Technological Decision-Making Under Scientific Uncertainty

Preventing Mother-to-Child Transmission of HIV in South Africa

Martin Weinel

Cardiff School of Social Sciences, Cardiff University

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ProQuest LLC
789 East Eisenhower Parkway
P.O. Box 1346
Ann Arbor, MI 48106-1346

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Summary

Between October 1998 and August 2000, the South African government refused to make AZT, an antiretroviral drug, available to pregnant women living with HIV/AIDS to reduce the risk of mother-to-child transmission of HIV. From October 1999 onwards, leading government figures justified this policy choice by claiming that the safety of AZT was the subject of a scientific controversy. President Mbeki played a pivotal role because he was the one who identified the alleged scientific controversy about AZT by reading scientific literature on the Internet.

The main objective of the this thesis is to establish whether President Mbeki and other leading government figures should have participated in making policy-relevant judgements related to the safety of AZT. A new STS approach, known as the 'Third Wave of Science Studies', provides the theoretical and conceptual tools that inform a normative model of technological decision-making, the '3W model', which guides the analysis of the South African case study. By relying on a realist theory of expertise, which links expertise with experience and tacit knowledge, the 3W model allows social analysts to make judgements about the extent of participation in making policy-relevant technical or scientific judgements.

The normative analysis focuses on three aspects. First, it is evaluated whether the government acted correctly when it ignored expert advice that suggested the benefits of using AZT to prevent the risk of mother-to-child transmission outweighed the risks. Second, by exploring Thabo Mbeki's level of expertise, it is explored whether he was in a position to make a reliable judgement about the state of the scientific discourse about the safety of AZT. Third, a proposal is made that prescribes how actors should proceed if they want to judge the authenticity of scientific controversies that are invoked in the context of technological decision-making processes.

Table of Contents

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Autobiographical Preamble

This thesis is a piece of social *science*, and its interpretations and conclusions ought not to be directly derived from my values and convictions. There is, however, inevitably a large personal element that has shaped it in certain ways. Issues such as the choice of topic or the choice of methodological perspective cannot be attributed to 'purely scientific considerations.' In this preamble, I explain why I have chosen to use the theoretical framework of the new and relatively controversial *Studies of Expertise and Experience* (SEE) research programme instead of a more established STS theory.

On reflection, I started grappling with a problem in 2002, which is at the heart of the thesis, called the 'problem of extension.' Coincidentally, this is the same year in which Collins and Evans (2002) published their discussion paper *The Third Wave of Science Studies: Studies of Expertise and Experience*, which presents their response to this problem but I did not know about it. I was at the University of Leipzig, in the old East Germany, a student of African Studies, and in 2002 I decided that the topic of the *Magister* thesis, which I would begin in 2003 would focus on HIV/AIDS policy in South Africa. While I knew that I wanted to write about this issue, I had no idea from which theoretical angle to approach the issue. African Studies is a regional discipline that, while scratching the surface of more established disciplines such as linguistics, history, economics and political sciences, is largely a 'theory free' enterprise. What was clear to me, however, was that a specific period of the South African HIV/AIDS policy presented an interesting problem for social analysts that I wanted to explore.

Despite being affected by the largest AIDS epidemic in any country in the world, for several months in 2000, Thabo Mbeki's government publicly questioned whether HIV really was the cause of AIDS. The problem, in general terms, was how to balance democracy and science. On the one hand, Mbeki was the elected Head of State and leader of the South African government, and surely had a democratic mandate to enact policies as he and his administration saw fit. On the other hand, the consensus among scientists was that AIDS is caused by

HIV. Therefore, I found myself agreeing with AIDS activists, scientists and journalists who criticised Mbeki for threatening to base South African HIV/AIDS policies on a theory of AIDS that did not attribute a causal role to HIV. Imagine for a moment that Mbeki and his government had decided to reverse the regulation that ensures that all blood donations in South Africa are routinely screened for HIV. After all, such a policy decision is consistent with the belief that HIV does not play a role in the causation of AIDS. While it is normal that a government might make 'wrong' policy-choices, it seemed to be entirely avoidable in the case of HIV/AIDS policy-making in South Africa.

Back then my favoured solution to the problem was simple and apparently commonsensical: Mbeki should restrict himself of dealing with the details of policy-making, while the technical knowledge that informed such policies should be left to scientists. In 2002, I was not aware that such an approach could be described as being technocratic. Fortunately, as I can say today with the benefit of hindsight, I never came to write a technocratic account.

Before I started writing my thesis I attended a seminar at the Institute for Ethnology at the University of Halle, in which Professor Richard Rottenburg introduced approaches to the sociology and ethnology of (scientific) knowledge which fundamentally changed my views. In the seminar, we read a range of literature including texts by Michel Foucault, Mary Douglas, Marilyn Strathern and Bruno Latour. It was Latour's (1999a) opening chapter of *Pandora's Hope* that caught my imagination. While he seemed to dismiss the whole sociology of scientific knowledge for reasons that I did not really understand, I was intrigued and started to read the little literature that Leipzig's university library had to offer: Scheler, Mannheim, Fleck, Bloor, Collins, Knorr-Cetina and Shapin provided me with a rough primary-source-based introduction to the discipline and to a new *Weltanschauung*. Things that I had taken for granted, such as that scientific knowledge is true, certain and value-free appeared uncertain and doubtful.

The only certainty I gained from reading the science studies literature was that *Wissenschaftssoziologie* or sociology of scientific knowledge (SSK) would provide me with the theoretical approach to inform my thesis on South African HIV/AIDS policy as I found this way of looking at the world both germane to the

problem, and highly stimulating. There was only one snag: the relativism that underpinned the sociology of scientific knowledge forced me to change my view on the Mbeki case. While my original intention was to write about the relationship between policy-making and science, this appeared to be less feasible now as it dawned on me that relativist science studies approaches did not allow me to criticise Mbeki for making policy-relevant technical judgements. Apart from the empirical findings that put the 'power' of science in perspective, the methodological rules of SSK did not allow me to take sides from the outset. My study had to be symmetrical and impartial.

Now that I had found an interesting theoretical approach, I was on the brink of losing my case study. Luckily, I came across Collins' (1981b) very brief introduction to EPOR, the empirical programme of relativism, which proved to be pivotal. This provided the impetus for reformulating my case study. Instead of criticising Mbeki for his meddling in science, which threatened to affect science-based policy-making, I redefined the political debate about AIDS causation as one of a 'scientific controversy.' Mbeki was then simply one player amongst many that were involved in determining the cause of AIDS. Given that the first step of an EPOR-informed study is to demonstrate interpretative flexibility related to the scientific data, I reconstructed some of the arguments of 'AIDS sceptics' and showed that they were not less 'rational' than the arguments used by the scientific mainstream; they just relied on different assumptions and standards of evidence. In a second step, I showed how the controversy was closed. The interpretative flexibility was not restricted through new or more compelling scientific evidence, but through mounting political pressure within and without South Africa, which made Mbeki's position on AIDS publicly untenable by October 2000.

