact factor, the better of course. And things with a disease simply sell better. But then one has to position it in a different field. (assistant professor)*

The third factor that changed the research agenda of toxicologists was the 'problem' that the most urgent practical problems were solved, so there was less governmental funding available for applied research. Industrial toxicology, for example, hardly exists anymore as an academic field, because industry has become cleaner, and there are now strict norms concerning exposure of industrial staff to toxic compounds.[102] As a result, toxicology remained linked with policy applications,[103] but to be able to maintain itself as a mature scientific field in a context of foresight and evaluation studies and to acquire sufficient funding, toxicologists had to address fundamental questions as well.

In the mid 1990s, when both the ministerial research program and the BION program ended, toxicology was supported again by a Stimulation Program, focusing on the effects of chemical pollution on ecosystems.[104] This new program focused more on the mechanisms behind toxicity and was less policy-oriented than the previous ones (Halffman 2003).[105] Currently, there is no dedicated NWO program for toxicology. In terms of funding, toxicology has never established itself as a mature field with its own steady supply of money. The limited amount of funding for toxicological research that is available at the research council nowadays solely comes from broader programs, requiring collaboration with other

101 Interview with full professor in toxicology
102 Interview with NWO-ALW representative
103 In 1989 SETAC Europe is founded, a forum for science and policy making (Halfmann, 2003)
104 NWO-ALW 1999
105 Confirmed in interview with NWO-ALW representative

fields. The other organizations that may fund toxicologists are individual governmental ministries, the EU, the medical research council ZonMw and medical charity foundations like the Cancer Foundation and Heart Foundation. It is, however, difficult to acquire funding from any of these without collaborating with other fields. Both ZonMw and the medical charity foundations will only pay for projects that are directed at a specific disease or patient group, and in most cases toxicologists cannot run such projects without the help of medical scientists. This serving role is illustrated with the following quote:

> *This Parkinson Foundation [project] is again a collaboration, in which we collaborate with a hospital in Tilburg. There is a Parkinson department at that hospital. I happen to know the neurologist there. He had some interest in pesticides and Parkinson. I had already written a couple of proposals for it. And we happened to meet each other actually. And then the idea was to do it at the Parkinson Foundation. (assistant professor)*

In conclusion, multidisciplinary research collaborations in toxicology have increased as a response to external factors. They can both facilitate application oriented research and help to develop more fundamental research lines. In a sense, the need for multidisciplinarity has been present in toxicology since its birth. The 'Dwarsverbandcommissie' that was established in BION to stimulate toxicological research intended to form a bridgehead between different fields. Notably, at Utrecht University toxicological research is still organized in an institute which is connected to the veterinary, the medical and the sciences faculties. Later on, the interaction with other fields served to give toxicological research more fundamental allure, as a response to societal pressure (in the form of performance assessments and funding programs) to develop a more academic research agenda. The tasks of exploring toxicity of compounds and developing toxicity tests have been taken over by commercial laboratories and public research institutes. To acquire funding for longer research projects, which yield PhD-theses and publications in high-impact journals, questions on the mechanisms of toxicity on the level of organisms, cells or molecules need to be addressed. These require collaborations with medical scientists or biochemists.

5.6 Conclusion

Over the past few decades, the importance of multidisciplinary research collaborations in the life sciences has grown. Multidisciplinarity is not a new phenomenon. In toxicology interactions with other fields have been vital ever since its inception. Moreover, disciplinary structures are dynamic and cross-disciplinary interactions can lead to the formation of new fields. Paleo-ecology itself can, for instance, be seen as the product of collaborations between archaeology and ecology. However, the relative share of multi- and interdisciplinary collaborations has grown significantly over the past 30 years (Porter & Rafols 2009).

The rise of multidisciplinary collaborations can be explained partly by internal factors, like the rise of molecular techniques, improved measurement equipment and informatics (Cassidy & Radda 2005, Loging et al. 2007). While these developments are beyond the main focus of this paper, our interview data indicate significant shifts in the dominant research systems over the past view decades. Toxicologists increasingly work with cell-lines and computer models instead of test animals. Paleo-ecologists still do a lot of individual microscope work, but they increasingly use molecular techniques as well. These developments have probably increased the necessity of collaborating with other fields. Thus our case studies confirm the significance of internal factors reported in earlier literature like personal contacts (Melin 2000), mentoring relationships (Bozeman & Corley 2004), specialization (Laudel 2001) and 'research ensembles' (Hackett et al. 2004) for the rise of multidisciplinary research collaborations.

Primarily, our analysis has identified three important changes in the 'contract' between biology and society that have induced multidisciplinary research collaborations. The first development, which has had a direct influence in both cases we have studied, is that multidisciplinarity has developed into an explicit policy goal, stimulated by various funding instruments. Second, society increasingly demands that the life sciences deliver practical applications, which indirectly stimulates multidisciplinarity in paleo-ecology (and to a lesser extent in toxicology, too). Third, the rise of quantitative performance assessments has increased the pressure for scientific publications and academic excellence. This has indirectly stimulated multidisciplinary research collaborations in toxicology.

Academic toxicology was born out of a societal need and has used input from a variety of disciplines from the start. Under the current social contract it also uses multidisciplinary collaborations to enhance its fundamental research. In response to the pressure from evaluations and funding instruments, toxicology needs to improve the external validity of its approaches. Because standard toxicity tests are offered by private labs and applied research institutes, academic toxicology now needs to focus on questions concerning the mechanisms of toxicity to legitimate its existence. This requires collaboration with other fields such as physiology and biochemistry.

Multidisciplinary research collaborations offer quite different benefits to paleo-ecology. In this field the collaborations with other fields mainly serve to facilitate strategic research. For paleo-ecology the changing social contract implied the need to develop a more application-oriented research line, connected to climate change. Public policy has strongly supported collaborations with earth scientists that are believed to yield new insights to inform environmental policy.

Before we close, let us discuss the generalisability of our findings. The findings reported in this chapter are restricted to the Netherlands. While specific conclusions about toxicology and paleo-ecology should not be generalized to other countries, there are indications that the general changes observed in the contract between biology and society have also taken place in other countries. Systematic performance assessments are evolving all across western science systems (Hicks 2009, Whitley & Gläser 2007), strengthening the 'publish or perish' norm (Weingart 2005). Shifts in available funding sources and selection criteria, and increased calls for practical relevance have also been reported elsewhere (Slaughter & Leslie 1997, Benner & Sandstrom 2000, Geiger & Sa 2008). For this reason, our observation that the changing societal context of academic research can give rise to a variety of incentives for multidisciplinary research collaborations can be generalized beyond the Netherlands.

To conclude, our analysis of these two cases shows that multidisciplinary collaborations can serve as means to different ends, and that the pressure exerted by the changing social contract with science can result in different outcomes. This implies that generalized claims about the changing relationship between science and society and the rise of 'Mode 2 knowledge production' (Gibbons et al. 1994) are severely vulnerable. The contract between biology and society has changed in various ways. It currently exerts several forces on academic scientists that seem to be (partly) in contradiction, reminiscent of the paradox around relevance observed in Dutch chemistry: scientists are rewarded for making promises about the (possible) relevance of their research, but not for realising these promises (see Chapters 3 and 4). The funding sources available for the life sciences increasingly demand research proposals to show the 'relevance' of the planned activities and to promise practical applications in the form of patents, products or policy advice. At the same time, the pressure for scientific publications has grown, due to the rise of systematic performance evaluations of university research. However, the relative influence of these two trends varied across the two cases studied here. Multidisciplinary research collaborations appear a strategy to cope with the changing societal demands. Toxicology, which started as an application oriented field uses multidisciplinarity to become more fundamental, in order to cope with the pressure for publications in prestigious scientific journals. Paleo-ecology, which started as an exotic, fundamental niche in biology moves, through collaborations, to a more practical orientation that fits well with the demands of current funding sources. In terms of Stokes's quadrant model (Figure 5.2) it seems that the changing societal contract pulls different fields in the same direction: both fields use multidisciplinary collaborations to move towards Pasteur's Quadrant. Possibly the current contract between society and academic biology is so demanding in terms of both practical applications and scientific output that extreme positions are not sustainable. Research fields in biology now need to combine basic and applied activities, in order to acquire sufficient funding and to score well in performance evaluations. For researchers this is a delicate balancing act, and multidisciplinary collaborations appear as versatile means to these ends.

Appendix B (chapter 5). Documents studied

Abbreviation	Publisher / Author	Year	Title	City
W74	Ministry of Science and Education (OC&W)	1974	Nota Wetenschapsbeleid	The Hague
B75-B93	Research council BION	1975-1993	Jaarverslag (Annual Report)	The Hague
W76-W97	Ministry of Science and Education (OC&W)	1976-1997	Wetenschapsbudget	The Hague
AR79	Academische Raad	1979	Beleidsnota Universitair Onderzoek	The Hague
R80	Biologische Raad	ca. 1980	Biologisch onderzoek in de subfakulteiten biologie en instituten voor fundamenteel biologisch onderzoek: rapport van de werkgroep 'Open Beraad'.	Amsterdam
R83	Biologische Raad	1983	Biologie van Levensbelang	Amsterdam
R85	Biologische Raad	1985	Bologisch Onderzoek voor Mens en Maatschappij	Amsterdam
K86	Dutch Royal Academy of the Sciences (KNAW)	1986	Sub-disciplineplan Biologie	Amsterdam
T88-T90	Dutch Toxicological Organization (NVT)	1988-1990	Toxpost	
N92	Research council NWO	1992	Een organisatie op maat: Meerjarenplan 1993-1997	The Hague
S94-S98	Research council SLW	1994-1998	Jaarverslag (Annual Report)	The Hague
V94	Association of Dutch Universities (VSNU)	1994	Quality Assessment of Research: Netherlands Biology in the Nineties	Utrecht
N95	NWO	1995	Trends in Wetenschap: Achtergrondnota bij beleidsnota 1996-2001	The Hague
M95	Ministry of Agriculture, Nature Conservation and Fisheries; Ministry of Public Housing, Spatial Planning and the Environment; and Ministry of Foreign Affairs	1995	Strategisch plan van aanpak biologische diversiteit – Nederlandse uitwerking van het verdrag inzake biologische diversiteit	The Hague

VB97	Verkenningscommissie Biologie	1997	Biologie: Het Leven Centraal	Amsterdam
N97	NWO	1997	Inspiratie en sturing van weten-schap: NWO en maatschappelijk georiënteerd en multidisciplinair onderzoek	The Hague
A98-A02	NWO-ALW	1998-2002	Jaarverslag (Annual Report)	The Hague
NJ99-NJ02	NWO	1999-2002	Jaarverslag (Annual Report)	The Hague
V99	VSNU	1999	Biology: Assessment of Research Quality	Utrecht
W00	OC&W	2000	Wie oogsten wil, moet zaaien: Wetenschapsbudget 2000	The Hague
C01	Commissie Disciplineplan Biologie	2001	Biologie: een Vitaal Belang	Amsterdam
N01	NWO	2001	Thema's met Talent: Strategienota 2002-2005	The Hague
G03	Verkenningscommissie Bio-geologie	2003	Tussen aarde en leven: een strategi-sche verkenning van de biogeologie in Nederland	Amsterdam
V03	VSNU, NWO & KNAW	2003	Standard Evaluation Protocol 2003-2009 for Public Research Organi-zations	
W04	OC&W	2004	Focus op Excellentie en meer waarde: Wetenschapsbudget 2004	The Hague
V04	VNO-NCW, VSNU & NFU	2004	Beschermde kennis is bruikbare kennis: Innovation Charter bedrijfs-leven en kennisinstellingen	
E04	Ministry of Economic Affairs	2004	Actieplan Life Sciences	The Hague
V05	VSNU	2005	Onderzoek van Waarde: Activiteiten van Universiteiten gericht op Ken-nisvalorisatie,	The Hague
N06	NWO	2006	Wetenschap Gewaardeerd! Strate-gienota 2007-2010	
W07	OC&W	2007	Voortgangsrapportage Weten-schapsbeleid	The Hague

Chapter 6.

Stakeholder interactions in Dutch animal sciences[106]

Abstract

This paper investigates the effects of the changing institutional environment on academic research practices in the case of Dutch animal sciences. The two most important changes in the Dutch agricultural research system in the past few decades have been shifts in the available funding and the rise of performance evaluations. Our analysis shows that these have only stimulated interactions with societal stakeholders in fields where this helped to sustain a basic research agenda. In other fields there turns out to be a tension between satisfying the needs of application-oriented funding sources and reaching high scores on evaluations that are dominated by bibliometric indicators.

6.1 Introduction

Two common trends can be identified in the institutional environment of academic research in developed countries over the past 30-40 years: a shift from block-grant support to earmarked funding for specific projects and programs (Lepori et al. 2007), and the rise of performance evaluations which – directly or indirectly – influence the availability of funding (Hicks 2009, Whitley & Gläser 2007). The question to what extent these structural changes have transformed actual research practices remains unsettled (Hicks & Katz 1996). It has been claimed that academic researchers have become more 'reflexive', collaborate more intensively with users, and are subject to novel forms of quality control, which reward societal contributions rather than scholarly products (Gibbons et al. 1994, Hemlin & Rasmussen 2006). However, the available empirical data suggest that we should take care not to overestimate the effects of institutional changes on research practices. Dutch research departments often succeed to continue their existing research activities, even under profound institutional changes in their environment

106 This chapter was co-authored with John Grin and Ruud Smits and has been submitted for publication.

(Leišytė et al. 2008). A study of American biologists suggests that the increase of industrial funding has only subtle effects on scientific problem choice (Cooper 2009). Also in a case study of a Finnish biotechnology department, research practices turned out to be remarkably stable, as it was difficult for academic researchers to combine their university activities with commercial development (Tuunainen 2005). In some fields even a movement away from practical concerns has been observed, due to the increasing pressure to publish in scientific literature (Albert 2003).

Against this background, the aim of our paper is to increase the understanding of the relationship between macro-level developments and micro-level research practices. By an analysis of the case of Dutch agricultural sciences we explore the ways in which institutional changes in a research system can lead to changes in the way science is conducted and organized on the lab-floor. Our analysis shows that institutional structures do not determine academic research practices. The effects of institutional changes are modulated by characteristics of scientific fields, such as existing research traditions and relationships with societal stakeholders.

In this paper we focus on three fields of agricultural (animal) science as performed at Dutch universities. As we explain below (section 2), we consider it important to differentiate between scientific fields and between national contexts. Dutch agricultural sciences have experienced a highly dynamic societal context. Over the past 50 years views on what agricultural research should be conducted, why it deserves support, and how it should be organized have changed dramatically. Contrary to popular notions such as Mode 2 knowledge production, the main shift in the views on fields of animal science does not concern an increasing pressure to contribute to practical applications. Rather, these fields are under pressure to contribute to *other* practical applications and to address different types of questions than before (Grin 2010, Grin et al. 2004).

The research questions guiding our analysis are the following:

1. What major changes have taken place in the institutional environment of Dutch academic agricultural research since 1975?
2. What is the impact of these institutional changes on the daily work of Dutch academic animal scientists?
   Do these institutional changes stimulate academic researchers to interact more intensively with societal stakeholders?

6.2 Theoretical framework

To study the daily work of agricultural (animal) scientists, we use the 'credibility cycle' (Latour & Woolgar 1986). This model explains how struggles for reputation influence the behavior of individual scientists. Its starting assumption is that a major motivation for a scientist's actions is the quest for credibility. Conceived in this way, the research process can be depicted as a repetitive cycle in which conversions take place between money, staff, data, arguments, articles, recognition, and so on (see also Chapter 3).

But scientists do not work independently; their activities take place in the context of a 'research system'[107]. Following Rip and Van der Meulen (1996), we regard a research system as consisting of 'research performers (individuals, groups, institutions), other organizations and institutions, interactions, processes and procedures' (Rip & van der Meulen 1996). This system contains universities, related research institutes and funding agencies, but also governmental organizations, firms and intermediary organizations to the extent that they are part of the institutional environment. This institutional environment provides research organizations with incentives and constraints to conduct (particular kinds of) research. Our notion of a research system is discipline-specific and it is delimited by national boundaries. It is important to differentiate between scientific disciplines, because of their social and cognitive differences (Bonaccorsi 2008, Whitley 2000, Knorr-Cetina 1999). As each discipline has its own (potential) stakeholders and funding sources, the effects of institutional changes can be expected to vary across disciplines (Gläser et al. forthcoming). Moreover, literature about national innovation systems has shown the importance of national science and innovation policies, national culture, and the presence of particular economic sectors (Lundvall 1992, Nelson 1993).

In line with the structuration perspective (Giddens 1984), the research system can be seen as the structure influencing the agency of individual researchers (Grin 2010). Existing structures are the product of practices and of dominant visions, such as the need to enhance agricultural productivity. The institutions of this system give shape to certain conversions of credibility, e.g. the possibilities to turn recognition into money (Packer & Webster 1996 , see also Chapter 3). Simultaneously, funding bodies presumably take into account the outcomes of research practices when formulating their future priorities. In this way, research practices can strengthen these institutions, but they can also neglect them and put them under pressure (Bos & Grin 2008). So the research system can be seen as a structure that shapes research practices, but that is at the same time (re)produced by these practices.

107 A research system can be seen as a subsystem of the national or sectoral innovation system. An innovation system is not only concerned with the development of new knowledge and technologies, but also with their adoption and diffusion (Lundvall 1992, Nelson 1993). So while (public) research organizations are central in a research system, in an innovation system they have to share this position with firms and other organizations that turn new knowledge into successful products, services or policy. Furthermore an innovation system also contains boundary conditions for innovative entrepreneurship such as infrastructure, venture capital, intellectual property rights etc.

6.3 Methods

The research reported here is a set of case studies of three fields of agricultural science in The Netherlands, all within the subset of (agricultural) animal sciences: animal breeding & genetics (ABG), animal production systems (APS), and cell biology (CB). We chose to study animal sciences because of their relatively applied nature, which makes them an interesting set of fields to study the effects of the increasing pressure for academically excellent performance (Leeuwis 2000). Moreover, Dutch animal sciences have experienced a highly dynamic societal context, with various successive conflicts about issues such as pollution, climate change and animal welfare, which facilitates the study of changing societal knowledge demands (Grin 2010). Focusing on three fields of animal science enhances the possibilities for mutual comparison, as they are all subject to the same contextual developments, i.e. the changing societal views on and practices of animal production. Within this subset, however, these three fields represent different 'search regimes' (Bonaccorsi 2008), characterized by convergence in ABG, and divergence in APS and CB. Related, ABG has a steady relationship with a homogeneous set of knowledge users (animal breeding firms), while the other two fields have diverse stakeholders.

Research question 1 was addressed based on document analysis, combined with expert interviews. Documents studied are listed in Appendix C. The documents were collected based on prior knowledge of the authors, tips from interviewees, and the 'snowball effect'. The selection includes governmental policy documents, reports and strategic plans of research councils, foresight studies, evaluations and other important publications about Wageningen University. The findings from these documents were triangulated in interviews with various experts directly or indirectly involved in agricultural science (see Table 6.1). Our analysis of the changing agricultural research system is delimited to the period of 1975 until about 2005. The starting year of 1975 roughly marks the beginning of governmental science policy in the Netherlands (M74). In order to properly situate the history of agricultural science, however, we provide an introduction to the agricultural research system that starts just after World War II (section 4).

Table 6.1 Expert interviews[108]

Name	Affiliation	Position / expertise
Bram Bos	WUR, Animal Science Group	Expert system innovations
Johan Bouma	Wageningen University	Professor emeritus soil science, TransForum advisory board
Annemieke van der Kooij	NWO	Senior program officer Earth and Life Sciences (ALW), and Social Sciences and Humanities (MaGW)
Henk van Latesteijn	TransForum	Director, formerly deputy director of directorate knowledge of ministry of agriculture
Niels Röling	Wageningen University	Professor emeritus agricultural extension and innovation
Gab van Winkel	Wageningen University	Former secretary of research school WIAS

To answer research questions 2 and 3 we carried out semi-structured in-depth interviews with 12 academic researchers in the selected fields. The respondents' ranks range from post-doc researcher to full professor. Most of them are employed by Wageningen University; two work at Utrecht University (see Table 6.2). We have asked them questions about their current and past research activities, their personal motivation, and their experiences and strategies concerning funding acquisition, publishing, scientific reputation, and performance evaluations. The questions had an open character, in order to minimize our influence on the answers and to benefit optimally from the respondents' own experiences. Because of the sensitivity of some of the issues addressed, quotes from the interviews will be presented anonymously, and all interviewees will be referred to as 'he', regardless of their sex.

Table 6.2 Interviews with academic researchers: distribution of respondents over sub-disciplines, universities and academic ranks

Animal production systems (4)	Wageningen University (10)	Full professor (3)
Animal breeding and genetics (4)	Utrecht University (2)	Associate professor (2)
Cell biology (4)		Assistant professor (4)
		Post-doc researcher (3)

108 WUR: Wageningen University and Research Centre (further in this paper denoted as AU), NWO: Dutch Organization for Scientific Research

6.4 Introduction to the Dutch agricultural research system

Dutch agricultural research got a boost in the period after WWII, when the Dutch government invested a lot of money in it with the aim to 'modernize' the agricultural sector in order to increase its productivity (Grin 2010). It is generally assumed that a key factor in the impressive productivity growth of the Dutch agricultural sector in the 1950s, 1960s and 1970s was the exceptional degree of consensus about the direction of change. Politicians, farmers and researchers all shared the aim of increasing agricultural productivity and agreed that mechanization was the key to reach this goal (Bieleman 2000, Roseboom & Rutten 1998). Thanks to the strong alignment of the different participating actors, the research system of this era has become known as the 'OVO-triptych', the strongly interlinked combination of research, education, and extension activities (Maat 2003). Within this configuration (see Table 6.3), a classical example of the linear innovation model, the agricultural university (AU)[109] was concerned with more fundamental research and the applied research was located in separate institutes and in a number of regional 'experimental stations' (WR91)[110] (Schot & van Lente 2003). The National Council for Agricultural Research (NRLO) functioned as an intermediary to coordinate the activities of the various research organizations (Dijksterhuis & van der Meulen 2007). The alignment of research agendas with practical knowledge needs was further strengthened by the fact that most agricultural researchers originated from farming families.

Academic agricultural research was financed mainly by lump-sum funding from the Ministry of Agriculture and Fisheries (Maat 2001)(WR91). Researchers at AU received unconditional support, as the usefulness of their work was not disputed. Because of the strong bonds between their (basic) research activities and agricultural extension, it was taken for granted that their research was attuned to the needs of farmers (and related industries) and that the results of their work would diffuse to the individuals or organizations that could apply them.

109 Until 1986 the agricultural university was known as Landbouwhogeschool Wageningen. It was founded in 1876 as the 'Rijkslandbouwschool', and became a university for professional education in 1918. Since 1986 it has the status of a research university. For the sake of clarity, we will use AU throughout this paper.

110 Codes in brackets like this refer to the documents listed in Appendix 1.

Table 6.3 Overview of the organizations of the agricultural research system and their relationships with the agricultural university (AU), around 1975

Organization	Relationship with AU
Ministry of Agriculture and Fisheries (MLV)	Main funding source, in the form of block grant support
Research council for Pure Scientific Research (ZWO)	Additional funding for fundamental research on competitive basis
Extension Service	Knowledge transfer to farmers
Institutes for applied research	Attunement of research agenda
Experimental Stations	Attunement of research agenda
National Council for Agricultural Research (NRLO)	Coordination of research activities of different organizations by keeping a project database and stimulating contact with applied research institutes and Experimental Stations

After a period of impressive productivity growth, this research system gradually came under pressure since the 1970s. Concerns about the side effects of agricultural modernization led the mainstream conviction that the impact of the high-intensive agriculture on nature, on the environment and on the Dutch scenery was unacceptably high (Bieleman 2000). In addition, there were concerns about animal welfare and overproduction and it became increasingly clear that on the long term, Dutch agriculture would not be able to compete internationally on price, because other regions possess comparative advantages in terms of the potential for scale enlargement and the costs of land, labor and energy (van der Ploeg & Ettema 1990). To overcome these major problems, a radical change seemed necessary from mass production of agricultural goods to sustainable production of knowledge-intensive specialties (Smits 2002).

The crisis in the agricultural sector implied that other kinds of knowledge were needed from the agricultural research system. In 1982, when the responsibilities of MLV were expanded in the areas of recreation, nature conservation and environmental issues, NRLO also broadened its scope (Dijksterhuis & van der Meulen 2007). In addition to knowledge about how to enhance the efficiency of existing systems of mass production, a need was developed for knowledge about animal welfare, about the environmental impact of agricultural activities and about alternative production systems.