While the thesis earned me very good marks and eventually a University degree, the original problem that drew my attention to the South African case of Mbeki's AIDS denial remained unresolved. Taking a relativist perspective to make an interesting point about how scientific controversies are closed is one thing, being sceptical about the causes of AIDS in the context of policy-making is another. The former is intellectually stimulating, the latter can cost lives as the recent history in South Africa has demonstrated. Relativism, I realised, was unable to deal with the normative dimension of the South African problem. At

the same time, my excursion into the sociology of scientific knowledge had also made it clear to me that a return to technocracy was indefensible; it seemed to be based on assumptions that SSK and related approaches had shown to be flawed. For different reasons, there appeared to be no way to critique Mbeki's involvement in the scientific discourse on AIDS causation on the basis of either positivist or post-positivist science study approaches. The former approach offers an intellectually unsatisfying solution to the normative problem; the latter affords no solution at all.

It was again Harry Collins, this time in collaboration with Robert Evans, who came to my 'rescue.' Just weeks before submitting my thesis in March 2004, I discovered the 'Third Wave' paper on the Internet. Collins and Evans' (2002) Studies of Expertise and Experience (SEE) research programme offers the possibility to critically evaluate Thabo Mbeki's ventures into the realm of science. SEE adopts a realist or essentialist perspective on expertise, which differs substantially from the realist conception of expertise that underpins technocratic approaches. By arguing that only those with particular expertises ought to be involved in making policy-relevant scientific judgements, SEE is able to distinguish between legitimate and illegitimate participation.

I contacted Collins and Evans, who agreed to take me on as a PhD student and I was lucky enough to secure some funding from the School of Social Sciences at Cardiff University, without which I would not have been able to complete the required Master's programme and the subsequent PhD. Upon arriving in Cardiff in 2005, I was surprised when Collins warned me to think hard about committing to SEE as he said he could not guarantee the future viability of the research programme. This warning was related to strong criticism drawn by SEE from some very influential STS scholars who described Collins and Evans' approach as being technocratic and anti-democratic. Reading the debate in *Social Studies of Science*, ironically, in the absence of what I will later call the 'domain specific discrimination' which might otherwise have led me to dismiss the SEE approach at the outset, I felt that the critics had entirely missed the point. While they reiterated their desire to pursue anti-essentialist research interests, Collins and Evans had a very different goal: to offer an approach that would make it possible to make policy-relevant scientific judgements before a scientific

consensus has been reached. In this sense, SEE is similar to some technocratic proposals, but the critics overlooked the fact that the central concept of expertise had been redesigned to differentiate it significantly from technocratic conceptions – I was surprised and disappointed by the critics' reaction.

Using the SEE approach has finally allowed me to deal normatively with Mbeki's transgressions into the realm of scientific advice. In this thesis, I have opted to analyse another episode of the controversy-ridden South African HIV/AIDS policy, one that focuses on Thabo Mbeki's involvement in making policy-relevant judgements about the safety of a particular antiretroviral drug used to treat HIV/AIDS and to prevent the mother-to-child transmission of HIV. I am, however, confident that if my earlier research focus, notably, Thabo Mbeki's denial of the causal relationship between HIV and AIDS, had instead formed the subject of this thesis, it would have been possible to show that he lacked the relevant expertise to be considered a legitimate participant in making policy-influencing scientific judgements in this realm too.

Encouragingly, in recent years, SEE has crossed disciplinary boundaries and has been applied in fields such as journalism, education, management, criminology, and geography. The broader application of this approach, as well as gaining greater acceptance within STS itself, suggests that perhaps the author was right not to pay too much heed to the warnings concerning SEE. Here, I not only apply the SEE approach but also try to develop it in a small way.

It ain't what you don't know that gets you into trouble. It's what you know for sure that just ain't so.

Mark Twain

Introduction

A Very Practical Problem

On 28 October 1999, Thabo Mbeki was due to address the *National Council of Provinces* (NCOP), the second chamber of Parliament, for the first time in his role as President of South Africa. Predictably, Mbeki's speech mainly focussed on future challenges facing local and provincial governments. In the last few minutes of the speech, however, the President turned unexpectedly to the issue HIV/AIDS and responded to public demands for antiretroviral treatment. Here is what Mbeki said:

> [W]e are confronted with the scourge of HIV-AIDS against which we must leave no stone unturned to save ourselves from the catastrophe which this disease poses. Concerned to respond appropriately to this threat, many in our country have called on the Government to make the drug AZT available in our public health system.
> Two matters in this regard have been brought to our attention. One of these is that there are legal cases pending in this country, the United Kingdom and the United States against AZT on the basis that this drug is harmful to health. There also exists a large volume of scientific literature alleging that, among other things, the toxicity of this drug is such that it is in fact a danger to health.
> These are matters of great concern to the Government as it would be irresponsible for us not to heed the dire warnings which medical researchers have been making. I have therefore asked the Minister of Health, as a matter of urgency, to go into all these matters so that, to the extent that is possible, we ourselves, including our country's medical authorities, are certain of where the truth lies.
> To understand this matter better, I would urge the Honourable Members of the National Council to access the huge volume of literature on this matter available on the Internet, so that all of us can approach this issue from the same base of information. (Mbeki 1999)

The aspect of his speech that is of the greatest relevance is President Mbeki's claim that AZT, a drug that has been used in several ways since 1987 as a treatment for HIV/AIDS, might be so toxic that it could present a danger to health.[1] Mbeki apparently became aware of a disagreement between medical researchers about the safety of AZT by reading scientific literature that he found

[1] AZT is the short form for azidothymidine. The drug is also known as 'zidovudine' and is marketed by GlaxoSmithKline, its manufacturer, as Retrovir.

on the Internet.[2] On this basis, he argued that demands made by some sections of the South African public to make AZT available to those suffering from HIV/AIDS would be irresponsible given the uncertainty with regard to the safety of the drug.

Mbeki claimed in his speech that before the government could consider the public demands for the use of AZT, an inquiry into the allegations of excessive toxicity of the drug had to be conducted. In direct response to Mbeki's speech, the South African Medicines Control Council (MCC), as part of its statutory responsibility to ensure the safety of drugs that had been granted a licence in South Africa, began to investigate. By mid February 2000, the MCC had submitted three separate reports evaluating the safety of AZT. The third report was the most relevant as it focussed explicitly and in detail on the assessment of benefits and risks with regard to different usages of AZT, which were necessary to establish the safety of the drug. As far as it is known, this report stated that the benefits of using AZT for the prevention of mother-to-child transmission of HIV (PMTCT) significantly outweigh the risks (Cherry 2000b, 2009, Heywood 2003). Asked in mid February about the recommendations of the MCC detailed in the third report, the Minister of Health only stated that she could not yet comment as she had just read the first couple of pages and that it was unlikely that the report would be published (Cherry 2000b). This was the last the South African public heard about this report from a member of government. In spite of the safety assessment conducted by the MCC, the government stuck to its position that the safety of AZT was uncertain for several months.