6.5 Institutional changes in the research system

The agricultural research system has undergone major changes over the past few decades. In short, one can observe a shift from the consensus-driven 'OVO-tryptich' to a heterogeneous agricultural research system. This shift consists of two major changes in the institutional environment of academic agricultural research: shifts in the available funding, and the rise of systematic performance evaluations. These will be subsequently addressed in this section.

Shifts in available funding

Over time, the relative share of governmental block-grant support has decreased. An increasing share of all funding available for agricultural researchers comes from projects and programs, paid by a variety of organizations. While the former left considerable autonomy to the researchers regarding the content of their work, the latter are often dedicated to making specific contributions to a more sustainable and competitive agricultural sector.

The 1980s brought increasing scarcity of funding and increasing programming of research activities. For the agricultural university (AU), the governmental operations called 'Task Division and Concentration', and 'Selective Shrinkage and Growth' implied budget cuts of 6.7 Million and 4.5 Million DFl, respectively (Faber 1993). In relation to the number of students, the governmental block-grant support decreased with about 56% in real terms (Faber 1993). Moreover, with the introduction of Conditional Financing in 1983 (Blume & Spaapen 1988), a substantial share of the remaining lump-sum funding became subject to programming and evaluation.

At the same time, the emphasis in the available funding shifted towards application oriented research. In the 1980s the national research council was reorganized and started to fund application oriented research as well[111] (Kersten 1996). Meanwhile contract research became increasingly significant for AU[112]. In 1981 a 'transfer point' was established, to stimulate and facilitate project acquisition and knowledge transfer (Faber 1993). The volume of contract research grew from 12 MDfl in 1982 to 74 MDfl in 1991. Around 1990 about 20% of all income of AU stemmed from contract research (Faber 1993).

Dutch agricultural sciences faced a further rise of performance-based funding and earmarked programs in the 1990s. Block-grant funding for research gradually gave way to more competitive funding arrangements (R96c). This general move from lump-sum funding to market-oriented funding took place in all western science systems (Levidow et al. 2002, Huffman & Just 1999)(I03) and fitted in Neo-liberalism, or the policy of 'steering at a distance' (De Boer et al. 2007). In the period between 1978 and 1995 the number of full-time equivalent research positions paid from block-grant support increased from 223 to 361, while those paid from other sources rose from 77 to 472. In 1995 the Ministry of Agriculture was responsible for less than half of all research income at Wageningen University (Roseboom & Rutten 1998). At the same time, the Ministry also privatized the agricultural extension service and its applied

111 Moreover Stichting Technologische Wetenschappen (research council for technical sciences) was founded in 1980.

112 The perception of contract research had radically changed since the 1970s, when contract research was considered 'not done'. In the 1970s influential left-wing student groups fiercely protested against collaborating with industry. In 1972 one of the parties stated in its election programme for the Faculty Council that it aimed to 'eliminate the contract research for the military-industrial complex' (Faber 1993, p233).

research institutes (Maat 2001), which implied a breakdown of their bridging function between farmers and AU.

To a smaller or lesser extent, many of the agricultural research programs that have substituted the lump sum funding express the need for sustainable development. After a turbulent phase, in which there was little consensus about the desired direction of Dutch agriculture and about the contribution of agricultural research, views began to converge in the 1990s around the concept of sustainability[113]. Although it has not completely replaced productivity as the central goal of the agricultural research system, sustainability has acquired a prominent position in arguments to defend investments in academic agricultural research[114].

In the course of years, an increasing share of research funding is governed by network structures rather than top-down steering (Klerkx & Leeuwis 2009, Klerkx & Leeuwis 2008b). The concept of the 'OVO triptych' has been gradually replaced by ideas like 'knowledge network' (M93), 'knowledge system' (R98a), 'innovation system' (WR91) and 'knowledge infrastructure' (P96), which emphasize the importance of feedback mechanisms and interaction between users and producers of knowledge, and to include a wider variety of users that goes beyond the traditional agricultural stakeholders.

Since the mid 1990s a new feature has entered the knowledge infrastructure: consortia in which public and private parties together finance a research program, partly conducted at Wageningen University. In 1997, for instance, the Wageningen Centre for Food Sciences was founded, a collaboration of several knowledge institutes (both within and outside Wageningen University) and food firms to carry out basic research and to generate knowledge for the Dutch food industry. Another example is the research and innovation program TransForum, which started in 2004 and which qualifies itself as a 'new knowledge arrangement', with a mandate to contribute to innovation and transition, and with the explicit intention to realize a transformation in the knowledge infrastructure, to make it more demand-driven and interdisciplinary (T07)(Hoes et al. 2008).

Another significant development regarding the available funding for academic research concerns the increasing emphasis within the national research council on stimulating individual talent, embodied by the 'Vernieuwingsimpuls' grants. Since its introduction in 2000 its relative share in NWO's total budget has increased to about 20% (N10). In the selection of proposals for these grants, the most determining factor is the individual quality of the requesting scientist, assessed mainly using bibliometric criteria.

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113 'Sustainable development, "sustainable" in ecological, economical, social and spatial sense, constitutes a central point of attention in the LNV-knowledge policy' (M93). 'The Agricultural University wants to develop and disseminate scientific knowledge which society needs to meet its demands for sufficient and healthy food and a good living environment for human, plants and animals in a sustainable way' (W92). 'In all sectors sustainability is becoming a self-evident and at the same time challenging part of (scientific) practice' (R96c).

114 Nowadays the notion of sustainability is mentioned in many documents dealing with agricultural research, but not often as its central goal. According to B06 'sustainable entrepreneurship' is one of the goals of agricultural research and 'Sustainable agriculture and fisheries' is an aspect of one the three core areas making up AU's domain (W08).

Moreover, it is important to note that the money that was left to AU increasingly became subject to university research policies. Since 1992 AU has allocated funding for PhD students to research programs, rather than to research departments (W92), which implied a loss of autonomy for departments to choose their own research agendas. Recently, AU has undergone a substantial reorganization in order to facilitate a stronger market orientation. In reaction to several critical reports (P96, R96c), the Minister of Agriculture decided in 1997 to merge its strategic research institutes[115] with the agricultural university into the Wageningen University and Research Centre (WUR). Since 2001, also the 'practice-oriented' agricultural research was integrated into WUR (B06). The result is a large matrix organization consisting of five 'Science Groups' (Plant, Animal, Agrotechnology & Food, Environmental, and Social), which each comprise both (fundamentally oriented) university departments and applied research departments. The aim of this new set up is to facilitate knowledge exchange between the fundamental and applied departments within each Science Group, but also to create synergy between their funding acquisition activities.

To conclude, in the complex dynamics of the funding sources of agricultural research, two trends are visible. First – and most significant - an increasing share of the funding is dedicated to projects that aim to make specific contributions to a more sustainable (or competitive) agricultural sector. Application oriented contract research, earmarked research programs and consortia, often including stakeholder interactions, now constitute a substantial part of all available funding. Second, the personal grants of research councils NWO and STW, which reward individual researchers with excellent publication records, have become a significant source of funding.

Rise of performance evaluations

As other fields of science, the agricultural sciences became subject to performance evaluations during the 1990s. In the 1970s research outcomes were mainly evaluated on the level of chair groups. Only very costly or long-lasting projects needed to be accounted at the faculty level (Faber 1993). In addition, the National Council for Agricultural Research (NRLO) monitored the research activities at AU, but this was oriented at mutual attunement of these activities with the components of the OVO-triptych, rather than assessing their performance (Dijksterhuis & van der Meulen 2007). The increasing pressure on available budgets and the growing need for accountability, however, have put 'quality' and 'effectiveness' on the agenda in the 1980s. In 1986 AU started to experiment with systematic evaluations of research quality and productivity (Faber 1993). The need for systematic quality evaluations was shared by NRLO (R86). In the early 1990s the Association of Dutch Universities (VSNU) constructed a national system for research evaluations of all scientific disciplines, which would also be used by AU (W92). Anticipating this development, AU also set up an internal quality control system, consisting of productivity analysis, impact analysis and an assessment of the content by peer review (W92).

115 Known under the common heading 'DLO' (Directorate for Agricultural Research).

The animal science groups were assessed for the first time in 1999 as part of a national evaluation of the 'Veterinary and Animal Sciences' (V99b), and later in the context of the evaluations of the graduate school Wageningen Institute of Animal Sciences (WIAS) in 2004 and 2009. The first evaluation was based on the second protocol of the Association of Dutch Universities (V98)[116], the latter two on the new 'Standard Evaluation Protocol' (V03). Both protocols prescribe a similar set of four main criteria, namely quality, productivity, relevance, and vitality & feasibility. The most important indicators for the first two criteria are citations and publication numbers. 'Relevance' is a container concept referring to both socio-economic and scientific impact. Vitality and feasibility are measured in terms of management and leadership of the program, funding acquisition, and ability to (re-)adjust the research program. These evaluation reports give recommendations for improvement both on the level of the entire graduate school and on the level of individual research groups.

The research system around 2005

As a result of the various changes just presented, today the agricultural research system is much more complex than in 1975. Wageningen University now interacts with a broader set of organizations (see Table 6.4). The ministry of Agriculture, Nature Conservation and Fisheries is still the main supplier of funds, but the relative contributions of other organizations have increased. Other ministries (responsible for economic affairs and environmental affairs), food and biotechnology companies, and non-governmental organizations fund a significant share of the research activities. The research council 'ZWO' has been replaced by the NWO, which – together with Technology Foundation STW – is less exclusively oriented at basic research, and which supplies a large share of its money in the form of thematic programs. Moreover, new sources of funding are European Framework Programmes (EU FPs) and public-private consortia, such as TransForum and the Wageningen Centre for Food Sciences. NRLO has been reorganized into the InnovatieNetwerk (Innovation Network Green Space and Agrocluster), responsible for conducting strategic foresight studies and for providing a breeding ground to 'system innovations'.

116 Interestingly, a sub-set of all agricultural sciences was evaluated with a novel methodology in a pilot study focusing explicitly on societal impact (R99b), but it remains to be seen whether indicators of societal impact will get a prominent position in systematic evaluations.

Table 6.4 Overview of the of the agricultural research system and their relationships with the agricultural university (AU), around 2005

Organization	Relationship with AU
Ministry of Agriculture, Nature Conservation and Fisheries	Main funding source, in the form of block-grant support and research programs
Other ministries	Funding for application oriented research
Dutch Organization for Scientific Research (NWO)	Funding for fundamental and application oriented research
Technology Foundation STW	Funding for application oriented research
European Framework Programmes	Funding in various forms
Extension Service	Knowledge transfer to farmers
institutes for applied research	Attunement of research agenda (within WUR)
Experimental Stations	Attunement of research agenda
InnovatieNetwerk	Strategic foresight studies; breeding ground for 'system innovations'
Food and biotech industry	Contract research and consortia
Environmental NGOs	Funding for applied research, user committees, consortia

6.6 Relevance in practice: a credibility cycle analysis

In this section we will analyze the work of individual researchers, using the perspective of the credibility cycle.

6.6.1 Animal Breeding and Genetics (ABG)

The institutional changes do not create any complications for animal breeding research. The rise of performance evaluations has been an encouraging experience for researchers in ABG. In all three reports this group received the highest score of the WIAS graduate school (V99b, W04, W09b). Interview data indicates that ABG has managed to adapt to the new demands of funding sources without compromising its fundamental research interest and its effective research strategies that helped it to reach high academic productivity. Thanks to the strong connections with animal breeding firms in combination with the strong publication record, the Wageningen ABG-group has managed to maintain the continuity of research funding. It currently has an impressive portfolio of projects, supported by a variety of funding sources[117]. Because its research has always been connected to animal breeding practice, the shifts in available funding sources have not significantly influenced the degree of application-orientation of the research agenda[118].

The need to connect specifically with the dominant sustainability agenda has not required radical changes in the research activities. Most of the new research questions are being addressed building on available molecular and quantitative genetic approaches. Historically, animal breeding research provided tools and knowledge that firms needed for breeding more productive cows, pigs and chickens. Currently many of their activities are framed not in narrow terms of agricultural productivity, but in terms of a better (or more sustainable) animal production system. This also concerns healthier meat, increasing animal welfare, lowering the environmental impact etc[119]. Some projects dedicated to these new goals were specifically designed accordingly and involve radically new activities that were not present in the old research system. For example, one post-doc researcher aims to develop breeding methods that select for chickens that are less aggressive and more 'sociable'. This requires ethological observation of animals, which traditionally was not conducted within the ABG group. However, many other researchers contribute to the new goals by further developing rather than replacing existing research techniques[120].

In spite of this continuity in the research activities, the shifts in available funding have stimulated ABG researchers to intensify the existing interactions with breeding firms and with the more applied researchers of the ABG division in Lelystad. The ABG divisions of AU and of the applied research institute in Lelystad have even issued a shared promotion flyer to advertise their services. Moreover, together they organize a 'round table meeting' every two years for all Dutch researchers and practitioners involved in animal breeding and genetics for networking purposes. Contacts with the firms help to attract research funding, both from the firms themselves and from public sources such as STW. Animal breeding firms are relatively large knowledge-intensive companies employing a lot of ABG's graduates. They engage in various types of collaborations ranging from bilateral collaboration projects, funded entirely by a company, to public-private consortia and STW-funding in which a company's interest is a necessary requirement, but need not be expressed in financial terms. In the latter case, firms participate in a 'Users

117 The group now consists of over 50 researchers, including PhD students (http://www.abg.wur.nl/UK/staff, accessed on February 18th, 2010)

118 One interviewee even argues that ABG's research has become more fundamental due to the growing size of research programs and the need for further specialization: 'First it was pretty much the application [which dominated] and also a bit of the foundation. And over the course of time the application has not moved ou of sight, but we are more intensively in the backgrounds.' (researcher 7).

119 The brief program design of ABG's quantitative research division states: 'Balanced selection programmes must be developed and implemented that respect the genetic variation in adaptive capacity of animals, as expressed in their resistance to disease, their reproductive rates and welfare.' (http://www.abg.wur.nl/UK/Research/Quantitative, accessed on February 18th, 2010)

120 Interviews 5 and 7.

Committee'. They are updated regularly on the progress of the project and get the opportunity to provide suggestions regarding the direction of the research.

The interactions with breeding firms appear to have some influence on the general direction of ABG's research activities, but they do not strongly restrict the researchers' autonomy[121]. For this reason, the increase of industrial funding does not imply more applied research. Moreover, interactions with breeding firms offer two other benefits, beside access to funding. First, the interactions with firms stimulate the researchers by providing challenging research questions and by turning fruitful research outcomes directly into practical innovations. All ABG researchers experience the direct relevance of the work for breeding companies as a motivating factor[122]. Second, firms provide access to valuable research data. Their large populations of animals enable university researchers to work with much larger datasets than they could do when they had to breed and keep all the animals themselves.

The high evaluation scores (V99b, W04, W09b) suggest that the increased publishing pressure stemming from performance evaluations is not problematic for the ABG group. Asked whether the pressure to publish has increased over the past 30 years, a senior researcher replies:

> *'That has always been the mainstay of everything. Has it increased? Well, there is certainly more attention for it. And of course now it is all more... Thanks to things like Web of Science it is increasingly easy [to measure publications] and that is also important, because I don't see it as a burden. Good work is wonderful, but if it is not being written down, you might just as well not have done it. And here we do have a public mission, we don't have the attitude of: enjoy yourself. I would say: enjoying yourself is wonderful, but do share it with others.' (researcher 7)*

The fact that the interviewee is not even sure whether there is currently a higher pressure to publish, indicates that he does not experience this pressure as threatening. The group manages to publish papers on all types of research projects, regardless of their funding source. Interviews with other researchers of this group[123], however, clearly indicate that more junior researchers do experience a strong pressure to publish. They regard producing many papers, preferably in high-impact journals, as crucial for their future career, either within our outside the ABG group.

121 'Well, in general they fairly tend to give us a free hand in order to see where that leads to and what it yields.' (researcher 6)

122 All four ABG researchers we have interviewed referred to the possible practical applications of their work by breeding firms when asked about their motivation.

123 Interviews 5 and 6.

6.6.2 Animal Production Systems (APS)

From the perspective of the changing societal knowledge demands, the APS research seems very timely. It addresses the need for research on sustainable agriculture and is relatively strongly oriented at practical applications. However, in this field interactions with stakeholders are not always productive in terms of scientific publications. There seems to be a contradiction between the increasing pressure for academic publications and the need to address questions of societal stakeholders.

The scores of APS on the three performance evaluations have been relatively low (V99b, W04, W09b). The most recent evaluation committee judged that 'the societal and cultural impact of the work was excellent but the citations and relative impact were relatively low' (W09b, p39) and it also commented that the volume of publications should increase. These evaluations are taken very seriously by the management of the graduate school and the faculty, and the scores can influence their strategic decisions[124]. Some APS researchers feel torn apart by the difference between criteria used in performance evaluations (and in local management decisions) and the demands of their funding sources:

> *'If you work 4-5 years on something and afterwards the conclusion is: we have contributed little to the livelihood of the local population, then that's is not good either. That is always a sort of clash between multiple interests. You will be judged on whether you will have delivered your work in four years like it has been promised as well, isn't it? We will also be judged on hard criteria just the same. But if you want to get projects again, for example in [the] INREF [program] or at [the] WOTRO [department of NWO], then you also have to come up with other criteria. So it always remains a kind of giving and taking between formal criteria that are imposed on you by your boss, so to say, and the criteria that others impose on you.' (researcher 1)*

However, another interviewee (researcher 15) argues that the practical impact of the APS activities is also appreciated by the wider Wageningen animal science community. Possibly more than at other universities, researchers at AU also give recognition to their colleagues for practical successes, even if they have not yielded impressive academic publications. This probably relates to the intrinsic motivation many of them feel to contribute to the development of agricultural production, in the Netherlands or abroad[125]. This suggests that the informal reputation of a researcher (or research group) can still be enhanced by contributions to human welfare or sustainable agriculture, but the formal reputation that determines funding decisions of university managers is dominated by bibliometric criteria.

124 'This year there is an assessment again, and its results will be pretty important this year, because it actually determines the opportunities you will get in the future. Your research is being managed through research schools. And if you get a low assessment, then that will have an impact on what you can do in the future.' (researcher 1)

125 All researchers in APS and ABG that we asked about their motivation referred to the possible applications of their work in agricultural practice. Researchers in CB turned out more motivated by a quest for fundamental understanding.

With regard to money, APS depends to a large extent on funding sources with a practical orientation. The opportunities at NWO are limited, due to APS's interdisciplinary and application oriented research portfolio.

'The final assessment at NWO, I always feel, is still very much based on the old norms. Then they say: "a great interdisciplinary project, but we don't have a clue what you are going to do". I also notice that I am often evaluated by people lacking interdisciplinary or application oriented education.' (researcher 12)

A further limitation to the access to NWO funding is the problem that few of the APS researchers have an exceptionally strong publication record.

The experiences with practically oriented projects vary, depending on their duration. One researcher, who is involved in many short research projects, feels that he is forced to engage in consultancy work rather than in-depth research that is suitable for academic publications. But another, who carries out more substantial projects, argues that practical and scientific goals can be fruitfully combined:

'Well these people are paying you, so they do have a certain interest. And so the challenge is to... then you make a proposal and when you go there to present it.... the challenge is to position it in such a way that you very clearly sketch their interest, but that you're still able to do what's important for you to do. That is the challenge. And then you end up in a win-win situation.' (researcher 3)

APS intensively interacts with 'agricultural practice', in various forms. These interactions help in gaining credibility, but not in such a fruitful way as in the ABG group. APS researchers have always had contacts with farmers as part of the research strategy. The APS-approach involves data collection in the form of monitoring or experiments at individual farms. Other interactions serve as part of funding acquisition, and they have become more important due to the decrease of block-grant support. Animal feed industry and applied research institutes such as Gezondheidsdienst voor Dieren and Animal Science Group Lelystad are of increasing importance as sponsors of the research activities of APS. However, the collaboration with Animal Science Group Lelystad is much less intensive than in the case of ABG.

Publishing sufficient numbers of articles in scientific journals seems more difficult in APS than in the other two fields, for several reasons. The first is the relatively practical orientation of many research projects. They often take place in the global south and serve local development goals, the relevance of which for scholarly debates is not self-evident. The second problem is that some of the funding comes in very small portions (e.g. the projects for the Science Shop). Combining the outcomes of several projects in order to produce a scientific paper is quite challenging and requires additional time which is difficult to get funded[126]. The third factor complicating scientific publishing is the fact that APS does not belong to an established discipline with a strong research tradition[127]. The APS approach combines elements of various schools, such as systems research, environmental science and development studies. For this reason there are few journals available as a stable platform for publishing results. APS researchers have numerous frustrating experiences with peer reviews of their manuscripts, in which they feel being evaluated with inappropriate standards. The development of a visible research tradition is complicated by the heterogeneity of APS's research activities. As the group combines research in tropical areas with research on Dutch agriculture, using a variety of methodologies, the group's members each have their own networks and audiences and they do not collectively contribute to the foundation of a new disciplinary tradition.

6.6.3 Cell Biology

Of the three fields under study, cell biology has the strongest tradition of fundamental research. When compared to ABG and APS, the research activities in agricultural cell biology are less strongly connected to practical applications: 'It is fundamental, but also with a wink to applicability. It's not that we say: if we are going to investigate this, we can apply it already within four years. That's not how it works. It is more like: we understand how it works, and we also understand of certain diseases what kind of molecular foundation it has. And then, once we understand it, we can start looking for a solution. That is step two.' (researcher 11) The primary interest of these activities is to enhance the fundamental understanding of biological mechanisms on the cell level. On the longer run, this understanding can contribute to more effective (or sustainable) fish production and to improved medical therapies to various diseases, either for humans or for animals.

Although the CB group in Wageningen scored high in the 2009 evaluation (W09b), it is generally difficult for the groups in cell biology to reach impressive scores on performance evaluations, because they have limited access to journals with a high impact factor. These journals are dominated by research with a medical orientation and publish few studies on agricultural animals:

> *'A clinical immunologist, or a medical immunologist who is working on mouse, often can reach much higher journals and therefore reach higher citation indices than a veterinary immunologist who is part of a much smaller scene. A veterinary immunologists is already happy with attaining an impact factor of 1.5. An*

126 For this reason one researcher (with an impressive publication list) chooses not to engage in projects that are so short or applied that they do not provide good perspectives on scientific publications in the first place.

127 The APS group also mentions the difficulty to publish interdisciplinary work in their SWOT analysis for their peer review evaluation (W09b, p39).

immunologist at a medical faculty or a biomedical faculty is not going to do it for a paper in a journal [with an impact factor] under 7.' (researcher 9)

For similar reasons, the agricultural cell biologists have a hard time getting funding from the research council NWO. At the life sciences department they have to compete for grants with researchers with a more fundamental or medical orientation and a more impressive publication list. Apart from the relative disadvantage in terms of publications, researchers also complain that referees and program committees of the research council do not consider research done on agricultural animals of much added value. Outside NWO it is also increasingly difficult for CB to find funding. Unlike ABG, CB does not have a steady relationship with a particular industrial sector, as the possible applications of its research and the labor market of its graduates are quite diverse[128].

In order to overcome this situation, the two groups pursue different strategies. The CB group at Utrecht University is gradually shifting its activities to more basic research, in order to publish more in high-impact journals which should help to get more NWO funding (or similar EU-grants) on the long run. Although embedded in a veterinary faculty, the research is increasingly concerned with human medicine. The Wageningen CB group spreads its activities over various themes, combining fish, livestock and human cell biology (and immunology), in accordance with the available funding. Focusing too much on human or medical research is not legitimate within WIAS, but it seems necessary to a certain extent in order to reach sufficient academic productivity. The following quote from the most recent evaluation report illustrates that the human cell biology research line is not considered immediately appropriate, but does have strategic value:

'The human research does not have a good 'fit' within WIAS. However, this is the main area of interest for the Chair, the work is externally funded and so the costs to WIAS are relatively modest, and there are tangible benefits for the remaining programmes, helping these to stay at the forefront of the field.' (W09b, p41).

The CB researchers do not interact intensively with societal stakeholders. The Utrecht interviewees report hardly any interaction outside the scientific community at all. The researchers in Wageningen have regular contact with firms that (co-)fund some of their projects or that participate in Users Committees or Feedback Groups of projects funded by public sources such as STW or ZonMW. These interactions are limited, however, and the 2009 evaluation committee commented that the 'downstream potential' of the CB research should be exploited better (W09b, p41).