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[2] The claims about 'pending court cases' disappeared without a trace from the public discourse in a matter of days and had no lasting impact on decision-making. As this was relatively easy to verify, it was never repeated again by Mbeki or any other member of the government. Anthony Brink, a South African AZT sceptic, confirmed that no legal cases were pending at that time: "In his address to the National Council of Ministers on 28 October 1999, during which he ordered an investigation into the safety of AZT, President Mbeki mentioned these lawsuits. GlaxoWellcome's representatives in South Africa immediately denied them. A few days later, the President's office asked me for details. I referred to the English cases of Threakall and others, and the American Nagel and McDonnell cases, all of which had been reported in the press. A month later however, in a telephone call from Susan Threakall's English solicitor Graham Ross, I was informed that her action, his lead case, had been withdrawn a couple of months earlier. In March 2000, Paul Headlund, the American attorney who had handled the Nagel and McDonnel cases, told me that the claims had not been pursued. GlaxoWellcome was therefore technically correct in disputing Mbeki's statement that there were cases concerning AZT pending against it at that time" (Brink 2001: 8-9)

Instead of being resolved, the question of AZTs safety simply lost its relevance in the context of the technological decision-making process, because the government decided in August 2000 that AZT ought not to be used in South Africa to reduce the risk of mother-to-child transmission (MTCT). Instead, the government decided to carry out a two-year pilot scheme on a new drug, Nevirapine. Nevirapine was both cheaper and easier to administer for PMTCT.[3]

At least between October 1999 and August 2000, the South African government justified its inaction regarding PMTCT by claiming that the safety of AZT was subject of a scientific controversy. This period was part of a longer, two year period in which the government remained completely inactive with respect to the issue of MTCT. A number of provincial PMTCT pilot projects could have started to deliver antiretroviral drugs in October 1998, but the government withdrew the required funding at the last moment.[4]

It is clear that the invocation of the alleged scientific controversy about the safety of AZT cannot be blamed for the two year delay in decision-making related to PMTCT, which was also justified by reference to a lack of financial resources, the inadequacy of health infrastructure, as well as bureaucratic obstacles. It is, however, also clear that the scientific argument contributed to the delay, because it provided the government with a powerful justification for doing nothing at a time when it came under public pressure. By locating the justification in the realm of science, it 'de-politicised' the matter and made it more difficult for those sections of the public who demanded the introduction of AZT for PMTCT to challenge the government. At the same time, the government appeared to have felt powerful enough to ignore the advice of scientific experts.

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[3] While this decision represented a fundamental change, it had no practical consequences until April 2001, when the first state-sanctioned pilot projects actually started to deliver. Moreover, it was not until April 2002, when the government, under pressure from the High Court in Pretoria, finally announced that it roll out a country-wide PMTCT programme on the basis of Nevirapine (see chapter 5).

[4] These pilot projects were located in Gauteng, but other provinces such as KwaZulu Natal were also considering implementing pilot projects. That providing PMTCT was feasible even in the most impoverished settings was demonstrated by a PMTCT pilot project in Khayelitsha, a township in Cape Town, which started on 1 January 1999. It was run by non-governmental organisations with the blessing of the provincial government in Western Cape, which was the only one without representatives from the African National Congress (*e.g.* Heywood 2003).

The delay in decision-making regarding the implementation of PMTCT programmes came with a cost. Those sections of the public who placed pressure on the government to provide AZT, including AIDS activists, human rights activists, health campaigners, doctors, nurses, scientists and a number of politicians, did so because the drug had been used throughout the world since 1994 to reduce the risk of mother-to-child transmission of HIV. Due to the size of the South African epidemic and due to fact that women of reproductive age bore the main burden of the epidemic, MTCT represented a significant problem in South Africa. According to government estimates, around 60,000 children acquired HIV annually in the late 1990s via vertical transmission, which meant that MTCT accounted for almost 10% of new annual HIV infections in the country. MTCT was one of the main reasons why South Africa, one the richest African countries, was one of the very few countries in the world that registered an increase of child mortality during the 1990s. Two independently conducted studies have estimated that the delay has led to the infection of tens of thousands of newborns with HIV (Chigwedere *et al.* 2008, Nattrass 2008). The general lack of long-term antiretroviral treatment in South Africa during the early years of this decade meant that the chances of long-term survival for those children who acquired HIV through MTCT were minimal.

A Very Theoretical Problem

The question for STS analysts is how to approach a case like this. The stance most in tune with contemporary STS mainstream, which is dominated by relativist methodologies and has been referred to as 'second wave of science studies' or Wave 2, would be to analyse the process of attribution of different labels, roles or identities to actors, which then impact on their ability or 'power' to make authoritative statements about the state of nature (Collins and Evans 2002).[5] For example, the analysis could show that there was a conflict between two groups that centred on the attribution of expertise to Thabo Mbeki. One group, including

[5] Collins and Evans (2002) have introduced the labels Wave 1, Wave 2 and Wave 3 as heuristics to refer to fundamental differences in methodological stance of three different research programmes in science studies. They are *not* meant to be an accurate description of the history of science studies.

Mbeki, attributed expertise to the President, which meant that Mbeki had sufficient epistemic authority to make judgements about the safety of AZT. The other group, in contrast, tried to depict the President as someone who did not know what he was talking about in an attempt to deflate his influence on determining the safety of AZT. It can then be shown that, over time, the second group was more successful and Thabo Mbeki was eventually pushed to concede 'defeat' on the matter.[6]

As an academic project, such a perspective on the case is valuable as it can reveal how boundaries between lay persons and experts are deconstructed, reconstructed and maintained. As a practical contribution to the question how technological policy can be made under conditions of uncertainty it is, however, of limited value. The problem is that adopting a relativist perspective, the social analyst is never in a position to say what would have been the best decision, not even with hindsight.

Of course, writing in 2010, hindsight has entirely vindicated those members of the South African public, including AIDS and human rights activists, journalists, scientists, and doctors and so on, who criticised the meddling of the government in the scientific discourse concerned with the safety of AZT. At no point over the last 10 years has the safety assessment by MCC experts, which concluded that that the benefits of using AZT for PMTCT outweighed the risks, been revised - even though safer and more effective means of preventing MTCT than the short-course of AZT are available today. The relativist STS scholar is, however, always aware that things could have been very different. With regard to the South African case, it was not clear from the outset that Mbeki would come to be seen as a lay person who got a technical judgement wrong.