128 The basic research tradition of the CB group in Wageningen can be explained by its origin. The group was founded in the early 1970s when a new education program in (general) biology was set up. As this education program did not have a specific agricultural orientation, scientists were hired with a fundamental biological background (interview researcher 9).

Although all CB researchers experience a strong pressure to publish sufficient papers, their reactions to this pressure vary, depending on their personal ambitions. One postdoc researcher argues:

> *'Personally, I don't have a strategy to publish as much as possible. I simply do my job. I don't want to write as many papers as possible, I find it more important to deliver sound research.' (researcher 8)*

But he admits that this stoicism is probably not beneficial for his future career. Another postdoc shows opportunism in his reaction:

> *'Lately I have focused pretty much on minimal publishable units. (...) I think you have to keep your eyes open very well: is this already publishable or not? Once you know sufficient, what kind of journal would be suitable, can it already go? That is sort of my latest strategy.' (researcher 10)*

He pursues this strategy in order to avoid creating 'gaps' in his publication list, which may inhibit his chances on a permanent research position.

6.7 Discussion

The aim of this paper is to increase the understanding of the relationship between macro-level changes in the governance of science and research practices in different scientific fields. We have observed two profound changes in the institutional environment of academic agricultural research in the Netherlands. Dutch agricultural science used to be part of a stable and smoothly running system, oriented at the enhancement of agricultural productivity through modernization. The relevance of the academic research was self-evident, thanks to the strong connections between research, extension and education activities. Since the 1970s, however, the available funding has shifted, and as a result a greater share of the available money has become dedicated to research that promises to make specific contributions to a more sustainable and competitive agriculture. At the same time, performance evaluations have developed into a systematic element of the research system, and provide input to decision making both by university managers and by external funding sources.

Our credibility cycle analysis shows that these developments have two generic consequences for daily research practices in all three fields of animal sciences we have studied (Figure 6.1). First, performance evaluations intervene in the conversion of articles into recognition. Their main consequence is that recognition is formalized and expressed in numerical values. Evaluations give scores to the work carried out by research groups. Although these are not the only indicators used, bibliometric indicators, like the numbers of publications and citation scores are the most visible, tangible and objective indicators. The development of indicators for systematically measuring social or economic impact is currently still in the pilot-phase (Spaapen et al. 2007). Besides, such societal indicators, which are measured to some extent under the criterion 'relevance', are usually not something to worry about in animal science. Most of the research lines are so strongly connected with agricultural practice that they are almost relevant by definition[129]. For these reasons, the bibliometric indicators tend to dominate in the interpretation of the evaluation reports, and in the discussion of their implications. As a result, bibliometric indicators are increasingly used to express a researcher's quality, also on the individual level. In other words, researchers earn recognition increasingly based on a quantitative perspective of their output:

> *'This pressure [to publish] is self-imposed. But if you do not do this, and you work for a longer period without projects, and without additional support from PhD students, postdocs, then at a certain moment you will contrast sharply with an other. Even if you are pretty good.' (researcher 8)*

As this quote illustrates, the content of a researcher's papers matters much less than the number of papers, number of citations, the 'impact factor' of journals one publishes in, or even the so-called Hirsch-index[130].

The second generic effect concerns the way recognition can be turned into money is changed by the shifts in available funding sources and their respective demands. In the 1970s research groups could count on sustained financial support, as long as they performed more or less adequately. AU supplied a basic level of funding for permanent staff, but also for some junior researchers and the equipment required to conduct a number of research projects. Researchers could attract additional funding from external sources, but this was not necessary for survival. Nowadays the block-grant support is hardly enough to finance the permanent staff of a research group. In order to secure their own position, researchers need to continuously write proposals and negotiate with stakeholders that may be willing to pay for contract research. This development implies that recognition from only a limited group of scientific peers is not sufficient for the acquisition of funding. One needs to convince a broader set of stakeholders, outside university, of the value and viability of one's research agenda. It has become more important to explicitly promise a practical impact. In particular, researchers need to align their plans

129 This is illustrated by the documentation WIAS has prepared for the 2009 evaluation (W09a), which is a 320-page report consisting only of scientific publication lists and academic CVs. It does not address the economic or societal impact of the WIAS research at all.

130 In Wageningen an 'H-index Dinner' with the university president seems to have taken place in 2009, for which the researchers with the highest Hirsch-index of each department were invited.

with dominant visions on the future of Dutch agriculture. There are much more funding opportunities for research that promises to contribute to a more sustainable and / or competitive agricultural sector than for research that only promises to enhance fundamental understanding (or that promises a contribution to the outdated agenda of productivity enhancement in the narrow sense). For research lines that can not be directly connected to stakeholder demands, the only escape route seems to be the 'Vernieuwingsimpuls' funding of NWO. This type of funding does not require promises of practical applications, as the selection is mainly based on personal 'excellence'. However, due to the strong competition, one needs an extremely strong publication record to be eligible for these grants.

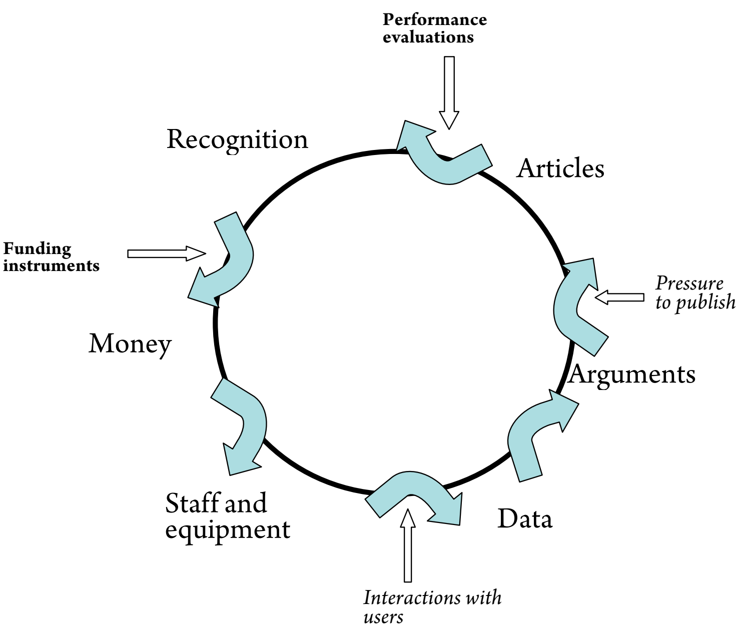

Figure 6.1 Today's credibility cycle of Dutch agricultural science. The institutional changes have two generic effects on the credibility cycle, which are indicated in bold. At two other positions in the cycle, effects can be observed that vary across the three fields studied (indicated in italic).

But has the daily work of university researchers changed? Do they interact more with 'users' of their knowledge, in order to contribute to the agenda of sustainable agriculture? Our analysis shows that this differs strongly across fields. Both the fields ABG and APS have increased their interactions with stakeholders outside university, but these interactions are of a different nature. In ABG researchers have frequent contact with breeding firms in order to select challenging research issues, to collect data, and to acquire research funding. In APS, however, contacts with farmers only contribute to data collection, while contacts with applied research institutes and with NGOs help to get access to funding. In Cell Biology, interactions with 'users' have decreased rather than increased, as there is a tendency to shift towards more fundamental research issues that are not of immediate interest to the agricultural sector.

In order to explain the differences across the fields three issues deserve attention. The first relevant factor is the agreement between leading research questions and the organization of the field. In all three fields a certain change in the leading research questions can be observed in the period studied, but the degree to which the new questions are compatible with disciplinary infrastructures varies. The question that is leading ABG today, i.e. how to breed animals for more sustainable agriculture, can be addressed by approaches similar to the ones addressing the former question oriented at more productive animal husbandry. The current activities can build on existing disciplinary infrastructures, such as scholarly journals and scientific networks. For this reason, ABG can accommodate the institutional changes without profoundly changing or reorganizing its own activities. The situation is different in APS. In this field there appears to be a mismatch between the societal knowledge demands and the existing disciplinary institutions to support the fulfilment of these demands. The new leading question of designing and monitoring sustainable agricultural systems requires a combination of knowledge dealing with animal feeding, animal housing, and animal behavior, which have traditionally been separate. Moreover, the cooperation of non-scientific actors such as farmers is also indispensible in APS's systems approach, but these do not always see the immediate benefits of this work. This transdisciplinary approach is currently developed in the APS group but it is not yet supported by a clearly visible international research community, scholarly organizations and established scientific journals. Journals with a respectable tradition (and a high impact factor) stem from the era of the old research system, which was oriented at productivity enhancement.

The second important issue is the readiness with which scholarly contributions can be measured by bibliometric indicators, in particular the journal impact factor. Apart from a strengthening of mutual competition, the application of bibliometric evaluations does not appear to have strong implications for ABG. Researchers have become more conscious of their bibliometric performance, but this does not seem to have a significant impact on their choice of research activities. In APS and CB, however, which have more diverse and less coherent research portfolios, bibliometric indicators give uneven rewards across different sub-fields[131]. In APS studies on western agro-systems are generally more suitable for publication in high-impact journals than studies on tropical areas. Similarly, high-impact journals in CB favour mouse or human tissue as a research model over agriculturally relevant animals like chicken. This situation can create a tension between research directions that are fruitful in bibliometric terms, and directions addressing societal demands.

The third explanatory variable concerns the characteristics of societal stakeholders. ABG has an unequivocal relationship with a homogeneous set of wealthy stakeholders. The knowledge-intensive breeding firms take such a serious interest in fundamental knowledge (and in new generations of highly skilled people) that they sponsor substantial research projects granting considerable autonomy to the

131 The lack of cross-disciplinary comparability is a well-known limitation of bibliometric analysis (Leydesdorff 2008).

scientists. For this reason, the increased interaction of ABG with animal breeding firms and with ASG Lelystad turned out not in conflict with the pressure for (more) scientific publications. Such a steady relationship with a homogeneous group of stakeholders is lacking in CB and APS. CB does not interact intensively with any stakeholder group. The APS group has increased its interactions with societal stakeholders, but, due to the diversity of their interests, this may threaten the viability of the group. Most projects for Dutch NGOs and for international charity foundations are too small and too diverse to form the basis of a convincing contribution to scientific literature. APS's stakeholders appear not wealthy enough to be able to afford investing in fundamental research projects that may only give returns on the longer term. Moreover, APS's sustainable systems approach cannot count on univocal support of farmers and agricultural organizations as it often challenges their current practices.

Our findings regarding the institutional changes in the academic research system suggest that scientific criteria still dominate in academic quality control. This confirms Leeuwis' suggestion that applicability and practice orientation are not important evaluation criteria in the current reward system of Dutch agricultural science (Leeuwis 2000, Klerkx & Leeuwis 2008a). These observations do not corroborate the shift to 'quality monitoring' that has been suggested by Hemlin and Rasmussen (2006). Our analysis does show an increasing importance of quality assessments in general, and also an increasing intensity of interactions between university researchers and stakeholders, but not a uniform shift towards more societal criteria and the inclusion of users in evaluation processes. In our study the inclusion of stakeholders has become only visible in the allocation of research funding. Performance evaluations turn out to be dominated by bibliometric criteria that stimulate an inward looking perspective in which the role of societal stakeholders is marginal. This can be explained by the limitations of the available indicators to evaluate social or economic impact[132].

With regard to science and innovation policy, this study shows that the governance of science is most effective when it adopts a long-term perspective. In the end, the unique quality of university research remains producing fundamental insights, which will help to solve practical problems or create economic benefits on the long rather than the short term (Kronjee & Nooteboom 2008). Addressing new knowledge demands requires the availability of sufficient supportive disciplinary infrastructure. If knowledge demands change much faster than the supportive infrastructure is able to adapt to, researchers will not be able to adequately address these demands. For new fields it is difficult to establish a stable position, especially due to the high and continuous pressure to deliver excellent academic performance. The case of ABG has shown that the most fruitful collaborations between academic research and societal stakeholders develop when the latter are ready to invest in knowledge that will only yield profits on the longer run. In fields with less generous and powerful stakeholders, scientists struggle to satisfy the needs of application-oriented funding sources and still reach high scores on bibliometric evaluations.

132 This may change in the near future, as there are currently attempts to develop indicators to systematically analyze the societal impact of academic research as well (Spaapen et al. 2007).

Appendix C (chapter 6). Documents studied[133]

Abbreviation	Publisher / Author	Year	Title	City
W68	Landbouwhogeschool Wageningen	1968	De Landbouwhogeschool op een Keerpunt: Jubileumboek ter gelegenheid van het 50-jarig bestaan van de Landbouwhogeschool Wageningen	Wageningen
R71	NRLO	1971	Waarheen met de structuur van het landbouwkundig onderzoek?	Den Haag
R72	NRLO	1972	Organisatiestructuur Landbouwkundig Onderzoek en Achtergronden van haar Totstandkoming	Den Haag
M74	Ministerie van Onderwijs en Wetenschap	1974	Nota Wetenschapsbeleid	Den Haag
A79	Academische Raad	1979	Beleidsnota Universitair Onderzoek	Den Haag
R86	NRLO	1986	Bevordering van de kwaliteit van het landbouwkundig onderzoek	Den Haag
I89	ISNAR	1989	The Agricultural Research-Technology Interface: A Knowledge Systems Perspective	Den Haag
R89	NRLO	1989	Rapport 89/32: Commercialisering van kennis en het functioneren van het landbouwkennissyteem	Den Haag
WR91	WRR / NRLO	1991	Technologie in de landbouw: Effecten in het verleden en beleidsoverwegingen voor de toekomst	Den Haag
W92	Landbouwuniversiteit Wageningen	1992	De strategie richting 2000: Strategisch Plan Landbouwuniversiteit Wageningen	Wageningen

133 This table only lists primary sources, which are not listed in the bibliography of this paper. Abbreviations:
ISNAR: International Service for National Agricultural Research
KNAW: Koninklijke Nederlandse Akademie voor de Wetenschappen (royal Dutch academy)
NRLO: Nationale Raad voor Landbouwkundig Onderzoek (national council for agricultural research)
NFU: Nederlandse Federatie van Universitair Medische Centra (federation of Dutch academic medical centres)
NWO: Nederlandse organisatie voor Wetenschappelijk Onderzoek (Dutch organization for scientific research)
OCV: Overlegcommissie Verkenningen (committee for foresight studies)
VSNU: Vereniging voor Samenwerkende Nederlandse Universiteiten (association of Dutch universities)
WRR: Wetenschappelijke Raad voor Regeringsbeleid (Scientific Council for Governmental Policy)
WIAS: Wageningen Institute of Animal Sciences

R92	NRLO	1992	Rapport 92/16: Implicaties van het begrip duurzame ontwikkeling voor de Nederlandse landbouw	Den Haag
M93	Ministerie van Landbouw, Natuurbeheer en Visserij	1993	LNV-kennisbeleid: Eenheid in Verscheidenheid	Den Haag
WR94	WRR	1994	Duurzame risico's: een blijvend gegeven	Den Haag
M95	Ministerie van Landbouw, Natuurbeheer en Visserij	1995	LNV-Kennisbeleid tot 1999	Den Haag
N95	NWO	1995	Trends in Wetenschap: Achtergrondnota bij beleidsnota 1996-2001	Den Haag
P96	Peper, Bram	1996	Duurzame Kennis, Duurzame Landbouw: Een advies aan de Minister van Landbouw, Natuur en Visserij over de Kennisinfrastructuur van de Landbouw in 2010	
R96a	NRLO	1996	Rapport 96/18 Kennisproduktie als wetenschap en praktijk: aard en verandering van de landbouwwetenschappen	Den Haag
R96b	NRLO	1996	Rapport nr. 96/15: Essays voor de verkenning "Landbouwwetenschappen in 2010: de positie van de LUW"	Den Haag / Amsterdam
R96c	NRLO/OCV	1996	Wageningen in profiel. Landbouwwetenschappen in 2010: de positie van de LUW	Amsterdam
R97	NRLO	1997	Rapport 97/17: Uitdagingen en concepten voor toekomstig landbouwkennisbeleid	Den Haag
N97	NWO	1997	Inspiratie en sturing van wetenschap: NWO en maatschappelijk georiënteerd en multidisciplinair onderzoek	Den Haag
R98a	NRLO	1998	Rapport 98/1: Een maatschappelijk perspectief voor de landbouw: Kennis- en innovatieopgaven voor de toekomst	Den Haag
R98b	NRLO	1998	rapport 98/20: Kennis- en innovatieagenda: Ambities voor de 21e eeuw	Den Haag
V98	VSNU	1998	Protocol 1998	Utrecht
M99	Ministerie van Landbouw, Natuurbeheer en Visserij	1999	Kracht en Kwaliteit: het LNV Beleidsprogramma 1999-2002	Den Haag

R99a	NRLO	1999	Rapport 99/1: Wetenschap en Technologie: Kansen voor de Agrosector, Groene Ruimte en Vissector	Den Haag
R99b	NRLO	1999	Rapport 99/12: De evaluatie van universitair onderzoek: methodiek voor het incorporeren van de maatschappelijke waarde van onderzoek	Den Haag
R99c	NRLO	1999	Rapport 99/17: Innoveren met Ambitie: Kansen voor de Agrosector, Groene Ruimte en Vissector	Den Haag
V99a	VSNU	1999	Assessment of Research Quality: Agricultural Sciences	Utrecht
V99b	VSNU	1999	Assessment of Research Quality: Veterinary and Animal Sciences	Utrecht
V99c	VSNU	1999	Biology: Assessment of research quality	Utrecht
NW01	NWO	2001	Thema's met Talent: Strategienota 2002-2005	Den Haag
V02	VSNU	2002	Assessment of Research Quality: Chemistry and Chemical Engeneering	Utrecht
I03	ISNAR	2003	Trends in the Organization and Financing of Agricultural Research in Developed Countries: Implications for Developing Countries	Den Haag
V03	VSNU, NWO and KNAW	2003	Standard Evaluation Protocol 2003-2009 for Public Research Organizations	
L03	Landbouw-Economisch Instituut	2003	Van OVO naar VOVI: Nieuwe institutionele arrangementen voor kennisverwerving en -ontwikkeling van agrarisch ondernemers	Den Haag
E04	Ministerie van Economische Zaken	2004	Actieplan Life Sciences	Den Haag
O04	Ministerie van Onderwijs, Cultuur en Wetenschappen	2004	Focus op Excellentie en meer waarde: Wetenschapsbudget 2004	Den Haag
V04	VNO-NCW, VSNU and NFU	2004	Beschermde kennis is bruikbare kennis: Innovation Charter bedrijfsleven en kennisinstellingen	
W04	WIAS	2004	International Peer Review of the WIAS Graduate School	Wageningen

B06	Berenschot	2006	Groene Kennis (de)Centraal? Evaluatie van de Wijzigingen in het Landbouwkundig Onderzoek	Utrecht
N06	NWO	2006	Wetenschap Gewaardeerd! Strategienota 2007-2010	Den Haag
T07	TransForum	2007	Jaarverslag TransForum: Duurzaam ondernemen met kennis	Zoetermeer
W08	Wageningen UR	2008	Science for Impact: on science, society and business	Wageningen
W09a	WIAS	2009	Documentation over 2003-2008 for Peer Review of the Graduate School WIAS	Wageningen
W09b	WIAS	2009	Report of the International Peer Review of the Graduate School WIAS	Wageningen
N10	NWO	2010	Begroting 2010 en meerjarencijfers 2011 tot en met 2014	Den Haag

Chapter 7.

Conclusions and discussion

7.1 Brief recapitulation

This thesis deals with the *struggles for relevance* of university researchers, their efforts to make their work correspond with ruling standards of relevance, and to influence these standards. It was written against the background of intensive debates about transformations in the knowledge infrastructure. Under labels such as 'Mode 2 knowledge production', 'Post-academic science', and 'The entrepreneurial university', prominent scholars have claimed that university research is increasingly oriented to practical applications in industry or policy, and that academic reward systems increasingly stimulate research that is relevant to society and the economy rather than purely scholarly contributions (see Chapter 2 for a systematic review). Although these diagnoses of the changing nature of university research have been quite influential both in scholarly thinking and in policy circles, they are also surrounded by uncertainty and confusion.

I have aimed to contribute to the understanding of transformations in the knowledge infrastructure by investigating struggles for relevance in three scientific disciplines in the Netherlands. After the introductory chapter, which explained this aim, specified the leading research questions, and introduced the methods and the general theoretical framework, I started with 'Re-thinking new knowledge production' (Chapter 2), a systematic analysis of the existing literature on changing science systems mentioned above. This review yielded a number of problems related to the notion of Mode 2 knowledge production and questions about its attributes transdisciplinarity, reflexivity and novel quality control. These questions served to further sharpen my perspective and to choose priorities. In Chapter 3 I developed a preliminary theoretical framework consisting of the credibility cycle and a contract between science and society, to guide my empirical studies. The following three chapters used and enriched (elements of) this framework in the analysis of case studies of Dutch chemistry, biology, and agricultural science. In each case I addressed similar questions about changing struggles for relevance. Chapter 4 reported on the chemical case study and explored the different roles of practical applications in the credibility cycles of catalysis, biochemistry and environmental chemistry. Chapter 5 compared two biological fields, paleo-ecology and toxicology, paying specific attention to growing multidisciplinary collaborations.

In Chapter 6 three fields of agricultural science were analyzed, again focussing on the changing role of stakeholder interactions in academic research practices.

This final chapter will provide a further reflection on the material presented in the previous chapters. The main research question of this thesis is:

> *How to understand changes in the struggle for relevance of Dutch academic researchers in chemistry, biology and agricultural science, in the period 1975-2005?*

In Chapter 1 I have specified three sub-questions:
1. What changes can be discerned in their struggles for relevance?
2. How can the similarities among the changes observed in different scientific fields be explained?
3. How can the differences among the changes observed in different scientific fields be explained?

These sub-questions will be subsequently addressed in the following three sections. At the end of Section 7.4 I will provide an answer to the main research question. In Section 7.5 I will reflect on the main concepts used in this thesis. Section 7.6 takes stock of the contributions this work makes to the literature about changing science systems and presents some recommendations for further research. I will close with a consideration of the policy implications of this study (7.7) and some concluding remarks (7.8).

7.2 Changes in the struggle for relevance

The starting assumption of this thesis was that academic researchers struggle for relevance: they try to make their work correspond with ruling standards of relevance while simultaneously influencing these standards (see Section 1.2). My first sub-question was what changes can be discerned between 1975 and 2005 in three scientific disciplines in the Netherlands. In this section I will answer this question, based on the empirical material presented in Chapters 4-6. I have observed common trends at three steps in the credibility cycle (see Figure 7.1), at the acquisition of data, recognition and money, respectively.

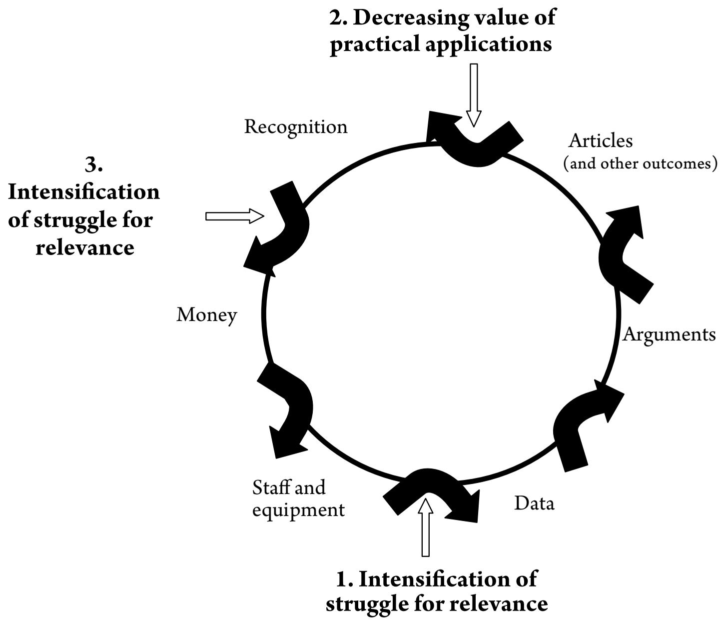

Figure 7.1 The three common trends in the struggles for relevance of eight fields of natural science, depicted in the credibility cycle (adapted from Latour and Woolar (1986)).

Intensification of struggle for relevance during data collection

First, in most fields the struggle for relevance during the collection of data (the actual research process) has intensified. Over the past few decades, the role of societal stakeholders in this process has grown. Nowadays, some researchers even collaborate so intensively with knowledge users that they conceive them as 'partners':

> *'We are free and they are free to go where they want. But you simply feel that they care about a long-lasting relationship. So we don't have a collaboration that is project-oriented. Then you would talk about customers. We talk about partners, [when we talk about] the firms.' (researcher A7).*

In agreement with the claims about the rise of 'Mode 2 knowledge production', an increasing share of all Dutch academic research is conducted in 'the context of application'. In several fields interactions with possible users of research outcomes were already common practice, but in general their frequency and salience have grown. In fields like catalysis, ABG, and toxicology most projects are supervised by an industrial sponsor or by a 'users committee'. These receive a regular update about the progress and provide feedback for future directions. In other fields, like APS and environmental chemistry, it has become common to conduct academic research projects in collaboration with applied researchers employed by public research institutes or private R&D labs. These types of interactions with stakeholders have increased the awareness of scientists of potential applications of their work and – to some extent - this awareness influences their choices on the lab floor. There are also fields in which researchers still hardly interact directly with societal stakeholders, in particular biochemistry and cell biology.

Decreasing value of practical applications as a source of recognition

The second major development that was visible in all fields I have studied relates to the way scientists earn recognition: on average, the value of practical applications as a source of academic recognition has decreased. Asked whether practical applications can help to get peer recognition, a biologist replied:

> *'Maybe it would count a bit, but also here the scientific content comes first and if it is applicable by accident, then that is so much to the good. I mean, it is like the cream on the pudding, but… It makes it more fun, yes. But it is not…' (researcher B20).*

Since the 1970s recognition has become more and more linked to the production of scientific papers (see Text box 7.1 for illustrations). Notably, this trend was hardly mentioned in the dominant 'diagnoses' of the changing science system (see Chapter 2). Writings about Academic Capitalism, post-normal science or Mode 2 knowledge production seem to neglect this salient development. Scientists are increasingly under pressure to be productive, both in quantitative and qualitative terms. In the credibility cycle, recognition has become so strongly based on numerical indicators of scientific productivity, that academic researchers simply face the choice 'publish or perish'. A strong publication list has become a crucial condition in order to qualify for particular kinds of funding, in particular grants from national research councils and from EU Framework Programmes. Moreover, scientific productivity is the main topic discussed during individual performance interviews, and it is also the main criterion for selecting candidates for academic positions. In metaphorical terms, a publication list needs length and depth. One needs to write many papers, and besides it is important that a certain share of one's

Text box 7.1 Interview quotes illustrating the increasing value of scientific publications as a source of recognition

'In the past it was like… yes… you knew that you were doing good stuff and it didn't matter so much whether you would have one or two publications a year. Nowadays that wouldn't be a lot.' (researcher B18).

'Yes, that is the most important principle: publishing in the best possible journals.' (researcher C13).

'First of all you want to make your findings publicly known. It is a way to get recognition from your peers. Second it is also dire necessity, to be able to give continuity to your funding. Because if you don't have publications… it is the way for the outside world to assess your group.' (researcher C2).

papers is of excellent quality. The latter is only partly evaluated by the content of a paper; it is usually measured in terms of the impact-factor of the journal it is published in, or the number of citations it has received. Recently, a new indicator combining both aspects of productivity has gained prominence, the so-called Hirsch-index. In spite of its methodological limitations (Costas & Bordons 2007, Glänzel 2006), this indicator has become popular both in informal communication, and in official evaluation processes[134].

Our interview data shows that scientists anticipate on the importance of scientific publications during the whole credibility cycle. Data collection is organized in such a way to optimize publication prospects. Some scientists choose particular strategies because they either need to publish in a high-impact journal (researcher B20) or because they perceive the need for a larger number of papers (researcher A10), depending on the current status of their publication list. In addition, several 'cheating' strategies are pursued, like the formation of writing 'task-forces' whose members grant each other co-authorships without any actual collaboration (researcher C5), submitting several papers each highlighting different aspects of the same research project, or dividing a particular contribution into its 'smallest publishable units' (researcher A10).

Over the years, publication achievements have (further) pushed away social or economic impact as a source of academic recognition. As I will specify below, practical applications are not always in competition with scientific papers, but in most fields they are. In the selection of manuscripts for publication in scientific journals, the societal relevance of the reported research does not play a significant role. Obviously, editors and peers decide about to publish papers based on purely scientific considerations, like consistency, novelty value and methodological quality. In many fields there is even a trade-off between research projects that are of high societal relevance and projects that are likely to result in (many) high-impact scientific papers.

Intensification of struggle for relevance in context of funding acquisition

The third generic trend that was visible in the period 1975-2005 concerns the intensification of the struggle for relevance in the context of funding acquisition. My findings confirm the claims in studies about Academic Capitalism (Slaughter & Leslie 1997)(also see Section 2.3.5 of this thesis) that funding acquisition has become an increasingly competitive affair. Earning sufficient income for continuing one's research activities is not anymore self-evident, but it has become the result of active acquisition efforts. The process of acquiring money for continuing and expanding research activities has become laborious and time-consuming. My interview data raise the impression that it has become common for senior re-

134 For example, in the documentation prepared for the most recent evaluation of the Wageningen Institute of Animal Sciences, the Hirsch-index of every scientist was listed (WIAS, 2009, 'Documentation over 2003-2008 for Peer Review of Graduate School WIAS', Wageningen).

searchers to spend between 10 and 20 % of their time on networking, exploring funding options, writing proposals and negotiating contracts.

Promises about practical applications often play a central role in the selection of project proposals. In other words: the meaning of relevance has changed. In all three disciplines I have studied, I could identify changes in the meaning of relevance (see Table 7.1). In chemistry and biology, in the 1970s relevance was mainly expressed in terms of the education of new generations of researchers, but later it was increasingly defined as producing strategic knowledge that can be used to improve industrial products and processes, to create medical innovations or to design public policy. In addition, nowadays academic involvement in the valorization of knowledge by creating spin-off firms, exploiting patents or consultancy services is also regarded as a sign of relevance. In the case of Dutch agricultural science practical applications have always been central in the notion of relevance. However, the content of this notion has shifted from contributing to productivity enhancement to contributing to sustainable farming.

Table 7.1 Shifts in the meaning of relevance in three scientific disciplines

	Chemistry	Biology	Agricultural science
Main conception of relevance ~ 1975	Education	Education	Productivity enhancement of agricultural production
Main conception of relevance ~ 2005	Industrial innovations Valorization	Industrial innovations Medical applications Environmental policy Valorization	More sustainable agricultural production

In all fields the scientists I have interviewed report that aligning their work with the knowledge needs of societal stakeholders has become increasingly important for gathering sufficient funding. In the credibility cycle, expected societal benefits strongly catalyze the conversion of recognition into money. Based on a certain amount of recognition (for example, expressed in one's publication list), the same researcher will more readily acquire funding if (s)he manages to convincingly specify the societal value of a proposed research project:

> *'Yes. It is much easier to get money, there are much more possible sources to get money, if you have something that is relevant to society.' (researcher C12)*

Still there are some possibilities to get funding for research without promising societal benefits, most at the national research council NWO. But for only a few of the 47 researchers I have interviewed these provide a substantial share of their budget.

The three trends just described are not necessarily related; the latter two even seem to point in opposite directions. In general, funding acquisition has become increasingly dependent on expected societal benefits, while peer recognition has become increasingly dependent on academic achievements. However, no general observations can be made about the interplay of these two developments, as the relationship between practical applications and scientific productivity turns out to vary strongly across fields. In the following the differences among the changing struggles for relevance in different scientific fields will be presented, including a further exploration of the possible tension between practical applications and scientific productivity.

Changing struggles in different scientific fields

To a certain extent the three common trends just presented were visible in all eight fields (in the Netherlands). However, a closer look also reveals significant differences across scientific fields in the manifestation of these changes. Surprisingly, the differences among fields within each discipline I have studied turn out even more significant than the differences among the aggregated disciplines themselves. Some interesting conclusions can be drawn from the comparison of chemistry, biology and agricultural sciences. But, thanks to the diversity observed within each discipline, the most powerful insights can be gained by comparing individual fields.

In my findings, four types differences can be discerned among the eight fields studied, regarding the changes in their struggles for relevance (see Table 7.2). The first three are directly linked to the general trends just described. A fourth concerns the tension that has arisen in some fields due to the combination of these trends. First, although the role of stakeholders in the academic research process has generally grown, the *extent* to which stakeholders have become involved in data collection varies strongly across fields. Second, there is variation in the (limited) *degree* to which practical relevance is rewarded in terms of academic recognition. Third, in all fields promising societal benefits can help to acquire funding, but the *degree* of involvement of societal stakeholders in the actual agenda-setting differs. A fourth dimension that deserves to be addressed here is the relationship between practical applications and scientific publications; in some fields there is a synergy between scientific productivity and practical relevance; in other fields these are in contradiction.

Table 7.2 Overview of the differences among struggles for relevance in eight scientific fields

Credibility conversion	Variable	Observed range
Acquiring data	Intensity of stakeholder interactions	Low – High
Acquiring recognition	The value of practical applications	Negligible – Considerable
Acquiring money	Influence of stakeholders on research agenda	Weak – Strong
(generic)	Relationship practical applications – scientific productivity	Strong tension - Synergy

Chemistry

Because chemistry is traditionally committed both to a quest for fundamental understanding and to considerations of use, in Section 1.5 I expressed the expectation that it could relatively easily accommodate the changes in its institutional environment. Given their tradition of 'use-inspired basic research' (Stokes 1997), it was assumed that chemical researchers would not have difficulties with the increasing pressure for publications and the practical orientation of many funding sources. On average, my findings confirm this expectation, but a closer look reveals considerable differences across the three fields.

In Dutch catalysis I observed a further increase in the intensity of interactions with chemical industry. The knowledge needs of chemical firms play a significant role in most types of funding. Moreover, in a large share of all projects, firms participate in a users committee or supervisory board that has a say in the progress and direction of data collection. Academic researchers in catalysis are often involved in patent applications and in high tech startup companies. In addition, their scientific publications and conference presentations also serve as transfer channels to industry, as industrial representatives (or industrial researchers) read scientific journals and attend scientific conferences. The pressure for publications does not inhibit interactions with industry, as application-oriented research is very suitable for academic publications. Contributions to practical applications are not rewarded in terms of scientific recognition, career opportunities, or evaluation scores, but they do not seem to hinder these rewards either. Overall, in this field the relationship between excellent performance in academic terms and producing relevant knowledge is synergetic rather than counterproductive.

In biochemistry interactions with stakeholders have not increased significantly since the 1970s. The context of application is still hardly present in this field. In order to enhance their chances to get funding, biochemical scientists have started to loosely refer to possible practical applications in research proposals, but they do not typically interact with stakeholders to set their research agenda. Most funding can be acquired based on rather general promises about future applications in health care or biotechnology. The data collection is carried out autonomously, without (frequent) interactions with stakeholders. Dutch biochemists are also not involved in knowledge transfer to industry or other end-users, they only collaborate with applied researchers, e.g. in the medical domain. To earn recognition, to receive high evaluation scores and to develop an academic career, it has become extremely important to publish many papers, to be cited and to acquire research grants. Overall, the increased pressure to publish and the scarcity of resources have further increased the competitiveness of this field, but the struggle for relevance has remained a side issue.

In Dutch environmental chemistry, the struggle for relevance has remained rather stable over the past few decades. Interactions with stakeholders have been common practice in this field, since its inception in the 1960s. Both during funding acquisition and data collection environmental chemists attune their activities to the needs of their users, i.e. policy-makers, firms, or NGOs. They also actively transfer their findings to users by writing policy reports, participating in advisory committees, and contributing to public debate. These aspects of the struggle for relevance have not changed a lot over the past few decades. However, the increased pressure for academic publications has complicated this struggle. The intensive interactions with stakeholders carry two dangers: they can threaten the objective position of academic researchers and they can erode the available time for writing scientific papers. In this field, (more than in most other fields), practical applications can help to receive recognition from academic colleagues. However, in formal quality control (performance evaluations, performance interviews, job interviews) only scientific publications count as real success.

Biology

In biology I expected to find relatively weak (existing) relationships with societal stakeholders. This would imply that it could be difficult to accommodate the increasing pressure for practical applications. However, in one of the two biological fields (toxicology) strong relationships with knowledge users already existed, while in the other (paleo-ecology) they have developed recently without any problems. In fact, the enduringly weak relationships with stakeholders expected in Dutch biology was more closely approximated in biochemistry and cell biology, two fields with close affinity with biology, but which were part of my case studies of chemistry and agricultural science, respectively.

In Dutch paleo-ecology the concern with practical applications has increased in the period studied. Traditionally this field had a quite academic orientation, directed at enhancing the understanding of taxonomy and evolution, but in the 1980s collaborations with oil companies were started in order to ensure sufficient funding. In addition, nowadays a major share of paleo-ecologists is involved in climate research. In this type of work, agenda-setting is strongly connected with policy needs. This new research direction does not require active involvement of knowledge users in the actual research process, but the collaboration with other disciplines (such as earth scientists) is crucial. By this type of multidisciplinary research, paleo-ecologists can provide direct or indirect support for environmental policy. Like in most other fields, career advancement, performance evaluations and informal reputations are based much more strongly on scientific productivity than on policy relevance. But the increasing publication pressure does not conflict with the increasing pressure for relevance. Paleo-ecologists seem able to produce prestigious papers by participating in climate-oriented programs or by contract research for industry just as well as by projects with a more fundamental orientation.

The dynamics of Dutch toxicology are similar to those of environmental chemistry, in the sense that the struggle for relevance has not intensified in these fields. Academic toxicology has been connected with practical applications since its emergence in the 1970s. Interactions with knowledge users (such as policy makers) have always played a significant role in funding acquisition, data collection and communication in this field. Over the years, toxicologists have experienced a growing pressure to perform better according to academic standards. Due to changing knowledge demands and a growing publishing pressure, a more fundamental research orientation has developed focusing on the mechanisms behind toxicity. Still, a major share of the available funding is connected to practical goals such as exploring the medical risks of a particular class of compounds. But within projects of this kind, the role of academic toxicologists is to produce generic models and understanding rather than performing standard toxicity tests. Knowledge exchange with users such as policy makers, firms, and medical scientists is still common practice, but efforts in this direction are less rewarding in terms of career advancement and peer recognition than producing papers for scientific journals.

Agricultural science

In agreement with my expectations (expressed in Section 1.5), in agricultural science the increased pressure for scientific publications of the past few decades has had the strongest impact. As this discipline was traditionally relatively weakly oriented to fundamental understanding, the demand for scientific productivity was more disrupting here than in chemistry and biology. But the effects of this development also varied across the three fields. In animal breeding and genetics (ABG), similar to catalysis, researchers had no problems with increasing their publication output while at the same time producing knowledge of strategic importance to their stakeholders. In animal production systems APS and cell biology, however, many researchers face a trade-off between activities that can help to produce prestigious publications and activities promising high practical utility for their stakeholders.

In ABG the interactions with stakeholders have further increased over the past 30 years. Similar to catalysis, this field has a strong relationship with a specific industrial sector, animal breeding firms. The university researchers have intensive contact with these firms in all stages of their work. Some of the projects are funded by industry; others are (mainly) funded from public sources. But in all cases, the long term needs of breeding firms are taken into account in the choice of research goals. In many cases animal breeding researchers also interact with firms during data collection, as these firms provide data or participate in users committees or similar bodies supervising the projects. ABG researchers intensively collaborate with the applied research institute Wageningen UR Livestock Research. This institute also mediates some of the relationships with industry. The outcomes of ABG's research seem to reach potential users easily, thanks to the good contacts, and thanks to the high absorptive capacity of the breeding firms. These companies are highly knowledge intensive and they employ a lot of ABG's graduates. ABG researchers are not particularly rewarded for (contributions to) practical applications of their knowledge in terms of career advancement or peer recognition. Still these contributions seem to catalyze rather than hamper academic publications. Industrial support facilitates research oriented to fundamental understanding, the outcomes of which are suitable for publication in prestigious scientific journals.

APS is a relatively young field, which has developed strong relationships with societal stakeholders. Its funding comes from a variety of sources, but it is most often linked to promised practical applications. APS's research agenda is strongly connected to the needs of stakeholders, although these stakeholders are quite diverse. Farmers, the end-users of this knowledge, do not provide funding, but they do contribute to the collection of data. The outcomes of APS research are not only published in scientific papers but also delivered to possible users, in the Netherlands or abroad. The increased pressure to publish is a challenge for APS. The interdisciplinary and application-oriented nature of its research makes it more difficult to publish in prestigious journals. In addition, due to the novelty of the APS approach, there is a limited disciplinary infrastructure available, in terms of specialized journals and research communities. In this field practical applications can help to earn informal recognition from colleagues, as they acknowledge them as legitimate outcomes. However, applications hardly help to receive high evaluation scores or to enhance career prospects.

Within Dutch agricultural science, cell biology has always been a relatively fundamental field and in the past 30 years it has not increased its interactions with societal stakeholders a lot. The research agenda is still driven by academic concerns. Stakeholders, such as firms, provide only a marginal share of the research funding. Also the processes of data collection and publication are carried out without frequent stakeholder contact. The increasing competition for research council funding and the increasing pressure to publish stimulate CB to shift towards more ‘prestigious’ research areas that are of less relevance for agricultural practice. More money is available for research on mouse or human models systems, and this also fits better in high-impact journals. This situation creates a difference between research that is most useful for agricultural stakeholders and research that provides the best prospects on academic rewards such as publications, research grants, evaluation scores and career advancement.

The relationship between practical applications and scientific productivity

Having presented my findings regarding the eight fields included in my case studies, let me now make some observations cutting across disciplinary boundaries. When comparing the changing struggles for relevance of the eight fields studied, a particularly interesting dimension is the relationship between practical applications (which have become dominant in the meaning of ‘relevance’) and scientific productivity. As the findings reported above indicate, in some fields there is a tension between practical applications and scientific productivity, while in other fields they have a synergetic relation. Table 7.3 presents a classification of the eight fields in my sample based on this dimension. In the following some the characteristics of the three categories of fields will be explored in terms of the other three ‘dimensions’ of the struggle for relevance.

Table 7.3 Classification of the eight fields based on the relationship between practical applications and scientific productivity

Relationship practical applications - scientific productivity	Fields	Intensity of stakeholder interactions during data collection	Value of practical applications for acquiring recognition	Influence of stakeholders on research agenda
Strong tension	Cell Biology	Slight increase	Negligible	Remains weak
	Biochemistry	Still low	Negligible	Remains weak
Weak tension	Environmental Chemistry	High but stable	Considerable	Strong but stable
	Animal Production Systems	High and growing	Considerable	Strong and increasing
	Toxicology	High but stable	Low	Strong, slight increase
Synergy	Catalysis	High and growing	Negligible	Strong and increasing
	Animal Breeding & Genetics	High and growing	Low	Strong and increasing
	Paleo-ecology	Slight increase	Low	Increasing

In the two fields with the least intensive stakeholder interactions, biochemistry and cell biology, I observed a trade-off between scientific productivity and practical relevance. In these fields scientists complain that engaging in application-oriented research projects and interacting with societal stakeholders 'distracts' them from the main focus of their field. Efforts or achievements of this kind do not significantly yield peer recognition here. In biochemistry and cell biology contributions to high-impact journals are usually based on projects paid by research councils granting considerable autonomy to the researchers to formulate their own research priorities and approaches. Enhancing the industrial, medical or agricultural relevance of one's work implies a move away from the central debates of these fields. To this end, one would have to shift to other model systems (for example chicken rather than mouse) or to address other research questions (for example relating to specific treatments rather than general understanding) which are less suitable for publishing in prestigious journals.

In a second class of fields, containing APS, environmental chemistry and toxicology, such a tension exists as well, but it is of a weaker nature. In this set of fields interactions with stakeholders are quite common, both during the acquisition of funding and during data collection. Here, under certain conditions, application-oriented research can lead to impressive publications. The most important requirements for successfully combining practical applications with scientific productivity seem to be substantial project size and consistency across projects. If these conditions are fulfilled, the results of application-oriented projects can lead to improved understanding on a fundamental level. In this way, the outcomes of (one or more) relatively practical projects can lead to scientific papers. For researchers involved in relatively short and diverse application-oriented projects it is difficult to develop fundamental insights that can be published in prestigious scientific journals.

The situation is different in catalysis, paleo-ecology and ABG. In this class of fields, in which stakeholder interactions have significantly grown, I observed a synergy rather than a trade-off between scientific productivity and societal relevance. Interactions with stakeholders do not only help to acquire funding, but they are also helpful in other credibility conversions. I identified three mechanisms that are responsible for this synergy. First, applied research projects for stakeholders can provide access to data that are useful for more fundamental investigations, too. Second, the interactions with stakeholders often give inspiration for challenging research questions. Third, some stakeholders simply sponsor fundamental research activities, of which they expect benefits on the longer run.

As Table 7.3 indicates, the relative value of applications (when compared to publications) as a source of recognition does not show a clear trend across the three sets of fields, that is, it does not correlate with the degree of synergy between practical applications and scientific productivity. It is the lowest in biochemistry and cell biology, the fields with the least intensive stakeholder interactions. In these fields, recognition is almost exclusively based on academic achievements, in terms of scientific publications and citations. In the two other classes of fields the relative value of applications ranges from negligible to considerable. Practical applications are most rewarding in terms of recognition in APS and environmental chemistry, which occupy a middle position in Table 7.3. In these two fields a scientific reputation is not only based on contributions to scientific debate, but also to contributions to environmental policy or to the development of more sustainable agriculture. This may be related to the fact that in these two fields scientists seem most strongly motivated to 'change the world'. More than in the other fields studied, they draw inspiration for their work from personal ambitions to contribute to external goals like sustainable development[135]. In the same vein, they also value their colleagues' practical contributions to such goals, more than scientists in fields like catalysis or toxicology.

Sub-question 1

To conclude this section, I have observed three general changes in the struggles for relevance of Dutch chemistry, biology and agricultural science, in the period 1975-2005:

1. The struggle for relevance during data collection has intensified.
2. The value of practical applications as a source of academic recognition has decreased.
3. The struggle for relevance in the context of funding acquisition has intensified.

135 For example: 'Yes, in the time that I started I had a strong passion. That there was a large problem which already received attention, but which was not known yet in its full proportions.' (interview researcher C15).

Moreover I have discerned significant differences in the way these changes became manifest in different scientific fields, and in the interplay between them. In some fields a tension has developed between scientific productivity and practical applications, in others a synergy.

7.3 Explaining the similarities among scientific fields

The work reported in this thesis shows that the three similarities among the changing struggles for relevance of the different fields can be understood as effects of the combination of three structural changes: shifts in the available funding, the rise of performance evaluations, and changing views on the societal position of the university. Chapters 4-6 presented an analysis of the changing structures influencing the work of academic researchers in chemistry, biology and agricultural science. Using the science-society contract as an entrance point, in Chapters 4 and 5 the main emphasis was on the discursive dimension of structure. Chapter 6 explored in more detail the institutional dimensions, based on the notion of a research system. From both angles, I have gained an understanding of the way structural conditions shape the agency of scientific researchers. The three structural changes I observed in all cases together provide a possible explanation for the generic changes I have observed in the credibility cycle: the intensifying struggle for relevance during data collection, the decreasing value of relevance as a source of recognition, and the intensifying struggle for relevance in funding acquisition (see Figure 7.2). Below I will summarize my findings regarding the three structural changes in the different case studies, complemented with some generic data where possible.