The central argument of this thesis is that even without the benefit of hindsight, it is possible to argue from a STS perspective that the government was acting irresponsibly. To make timely judgements about the best course of technological policy even though the relevant science has not been fully settled requires a normative approach that is able to determine which actors are most

[6] This follows Jasanoff's (2003a: 398) definition of research activities on which 'critical science studies' ought to concentrate.

likely to make the best policy-relevant technical or scientific judgements. Wave 2 does not provide adequate theoretical tools to do this.

The failure of Wave 2 approaches to allow STS analysts to make interventions into technological decision-making has been noted by Collins and Evans (2002, 2003, 2007) and has prompted them to propose a 'third wave' (Wave 3) of science studies, which they call Studies of Expertise and Experience (SEE).[7] At the heart of SEE is a new conceptualisation of 'expertises', which allows for the identification of a variety of different forms and types of expertises. The different expertises can be related to different kinds of experiences and different levels of socialisation. Collins and Evans (2002, 2007) argue that different experiences and levels of socialisation are characterised by actors' acquisition of certain 'bodies' of tacit knowledge. Without relevant tacit knowledge, actors are unable to apply explicit knowledge in novel and unexpected situations. While explicit knowledge can be acquired in a variety of ways, for example through reading books, the only known way to acquire tacit knowledge is through direct and active contact with members of social groups who already possess it. The link with tacit knowledge turns 'expertise' into a realist or substantive concept. In other words, the link between expertise and tacit knowledge makes it possible to 'measure' expertise and to distinguish between different forms and types of expertise.

The ability to recognise those with expertise is a crucial component of the wider normative argument made by SEE as to how technological decision-making in conditions of uncertainty about the correctness or 'truthfulness' of scientific or technical claims ought to be made. Collins and Evans (2002, 2007) argue that only those with appropriate expertise, *e.g.* those who know what they are talking about, ought to make policy-relevant scientific or technical judgements. This argument is based on the assumption that this will lead to the best possible technical or scientific judgements, because it can be expected that their technical

[7] Since its introduction in 2002, SEE has inspired explorations into the nature of expertise (*e.g.* Collins 2004b, 2010b, Collins and Evans 2002, 2003, 2007, Collins and Sanders 2007, Selinger, Dreyfus, and Collins 2007, Weinel 2007); it has given rise to a new method in social science, *e.g.* the imitation game (*e.g.* Collins *et al.* 2006, Boyce 2009, Collins and Evans 2010, Evans and Collins 2010); it has provided the theoretical basis for empirical studies in a variety of disciplines such journalism, marine conservation, criminology and agricultural studies and so on (*e.g.* Boyce 2006, Carolan 2006, Collins 2007a, Edwards and Sheptycki 2009).

or scientific advice "is likely to be no worse, and maybe better, than [advice provided by] those who do not know what they are talking about" (Collins and Evans 2007: 2). The normative framework of SEE enables STS analysts to decide or assist others to decide who has the necessary experience in dealing with particular technical or scientific issues. On this basis, SEE can help policy-makers to solicit the most relevant scientific or technical advice.

It has to be stressed that while Wave 3 might be useful in identifying the most relevant technical or scientific advice, it is unable to identify the 'truest' or 'most correct' advice. This is one of the aspects that differentiates the current approach from what Collins and Evans (2002) have called the first wave of science studies or Wave 1. Under Wave 1, it was widely believed that accredited scientists and engineers were able to provide technical and scientific advice that represented true and politically neutral knowledge. SEE aims to say something about how to make the *best* judgements, not the *truest* judgments; the latter can only be made with hindsight.

Purpose of the Thesis

The two main objectives of this thesis are to develop and then to analytically apply a model of technological decision-making that is based on the assumptions and concepts of Wave 3 or SEE. Accordingly, the new model will be called the '3W model.'

The 3W model, which is proposed here, is capable of addressing two specific problems. One is the so-called 'problem of extension.' Collins and Evans (2002) use this phrase to describe the challenge for social analysts when trying to demarcate between those who should be involved in making policy-relevant technical judgements from those who should be excluded. The problem of extension has, in principle, already been solved with regard to technical judgements, given that Collins and Evans (2002) suggest using the criterion of relevant 'technical expertise' to inform this kind of demarcation work.[8] In

[8] It is important to note that 'technical expertise' in the context of SEE does not mean something like the 'knowledge of scientists or technologists.' In the context of SEE, actors who are typically regarded as lay-persons such as sheep-farmers or farm workers can be 'technical experts' with

offering an original contribution, this thesis seeks to work out whether and how the problem of extension can be resolved with regard to *judgements about* technical judgements. These kinds of judgements involve what Collins and Evans have called 'meta-expertises.'

The other problem is what can be called the 'problem of integration', which arises as a direct consequence of the 3W model's ability to solve the problem of extension. Implicit in demarcating between experts and non-experts is the construction of a niche or 'phase', as it has been called by Collins and Evans (2002), in which technical experts can make policy-relevant technical judgements. Separating such a specific 'technical phase' from a larger 'political phase' makes it logically necessary to establish rules according to which the technical advice formulated by experts feeds into or informs the political phase. Thus, another original contribution of this thesis is the development of the Minimal Default Position (MDP), which sets out how technical advice should be integrated into the wider process of technological decision-making.

The ultimate and probably utopian aspiration of the 3W approach is to inform technological decision-making in practice. In other words, the aim of the 3W approach is to provide a template that informs all those involved in technological decision-making how such decisions should be thought about. While it is beyond the direct control of STS scholars to determine how technological policy is made in practice, a direct contribution academics can make is to develop a coherent model of technological decision-making that has the potential to improve the existing practice of decision-making. Such a contribution is made in this thesis by proposing the 3W model.

In order to convince others about its prescriptive value, the proposed 3W model is then used to analyse the involvement of South African government officials in the assessment of the safety of AZT between October 1999 and August 2000. In accordance with the capabilities of the 3W model, the normative

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regard to certain aspects of the natural world. This will be explained in detail in chapter 1. Moreover, 'technical expertise' in the context of SEE includes two types of expertises: interactional expertise and contributory expertise. The meaning of those terms will also be explained in detail in chapter 1.

potential of the MDP is tested by comparing the reality of the technological decision-making process about PMTCT with its prescriptions.

Structure of the Thesis

The two main purposes of the thesis are engaged in dialectic interplay, which means that the development of the model serves to aid its analytic application, while its analytic application provides insights that feed back into the development of the model. The first three chapters are dedicated to model-building, while chapters 5 to 7 deal with the application of the model to the case study. Chapter 4 is descriptive in nature and provides some additional information about the context in which the technological decision-making about PMTCT occurred.