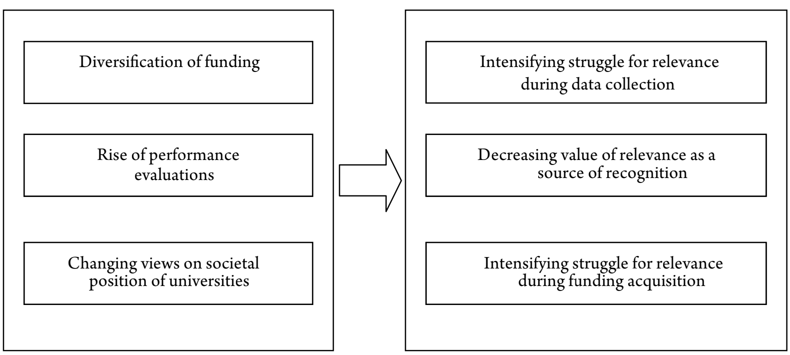

Figure 7.2 Overview of the effects of structural changes on the struggle for relevance

Diversification of funding

The first general structural change influencing struggles for relevance in all fields of science, is trend of funding diversification. In all fields studied the relative share of public funding for basic research has decreased. Moreover, over the years the relative share of unconditional funding (first money stream) has decreased. With the general expansion the public science systems, budgets have become under pressure and the need to account for public investments in academic research has grown (Ziman 1994). In line with the ideologies of Neo-liberalism and 'New Public Management', the government has loosened state control and introduced market mechanisms to enhance efficiency and effectiveness (De Boer et al. 2007, Schmoch & Schubert forthcoming). Since 1975, the starting point of my analysis, the government has transferred an increasing share of public funding to competitive arrangements, organized by research councils or other intermediary organizations. Between 1975 and 2005 the total amount of block grant support for universities has grown almost twofold in real terms (Versleijen 2007), but its relative share in relation to more competitive funding sources has decreased. In 1985 governmental block grants still constituted 84% of all revenues of Dutch universities, but in 2000 this was only 70%. In relation to the number of enrolled students, the block grant support has even decreased with 30% between 1980 and 2000[136] (Jongbloed & Salerno 2003). Around 1975, this money stream was still sufficient for research groups to buy the necessary equipment and hire some temporary staff, but nowadays even some of the permanent academic staff needs to be paid from project funding. Even the funding that has remained in this category has become less secure, as it has become subject to university policy, and it is often needed to 'match' externally acquired funding (Jongbloed & Salerno 2003, AWT 2004).

The 'second money stream' has also changed dramatically. Research councils were initially organized in sub-disciplinary Working Committees, but they have been merged and reorganized into a general matrix organization supplying most funding in the form of multidisciplinary research programs. While Dutch research councils originally exclusively funded basic research, they have expanded their territories to application-oriented activities, too. Moreover the Dutch Organization for Scientific Research (NWO, the new umbrella organization of all research councils) has developed a variety of hybrid funding configurations in collaboration with ministries, firms or other knowledge users.

In addition, the third money stream (all contract funding except from NWO), which is more strongly oriented to practical applications, shows a spectacular increase in the 1980s and 1990s at all Dutch universities (see Figure 7.3). Between 1983 and 2000 the total size of this 'stream' has increased from about 125 to about 638 Million Euros. This amounts to an increase of a factor 3.85 in real terms (Jongbloed & Salerno 2003).

136 At Wageningen University, one of the few Dutch universities with decreasing student numbers, governmental block grant support even decreased with about 56% in relation to the number of students during the 1980s (in real terms (Faber 1993), see also Chapter 6). Between 1978 and 1995, the number of research positions paid from block grant funding at this university increased from 223 to 361, while those paid from external funding increased 77 to 472 (Roseboom & Rutten 1998).

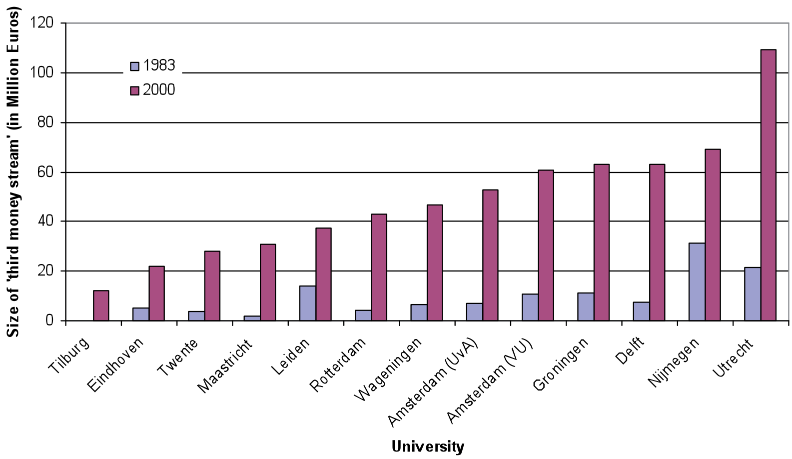

Figure 7.3 The size of the 'third money stream' in 1983 and 2000 (in current prices) at Dutch universities. Source: Jongbloed & Salerno, 2003.

In the case of chemistry the willingness of industry to support academic research has grown a lot. Due to the adoption of new R&D strategies involving less in-house basic research, firms have become more dependent on knowledge produced outside their own laboratories, so they provide a substantial share of all funding for academic chemistry. Biology has a less straightforward relationship with one particular industrial sector, but it also receives increasing support from third parties, such as governments, NGOs, medical charities, and the European Framework Programmes. In the case of agricultural science, biotechnology firms, NGOs and new intermediary organizations have taken positions as significant funding sources next to the traditionally dominant Ministry of Agriculture.

These shifts help to explain the intensification of the struggle for relevance during data collection and during funding acquisition. In all three disciplines a strong diversification of funding sources has taken place between 1975 and 2005 and the relative share of unconditional support granting complete autonomy has decreased. Overall the funding system has become more competitive. Academic recognition as such is not anymore a guarantee for continuous funding. Nowadays the large majority of projects is paid from external funding, based on carefully written research proposals and / or negotiations. And within the palette of available additional funding, the share of money tied to specific practical or strategic goals has increased from a marginal element to the dominant majority. In this situation, academic researchers need to maintain relationships with all types of organizations that may serve as future funding sources.

Simultaneous to this major shift towards application-oriented funding, however, a smaller trend took place in the opposite direction. Over the last decade, the research council's policy to nurture and stimulate excellent researchers has created a small but significant subset of funding arrangements lacking any consideration of practical utility. In 2000 NWO introduced the highly competitive 'Vernieuwingsimpuls' grants, as a policy-instrument for supporting talented researchers. In the selection of proposals for these grants, the most determining factor is the individual quality of the requesting applicant, assessed mainly using bibliometric criteria. With its emphasis on bibliometric quality indicators, this type of funding has probably contributed to the decreasing value of practical applications as a source of recognition. At the moment the grants under this scheme together consume about 20% of NWO's total budget[137]. However, the relative impact of this funding instrument is probably larger than its financial share, thanks to its prestige and popularity. Because these grants do not provide any thematic restrictions and they offer a large degree of autonomy during the research process, they are generally considered as one of the most attractive funding sources available. Between 2000 and 2006 the average number of applications for the Vernieuwingsimpuls was about five times the number of available grants (Technopolis & Dialogic 2007). This implies that the success rate of this funding instrument is only 20%, while the overall average success rate at NWO is about 50%[138]. Moreover, because of their competitive nature, successful acquisition of Vernieuwingsimpuls-grants counts as a strong quality indicator both in formal evaluations (performance interviews, job interviews, research evaluations) and in informal reputation. For these reasons, the stimulating effect of this funding scheme reaches a much larger population of researchers than the grant-winners only.

Rise of performance evaluations

The second structural change that has shaped struggles for relevance in all scientific fields studied, is the rise of performance evaluations. This development has contributed to the increasing pressure for scientific publications and the decreasing value of practical applications as a source of academic recognition. Over the past 30 years society's general need for accountability has led to the development of an elaborate system of performance evaluations of all academic research. Inspired by the ideology of New Public Management, the government has gradually developed an approach of 'steering at a distance', granting more autonomy to universities as organizational units (De Boer et al. 2007). This new governance model implies the need for more management information, such as numerical indicators about the performance of specific organizational units (Schmoch & Schubert forthcoming). Research evaluations could meet this need. After a number of pilot evaluations and foresight studies in the 1980s, a more or less standard approach has been developed for systematic evaluations of academic research groups (van der Meulen 2008). This trend can be seen as a manifestation of a wider 'audit explosion' in the late 1980s, in response to increasing demands for accountability and transparency (Power 1999). Nowadays, every research group in the Netherlands is subject to regular evaluations. Research quality assessments officially use a variety of criteria[139], but in practice they tend to be ruled by bibliometric quality indicators. Even if other dimensions such as viability or relevance are measured as well, in the interpretation of

137 NWO, 2010, 'Begroting 2010 en meerjarencijfers 2011 tot en met 2014', NWO, Den Haag
138 This was the average success rate in 2004 (NWO, 2005, 'Jaarboek 2004', NWO, Den Haag)
139 VSNU, NWO & KNAW 2003, *Standard Evaluation Protocol 2003-2009 for Public Research Organisations*

the evaluation scores, numbers of publications and citation rates dominate[140]. The availability of digital bibliometric databases, and the relative generic validity and cross-comparability of these indicators made these into a success that is not equalled (yet) by any other indicators (Gläser & Laudel 2007). Unlike the Research Assessment Exercise in the UK (Barker 2007) the results of Dutch research evaluations do not have direct financial consequences. However, the outcomes do influence strategic decisions by deans and university boards, and high scores can also contribute to successful acquisition of external funding.

Changing views on the societal position of the university

The third important structural change concerns the dominant perspective on the mission of the university in the Netherlands. In the period between WWII and the start of science policy in 1975, the ruling values connected with academic research were autonomy and independence. In the next 30 years, these values have been gradually replaced by accountability, valorization and entrepreneurship.

These shifting views can be traced in the rationales for funding academic research. In the rationales for funding academic chemistry and biology, the emphasis has shifted from education and cultural value to practical applications. Before 1975 chemical and biological research was mainly considered valuable because it facilitated the training of new generations of researchers. As a minor argument one sometimes referred to the intrinsic (cultural) value of basic research. Since the 1970s public support for these disciplines has increasingly been justified with reference to their contributions to the solution of societal problems and their contribution to industrial innovations. Over the course of the years, society has developed different expectations of the contribution academic scientists should make to social and economic goals, giving them an increasingly active role in reaching these goals. In the early days of science policy, in the late 1970s, the rise of environmental concerns and critical views on scientific progress created a demand for chemical expertise to contribute to the solution of environmental problems and the development of cleaner technologies. Later, in the 1980s, when innovation became an explicit policy goal and when firms started to reorganize their R&D activities, funding for academic chemistry became connected to the expectation that its outcomes provide starting points for chemical industry to develop new products and processes. In the case of biology, the legitimacy for supporting academic toxicology was already connected to environmental problems since the emergence of this field in the early 1970s. Support for paleo-ecology became connected to economic rationales in the 1980s, when it started to contribute to the identification of potential oil drilling locations. The commercial success of biotechnological industry has also provided additional rationales for supporting academic biology, but these are of little significance for the two fields focused on in Chapter 5 (toxicology and paleo-ecology). Since the

140 The newest protocol for Dutch research evaluations more explicitly demands the assessment of 'societal relevance' (VSNU, KNAW & NWO 2009, *Standard Evaluation Protocol 2009-2015: Protocol for Research Assessment in the Netherlands*), and a recent set of pilot-studies has shown the potential of indicators for this criterion (ERiC 2010, *Handreiking Evaluatie van maatschappelijke relevantie van wetenschappelijk onderzoek*, ERiC publicatie 1001.), but the effects of this development on academic research practices were not yet visible in my case studies.

early 1990s, the need for sustainable development is mentioned a lot in arguments for funding research in both disciplines. This umbrella term covers potential contributions to both environmental issues and economic prosperity. These contributions include enhanced understanding of climate change (by paleo-ecology), higher efficiency of chemical production (by catalysis) or lower uncertainty about the risks of synthetic compounds (by environmental chemistry). Recently, an additional set of rationales for supporting academic science has emerged under the label of 'valorization', connected to the potential direct economic benefits biologists and chemists can generate. Patents and spin-off creation have become signs of successful valorization, providing access to future funding from innovation-oriented policy instruments (Zomer et al. 2010).

A similar development is visible in agricultural science, although with a different point of reference, as funding in this area has always been connected with expected practical applications. However, also in this case a significant shift can be observed in the specific applications demanded and in the visions on the optimal organization of the knowledge system. In the first post-war decades, support for academic agricultural science was based on the widely shared need to enhance agricultural productivity by modernization. Agricultural researchers could count on unconditional support, based on the linear-model-based belief that the basic insights they generated would be unproblematically be turned into technological innovations by applied researchers and diffused to farmers by the celebrated governmental extension service. After Dutch agricultural production reached its geographical, economic, and environmental limits and undesirable side effects of agricultural modernization became visible, views on the future of this sector radically diverged. Although the level of consensus of the 1950s and 1960s has never returned, nowadays the dominant perspective is the need for a (more) sustainable agriculture, which is internationally competitive, without comprising the environment, animal welfare, or human health. Moreover, the dominant views on an effective agricultural knowledge system have changed from a consensus-driven homogeneous structure to a loosely coupled heterogeneous network representing a variety of stakes. In the former, academic researchers had a clear role as the producers of fundamental insights in agricultural production, and did not have to be directly involved in the transformation and translation of these insights into actual farming practices. In the latter system, however, their role is much larger and more diffuse than only knowledge production and transfer to applied researchers and extension agents. They are expected to interact with all actors in the agricultural 'innovation system', including farmers, supplying firms, consumer organizations, environmental NGOs, and regulating bodies. What is more, these interactions should go beyond one-way communication as agricultural scientists are invited to participate in 'co-production' and 'transdisciplinary learning' (Hoes et al. 2008, Regeer 2010).

The changing views on the societal position of the university have two important implications. First, while the values autonomy and independence encourage scientists to isolate themselves from the outside world, values such as valorization and entrepreneurship invite them to collaborate, participate and interact. This has contributed to the intensification of struggles for relevance during the collection of data. Second, the rise of accountability as a value implies that scientists need to show that they deliver value for money, both in terms of *what* they are doing and *how much* they produce. This need has been fulfilled with performance evaluations, which have changed the norms guiding peer recognition, as I have explained above.

Structures are both medium and outcome

Please note that these explanations do not imply a unidirectional causality between the structural conditions and the agency of university researchers. Increasing interactions with societal stakeholders can also lead to changes in the dominant view on the societal mission of universities, and to modifications in the available funding for academic research. Paleo-ecology, for example, was traditionally seen as rather fundamental, but got a more strategic image due to the development of stronger connections with oil companies. This created new funding possibilities, both from public and from private sources.

Moreover, changing publication patterns influence the way performance evaluations are organized and also the quality criteria used by research councils to distribute their funding. Bibliometric criteria would never have become so dominant in the allocation of personal grants in the context of NWO's Vernieuwingsimpuls if journal papers had not developed into the most universally accepted research outcome. The declining prominence of Dutch publications in fields like environmental chemistry and toxicology is not only an effect of the rise of bibliometric evaluation criteria, but it also strengthens their validity and legitimacy. It is probably appropriate to speak of a co-evolution of publication practices and evaluation structures. Due to limitations in time and space, feedbacks from agency to structures have received less attention in this thesis, but I do not want to suggest that they are not significant.

Sub-question 2

The conclusion of this section is that a possible explanation for the similarities among the changes in the struggles for relevance in different scientific fields can be found in changes in the structural conditions of academic research, in particular the diversification of funding, the rise of performance evaluations, and changing views on the societal position of the university.

7.4 Explaining the differences among scientific fields

A notable result of this thesis is that struggles for relevance differ more strongly among fields than among aggregated disciplines. While my case studies were selected on the level of disciplines, the comparison of their constituent yields more interesting outcomes than the comparison of whole disciplines. In all three disciplines the changes in the struggle for relevance varied considerably across fields. For example, interactions with stakeholders have increased in both catalysis and environmental chemistry, but in catalysis these help scientists to build excellent publications record, while in environmental chemistry these tend to constrain prestigious publications. It seems that the struggle for relevance of catalysis shares more characteristics with the one in animal breeding & genetics than with biochemistry or environmental chemistry. How can we explain the differences among the changing struggles for relevance of scientific fields? This section will present a possible explanation based on socio-organizational, cognitive and cultural field-characteristics, combined with the characteristics of societal stakeholders.

Explaining variation in stakeholder interactions

In the previous section we have seen that the degree to which stakeholder interactions have increased (both during data collection and during funding acquisition) varies strongly across fields. This can be partly explained by cognitive and socio-organizational characteristics of scientific fields, in particular their search pattern (Bonaccorsi 2008) and strategic task uncertainty (Whitley 2000). I have found that some fields with 'convergent' search patterns, namely biochemistry and cell biology, have developed relatively few interactions with societal stakeholders (see Tables 7.3 and 7.4). This is understandable, because convergent fields have a relatively sharp focus in terms of research problems and approaches. In these fields the strategic task uncertainty is low. This implies that there is a strong overall consensus about the intellectual priorities and scientists can not easily develop a new, application-oriented research direction.

Table 7.4 Classification of fields based on the degree of convergence of their search pattern

Search pattern	Chemistry	Biology	Agricultural science
Convergent	Biochemistry Catalysis	Toxicology	Cell biology Animal breeding and genetics
Divergent	Environmental	Paleo-ecology	Animal production systems

In divergent fields, such as APS an environmental chemistry, it may be more likely that niches develop which fit the knowledge needs of societal stakeholders. Divergent fields typically have a high strategic task uncertainty. This means that a large diversity of research directions is accepted simultaneously because there is no overall consensus about the intellectual priorities. Researchers and employers are able to pursue distinct strategies and orientations without being penalized for theoretical deviance. In such fields it is easier to develop new research directions that fit the needs of societal stakeholders.

Explaining variation in the value of practical applications as a source of credibility

The extent to which practical applications count as a source of credibility, another dimension of changing struggles for relevance, can be understood when taking into account the traditional communication culture of a scientific field. Fields with a divergent search pattern generally have a 'rural' communication style (Becher & Trowler 2001), as they are not very competitive and have a low people-to-problems ratio. In rural fields, such as APS an environmental chemistry, relatively few researchers work on a large number of dispersed problems, and the mutual competition is limited. As there is no broad consensus about overall quality standards, it is also difficult to formulate general evaluation criteria. Due to the theoretical diversity knowledge accumulation is less efficient than in 'urban' fields and there is usually a lower citation density. We can assume that in these fields less high-impact journals are available, which makes it more difficult to score high on bibliometric evaluations. This implies that bibliometric quality indicators have limited validity, so that it is less likely that recognition will be based on publications only and more likely that practical outcomes such as policy advice, patents or spin-off firms count as well. In urban fields, with a high citation density, bibliometric quality indicators have a higher validity. On this end of the spectrum these indicators will be more readily used, not only in formal evaluations and management decisions, but also in informal processes of exchanging recognition.

Explaining variation in the relationship between practical applications and scientific productivity

The variation in the relationship between practical applications and scientific productivity, which seems crucial for the fate of scientific fields, can be explained by taking into account characteristics of other actors in the research system, in particular the end-users of academic research. Depending on its cognitive content, each field has different potential users outside university. Of particular importance are 'upstream end-users', stakeholders with formal channels to influence the strategies and programs of a scientific field through research funding, regulation, or policy (Lyall et al. 2004 p. 79). Examples of such users are animal breeding firms, environmental policy makers and patient organizations.

In my case studies, the fields that were most successful in combining stakeholder interactions with academic performance, were fields with wealthy and powerful upstream end-users with a long-term vision on the utility scientific research (see Table 7.5). Chemical industry (in the case of catalysis) and animal breeding industry (ABG) both invest substantial sums in academic research, based on the expectation that these will pay back on the longer run. These companies support academic researchers in fundamental research activities, which provide good opportunities for high-impact publications. In this way, they support scientists along the complete cycle of credibility. The same goes for environmental policy makers and oil companies in the case of paleo-ecology.

Table 7.5 The upstream end-users of different scientific fields. The fields are ranked according to their degree of synergy between practical applications and scientific productivity

Field	Upstream end-users
Catalysis	Industry
ABG	Animal breeding firms
Paleo-ecology	Policy makers Oil companies
Environmental chemistry	Policy makers Industry NGOs
APS	Farmers Policy makers
Toxicology	Policy makers Industry NGOs
Cell biology	- (some agro-food companies)
Biochemistry	-

Biochemistry and cell biology, in contrast, hardly have any upstream end-users. Of course, stakeholders can be identified that may eventually benefit from these research activities, such as patient groups, farmers or veterinary surgeons. These, however, rather function as 'downstream' users, as they are no active players in the academic research system; they do not directly commission research or influence its directions. The only actors generously supporting these fields and directly influencing their directions are research councils (on both national and European level), but these rather function as intermediaries providing channels to transfer knowledge to and from downstream end-users.

In the third class of fields, including environmental chemistry, toxicology, and APS, upstream end-users can be identified as part of the research system, but these mainly support application-oriented research. These stakeholders definitely care about the research in these areas, but they cannot afford investments with a long time-horizon. For instance, support from a governmental body for academic research in the area of toxicology or environmental chemistry is usually connected to a knowledge need on a specific problem. This explains why researchers in these fields often experience a tension between end-user relevance and scientific productivity. The short time-horizon of the projects commissioned by upstream end-users is incompatible with the fundamental nature of dominant debates in scientific literature. In such cases, interactions with stakeholders catalyze some conversions of credibility (funding acquisition), but it inhibits others (publishing).

Another significant variable is the homogeneity of the upstream end-users of a particular field. In the cases of catalysis and ABG, the set of upstream end-users is quite homogeneous, but in environmental chemistry, toxicology, and APS it is heterogeneous. It seems that a homogeneous set of end-users makes it easier to build a consistent project portfolio, which will help to find synergy between practical applications and scientific productivity.

Sub-question 3

This section shows that a possible explanation for the differences among the changes in the struggles for relevance observed in different scientific fields can be found in variations in the degree of convergence in their search pattern, their strategic task uncertainty, their traditional communication culture, and characteristics of their upstream end-users.

General research question

Combining the answers to the three sub-questions, the following answer to the general research question of this thesis can be formulated:

In the period 1975-2005, three general trends were visible in the struggles for relevance of Dutch academic researchers in chemistry, biology and agricultural science: the intensification of the struggle for relevance during the collection of data, the decreasing value of practical applications as a source of recognition, and the intensification of the struggle for relevance in the context of funding acquisition. However, the changes also varied across scientific fields, regarding the intensity of stakeholder interactions during data collection, the precise value of practical applications as a source of recognition, the actual influence of stakeholders on the research agenda, and the relationship between practical applications and scientific productivity. The changes observed can be understood as effects of structural changes, in particular the diversification of funding, the rise of performance evaluations, and changing views on the societal position of the university, in combination with characteristics of scientific fields and their stakeholders.

7.5 Reflection on central concepts

Having answered my research question, let me now reflect on the strengths and weaknesses of the three central concepts I have used and provide recommendations for further research. In the next sections I will discuss my contribution to the debate about changing science systems (7.6) and formulate recommendations for science and innovation policy (7.7).

The credibility cycle

In this thesis the credibility cycle has turned out to be a helpful concept for investigating daily research practices. My analysis has focused on the reward structure of academic science, rather than on actual research activities. The credibility cycle proved to be a useful concept for this endeavor, as it guided my attention to the pressures and incentives that scientists experience in their daily work. In this way, it helped to get an overview of the general sequence of steps needed in building and conserving a scientific reputation.

Beside its function as a generic model of science as an ongoing process of resource conversions, the credibility cycle can also be regarded as a historical-specific description of academic research practices. Based on my empirical findings I would like to suggest a number of adaptations to the cycle in this second function. First, two modifications should be considered regarding the structure of the cycle.

My first recommendation is to include other research outcomes beside scientific articles. Today in some fields patents, prototypes, spin-off firms and consultancy reports count as legitimate outcomes as well (see also (Packer & Webster 1996)). To what extent these alternative outcomes can be used as a source of recognition is a further empirical question; my research suggests that in most scientific fields their yield in terms of recognition is relatively small. In some cases, however, entrepreneurial scientists can 'by-pass' peer recognition, and turn such alternative research outcomes directly into money by licensing patents, consultancy or selling a spin-off firm to a larger company (see Section 4.5). Although academic entrepreneurship is being stimulated by both public policy and university management, I have seen only few examples of scientists succeeding to earn substantial income for research activities in this way.

Second, I suggest broadening the concept of recognition beyond informal acknowledgements to include a more formal component of 'recognition' that accounts for citation scores and the outcomes of performance evaluations. My case studies have shown that today recognition is not anymore based on individual assessments of the quality of one's work, but to a large extent the product of numerical procedures. Because (informal) reputations are strongly influenced by numerical analyses, it seems not necessary to add another element of 'formal reputation'. It seems more fruitful to expand the element 'recognition', and make it depend not only on (the number and quality of) articles, but also on citation scores (like the H-index) and other evaluations. In turn, this composite pool of 'recognition' can be used as a resource for acquiring money.

My outcomes have also provided insights into the processes and norms guiding three specific credibility conversions. These can be used to further specify and qualify the model:

1. In many fields the process of collecting data has expanded to include interactions with potential knowledge users. In some fields this facilitates efficient and effective data collection, but in others it is a complicating factor[141].
2. The value of articles as a source of recognition has (further) increased, at the expense of practical applications.
3. In the conversion of recognition to money, promises about potential practical applications have become an influential factor. Being able to express the future societal benefits that one's work will generate, is a valuable asset for today's scientists. Related, contacts with potential knowledge users are a valuable resource to acquire funding. Moreover, due to the increasing programmatic nature of public funding for university research, good contacts with other researchers can help a great deal in acquiring money.

Science-society contract

The idea of a contract between science and society is frequently alluded to in science (policy) studies literature, but to date it has hardly been given a proper definition and operationalization. This thesis has further developed this concept into a heuristic for analyzing changing science-society relationships of specific disciplines in a particular country. This heuristic helped to acquire an overview of the changing societal position of academic research, or – in other words – the changing meaning of relevance. The discourse on the science-society relationship influences the actors involved in science. Shared visions on the identity of science and the legitimacy of public investments in science enable and constrain the activities of individual researchers, science policy makers, and other sponsors of academic research (see Chapter 3).

In future research the mutual relationships among the three components of the contract (identity, rationales, conditions) could be further explored. My assumption that compatibility among these three components is a necessary condition for a stable research system could be empirically tested by analyzing historical episodes in which there are tensions among the three components. Another interesting question is which of the components has the primacy. Is the public debate about the societal position of university research always guided by the rationales relating to expected societal benefits? Or can one also find instances in which desirable conditions are taken as a starting point, and rationales are simply formulated to legitimize these conditions? In this thesis, the main interest was in the consequences of the changing contract on research practices, and not on interactions and possible tensions among the elements *within* the contract.

141 Another recent change in this process, which has received relatively little attention in this thesis, is the increasing value of collaborations during the research process, as large-scale collaborative projects are becoming a salient feature in many fields of science (Penders et al. 2009, Vermeulen 2009).

Another direction for further improvement could be to experiment with differentiating the contract for individual fields within a discipline. This thesis has shown that even within one discipline there can be huge differences in expectations about societal benefits generated by scientific fields. For example, funding for biochemistry is often legitimized by loosely referring to possible long term benefits for biotechnology or medicine, while funding for environmental is typically justified by pointing to short term improvement of environmental policy. This thesis has shown that the structuring influence of such specific promises on the agency of individual scientists is as least as powerful as the influence of generic promises of scientific disciplines.

Research system

Building on existing literature, the notion of a research system has helped to analyze the changing institutional environment of academic research, in the case of agricultural science (Chapter 6). The institutions in the research system shape certain conversions of the credibility cycle, but they are at the same time (re)produced by research practices. The most profound changes that I have identified in the agricultural research system concern shifts in the available funding and the rise of systematic performance evaluations.

The research system is sometimes conceptualized in terms of three levels, a top level of government agencies, an intermediary level of funding bodies, advisory bodies and research organizations, and a research performance level (van der Meulen & Rip 1998, Morris & Rip 2006). The prominent position of the government in such a concept of research system suggests that it has a privileged position as the central steering actor. However, given the fact that the position of the government varies across fields and across countries, I prefer a horizontal concept of the science system in which research organizations are surrounded by a variety of institutions, some of which are governmental policy instruments and others are not. With this horizontal concept one can avoid the misleading suggestion that top-down steering is the primary coordination mechanism. It leaves more room for bottom-up forces of researchers (individually or collectively) giving shape to their institutional environment.

Analogous to my suggestion of a field-specific contract, I would also suggest conceptualizing a field-specific research system. Following Roseboom and Rutten (1998) in this thesis I have applied the notion of a research system to agricultural science (see Chapter 6). But within this discipline I have observed considerable variations in the incentives and pressures experienced in different scientific fields. This variation seemed to relate to a great deal to characteristics of the (upstream) end-users of scientific fields. Due to their differences in cognitive content, fields have different stakeholders with knowledge needs on a shorter or longer term horizon. Building on literature about sectoral or technological innovation systems, a field-specific notion of the research system could be developed, that takes these characteristics into account. A research system containing the end-users relevant for a specific field gives more direct insight into the institutional environment of the scientists in this field.

7.6 Contributions to the literature about changing science systems

The starting point of this thesis was the scholarly debate about changing science systems. Several bodies of literature claim that the position of universities in modern science systems is changing, and that academic research is increasingly oriented to producing societal benefits. A number of diagnoses of this development have been presented that explore its causes and its consequences for the cognitive and organizational characteristics of scientific knowledge production and for the relations with the outside world (see Chapter 2 for a review). Having answered the research questions of this thesis (Section 7.2-7.4), and having briefly reflected on the main theoretical concepts used (Section 7.5), it is now time to take stock of my contribution to this debate. I will first discuss a couple of general contributions, and then give a specific reaction to the concept of Mode 2 knowledge production, which is probably the most influential of all diagnoses of changing science systems.

In general, my two most important findings in relation to the literature about transformations in the knowledge infrastructure are:

1. Simultaneous to the increasing pressure for practical utility, Dutch university researchers experience an increasing pressure for productivity, as measured in bibliometric terms.
2. Science is much more diverse than most of this literature suggests.

Potential tension between practical applications and scientific productivity

The common thread in most literature on changing science systems is the increasing orientation of university research to practical utility. Indeed, in my case studies I have found that in many fields interactions between scientists and stakeholders have increased, both in the context of funding acquisition and during actual research activities. However, my analysis has also explored this development in combination with another dominant trend with far-reaching consequences, the increasing pressure for scientific publications. The interplay between these two developments hardly receives any attention in existing diagnoses of changing science systems, but it is of crucial importance because the growing pressure for scientific publications potentially inhibits the shift towards contextualized, reflexive or transdisciplinary research. Ironically, this development stems from society's need for accountability. Systematic performance evaluations, developed to address this need (van der Meulen 2008), have strongly increased the pressure to publish, because of their emphasis on bibliometric quality indicators (Weingart 2005). Although several diagnoses have touched upon the growing pressure for accountability, they have largely neglected this effect. Books on Post-academic science (Ziman 2000), Mode 2 (Gibbons et al. 1994) and Academic Capitalism (Slaughter & Leslie 1997) all report society's increasing need to have 'value for money', but they only discuss the consequences of this pressure in terms of the increasing orientation to practical utility and the growing competitiveness of funding acquisition.

This thesis has shown that there is a potential tension between the increasing pressure to publish and the increasing stress on generating broader impacts. In some fields, such as catalysis and ABG, I have observed a synergetic relationship between societal impacts and scientific excellence. In other fields, however, like biochemistry and toxicology, the pressure for academic productivity is at odds with the pressure of practical applications. In these fields scientists have increased their efforts to produce papers in high-impact journals at the expense of the practical utility of their work. In these areas research activities addressing knowledge needs of societal stakeholders are not easily published in scientific journals. Here the increased publication pressure inhibits the shift towards application-oriented research modes. To my knowledge, of all diagnoses of transformations in the science system reviewed in Chapter 2, only the Triple Helix corpus explicitly addresses this potential tension[142].

Diversity of science

This thesis shows that the dynamics of the relationships between academic research and its societal environment vary strongly across scientific fields. In this respect my work adds to a number of other recent studies that have reported varying reactions to institutional changes across scientific disciplines (Albert 2003, Gläser et al. forthcoming, Reale & Seeber forthcoming). This thesis confirms their call for disciplinary differentiation in science (policy) studies. Moreover, it reinforces it to a call for an even more fine-grained perspective that does not only distinguish among complete disciplines, but also among specific fields. Some of the diagnoses of changing science systems differentiate across scientific fields or disciplines, in particular literature about post-normal science, Finalization science, Triple Helix and innovation systems. However, none contains a satisfying framework to understand the varying dynamics of scientific fields.

Literature about post-normal science distinguishes categories of 'problem solving'. Funtowicz and Ravetz (1993) distinguish 'core science' (pure or basic), applied science, 'professional consultancy', 'post-normal science', with increasing decision stakes and increasing systems uncertainties. My findings could stimulate this literature to further explore the stakeholder characteristics of different scientific fields, as the inclusion of users in 'extended peer review' will be much easier in fields with clearly identifiable upstream end-users.

'Finalization theory' would explain changes in the struggle for relevance by the internal dynamics of scientific fields. According to finalization theory scientific fields undergo a generic development from an explorative to a paradigmatic and a post-paradigmatic phase (Böhme et al. 1983). Their claim is that once a field has matured and sufficient consensus has been attained about the ruling paradigm, the optimal conditions are fulfilled for fruitful interactions with firms or other knowledge users. This reason-

142 For example, an empirical analysis of university-industry-government relations in South-Korea has shown that governmental policy to stimulate publications in international scientific journals has contributed to the erosion of the synergy in Triple Helix relations (Park & Leydesdorff 2010).

ing, however, is in contradiction with my finding that relatively young field of APS has developed more intensive stakeholder relationships than mature fields like cell biology and biochemistry.

Most of the literature about innovation systems and Triple Helix interactions is of a generic nature and does not distinguish among industrial sectors or scientific fields (e.g. Motohashi (2005)), but these bodies of literature also contain studies that show that in some sectors there are stronger links between academic science and industry than in others. Their explanations are not restricted to characteristics of the scientific fields involved, but also involve economic and social characteristics of the industrial (or governmental) organizations. For example, a Triple Helix study on micro-electronics showed a synergetic relationship between scientific productivity and industrial collaborations similar to my case studies of catalysis and ABG (Balconi & Laboranti 2006). The authors explain this (partly) by the close social relationships between firms and academic research groups, and by the industrial need for recruitment of productive researchers. An important limitation of Triple Helix and innovation system literature is that both tend to 'black-box' research *practices*. The understanding I have gained of the incentive structure of university research could help them to improve the understanding of why science-industry relationships are much more successful in some fields than in others.

In contrast with these four diagnoses, The New Production of Knowledge (NPK), Academic Capitalism and Post-Academic Science lack a proper differentiation among scientific fields. Their main concern seems the identification and investigation of general phenomena rather than exploring and explaining the differences among fields. NPK (Gibbons et al. 1994) only contains generic claims; it does not systematically differentiate across scientific fields. Its arguments mainly deal with science, technology and medicine; one chapter of the book makes a comparison with the (aggregated) humanities. The authors use examples from different scientific fields, these all serve the same general argument. Ziman's book about Post-Academic Science (2000) also has a rather loose empirical foundation and does not differentiate among scientific fields or disciplines. 'Academic Capitalism' (Slaughter & Leslie 1997) presents data on specific departments representing different fields/disciplines, but the authors hardly elaborate on their differences. Altogether these three books raise the suggestion that all fields of science are subject to a generic shift, and that there are only differences in degree and not in kind.

In conclusion, the debate about changing science systems can benefit from my findings regarding the different dynamics of scientific fields. This thesis has shown how, under similar institutional conditions, some fields develop a stronger practical orientation, while others strengthen their academic identity. By providing an explanatory framework for this variation based on characteristics of fields and their stakeholders (Section 7.4), this thesis contributes to the development of a more differentiated view on the changing relationship between science and society.

A shift towards Mode 2 knowledge production?

The diagnosis about 'Mode 2 knowledge production' (Gibbons et al. 1994) deserves special attention, because it is probably the most influential writing on changing science systems of the past few decades[143]. Our systematic literature study raised the suggestion that Mode 1 and Mode 2 should be seen as ideal-types rather than categories of existing research practices (Chapter 2). What is more, because of the limited coherence of the five major attributes of Mode 2 (see Table 7.6), it seemed appropriate to investigate them separately. Although the central research questions of this thesis were not expressed in terms of Mode 2 attributes, my findings give an indication regarding the degree to which they have changed in the Netherlands. In short, my empirical results confirm two of the trends announced by Gibbons et al. (1994), but challenge the three others, in particular the claim that quality control is increasingly dominated by societal criteria.

Table 7.6 Results in terms of Mode 2 attributes

Attribute	Status in literature*	Trend in my case studies
Context of application	Supported	Increasing in most fields
Heterogeneity	Supported	Increasing in most fields
Transdisciplinarity	Contested	Increasing, but still marginal, due to lack of incentives and rewards
Reflexivity	Contested	Increasing, but accompanied by increasing *scientific* accountability
Novel quality control	Contested	Increasing in funding distribution, but not in other processes

*see Chapter 2

In agreement with other literature, my case studies indicate that in most fields the *'heterogeneity'* of knowledge production is increasing, and that a growing share of it is conducted in the *'context of application'*. In most fields knowledge production is increasingly organized around possible practical applications. For example, research in paleo-ecology is more and more inspired by knowledge needs of environmental policy, and animal breeding & genetics research is increasingly oriented to industrial applications. This trend seems mainly driven by shifts in available funding. It also relates to the increasing heterogeneity of knowledge production I observed in most fields. Beside academic researchers, industry, policy makers and other (possible) knowledge users make active contributions to the research process. In funding arrangements, such as the Dutch Technology Foundation STW and Technological Top Institutes, users committees are set up to give feedback on the progress of individual projects or

143 Currently (June 2010) 'The New Production of Knowledge' (Gibbons et al. 1994) has been cited almost 1900 times, and its popularity seems still increasing (Hessels & van Lente 2010a)

aggregated programs. My interview data suggest that such input significantly contributes to the production of knowledge. Also, in ABG and catalysis, a significant share of all research activities is organized in teams including both academic and industrial researchers. In other fields, like APS and environmental chemistry, active contributions of researchers from public research institutes are increasingly essential to the research process.

However, the rise of the other three attributes of Mode 2, transdisciplinarity, reflexivity, and novel quality control, has less support in the literature (see Chapter 2). My case studies provide further reasons to doubt the idea of a homogeneous rising trend of these characteristics. Regarding all three attributes, my empirical data indicate a mixed picture: a certain increase is visible, but it is accompanied by an undercurrent in the opposite direction, most significantly regarding quality control.

My findings do not support the view of a general increase of the dynamic integration of elements of several disciplinary backgrounds. *Transdisciplinarity* can only clearly be recognized in one of my cases, animal production systems (APS). Here I observed that novel combinations are made of methods and theories from other disciplines, in order to develop knowledge to support sustainable animal production. Researchers develop 'distinct theoretical structures, research methods and modes of practice' (Gibbons et al. 1994 p. 168) that can not be easily located on the prevailing disciplinary map. In a weaker sense, some of these aspects have also become visible in ABG, toxicology and paleo-ecology, but for this type of research 'multidisciplinarity' or 'interdisciplinarity' seem more appropriate terms than 'transdisciplinarity'. In these fields the integration of elements from different disciplinary backgrounds is less dynamic and interactive than in APS. Toxicologists collaborate increasingly with medical scientists; paleo-ecologists with geo-scientists, but their resulting approaches seem still reducible to their disciplinary parts. In correspondence with suggestions in the literature (Merkx & van den Besselaar 2008, Kaufmann & Kasztler 2009), the academic reward system does not stimulate transdisciplinarity in APS. The pressure for academic publications stemming from dominant mechanisms of quality control tends to work as an inhibitor for transdisciplinary research, as this does not typically result in prestigious journal papers. Altogether, my case studies suggest that the share of transdisciplinary research is growing, but that it is far from a mainstream approach in Dutch university research.

With regard to *reflexivity* (or 'social accountability'), my case studies indicate an ambivalent development. Judging from my interviews, in general the reflexivity of Dutch academic researchers seems to have increased over the past 30 years. Due to the need for promising 'relevant' contributions in order to get funding and the shifting societal views on academic research, scientists have become more sensitive to the broader implications of their work. Regardless of the basic or applied nature of his/her research, a certain awareness of potential applications has become a valuable asset for every academic scientist. Even most biochemists, who interact little with societal stakeholders and whose work does not regularly lead to direct policy solutions or technological innovations, nowadays have a view on the societal impact their work could have on the long run[144]. The reflexivity is obviously much higher in fields that are more strongly connected with practical applications, such as APS, paleo-ecology and catalysis. In these fields, a consciousness of possible impact is continuously present, and this is stimulated by the frequent interactions with knowledge users in industry or the government. In more fundamentally oriented fields, like biochemistry and cell biology, such consciousness is present mainly during the funding acquisition phase, and fades away once the actual research work is started.

The increased reflexivity I have observed can be understood as a response to a societal pressure for accountability. My three case studies indicate that society has developed a need for more reflexive science, science that is aware of (or even anticipates on) its own intended and unintended (side-)effects. The increasing policy steering of research priorities and stimulation of interactions with stakeholders can (also) be seen as a reaction to public concerns about undesirable side-effects of technoscientific development. The effect of these policies can be interpreted as a de-differentiation of different societal realms (Nowotny et al. 2001). Beside scientists, academic researchers increasingly take up the roles of public experts and of entrepreneurs[145].

Yet, my research shows that the (growing) reflexivity of scientists is limited by characteristics of the research system, especially the pressure for high scientific productivity. This pressure has led to an increasing *scientific* accountability, that is, awareness of the *scientific* value of academic research. The increased pressure for excellent academic performance has incited an increased consciousness of possibilities to turn one's outcomes into scientific publications, preferably in high-impact journals. This parallel development should definitely be taken into account when discussing the increasing reflexivity, especially because in some cases (like toxicology and cell biology) the scientific accountability seems to have increased much more than the social accountability.

The strongest disagreement between my empirical results and the Mode 2 diagnosis concerns the question of changing academic *quality control.* My findings contradict the claims by Gibbons et al. (1994) and by Hemlin and Rasmussen (2006) that academic quality control is increasingly ruled by societal considerations and conducted by non-scientific actors. In most fields, the various mechanisms of academic quality control turn out to have developed in different, partly opposite directions. Similar to

144 E.g. 'With this method you can look at protein quantities in response to external changes. Eventually you can summarize this in a computer model. So then you actually have a sort of model of a cell. And if you administer drugs, then what will happen in the cell? Maybe you can then predict what effects it has. With a model you know what processes correlate within a cell. So when something changes, then this will change and that. If you develop a certain drug against a particular disease and you administer it, then you assume that it only has the effect you are aiming for. But it could also influence other processes. And that is what you could predict with such a model.' (Researcher C4)

145 Although these new roles certainly enhance the reflexivity of scientists, because they widen their perspective, they can also create new side-effects, due the new stakes and interests that scientists develop. Active engagement in commercial activities or policy advice can threaten the disinterestedness of scientists (Ziman 2003, Jacob forthcoming). Because of their personal stake in particular research outcomes, the objectivity of their research could be in danger and this could eventually lead to undesirable outcomes, like technologies carrying unacceptable risks to human health or the environment.

Schmoch and Schubert (forthcoming), I found that the different practices of quality control each have their own criteria, which are often contradictory. In agreement with the claims by Gibbons et al., the procedures used in the distribution of research funding are multidimensional and increasingly use valorization, societal relevance or equivalents as a selection criterion. However, job interviews, performance interviews and performance evaluations most strongly reward scientific productivity and scientific impact. In these practices of quality control, a narrow set of (disciplinary) scientific criteria dominate, in particular publications numbers, journal impact-factors, and citation scores. Altogether my findings do not indicate that in the Netherlands societal considerations and users are increasingly powerful in academic quality control. Peer review remains a dominant steering mechanism controlling problem choice, career advancement and reputations. The wider set of criteria and broader social composition of the review system that Gibbons et al. predicted have only become visible in the mechanisms controlling who receive funding, but not in mechanisms controlling what precise topics are investigated, what research methods are used or what papers are published. In short, nowadays societal considerations do play a significant role in quality control in the funding phase, but not during or after the actual research phase.

In conclusion, my empirical findings regarding Mode 2 further substantiate the theoretical criticisms uttered in Chapter 2. The fieldwork reported in this thesis shows that the dynamics of contemporary science systems are neither homogenous nor unidirectional. The issues of reflexivity and quality control are ambivalent and vary strongly across scientific fields. Based on these findings I conclude that the idea of a homogeneous transformation of the science system is misleading.

7.7 Policy recommendations

The research reported in this thesis was primarily a scholarly endeavor. My first aim was not to assess the effectiveness of science and innovation policies, but rather to enhance the understanding of changing science systems. Still, given the topic of this thesis my analysis dealt in quite some detail with policy instruments like funding mechanisms and evaluation processes, and with their effects on academic research practices. For this reason, it seems appropriate to reserve some space for a consideration of the practical implications of my findings.

The two most salient developments in the governance of academic research discussed in this thesis are the pressure for valorization related to the diversification of funding and the rise of performance evaluations. In the following I will make a number of critical remarks on both topics. With regard to funding, I comment on the dangers of a restrictive notion of 'valorization'. Concerning evaluations, I point to the dangers and limitations of bibliometric quality indicators. I will conclude with a list of recommendations.

However, I start with a more general statement. For a long time, the distinction between basic and applied research has served as a landmark in science and innovation policy. Nowadays this dichotomy is denied in most policy documents. This denunciation is inspired by literature about Mode 2 knowledge production, strategic science and Pasteur's Quadrant. These contributions claim that fundamental and applied research are not mutually exclusive and that research projects can be perfectly fundamental and applied at the same time. However, my results indicate that in many fields there is still a tension between enhancing fundamental understanding and generating practical benefits, and in some fields this tension is pretty strong (e.g. in environmental chemistry). The idea that these two go very well together in all fields of science may be wishful thinking, based on the extrapolation of observations on a limited number of fields such as catalysis or biotechnology. May this thesis stimulate awareness of the possible tension between practical applications and scientific excellence: a strong pressure for valorization can threaten the viability of fundamental research lines and a strong pressure for academic excellence can threaten the alignment of research activities with stakeholder needs. This general concern forms the starting point for the more specific considerations below.

Knowledge valorization

As my analysis has shown, the latest conception of the relevance of university research in the Netherlands is strongly connected with the idea of 'knowledge valorization' (see Table 7.1). This term refers to 'making sure that research results are being applied and converted into economic, financial and societal value'[146]. Nowadays valorization figures as a goal in a large share of public funding arrangements for Dutch university research[147].

In principle, knowledge valorization seems a perfectly legitimate goal. As academic research is still paid predominantly from public money, the generation of societal benefits seems a logical requirement. Valorization as defined above is so general that one can hardly object to it. It is even similar to 'relevance' in the sense that actors can give it different meanings, according to their own preferences and interests.

However, there is a tendency to restrict the meaning of valorization by placing specific emphasis on *economic* value, generated on the *short* term (Benneworth & Jongbloed 2010). For example, the Ministry of Education, Culture and Science defines valorization as 'the conversion of research results into economic value'[148]. The restriction to short term economic contributions carries four dangers.

First this limited notion of valorization suggests that the specific benefits of university research can be predicted, or even planned. This perspective is incompatible with the view of most researchers I

146 VSNU, 2005, 'Onderzoek van waarde: Activiteiten van universiteiten gericht op kennisvalorisatie', Utrecht.

147 In its most recent strategy paper, research council NWO has announced that it will pay attention to possible 'knowledge utilization' (which is essentially a synonym for valorization) within all research it is funding (NWO, 2010, 'Groeien met kennis: Strategienota NWO 2011-2014', Den Haag)

148 Ministry of Education, Culture and Science, 2004, 'Focus op excellentie en meer waarde: Wetenschapsbudget 2004', Den Haag., p. 12

have interviewed, who experience writing valorization plans (and the like) as window-dressing[149]. Most procedures for acquiring (application-oriented) funding from public sources do not do justice to the diversity and unpredictability of science. Many of the programs of the Dutch research council NWO and European Framework Programmes require detailed projections of the expected societal benefits or even a plan or strategy to ensure such benefits are realized. In some fields it seems appropriate to anticipate on collaborations with specific firms or the development of a particular product, but in others this does not make sense.

Second, the value of academic patents is systematically overrated. University researchers experience a pressure from their employers and by research councils to patent potentially commercially relevant findings. However, my impression is that this is driven by the need of these organizations to legitimate their own societal position, rather than by truly financial motives. Academic patents are often regarded as an indicator for successful valorization. As a result, many patents may be filed for symbolic reasons[150]. Very few of my respondents received significant income from patents. This fits the global picture in which only a few universities manage to attract substantial revenues from patents; for most universities patenting is not profitable (Geuna & Nesta 2006).