As the first step, the 3W model has to be developed. Important parts of such a model are already in place as they have been introduced by others since 2002 (*e.g.* Collins and Evans 2002, 2007 and Evans and Plows 2007). In particular, as will be shown in chapter 1, a distinction between 'technical' and 'political phases' of technological decision-making has already been proposed. Moreover, a demarcation criterion, 'technical expertise', which is based on a new and innovative theory of expertise and which allows analysts to solve the problem of extension with regard to technical claims, has also already been developed.

Having become acquainted with the basics of the 3W model as well as with the SEE specific concept of technical expertise, chapter 2 shows how exactly the 3W model differs from technocracy and technological populism that can be associated with Wave 1 and Wave 2 theories of scientific knowledge and expertise respectively.

Chapter 3 develops the Minimal Default Position (MDP). According to the rules of the MDP, policy-makers are in control of *policy choices*, but their *justifications* for the policy-choices are constrained by consensual expert advice. Again, the differences between this specific solution to the problem of integration and the solutions provided by technocracy and technological populism are made transparent.

Chapter 4 describes the extent of the problem of mother-to-child transmission, provides an overview of the potential range of solutions to the problem and justifies the narrow focus of subsequent chapters on Thabo Mbeki by outlining how political power in South Africa was distributed in the late 1990s.

In chapter 5, the technological decision-making process concerning the prevention of MTCT in South Africa is described and the various justifications utilised by the government to delay the introduction of AZT for PMTCT are analysed. By converting the MDP into an analytic tool, it can be shown that only a short period of two years of complete inactivity regarding decision-making concerning PMTCT can be attributed to government's misrepresentation of consensual scientific advice.

In chapters 6 and 7, the focus shifts to the problem of extension related to policy-relevant propositional questions that only indirectly or implicitly contain technical judgements. This type of question is referred to as 'meta-questions.'

Chapter 6 explores the extent of permissible participation with regard to the meta-question that asks whether a particular scientific discourse is in a state of controversy or consensus. Focussing on the meta-question concerning the identification of a scientific controversy is new territory for the 3W model. Exploring this meta-question is, however, relevant, because judgement of the state of a scientific discourse can impact on the technological decision-making process. This was the case in South Africa, where Mbeki's comments in October 1999 triggered a lengthy investigation.

In the chapter, the public reasoning provided by Mbeki to justify his judgement that the safety of AZT was the subject of a scientific controversy is compared to the reasoning of scientific experts. It is established that scientific experts can base their judgements not only on technical expertise, but also on social understanding, which they acquire routinely in the course of becoming an expert. This form of social understanding, which complements technical expertise, is called 'domain-specific discrimination' (DSD) and represents a kind of expertise not so far represented in the Periodic Table of Expertises. In contrast, Mbeki based his judgement on the most common form of meta-expertise 'ubiquitous discrimination.' Given that those relying exclusively on ubiquitous

expertise add nothing to the judgement of the state of scientific discourses that is not offered by other types of experts, it is argued that this specific class of meta-experts ought to be excluded from making such policy- relevant judgements.

Chapter 7 builds directly on what has been outlined in chapter 6. Given that policy-process-relevant judgements about the state of a scientific discourse can legitimately be made by a heterogeneous set of actors, policy-makers might find themselves faced with conflicting assessments regarding the state of a scientific discourse. To enable policy-makers to assess whether an invoked scientific controversy is authentic or not, a set of criteria is proposed. The criteria are operationalised in accordance with a specific understanding of the nature of science, which is based on insights provided by Wave 2 and Wave 3 of science studies. This STS understanding of the nature of science, which guides the operationalisation of the four criteria, is referred to as 'sociological discrimination.' Due to the continued popularity of an understanding of the nature of science that can be associated with Wave 1, sociological discrimination represents currently a special type of meta-expertise, which has not previously been named or defined. Sociological discrimination could, in principle, be transformed into 'ubiquitous discrimination.' This requires an educational effort which ought to aim at teaching people about the nature of science, not specific scientific findings. It is to be hoped, that educational efforts to disseminate the 'public understanding of the nature or process of science' will eventually contribute to better technological decision-making – not by transforming lay-people into specialists, but by enabling them to critically evaluate the conduct of technological decision-making processes.

The conclusion of this thesis completes the dialectical circle as the empirical findings of the analysis help to improve both the theory of expertise at the heart of SEE and the 3W model. For example, the application of the MDP to the technological decision-making process about the provision of antiretroviral drugs for PMTCT, as set out in chapter 5, brings the limits of the 3W model in general and of the MDP in particular to attention. This suggests that the implementation of the 3W model in practice can only succeed if certain contextual conditions regarding the political environment are in place. The discussion in chapters 6 and 7 indicates that SEE has, so far, put too much emphasis on technical knowledge.

As a consequence, the importance of social knowledge for making technical judgements, an aspect that has received considerable attention by relativist science studies over the last few decades, has to be incorporated into SEE's theoretical framework.

On Method

There is no need for an extensive discussion of methods in this thesis. The first three chapters are concerned with theoretical considerations. Chapter 1 is essentially an exposition of the already established parts that make up the 3W model. Chapter 2 can be described as a purpose-driven literature review as the basic principles and assumptions that inform technocracy and technological populism are reconstructed on the basis of what has been established in the wider STS literature. Chapter 3 derives the MDP by reconciling two principles of SEE – the only requirement is to produce an account that is coherent and consistent.

Chapter 4 provides a description of contextual features, concerning the extent of the problem of MTCT in South Africa in 1999 and the distribution of political power within the South African society. This description is based mainly on factual accounts provided in the literature concerned with South Africa's HIV/AIDS policy. The central challenge is to provide a synthesis of these factual accounts as there is broad agreement among a diverse set of observers about the relevant facts.

Chapter 5 is concerned with a reconstruction of the sequence of events regarding the technological decision-making process that concentrates predominantly on the period between mid 1997 and August 2000. Again, a considerable body of social and political science literature provides a relatively uncontroversial description of the sequence of events. The main task is to piece different accounts together and to verify certain assertions made in individual accounts. This is achieved by cross-checking or 'triangulating' the academic accounts with reports in newspapers and statements provided by interviewees.

Chapters 6 and 7 are mainly concerned with assessing the expertise, and reconstructing the justifications for certain actions of a small number of actors

involved in the debate about PMTCT between October 1999 and August 2000. In addition, Chapter 7 also introduces two very brief case studies that are not related to the technological decision-making process about PMTCT in South Africa. The analysis conducted in these two chapters is informed by the interpretative approach pioneered by Max Weber (1980[1921], Collins 2008). Weber argues that the interpretative method consists of two important steps. Firstly, the social analyst has to 'understand' how actors perceive their surroundings. The analyst has to make the perspectives of actors intelligible to an audience. Secondly, analysts have to detach themselves from the perspective of the actors to provide an explanation for or interpretation of aspects of actors' understanding of the world. In other words, analysts' are free to develop their own categories to explain the understanding of actors.