Third, the emphasis on short term benefits endangers the viability of research lines oriented to fundamental understanding. Valorization policy tends to restrict the autonomy of scientist to choose their own research questions and approaches. My fieldwork shows that in some fields the demand for practical applications leads scientists away from fundamental questions. In my opinion there are at least three reasons to protect fundamental research lines:

1. In spite of the current hype of high-tech spin-off companies, the most effective channel for knowledge valorization probably remains the training of new generations of scientists (Salter & Martin 2001). Scientists trained in basic research are a crucial requirement for innovative performance. If all PhD-projects are tied to practical goals, the new generation of researchers could lack a basis of fundamental understanding, especially in fields where there is a tension between practical applications and scientific excellence.
2. One of the crucial functions of academic research activities is securing the quality of higher education programs by keeping 'teachers' at the forefront of academic debates. It strikes me that the education function of universities receives little attention in the current valorization discourse. Some research areas which are not particularly promising in terms of generating direct social or economic impacts are highly relevant with regard to education, most strikingly in the social sciences and the humanities. On the longer run, too much steering on valorization could endanger the quality of higher education in these areas.

149 This was also shown by Morris (2000) and Roberts (2009)

150 A survey among scientists at German Max Planck Institutes has shown that their patenting behavior is more strongly influenced by a quest for reputation than by expectations of real financial benefits (Göktepe-Hulten & Mahagaonkar forthcoming)

3. Third, basic research also has a cultural function. In the numerous conversations I have had about the societal relevance of science with people inside and outside the science system I noticed that there is widespread sense that enhancing the understanding of matter, life, or the universe is part of our culture, regardless of its practical benefits[151].

The fourth danger of the restricted concept of valorization relates to the importance of external funding sources for the survival of research groups. The overall decrease in block grant support for university research threatens the stability of academic research groups and the coherence of their activities. Research groups now have to combine a diverse collection of funding sources, each with their own demands. Given the limited total sum of public funding available for academic research, many scientists depend to a large extent on external, often commercial funding sources. However, empirical evidence indicates that industrial funding is not a decisive factor for successful valorization[152]. In some fields (like toxicology and environmental chemistry) I have found instances where external sponsors threaten the independent position of university researchers. Commercial sponsors often delay the free communication of research results. Moreover the integrity of scientists is endangered in the case their sponsors have a strong stake in particular research outcomes.

In conclusion, a restricted concept of valorization is a danger for the long term viability of science, as it threatens several of its vital functions. On the longer run, this perspective impedes rather than stimulates the conversion of research results into economic and societal value. Truly successful valorization requires a long-term horizon. Because most societal benefits become visible long after research projects are finished, it makes more sense to govern by strategic priorities than by immediate impact.

Bibliometric quality indicators

The second issue that I want to comment on is the dominance of bibliometric quality indicators in scientific performance evaluations. In my fieldwork I observed that bibliometric data (like publication numbers, impact factors, citations) enjoy an ever growing popularity as indicators of the quality of individual scientists, organizations or journals. When used with care, bibliometric analysis can be a valuable tool to evaluate the performance of individuals, research groups or institutes. Bibliometric indicators have been adopted so abundantly both in formal and informal quality assessments, without proper acknowledgement of their limitations, that they have radically changed the meaning of a scientific paper. Originally a means of communication, publications have become an end in itself. This development carries at least three dangers:

151 A possible fourth reason for cherishing fundamental research is that autonomous fundamental research can also lead to unpredictable outcomes with a social or economic impact that is much higher than research that was originally designed to contribute to particular practical goals. However, because this effect is difficult to prove, it is difficult to draw conclusions about the amount of basic research needed to this end.

152 Between 1997 and 2004 Stanford University, generally considered as one of the most successful examples of academic entrepreneurship, has received less than 5% of its funding from industry (Tindemans & Soete 2007).

First, I have seen that bibliometric indicators tend to restrict the concept of academic excellence. The excessively high value attributed to numbers of journal papers and citations has left little room for other aspects of academic performance, in particular teaching and knowledge transfer or dissemination activities (Hessels & van Lente 2010b). Also, applied research and interdisciplinary research (even if it is of a rather fundamental nature) is undervalued, because it usually yields fewer papers in high-impact journals.

The second risk of bibliometric quality indicators is the promotion of a monoculture of science. Across all fields, the scientists I have interviewed experienced a pressure to assimilate to the 'urban' communication style (Becher & Trowler 2001) that in principle fits only a limited share of the sciences. The generic incentives for more publications in scientific journals endanger fruitful publication traditions in some fields that involve more book publications and papers in native languages (like animal production systems). Forcing these fields to assimilate to the dominant publication form could increase the efficiency of their communication mechanisms, but it threatens the relevance of their work for local stakeholders and it could also threaten the viability of larger (and more risky) research projects, the results of which can not be easily be communicated in brief journal papers.

Third, the high pressure for publications creates an inflation effect on the value of a scientific paper. In the pressure to publish the sheer number of papers often seems more important than their precise content or quality. Several respondents have reported to divide their results into the 'smallest publishable units' to enhance their publication rate[153]. Sometimes the same findings can be published in several different journals, if the author manages to opportunistically emphasize different aspects of the same study for different audiences. This trend probably decreases the efficiency of the scientific communication system.

To conclude, bibliometric quality indicators are useful tools in principle, but their universal application has several perverse side-effects. This problem can not be solved by only developing more sophisticated indicators, it is also necessary to reduce their general influence on decisions in the academic world. I see two possibilities to do so: complementing quantitative bibliometric indicators with qualitative assessments, and complementing them with indicators of social and economic impact.

153 My qualitative observations on this point can be supported with quantitative evidence from Australia, where the introduction of performance-based funding formulae resulted in an increase of the average productivity, but a decrease in the average impact of the papers published (Butler 2003).

Practical recommendations

I finish with two sets of recommendations that may help to alleviate the problems identified.

Regarding research funding:

1. Loosen the 'valorization' requirements for grants from the national research council NWO. It makes sense to stimulate applicants to think about possible practical applications, but not to force them to plan specific knowledge dissemination activities, in a phase they don't know yet what type of knowledge they will generate.
2. Make a more careful cost-benefit analysis before patenting academic inventions. Do not use patents as an exclusive indicator of valorization success. In most cases, commercially valuable inventions can better be sold directly to an interested company rather than patented.
3. The governance of academic research activities should be based on expected long-term benefits rather than short-term benefits. Organize processes of strategic foresight in order to explore and choose long-term priorities for scientific research[154]. Such exercises should be protected from dominant incumbent forces, in order to prevent the reproduction of existing (economic) powers into future priorities. Moreover consistency should be strived for, by setting strategic goals for periods beyond the regular term of a governmental administration.
4. Restore and protect a healthy share of unconditional block grant support for university research. A further decrease of this money flow would endanger the long-term viability of the Dutch science system.

Regarding research evaluation:

1. Use bibliometric indicators with care. In any quality judgment (whether dealing with individuals, organizations or journals), bibliometric indicators should be accompanied by a qualitative analysis. A direct linkage between funding and bibliometrics should be avoided under all circumstances. Bibliometric quality indicators have important methodological limitations, especially when evaluating individuals and when comparing across fields. What is more, if funding decisions are left to automated procedures a crucial opportunity for reflection is missed. Decisions about the future should not be based only on past successes but also on a consideration of future priorities, based on foresight studies and normative goals.
2. Broaden the criteria of performance evaluations. As a recent series of pilot studies has shown, there are also possibilities to measure social and economic impact of academic research[155]. As most societal benefits only become visible on the longer run, such indicators are not applicable to all scientific

154 Inspiration can be drawn from the UK Foresight Programme, which conducts in-depth studies 20-80 years into the future (http://www.foresight.gov.uk).

155 ERiC 2010, *Handreiking Evaluatie van maatschappelijke relevantie van wetenschappelijk onderzoek*, ERiC publicatie 1001

fields. However, in numerous fields they can have an equal position next to scientific quality, productivity and relevance.

3. Broaden the criteria for the promotion and hiring of academic staff. They should be less dependent on quantitative indicators of a candidate's research performance, and more on a qualitative assessment of the *content* of his/her work, including any societal impacts. In addition teaching qualities deserve an equal position next to research qualities in such decisions. This could stimulate scientists to invest more time and energy in teaching.

7.8 Conclusion

When and how can science be called relevant for society? Science promises considerable societal benefits: it will help to improve public health, enhance global security and protect the environment. Still the outcomes of research activities are inherently uncertain and unpredictable. How to govern science in order to optimize its relevance for society? The understanding of the complex relationships between academic research and its practical applications is limited. What is more, the question of relevance is particularly urgent against the background of the current debate about changing science systems.

In this thesis I have studied the various *struggles for relevance* of university researchers, in other words, their efforts to make their work correspond with ruling standards of relevance, and to influence these standards. I have conceptualized these struggles as part of the credibility cycle. The agency of scientists in this cycle is influenced by structural conditions, which can be seen in terms of a science-society contract or as a research system. My empirical investigations have shown how struggles for relevance have changed over the past 30-35 years in eight fields of natural science in the Netherlands and provided starting points for explaining the changes observed.

Across all scientific fields, I observed three common developments: the intensification of the struggle for relevance during the collection of data, the decreasing value of practical applications as a source of recognition, and the intensification of the struggle for relevance in the context of funding acquisition. These general trends can understood as effects of the combination of three major changes in the structural conditions of academic research in the Netherlands: the diversification of funding, the rise of performance evaluations, and changing views on the societal position of the university.

The changes in the struggle for relevance also varied across scientific fields, along four dimensions: the intensity of stakeholder interactions during data collection, the precise value of practical applications as a source of recognition, the actual influence of stakeholders on the research agenda, and the relationship between practical applications and scientific productivity. A possible explanation for these differences can be found in the combination of the degree of convergence in their search pattern, their strategic task uncertainty, their traditional communication culture, and characteristics of their upstream end-users.

This study has two major implications for the debate about changing science systems. First, it shows that the increasing pressure for productivity, as measured in bibliometric terms, can counteract the pressure for practical utility. In many fields I observed a tension between research activities that are useful for societal stakeholders and activities that are most promising in terms of scientific publications. Second, my work indicates that a further differentiation is needed, as the dynamics of science vary much more across scientific fields than most literature suggests. Both findings challenge the empirical validity of some central claims around the notion of Mode 2 knowledge production, in particular regarding transdisciplinarity, reflexivity, and novel quality control.

Both the meaning of relevance and the ways in which scientists struggle for relevance are contingent; they change over time and vary across scientific fields. This thesis has mapped some of these changes and variations, and the tensions they may create. By providing conceptual clarity and empirical insights, this thesis contributes to the scholarly debate about changing science systems. On the longer run, I hope this thesis will, directly or indirectly, also contribute to the possibilities of science governance to effectively increase the societal benefits of science. After all, academic research is intimately entangled with relevance, and it probably always will be.

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Summary

The starting assumption of this thesis is that academic researchers *struggle for relevance*. To a lesser or greater extent, they need to make sure that their work is valuable to society, either directly or indirectly. This can give personal satisfaction, but it also helps to legitimate one's work to the outside world and to get access to resources. However, striving for relevance is not a straightforward exercise. First, researchers may face difficulties when trying to reconcile the need for relevance with other plans, ambitions and values. Second, the meaning of relevance is not fixed; it is the product of social interactions among scientists, policy makers and other stakeholders. Because of these aspects it makes sense to speak of a 'struggle'.

In this thesis, the struggle for relevance is defined as the combination of the efforts of scientists to make their work correspond with ruling standards of relevance and their efforts to influence these standards of relevance. With this study of the relevance of science I aim to contribute to the understanding of transformations in the knowledge infrastructure, as discussed in a large and expanding literature using concepts like Mode 2 knowledge production, post-academic science and the Triple Helix of university-government-industry relationships.

The main research question of this thesis is: How to understand the changes in the struggle for relevance of Dutch academic researchers in chemistry, biology and agricultural science, in the period 1975-2005?

My theoretical starting point is a distinction between the agency of scientists and the structures patterning their behavior. In line with structuration theory, I assume that these structures are both medium and outcome of research practices. Further I regard science as a social activity, and the progress of science as the outcome of socially embedded actions of researchers. Another starting assumption is that scientists are involved in a continuous struggle for resources, as they depend on their environment for funding and legitimacy. Based on this theoretical foundation, in this thesis I develop and use three central concepts to analyze struggles for relevance: the credibility cycle, the science-society contract, and the research system.

I use the credibility cycle to conceptualize the struggles for relevance of individual scientists. This model explains how struggles for reputation influence the daily work of individual scientists and depicts

the research process as a repetitive cycle in which conversions take place between money, staff, data, arguments, articles, recognition, and so on. I investigate the changing structures influencing scientific agency from two different angles, using the concepts of a science-society contract, and a research system, respectively. The contract refers to all implicit and explicit agreements between science and societal parties about what science should do, why it should do this, and what are the appropriate conditions for science to function well. The research system draws attention to the institutional environment of academic research, consisting of research councils, firms, public research institutes, governmental departments, and other organizations that provide incentives and constraints to conduct particular kinds of research.

This research follows a case study approach and deals with chemistry, biology and agricultural science in the Netherlands. Within each case I focus in particular on two or three fields representing the breadth of the discipline in terms of possible stakeholders. This thesis deals primarily with *academic* knowledge production, that is, research conducted at universities. Data have been drawn from in-depth interviews with 47 academic researchers, and with a number scholarly experts and representatives of various organizations in the research system. Moreover I have analyzed a selection of governmental policy documents, reports and strategic plans of research councils, foresight studies, evaluations and other important publications about the disciplines.

After an introductory chapter presenting the background of this study, its theoretical framework and methodology, Chapter 2 provides a state-of-the-art of the current literature on the present debate on transformations in the knowledge infrastructure. The notion of 'Mode 2 knowledge production' (Gibbons et al, 1994) was chosen as a starting point in this literature review, because of its prominence in this debate, both in academia and policy circles. I systematically review its reception in scientific literature and compare this concept to a number alternative diagnoses of changing science systems, such as the 'Triple Helix' and 'Post-normal science'. I conclude that the strength of The New Production of Knowledge (NPK) is its exceptionally wide scope, as it deals with changes on the cognitive, organizational and societal level. Thanks to its breadth, NPK has created a forum to discuss a wide range of putative trends in the science system. However, based on the review of critical reactions in scholarly literature I also identify a number of problems, regarding the empirical validity, the conceptual strength and the political value of NPK. The most important problems are the lack of empirical support and the questionable coherence of the five constitutive features of 'Mode 2'. For this reason I recommend separating these features and investigating each separately. The review is concluded with a research agenda for scholars in Science, Technology and Innovation Studies, dealing with the three most controversial features of Mode 2 knowledge production: transdisciplinarity, reflexivity, and quality control. This agenda has formed the starting point for formulating the research question of this thesis, as presented above.

After the literature review, my next step is a further conceptualization of (struggles for) relevance in terms of my theoretical framework. Chapter 3 develops the first two building blocks of this framework: the science-society contract and the credibility cycle. Based on a brief review of the usage of the contract metaphor in the literature, I propose to use it as a heuristic for studying the changing relationship between academic science and society. I suggest regarding its content in terms of three main components, dealing with the identity of science, the rationales for supporting it, and the conditions under which university researchers work, respectively. The second element of my framework is the concept of the credibility cycle. In a brief case study of Dutch academic chemistry the usefulness of these concepts is illustrated. In this case study I explore how changing concepts of 'relevance' in the science-society contract influence scientific practices, with a particular focus on the institutions governing the credibility cycle. This analysis gives a first indication of the tension between the increasing pressure for scientific publications and the increasing pressure for relevant outcomes – increasingly defined in terms of practical applications.

The following three chapters (Chapters 4-6) use and enrich (elements of) this framework in the analysis of case studies of Dutch chemistry, biology, and agricultural science, respectively. Each study addresses similar questions about the changing struggle for relevance. Chapter 4 presents a more elaborate study of the chemistry case. Between 1975 and 2005 four major changes can be identified in its science-society contract:

1. the relative share of funding for application oriented university research has grown;
2. the interactions with industry have become more intensive;
3. universities attempt to play an active role in the valorization of research outcomes; and
4. systematic performance evaluations have become a powerful institution governing academic research.

The consequences of these developments for the struggles for relevance of researchers vary across the three chemical fields studied. In catalysis linking with industrial applications helps in many steps of the credibility cycle. Practical applications yield much less credibility in environmental chemistry, where application oriented research agendas help to acquire funding, but not to publish prestigious papers or to earn peer recognition. In biochemistry practical applications hardly help in gaining credibility, as this field is still strongly oriented at fundamental questions. The differences between the fields can be explained by the presence or absence of powerful upstream end-users that can afford to invest in academic research that promises *long term* benefits.

Chapter 5 addresses the influence of changes in the contract on the actual behavior of academic researchers and the content of their work. It focuses in particular on multidisciplinary collaborations in paleo-ecology and toxicology. This chapter shows that the societal contract for biology has undergone a development similar to the one of chemistry. The identity of academic biology has changed from a basic to a strategic science; rationales for funding biology increasingly emphasize practical applications; the available funding has shifted and performance evaluations have emerged. These developments turn out to provide both pressures and incentives for multidisciplinarity that differ between the two fields under study. In paleo-ecology a rise in multidisciplinary research collaborations can be understood as a response to outside pressures to do more 'relevant' research, particularly in the area of climate change. In toxicology I observe the opposite: multidisciplinarity is used to strengthen the fundamental research on physiological mechanisms of toxicity. I conclude that multidisciplinary research collaborations can serve various ends, ranging from enhancing the practical value of a fundamental field to strengthening the fundamental basis of an applied field.

The central question of Chapter 6 is: how do changing institutions influence academic research practices in Dutch agricultural sciences? This chapter deals in particular with three fields of animal science. These fields have experienced a highly dynamic societal context. Dutch agricultural sciences are under pressure to change the content and the organization of the research in order to contribute to more competitive and sustainable agricultural production. In this chapter I analyze the changes in the institutions of the agricultural research system and their consequences on the daily work of Dutch animal scientists. In the period studied, I observe a shift from the consensus-driven 'OVO triptych' to a heterogeneous knowledge system. This shift entails two major changes in the institutional environment of academic agricultural research: shifts in the available funding, and the rise of systematic performance evaluations. As a result, in two fields the interactions with stakeholders have increased, but for different reasons. In Animal Breeding & Genetics (ABG) researchers have frequent contact with breeding firms in order to select challenging research issues, to collect data, and to acquire research funding. Thanks to these interactions, ABG manages to obtain sufficient external funding, without compromising its basic research agenda. In Animal Production Systems (APS) contacts with farmers only contribute to data collection, while contacts with applied research institutes and with NGOs help to get access to funding. This seems a vulnerable combination, as APS struggles to build a coherent research agenda and to publish sufficient scientific papers. In Cell Biology, the third field studied, interactions with 'users' have decreased rather than increased, as there is a tendency to shift to more fundamental research issues with a higher potential for prestigious publications, but which are not of immediate interest to the agricultural sector.

In Chapter 7 I provide a further reflection on the material presented in the previous chapters. I answer the general research question by subsequently addressing three sub-questions.

1. *What changes can be discerned in the struggles for relevance of Dutch academic researchers in chemistry, biology and agricultural science, in the period 1975-2005?*

Across all scientific fields, I observed three common developments. First, in most fields the struggle for relevance during the actual research process has intensified. In several fields interactions with possible users of research outcomes were already common practice, but in general their frequency and salience have grown. The second common trend relates to the way scientists earn recognition: on average, the value of practical applications as a source of academic recognition has decreased. Since the 1970s publication achievements have (further) pushed away social or economic impact as a source of academic recognition. Third, the struggle for relevance in the context of funding acquisition has intensified. In all fields the scientists I have interviewed report that aligning their work with the knowledge needs of societal stakeholders has become increasingly important for gathering sufficient funding. T

The changes in the struggle for relevance also varied across scientific fields. First, although the role of stakeholders in the academic research process has generally grown, the *extent* to which they have become involved in data collection varies strongly across fields. Second, there is variation in the (limited) *degree* to which practical relevance is rewarded in terms of academic recognition. Third, in all fields promising societal benefits can help to acquire funding, but the *degree* of involvement of societal stakeholders in the actual agenda-setting differs. A fourth dimension concerns the tension that has arisen in some fields due to the combination of these trends. In some fields there is a synergy between scientific productivity and practical applications because interactions with stakeholders do not only help to acquire funding, but they are also helpful in other credibility conversions. In other fields there is a tension between scientific productivity and practical applications, as enhancing the industrial, medical or agricultural relevance of one's work implies a distantiation from these fields' central debates. To this end, one would have to shift to other model systems or to address other research questions which are less suitable for publishing in prestigious journals and which are less likely to yield academic recognition.

2. *How can the similarities among the changes in different scientific fields be explained?*

The general trends across the fields studied can be understood as effects of the combination of three major changes in the structural conditions of academic research in the Netherlands. The first is the diversification of funding sources. Between 1975 and 2005 the share of application oriented funding has increased. Simultaneously, support for talented researchers has emerged, rewarding scientists who are highly productive in terms of scientific publications. The second structural change that has shaped struggles for relevance in all scientific fields studied is the rise of performance evaluations, which are dominated by bibliometric quality indicators. The third important structural change concerns the dominant perspective on the mission of the university in the Netherlands. Since 1975 notions like accountability, valorization and entrepreneurship have gradually replaced autonomy and independence as the ruling values connected with academic research.

3. *How can the differences among the changes in different scientific fields be explained?*

One of the most striking findings of this thesis is that the changes in the struggle for relevance vary strongly across scientific fields. A possible explanation for the differences among scientific fields can be found in the combination of the degree of convergence in their search pattern, their traditional communication culture, and characteristics of their upstream end-users.

First, in divergent fields, which have a high strategic task uncertainty, it seems more likely that niches develop which fit the knowledge needs of societal stakeholders than in convergent fields in which there are higher penalties on deviating from the shared research agenda.

Second, the extent to which practical applications count as a source of credibility can be understood when taking into account the traditional communication culture of a scientific field. In fields with a 'rural' communication style in which there is a low people-to-problem ratio, there is usually a lower citation density. This implies that bibliometric quality indicators have limited validity, so that it is less likely that recognition will be based on publications only and more likely that practical outcomes such as policy advice, patents or spin-off firms count as well. In more competitive, urban fields (high people-to-problem ratio), with a high citation density, bibliometric quality indicators will be more readily used, not only in formal evaluations and management decisions, but also in informal processes of exchanging recognition.

Third, a possible explanation for the variation in the relationship between practical applications and scientific productivity can be found in characteristics of other actors in the research system, in particular the end-users of academic research. The fields that turned out most successful in combining stakeholder interactions with academic performance, were fields with wealthy and powerful upstream end-users with a long-term vision on the utility scientific research. By sponsoring fundamental research activities, which provide good opportunities for high-impact publications, chemical industry (in the case of catalysis) and animal breeding industry (ABG) support scientists along the complete cycle of credibility.

My findings have two major implications for the debate about changing science systems. First, my thesis shows that the increasing pressure for productivity, as measured in bibliometric terms, can counteract the pressure for practical utility. In many fields I observed a tension between research activities that are useful for societal stakeholders and activities that are most promising in terms of scientific publications. Second, my work indicates that a further differentiation of science (policy) studies is needed, as the dynamics of science vary much more across scientific fields than most literature suggests. Both findings challenge the empirical validity of some central claims around the notion of Mode 2 knowledge production, in particular regarding transdisciplinarity, reflexivity, and novel quality control.

Based on my findings I have formulated two sets of recommendations for science and innovation policy. Regarding the funding of university research, I warn for a restricted (short-term economic) concept of valorization. The emphasis on short-term benefits leads to ineffective funding instruments, overrates the value of academic patents, endangers the viability of more fundamental research lines, and it threatens the stability of academic research groups and the coherence of their activities. I recommend to:

- govern academic research based on expected long-term benefits rather than short-term benefits;
- loosen the 'valorization' requirements for grants from the national research council NWO;
- make a more careful cost-benefit analysis before patenting academic inventions; and
- restore and protect the size of unconditional block grant support for university research.

With regard to research evaluation, I point to the perverse side-effects of bibliometric quality indicators. They restrict the concept of academic excellence, promote a monoculture of science and create inflation of the value of a scientific paper. To overcome these problems, bibliometric indicators should be complemented both with qualitative assessments and with indicators of social and economic impact. I recommend to:

- avoid direct linkage of funding decisions with bibliometric indicators;
- broaden the criteria of performance evaluations and include indicators of societal impact when applicable; and
- broaden the criteria for hiring and promoting academic staff: take into account teaching qualities and societal impact as well.