Given that the analysis of the case study is informed by SEE and the 3W model, analyst's categories are readily available. It is the first step of interpretation, the reconstruction of the actors' perspective, which has to be conducted. The purpose of the reconstruction was mainly to establish what certain actors knew and when or how certain actors publicly justified particular judgements. It is argued that the purpose of the reconstruction does not warrant the use of highly sophisticated qualitative methods. Two reasons can be specified. First, the level of detail required to reconstruct certain actors' perspectives is not very high. For example, with regard to establishing Mbeki's expertise on the safety of AZT, it is important to know whether his understanding was developed through reading texts in isolation or whether he spent considerable time discussing the matter with experts. If, as has been the case, it is established that his knowledge was acquired through self-study, it is not necessarily relevant to establish what exactly he was reading; one can already draw the conclusion that he could not have acquired tacit knowledge on the matter due to his lack of interaction with experts.

Second, there is a great deal of agreement within a variety of sources, ranging from newspaper articles over narratives in academic texts to interviews, when it comes to certain relevant events. In rare cases, where there is disagreement, it is often irrelevant to establish which account, if any, is true. For example, there is a disagreement about how Mbeki first became aware of claims

that AZT is toxic. Was he informed by a barrister or was he informed by a business man? For the purposes of the analysis of Mbeki's expertise, this discrepancy does not make the slightest bit of difference.[9]

The purpose of the reconstruction of certain actors' contemporary perspective does not require much in way of 'qualitative analysis.' More important is the acquisition of information about certain actors and particular events. The choice of information sources is influenced by three factors: first, the case study is of a historic nature; second, the benefit of hindsight is not supposed to be relied on; and third, the main actors involved in the episode were, for the greater part, not prepared to contribute to this study.

These factors rule out the possibility of acquiring information through direct participation as it is impossible to participate in events which lie in the past. Some of my understanding of historic events surrounding the specific episode of technological decision-making in South Africa stems, however, from participating in the academic discourse about those events. In particular, I have shown a continued academic interest in South African HIV/AIDS policy since at least 2002. I have published several papers about aspects of South Africa's HIV/AIDS policy that are more or less related to the controversy about the safety of AZT (*e.g.* Weinel 2005a, b, 2006, 2007, 2008, 2009, Von Soest, Calcagnotto and Weinel 2006 and Von Soest and Weinel 2007). I have attended public talks related to the issue and given a seminar at the University of Leipzig in 2005. I have spoken to a number of people who were either directly involved in the episode or who possessed knowledge that was relevant to the 'bigger picture.' In short, over the course of the last 8 years, I am confident in having developed some expertise of these historical events, which cannot be neatly traced back and attributed to individual sources.

The bulk of 'reconstructive' work concerning the contemporary understanding of certain key actors has had to be done on the basis of texts and documents (Silverman 2000, Atkinson and Coffey 2004). For the purposes of reconstructing events and the perspectives of key actors, I have relied on a wide

[9] It is, however, a decisive detail in the story of Virodene, a supposed cure for AIDS, in which Mbeki got entangled (see Myburgh 2007, 2009).

variety of sources, ranging from academic accounts of the PMTCT controversy over scientific papers to discussion in newspaper articles. Most helpful for finding these sources have been numerous internet websites dedicated to the issue of PMTCT, the safety of AZT and other relevant issues. Also of great importance were regional libraries and a variety of book shops in South Africa.

In addition, over the course of a three and a half weeks stay in the country in April and May 2008, I conducted nine interviews with scientists, doctors, AIDS activists, and political observers; each of these actors had been involved in the technological decision-making process, albeit to differing extents and in different roles.[10]

One obvious problem of conducting interviews about historical events is that actors' memories are often only patchy and blurred. I found myself often in a situation where I was better informed about certain events than some of the interviewees. This is to be expected, given that to me the events have special significance, while for interviewees they were often just one of many events they had experienced.

Another major problem is that some of the central characters were either unwilling or unable to be interviewed. Various expressions of 'unwillingness' can be distinguished. For example, former President Mbeki, who sits at the heart of this case study, failed to respond to any interview requests. Others, such as a few scientists who had great insight into what was going on behind the scenes, responded, but flatly refused to be interviewed on the subject matter due to their personal preference of not being dragged into the matter again. Others, such as South African-based critics of AZT, initially agreed to be interviewed, but later, upon realising that I did not agree with them with respect to the legitimacy of the government's continued insistence that AZT was unsafe, changed their minds. One can also distinguish various expressions of 'inability.' Some of the central

[10] I do not reveal any further information about the interviewees. In cases where passages are drawn from interviews, only the profession of the interviewee is stated. No names, workplaces, or locations and times of when the interview was conducted are revealed. At times, parts of quotes from interviews were removed to preserve the anonymity of the interviewee. Confidentiality with regard to the interviewees is covered by the consent sheet signed by them. The need to preserve the confidentiality of individuals in such cases has become increasingly apparent to the author through the research process, in particular following difficult exchanges prompted by a number of AZT critics, who do not share the conclusions arrived at in this thesis.

actors had passed away between 2000 and 2008. Others could not be tracked down. For example, I was unable to locate a former government adviser, whose name was frequently cited in newspapers and who played an important role in the Department of Health. Others were out of the country as it is often the case with South African scientists and academics.

A third problem, which limits the value of the interviews, was time constraints. Most of the interviewees were prepared to give me an hour, although some of them spontaneously provided more of their time. During this hour, I had to introduce myself and the research project as well as establish some information regarding the expertise of my interviewees. This left less time for substantive questioning on certain aspects of technological decision-making process than would have been ideal.

Despite generic and specific difficulties related to conducting interviews, the conversations (the interviews were open) with South Africans were of value insofar as they confirmed that certain events had taken place or that certain facts had been known at the time. In terms of bringing out genuinely new facts related to the case study, the interviews were of limited significance.

1. Technological Decision-Making and the Problem of Extension

1.1 Introduction

The purpose of this chapter is to introduce a solution to a common problem in science-based policy-making that arises out of differences in the speed of consensus finding in politics and science. A popular criticism of democratic policy-making is that finding consensus is often very slow as certain procedures have to be followed and many different actors with different interests have to be consulted. It has been observed, however, that consensus finding in the realm of science is often much slower (*e.g.* Collins 1981a, Collingridge and Reeve 1986, Martin 1989, Collins and Pinch 1998[1993]). In some cases, it can take years, decades or even centuries before certain claims are regarded as 'true statements' by the majority of scientific practitioners. The difference in the pace of consensus finding between politics and science has been summarised by Collins and Evans (2007: 8) in the phrase: "In general, the speed of politics exceeds the speed of scientific consensus formation." This observation points to a fundamental problem in the context of science-based policy-making or, as it will be called here, technological decision-making: political decisions that depend to a certain degree on new scientific knowledge will often have to be made without science having established consensus and certainty with regard to the policy-relevant scientific knowledge.

This problem has been addressed in different ways by different STS approaches. It is possible to argue under Wave 2 that scientific experts no longer ought to play a privileged role in making policy-relevant technical or scientific judgements, since they, just like anyone else, do not know the truth about policy-relevant scientific matters. In other words, given the inherent uncertainty of scientific knowledge, making policy-relevant technical and scientific judgements should be 'democratised' through an extension of public participation (*e.g.* Liberatore and Funtowicz 2003b, Wynne 2003, Millstone 2009).

While such a 'participatory solution' is internally consistent, it compels those who subscribe to it to assume that abolishing a privileged role for scientific experts in making policy-relevant technical or scientific judgements provides advice that errs less frequently or that incorporates fewer bad errors compared to expert advice. The case of Thabo Mbeki's involvement in making judgements about the safety of AZT, described in the Introduction to this thesis, suggests that this assumption is to some extent flawed. The infection of thousands of children with HIV could have been avoided if President Mbeki and his government had listened to scientific experts, who advised the government that the benefits of AZT outweighed the risks when the drug was used for PMTCT.

Once it is recognised that the indiscriminate extension of public participation in providing policy-relevant technical or scientific advice is not a desirable option, the so-called problem of extension has to be addressed. The problem of extension refers to the challenge of finding a suitable criterion or rationale to determine who should be allowed to provide scientific advice to policy-makers and who should be excluded. To deal with the problem of extension, Collins and Evans (2002, 2003, 2007) have proposed a new approach to technological decision-making as part of their Studies of Expertise and Experience (SEE) framework. Accepting the impossibility that science can always provide policy-makers with true and certain scientific knowledge, they nevertheless argue that the task of providing policy-relevant scientific or technical knowledge should still be restricted to those with relevant expertise. The reasoning behind this position is that the policy-relevant judgements of those with relevant expertise, *e.g.* those 'who know what they are talking about', are likely to be superior compared to judgements of those who do not understand the relevant technical or scientific issues.

Collins and Evans (2002, 2007) have completely re-conceptualised 'expertise.' While their concept of expertise retains a realist or essentialist character, the realism is derived from a link with tacit knowledge. This link enables analysts to recognise and measure expertise and therefore to solve the problem of extension. One consequence of this re-conceptualisation of expertise is that any association with technocratic approaches to technological decision-making which use 'scientific expertise' as a demarcation criterion, is avoided.

This chapter is organised in three broad parts: The first two introduce the specific meaning of 'technological decision-making' and 'problem of extension' respectively. The third part introduces the SEE-specific concept of expertise in general and of scientific expertise in particular. First, the link between expertise and tacit knowledge is explained. Second, a classification of different types of expertise, summarised in the so-called Periodic Table of Expertises (PTE) is introduced. Finally, I set out how a specific concept of 'scientific expertise', which affords the inclusion of both accredited scientists and actors without formal scientific education, is derived from the more general notion of expertise.

1.2 The Problem of Extension

What is exactly the problem of extension? And whose problem is it? Collins and Evans (2002) describe the problem of extension as a problem directly related to the use of relativism in the context of technological decision-making. According to Collins and Evans (2002), anyone who wants to retain a special role for 'experts' or 'specialists' or 'those who know what they are talking about' when it comes to making policy-relevant technical judgements, has to answer the following question:

> To what extent, under which circumstances, and according to which rationale should 'the public' or 'publics' be able to actively participate in technological decision-making? (Collins and Evans 2007: 10)

Before outlining how Collins and Evans believe that the problem of extension can be solved, some terminological and conceptual work has to be done to avoid misunderstandings.

1.3 The Meaning of Technological Decision-Making

Collins and Evans (2002: 236) have defined technological decision-making as,

> ...decision-making at those points where science and technology intersect with the political domain because the issues are of visible relevance to the public.

This definition resonates with many similar concepts of decision-making processes that have been given different names in the literature, for example,

'science for policy', which Brooks (1964: 76) defines as "matters that are basically political or administrative but are significantly dependent upon technical factors." Similar concepts include 'mandated science' (Salter 1988), 'policy-decisions in which expertise plays a non-trivial role' (Krimsky 1984), 'risk analysis' (Fiorino 1990) 'science-based public policy-making' (Millstone 2009) or 'public decision-making that involves science' (Wynne 2007). Thus, technological decision-making refers to a hybrid decision-making process that is (predominantly) political but includes technical or scientific components.

Collins and Evans take the hybrid character to mean that technological decision-making contains two different components or 'phases', which have to remain at least analytically distinguishable (Collins, Weinel and Evans 2010).[11] One is called the 'technical phase'; making decisions in this phase will be referred to as *technical decision-making*. The other phase is called the 'political phase' and making decisions in this phase will be referred to as *technological decision-making*.[12]

Collins and Evans (2002) have sketched out the differences between the technical and the political phases of technological decision-making along four dimensions. These are: type of questions addressed, type of actors involved, role of 'politics' and type of values involved. As the discussion below illustrates the distinction between values and politics is superfluous. Taking recent work by Douglas (2009) into account, these two dimensions distinguished by SEE can be integrated into one dimension dealing with values. The following outline of the differences between technical and political phase has to be understood as prescriptive, not descriptive. The prescriptions can be understood as aspirations that might remain unfulfilled in practice, although actors should do their utmost to meet the ideals.

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[11] As will be shown later, Collins and Evans's distinction between 'two phases' in technological decision-making must not be confused with the fact-value distinction used by positivists to characterise the hybrid character of technological decision-making.

[12] Although I will make this clearer in Chapter 3, it is important to realise that 'technical decision-making' is merely a little part of the wider process of 'technological decision-making.' That means it is *not* argued that technical decision-making exhausts science-based public decision-making as Brian Wynne (2007, 2008) still appears to believe.

Participating Actors

The technical phase has also been called 'expert phase' by Evans and Plows (2007), since SEE postulates that only those with sufficient technical expertise should participate in this phase. While the category of 'technical expert' will often include scientists who have received formal training and have worked for some time on a specific issue, it can also include actors who have not received any formal training, but have gained technical understanding that is relevant to an issue under consideration.[13] Participation in this context refers to active involvement in technical decision-making.[14]

In contrast, participation in the political or 'democratic' phase should be as extended as possible.[15] How exactly participation in the political phase is regulated is not of concern to SEE, although Collins *et al.* (2010) express a preference for democratic decision-making procedures that allows for a broad and informed debate. In accordance with democratic principles both experts and non-experts should be able to participate in the political phase in their role as citizens (*e.g.* Barber 1984, Held 1999).