Samenvatting

Het uitgangspunt van dit proefschrift is dat academische onderzoekers *strijden om relevantie.* In meer of mindere mate proberen ze ervoor te zorgen dat hun werk waardevol is voor de samenleving, direct of indirect. Dit kan ze persoonlijke voldoening geven, maar het helpt ze ook om hun werk te legitimeren naar de buitenwereld en om toegang te krijgen tot de benodigde middelen. Het streven naar relevantie is echter niet altijd gemakkelijk; het heeft vaak het karakter van een worsteling. Ten eerste kan het lastig zijn om het najagen van relevantie te verzoenen met andere plannen, ambities en waarden. Ten tweede heeft relevantie geen vaste betekenis; het is het product van sociale interacties tussen wetenschappers, beleidsmakers en andere betrokkenen. Kortom, de strijd om relevantie is een interessant maar ingewikkeld krachtenspel.

In dit proefschrift heb ik de strijd om relevantie gedefinieerd als het geheel van inspanningen van wetenschappers om hun werk in overeenstemming te brengen met gangbare standaarden van relevantie en hun inspanningen om die standaarden te beïnvloeden. Het doel van dit onderzoek naar relevantie is om bij te dragen aan het begrip van veranderingen in de kennisinfrastructuur, die de afgelopen 15 jaar met concepten als Mode 2 kennisproductie, post-academische wetenschap, en de Triple Helix van universiteit-overheid-bedrijven relaties werden aangeduid.

De onderzoeksvraag van dit proefschrift luidt: hoe kunnen we veranderingen in de strijd om relevantie van Nederlandse academische onderzoekers in de chemie, biologie en landbouwwetenschappen in de periode 1975-2005 begrijpen?

Mijn theoretische vertrekpunt is het onderscheid tussen de *agency* van wetenschappers en de structuren die hun gedrag vormgeven. In lijn met structuratietheorie neem ik aan dat deze structuren tegelijk medium en uitkomst zijn van onderzoekspraktijken. Verder vat ik wetenschap op als een sociale activiteit, en de voortgang van wetenschap als de uitkomst van sociaal ingebedde handelingen van onderzoekers. Een andere basisaanname is dat wetenschappers betrokken zijn bij een voortdurende strijd om hulpbronnen, omdat ze afhankelijk zijn van hun omgeving voor financiering en legitimatie. In dit proefschrift ontwikkel en gebruik ik drie centrale concepten om de strijd om relevantie te analyseren: de geloofwaardigheidcyclus (*credibility cycle)*, het contract tussen wetenschap en samenleving, en het onderzoekssysteem.

Ik gebruik de geloofwaardigheidcyclus om de strijd om relevantie van individuele wetenschappers te conceptualiseren. Dit model legt uit hoe het dagelijkse werk van individuele wetenschappers verbonden is met het opbouwen van een reputatie. Het beeldt het onderzoeksproces af als een repeterende cyclus waarin omzettingen plaatsvinden tussen geld, personeel, data, argumenten, artikelen, erkenningen, enzovoorts. De veranderende structuren die het gedrag van wetenschappers beïnvloeden, onderzoek ik vanuit twee verschillende invalshoeken, gebruikmakend van de begrippen 'contract' (tussen wetenschap en samenleving) en 'onderzoekssysteem'. Het contract verwijst naar alle impliciete en expliciete overeenkomsten tussen wetenschap en maatschappelijke partijen over wat wetenschap moet doen, waarom het dit zou moeten doen, en wat de geschikte omstandigheden zijn voor wetenschap om goed te functioneren. Het onderzoekssysteem vertegenwoordigt de institutionele omgeving van academisch onderzoek, bestaande uit onderzoeksfondsen, bedrijven, publieke onderzoeksinstituten, ministeries en andere organisaties die prikkels en beperkingen opleggen om een bepaald soort onderzoek uit te voeren.

Dit onderzoek is een combinatie van casestudies naar scheikunde, biologie en landbouwwetenschappen in Nederland. Binnen elke case richt ik me in het bijzonder op twee of drie onderzoeksgebieden die samen de breedte van deze discipline vertegenwoordigen in termen van mogelijke belanghebbenden. Dit proefschrift gaat in de eerste plaats over *academische* kennisproductie, dus onderzoek dat wordt uitgevoerd aan universiteiten. Gegevens voor het onderzoek zijn verzameld door middel van diepte-interviews met 47 universitaire onderzoekers, en met een aantal deskundigen en vertegenwoordigers van verschillende organisaties in het onderzoekssysteem. Daarnaast heb ik een verzameling beleidsdocumenten, rapporten en strategische plannen van onderzoeksfondsen, verkenningen, evaluaties en andere belangrijke publicaties over de drie disciplines geanalyseerd.

Na een inleidend eerste hoofdstuk dat de achtergrond, het theoretische kader en de methodologie van deze studie behandelt, biedt Hoofdstuk 2 een overzicht van de recente literatuur over transformaties in de kennisinfrastructuur. Voor deze literatuurstudie heb ik het begrip 'Mode 2 kennisproductie' (Gibbons et al., 1994), gekozen als vertrekpunt vanwege haar prominente positie in het debat, zowel aan de universiteit als in beleidskringen. Ik geef een systematisch overzicht van de ontvangst van The New Production of Knowledge (NPK) in de wetenschappelijke literatuur en vergelijk het begrip Mode 2 met een aantal alternatieve diagnoses van veranderende wetenschapssystemen, zoals de 'Triple Helix' en 'Post-normal science'. Mijn conclusie luidt dat de kracht van NPK haar uitzonderlijk grote reikwijdte is: het gaat over veranderingen op het cognitieve, organisatie- en maatschappelijke niveau. Dankzij deze reikwijdte heeft NPK een forum geschapen voor het bespreken van verschillende vermoedelijke trends in het wetenschapssysteem. Op basis van een overzicht van de kritische reacties in de wetenschappelijke literatuur, identificeer ik echter ook een aantal problemen aangaande de empirische validiteit, de conceptuele kwaliteit en de politieke kleur van NPK. De belangrijkste problemen zijn het gebrek aan empirische ondersteuning en de twijfelachtige samenhang van de vijf essentiële kenmerken van 'Mode 2'. Daarom raad ik aan om deze kenmerken van elkaar te scheiden en ieder apart te onderzoeken. Het literatuuroverzicht wordt afgesloten met een onderzoeksagenda voor onderzoekers op het gebied van Wetenschaps-, Technologie- en Innovatiestudies met betrekking tot de meest controversiële kenmerken van Mode 2 kennisproductie: transdisciplinariteit, reflexiviteit en kwaliteitszorg. Deze agenda heeft als vertrekpunt gediend bij het formuleren van de onderzoeksvraag van dit proefschrift, zoals hierboven weergegeven.

Na deze literatuurstudie is mijn volgende stap een nadere conceptualisering van (de strijd om) relevantie in termen van mijn theoretisch kader. Hoofdstuk 3 ontwikkelt de eerste twee bouwstenen van dit kader: de geloofwaardigheidcyclus en het contract tussen wetenschap en samenleving. Op basis van een kort overzicht van het gebruik van de contractmetafoor in de literatuur, stel ik voor om deze als een heuristiek te gebruiken om de veranderende verhouding tussen wetenschap en samenleving te bestuderen. Ik stel voor om de inhoud van het contract te beschouwen in termen van drie hoofdcomponenten, die respectievelijk de identiteit van wetenschap, de beweegredenen om haar te steunen, en de omstandigheden waaronder onderzoekers werken behandelen. Het tweede element van mijn theoretisch kader is de geloofwaardigheidcyclus. Middels een korte casestudie van de Nederlandse academische chemie wordt de bruikbaarheid van deze twee concepten geïllustreerd. In deze casestudie verken ik hoe veranderende opvattingen van 'relevantie' in het contract tussen wetenschap en samenleving invloed uitoefenen op de wetenschapspraktijk, met speciale aandacht voor de instituties die de geloofwaardigheidcyclus beheersen. Deze analyse geeft een eerste indicatie van de spanning tussen de toenemende druk om veel te publiceren en de toenemende druk om maatschappelijk relevante resultaten – de laatste jaren steeds meer gedefinieerd als praktische toepassingen - te genereren.

De volgende drie hoofdstukken (4 t/m 6) gebruiken en verrijken (elementen van) dit kader in de analyse van casestudies van achtereenvolgens de Nederlandse chemie, biologie en landbouwwetenschappen. Elke studie behandelt soortgelijke vragen rondom de veranderende strijd om relevantie.

Hoofdstuk 4 presenteert een uitgebreidere studie naar de chemie-casus. Tussen 1975 en 2005 kunnen vier belangrijke veranderingen worden aangewezen in haar wetenschap-samenleving contract:

1. de financiële middelen voor universitair onderzoek bieden meer ruimte voor gerichte inspanningen op toepassingsgerichte gebieden;
2. de interacties met de industrie zijn intensiever geworden;
3. universiteiten proberen een actieve rol te spelen in de valorisatie van resultaten van wetenschappelijk onderzoek, en
4. systematische evaluaties zijn uitgegroeid tot een machtige institutie rondom wetenschappelijk onderzoek.

De gevolgen van deze ontwikkelingen voor de strijd om relevantie van onderzoekers verschillen tussen de drie bestudeerde chemische vakgebieden. Praktische toepassingen leveren bijvoorbeeld veel minder geloofwaardigheid op in de milieuchemie, waar toepassingsgerichte onderzoeksagenda's helpen om financiering te verwerven, maar niet om prestigieuze artikelen te publiceren of om erkenning te verdienen van collega's. In de biochemie helpen praktische toepassingen nauwelijks bij het verwerven van geloofwaardigheid, want dit gebied is nog steeds sterk gericht op fundamentele vragen. De verschillen tussen de gebieden kunnen worden verklaard door de aan- of afwezigheid van machtige 'upstream-eindgebruikers', die zich investeringen kunnen veroorloven in wetenschappelijk onderzoek die zich pas op de lange termijn terugbetalen.

Hoofdstuk 5 behandelt veranderingen in het contract en de invloed daarvan op het daadwerkelijke gedrag van universitaire onderzoekers en de inhoud van hun werk. Het richt zich met name op multidisciplinaire samenwerkingen in de paleo-ecologie en toxicologie. Dit hoofdstuk laat zien dat het maatschappelijk contract van biologie een ontwikkeling heeft doorgemaakt die vergelijkbaar is met die van het contract van chemie. De identiteit van academische biologie is veranderd van een fundamentele naar een strategische wetenschap; praktische toepassingen liggen steeds vaker ten grondslag aan de beweegredenen voor het steunen van biologie; de beschikbare financiering is verschoven en onderzoeksevaluaties zijn opgekomen. Deze ontwikkelingen blijken zowel prikkels als beperkingen voor multidisciplinariteit te genereren die verschillen tussen de twee onderzochte vakgebieden. De toename van multidisciplinaire samenwerking in de paleo-ecologie valt te begrijpen als een reactie op druk van buitenaf om meer 'relevant' onderzoek te doen, met name op het gebied van klimaatverandering. In toxicologie zie ik het tegenovergestelde: hier wordt multidisciplinariteit gebruikt om het fundamentele onderzoek naar fysiologische mechanismen van toxiciteit te versterken. Ik concludeer dat multidisciplinaire samenwerking verschillende doelen kan dienen, uiteenlopend van het verhogen van het praktische nut van een fundamenteel vakgebied tot het versterken van de fundamentele basis van een toegepast vakgebied.

De centrale vraag van Hoofdstuk 6 is: wat is de invloed van veranderende instituties in het onderzoeksysteem op onderzoekspraktijken in Nederlandse landbouwwetenschappen? Dit hoofdstuk gaat in het bijzonder over drie dierwetenschappen. Deze vakgebieden hebben te maken met een sterk dynamische maatschappelijke context. De Nederlandse landbouwwetenschappen staan onder druk om de inhoud en de organisatie van het onderzoek te veranderen om een bijdrage te kunnen leveren aan een meer concurrerende en duurzame landbouw. In dit hoofdstuk analyseer ik de veranderingen in de instituties in het landbouwkundig onderzoekssysteem en de consequenties daarvan voor het dagelijkse werk van Nederlandse dierwetenschappers. In de periode die ik heb bestudeerd, valt een verschuiving op van het consensus-gedreven, top-down 'OVO-drieluik' naar een heterogeen kennissysteem. Deze verschuiving brengt twee belangwekkende veranderingen in de institutionele omgeving van het academische landbouwonderzoek met zich mee: verschuivingen in de beschikbare financiering, en de opkomst van systematische onderzoeksevaluaties. Als gevolg van de genoemde ontwikkelingen zijn in twee vakgebieden de interacties met stakeholders toegenomen, maar om verschillende redenen. In Dierfokkerij & Genetica (ABG) hebben onderzoekers veelvuldig contact met fokkerijbedrijven voor het uitkiezen van uitdagende onderzoeksthema's, het verzamelen van data en de werving van onderzoeksfinanciering. Dankzij deze interacties slaagt ABG erin om voldoende externe financiering te verkrijgen zonder haar fundamentele onderzoeksagenda in gevaar te brengen. In Dierlijke Productiesystemen (APS) dragen contacten met boeren alleen bij aan de dataverzameling, terwijl contacten met toegepaste onderzoekers en met non-gouvernementele organisaties helpen om toegang te krijgen tot financiering. Dit lijkt een kwetsbare combinatie, want APS heeft moeite met het opbouwen van een coherente onderzoeksagenda en met het publiceren van voldoende wetenschappelijke artikelen. In het derde bestudeerde vakgebied, celbiologie, zijn interacties met 'gebruikers' juist eerder afgenomen dan toegenomen, want daar is een neiging om op te schuiven naar meer fundamentele onderzoeksthema's die een grotere kans bieden op prestigieuze publicaties, maar die niet van onmiddellijk belang zijn voor de landbouwsector.

In Hoofdstuk 7 bied ik een nadere reflectie op het materiaal dat is gepresenteerd in de voorafgaande hoofdstukken. Ik beantwoord de algemene onderzoeksvraag aan de hand van drie sub-vragen.

1. *Welke veranderingen kunnen worden waargenomen in de strijd om relevantie van Nederlandse academische onderzoekers in de chemie, biologie en landbouwwetenschappen, in de periode 1975-2005?*

Wie alle bestudeerde vakgebieden samen in beschouwing neemt, kan drie gemeenschappelijke ontwikkelingen waarnemen. Ten eerste is in de meeste vakgebieden de strijd om relevantie tijdens het daadwerkelijke onderzoeksproces geïntensiveerd. Interacties met mogelijke gebruikers van onderzoeksuitkomsten waren in sommige vakgebieden al langer gangbaar, maar in het algemeen is de frequentie en het belang van dit soort interacties gegroeid. De tweede gemeenschappelijke trend heeft te maken met de manier waarop onderzoekers erkenning verwerven: gemiddeld genomen is de waarde van praktische toepassingen als bron van academische erkenning gedaald. Sinds de zeventiger jaren hebben publicatieresultaten sociale en economische impact (verder) verdrongen als bron van academische erkenning. Ten derde is de strijd om relevantie tijdens de werving van financiering geïntensiveerd. In alle vakgebieden melden de onderzoekers die ik heb geïnterviewd dat de aansluiting van hun werk op kennisbehoeften van maatschappelijke stakeholders steeds belangrijker is geworden voor het verzamelen van voldoende financiering.

De veranderingen in de strijd om relevantie varieerden ook tussen de vakgebieden. Ten eerste, hoewel de rol van stakeholders in het academische onderzoeksproces in het algemeen is gegroeid, varieert de *mate* waarin zij betrokken zijn bij het verzamelen van data. Ten tweede is er variatie in de (beperkte) *mate* waarin praktische relevantie wordt beloond in termen van academische erkenning. Ten derde helpt het beloven van maatschappelijke opbrengsten in alle vakgebieden om financiering te verkrijgen, maar de *mate* waarin maatschappelijke stakeholders betrokken zijn bij het bepalen van de daadwerkelijke onderzoeksagenda verschilt. Een vierde dimensie betreft de spanning die in sommige vakgebieden is ontstaan door de combinatie van deze ontwikkelingen. In sommige vakgebieden is er synergie tussen wetenschappelijke productiviteit en praktische toepassingen omdat interacties met stakeholders niet alleen helpen bij het werven van financiering, maar ook behulpzaam zijn bij andere geloofwaardigheidomzettingen. In andere velden is er een spanning tussen wetenschappelijke productiviteit en praktische toepassingen omdat daar het versterken van de industriële, medische of agrarische relevantie van je werk een verwijdering van de centrale academische debatten impliceert. Men zou dan moeten overstappen op andere modelsystemen of andere onderzoeksvragen die minder geschikt zijn om over te publiceren in prestigieuze tijdschriften en die minder snel academische erkenning zullen opleveren.

2. *Hoe kunnen de overeenkomsten tussen de veranderingen in verschillende vakgebieden worden verklaard?*

De trends die gedeeld worden tussen de vakgebieden kunnen worden begrepen als gevolgen van de combinatie van drie belangrijke veranderingen in de structurele condities rondom het universitair onderzoek in Nederland. De eerste is de diversificatie van financieringsbronnen. Tussen 1975 en 2005 is het aandeel van toepassingsgerichte financiering toegenomen. Tegelijkertijd zijn er speciale subsidies ontwikkeld voor getalenteerde onderzoekers, die een beloning vormen voor wetenschappers die bijzonder productief zijn in termen van wetenschappelijke publicaties. De tweede structurele verandering die de strijd om relevantie in alle wetenschappelijke vakgebieden heeft gevormd is de opkomst van onderzoeksevaluaties, die worden gedomineerd door bibliometrische kwaliteitsindicatoren. De derde invloedrijke structurele verandering betreft de heersende opvatting over de missie van universiteiten in Nederland. Sinds 1975 zijn autonomie en onafhankelijkheid geleidelijk vervangen door begrippen als verantwoording, valorisatie en ondernemerschap als de heersende waarden verbonden met universitair onderzoek.

3. *Hoe kunnen verschillen tussen de veranderingen in de verschillende vakgebieden worden verklaard?*

Een van de opvallendste uitkomsten van dit proefschrift is dat de veranderingen in de strijd om relevantie sterk verschillen tussen vakgebieden. Een mogelijke verklaring voor de verschillen tussen vakgebieden kan worden gevonden in de combinatie van de mate van convergentie in hun zoekpatroon, hun traditionele communicatiecultuur en eigenschappen van hun 'upstream' eindgebruikers. Ten eerste lijkt het in divergente velden, met een hoge strategische taakonzekerheid waarschijnlijker dat zich niches ontwikkelen die passen bij de kennisbehoeften van maatschappelijke stakeholders dan in convergente velden waarin meer consensus bestaat over de wetenschappelijke agenda en ontsnappen hieraan zwaarder wordt gestraft.

Ten tweede kunnen we de mate waarin praktische toepassingen als bron van geloofwaardigheid dienen, begrijpen als we rekening houden met de traditionele communicatiecultuur van een vakgebied. In vakgebieden met een 'landelijke' communicatiestijl waar een lage 'people-to-problem'-ratio heerst is er normaal gesproken een lagere citatiedichtheid. Dit impliceert dat bibliometrische kwaliteitsindicatoren een beperkte validiteit hebben. Daardoor is het minder waarschijnlijk dat erkenning puur op publicaties wordt gebaseerd, en waarschijnlijker dat praktische uitkomsten zoals beleidsadvies, patenten of spin-off bedrijven ook meetellen. In de meer competitieve 'stedelijke' vakgebieden, met een hoge 'people-to-problem'-ratio en een hoge citatiedichtheid, zullen bibliometrische indicatoren meer doorslaggevend zijn, niet alleen in formele evaluaties en managementbeslissingen, maar ook in het toekennen van informele erkenning.

Een mogelijke verklaring voor de variatie in de verhouding tussen praktische toepassingen en wetenschappelijke productiviteit kan worden gevonden in eigenschappen van andere spelers in het onderzoekssysteem, met name de eindgebruikers van academisch onderzoek. De vakgebieden die het meest succesvol bleken in het combineren van stakeholder interacties met academische prestaties waren velden met vermogende en machtige upstream eindgebruikers met een langetermijnvisie op het nut van wetenschappelijk onderzoek. Door het subsidiëren van fundamentele onderzoeksactiviteiten die goede kansen bieden op high-impact publicaties, steunen de chemische industrie (in het geval van katalyse) en dierfokkerijbedrijven onderzoekers gedurende de gehele geloofwaardigheidcyclus.

Dit onderzoek heeft twee belangrijke implicaties voor het debat over veranderende wetenschapssystemen. Ten eerste maakt het duidelijk dat de toenemende druk om te publiceren de druk om praktische toepassingen te genereren kan tegenwerken. In veel vakgebieden heb ik een spanning waargenomen tussen onderzoeksactiviteiten die nuttig zijn voor maatschappelijke stakeholders en activiteiten die het beste perspectief bieden op prestigieuze wetenschappelijke publicaties. Ten tweede geeft mijn werk aan dat een nadere differentiatie in wetenschaps(beleid)studies nodig is, omdat de dynamiek van wetenschap veel sterker per vakgebied verschilt dan de meeste literatuur doet vermoeden. Beide bevindingen stellen de empirische validiteit van enkele centrale beweringen rondom het begrip Mode 2 kennisproductie op de proef, met name met betrekking tot transdisciplinariteit, reflexiviteit en vernieuwde kwaliteitszorg.

Op basis van mijn bevindingen heb ik twee sets aanbevelingen voor wetenschap- en innovatiebeleid geformuleerd. Ten aanzien van de financiering van universitair onderzoek waarschuw ik voor een beperkte (kortetermijn-, economische) opvatting van valorisatie. Een te sterke nadruk op kortetermijnopbrengsten leidt tot ineffectieve financieringsinstrumenten en tot overschatting van de waarde van universitaire patenten. Bovendien bedreigt zo'n opvatting de levensvatbaarheid van meer fundamentele onderzoekslijnen, de stabiliteit van universitaire onderzoeksgroepen en de coherentie van hun activiteiten. Ik beveel daarom aan om:

- wetenschapsbeleid te baseren op verwachte langetermijnopbrengsten in plaats van kortetermijnopbrengsten,
- de valorisatie-eisen voor subsidies van het nationale onderzoeksfonds NWO te versoepelen,
- een zorgvuldigere kosten-batenanalyse te maken alvorens universitaire uitvindingen te patenteren, en
- de omvang van onvoorwaardelijke lumpsum subsidie voor universitair onderzoek te herstellen en te bewaken.

Met betrekking tot onderzoeksevaluaties wijs ik op de perverse neveneffecten van bibliometrische kwaliteitsindicatoren. Zij beperken de opvatting van academische kwaliteit, bevorderen een monocultuur van wetenschap en veroorzaken inflatie van de waarde van een wetenschappelijk artikel. Om deze problemen op te lossen, zouden bibliometrische indicatoren moeten worden aangevuld, zowel met kwalitatieve beoordelingen als met indicatoren van sociale en economische impact. Ik beveel daarom aan om:

- een rechtstreekse verbinding van financieringsbesluiten aan bibliometrische indicatoren te allen tijde te vermijden,
- de criteria voor onderzoeksevaluaties te verbreden en ook indicatoren voor maatschappelijke impact op te nemen (indien van toepassing), en
- de criteria voor het aanstellen en bevorderen van universitair personeel te verbreden: houd ook rekening met didactische kwaliteiten en met maatschappelijke impact.

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Curriculum Vitae

Laurens Hessels was born in Groningen (1980). In 1999 he started his studies at University of Amsterdam. After a research project in microbiology at the Swammerdam Institute for Life Sciences, he got his Bachelor's degree in Chemistry in 2004. Laurens worked as an intern at the National Institute for Public Health and the Environment (RIVM, Bilthoven) and obtained a Master's degree in Environmental Chemistry in 2005. In the same year, he also completed a philosophical Master thesis on the singularity of science, based on the constructivist philosophy of Isabelle Stengers, and graduated in the Philosophy of Science (with honours).

From 2006 until 2010 Laurens worked on the PhD-project presented in this thesis at the Innovation Studies Group, Utrecht University. This project was part of the Knowledge Network on System Innovations and Transitions (KSI). Laurens completed the education program of the graduate school for Science, Technology and Modern Culture (WTMC). He served as a student advisor in WTMC's teaching committee in 2007 and 2008. Beside his research work, he supervised master students and provided tutorials in innovation theory, research methods and innovation policy. Early 2008 Laurens initiated the Utrecht Innovation Colloquium, a series of guest lectures on the dynamics and governance of innovation. Laurens presented his work at numerous international conferences and gave invited lectures at Maastricht University, University of Amsterdam and York University (Toronto).

Since September 2010, Laurens works as a researcher at the Rathenau Institute (Den Haag).

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