Questions to be addressed

One of the most important characteristics of a technical phase is that it is limited to dealing with very narrowly framed 'questions of fact' or 'propositional questions' (Collins and Evans 2002, Evans and Plows 2007). Given that the expertise of actors participating in the technical phase is narrow, the questions should also be narrow to reduce the risks of transgression of expertise (Nowotny

[13] For example, Wynne (1989a, 1992) has shown that the knowledge of Cumbrian sheep farmers about sheep would have been a crucial ingredient in the design of experiments conducted by scientists to assess the exposure to radiation. Farmers knew due to their knowledge about sheep that the experimental design was flawed from the outset. The scientists were not prepared to listen, but they had to later abandon the experiments due to the problems anticipated by the sheep farmers.

[14] Of course, participation can be understood differently. For example, lay persons might 'participate in a technical phase as 'consultants.' For example, in a project on drawing up new guidelines for animal welfare in the European Union, experts presented some of their work-in-progress to citizen juries to get feedback from 'consumers' (*e.g.* Miele and Evans 2010). While lay persons undoubtedly 'participate' in the technical phase, they accrue no decision-making rights in the sense that they will be ask to make technical judgements. 'Active participation' in the context of Wave 3 means 'having the right to make technical judgements.'

[15] That is because, in terms of the 3W model, no specialist tacit knowledge is required to participate in the political phase. In other words, 'expertise' or 'experience' is not a relevant criterion for participation in the political phase.

2003). According to Evans and Plows (2007: 835), a formative intention associated with technical phases is "to arrive at an agreed understanding of some aspect of the natural world."

It is critical to note a clear difference between technical decision-making and what might be called 'scientific decision-making', *e.g.* making scientific judgements in the context of 'doing science.' The aspiration that underpins scientific decision-making is to arrive at true statements about the world. Looking for the 'truth' or for 'definite facts' can take a lot of time; time that is usually not available in the context of technological decision-making, which is driven by the pace of political decision-making (Collins and Evans 2002, 2007). In contrast, technical decision-making serves a practical purpose within a political context. Given that technical judgements have to be made urgently to remain relevant in the context of technological decision-making, they only have to reflect the contemporary available technical and scientific knowledge, which might be incomplete and relatively uncertain. *Technical decision-making* therefore aspires solely to provide those in the political phase with what might be called 'best possible technical judgement based on contemporary knowledge.' While those involved in the technical phase cannot and are not expected to deliver scientific truths, they nonetheless have to provide consensual answers to propositional questions.

In contrast, the political phase is supposed to deal with much wider 'questions of preference.'[16] Instead of finding an agreed understanding of 'how the world is', the overall intention is to find an agreed understanding of 'how the world should be.' Scientific knowledge cannot help directly with regard to this explicitly normative task, although it can provide political actors with some 'background constraints' such as that it is impossible to build perpetual motion machines and so on.

Values and 'Politics'

Collins and Evans (2002, 2007) advance two similar, although seemingly separate arguments by distinguishing between 'values' and 'politics.' With regard to

[16] In chapter 3, the respective tasks or functions attributed to the two phases will be sketched out in more detail.

values they claim that only certain values are permissible within the technical phase, while all other values cannot legitimately influence the answering of propositional questions. Specifically, Evans and Plows (2007) argue that the values identified by Merton (1942) such as universalism, communalism, disinterestedness, and organised scepticism, are appropriate for answering propositional questions and therefore represent 'good' epistemic values. With regard to politics, by which they seem to mean 'non-scientific interests' or 'preferences', Collins and Evans (2007) argue that these should never extrinsically or overtly influence technical or scientific judgements.[17] Since the terms 'values' and 'politics' remain largely undefined, it is not entirely clear what the exact differences between the two are.

A more detailed discussion of the role of values within science by Douglas (2009) appears to suggest that Collins and Evans (2002, 2007) as well as Evans and Plows (2007) might talk essentially about the same thing when they invoke values and politics. Douglas's critique of the fact-value dichotomy can be understood as a clarification and as such, can be incorporated into SEE. Douglas (2009) shows with regard to the development of the 'value-freedom doctrine' that philosophers of science hardly ever argue that complete value-freedom is possible. Instead, they have constructed a set of so-called 'epistemic values', which are regarded as necessary and useful within the context of science. It appears that Collins and Evans (2002) and Evans and Plows (2007) fall back on the idea of permissible 'epistemic values' when they suggest that certain values appear to be reconcilable with the formative intentions underlying the technical phase.

Douglas (2009), in contrast, rejects the idea of permissible epistemic values as an arbitrary and self-serving class of values. Instead, she introduces the notion of epistemic criteria, which is differentiated from values. Criteria such as internal consistency and predictive competency of scientific theories are not values but essentially define the scope of science. If a theory does not meet epistemic criteria, it does not belong to science at all. Douglas then distinguishes three classes of values: cognitive, ethical and social values. These different types of

[17] This means essentially that non-scientific interests should never function as evidence, *e.g.* a scientist should not be allowed to reject a theory because it collides with her or his political preferences.

values seem to correspond with Collins and Evans' notion of 'politics' as Douglas regards them as generally non-scientific. According to Douglas, these values should play different roles at different stages of a scientific process. Values have to play a direct (or extrinsic) role when it comes to choosing and framing of research questions. At this stage, values themselves can function as reasons to justify choices.[18]

Douglas insists, however, that ethical, social and cognitive values must not play a direct or extrinsic role when it comes to the stage where scientists have to decide whether to accept or reject a theory on the basis of given evidence. Here, values cannot function as evidence; that is, one cannot legitimately reject a theory by pointing to an ethical or social preference. Exactly the same argument is made by Collins and Evans (2007) with regard to 'politics.' Douglas claims that values can play an indirect (intrinsic) role; for example, when they are invoked to determine the 'importance of inductive gaps left by the evidence' (2009: 96).

While cognitive, social and ethical values must not function as reasons in *technical* decision-making, they can legitimately influence *political* choices made in the political phase. It can be expected that within a polity different actors subscribe to a range of personal and cultural values. This might lead to political conflicts which have to be solved through negotiations and compromises.

According to this modified account of SEE, the difference between the technical and political phases of technological decision-making can be outlined with regard to three dimensions. First, participation in the respective phases is open to different social groups. While SEE does not prescribe any restrictions in participation in the political phase, active participation in technical decision-making should be reserved for those with a proper and relevant technical understanding, *e.g.* appropriate levels of expertise. Second, there are differences in the nature of questions addressed in the respective phases. The participants in the technical phase are expected to deal with and find answers to propositional

[18] This is an area that is not discussed by Collins and Evans (2002, 2007), but it does not appear to be incompatible with SEE. Interestingly, Evans and Plows (2007) argue that in the context of technological decision-making activities such as choice of research topics and framing of propositional questions belong into the political phase, which suggests an extrinsic political character.

questions or questions related to matters of fact. In contrast, participants in the political phase are supposed to deal with questions of preference. Third, there is a difference with regard to the role of values. Values can play an open and direct role within the political phase. This means, participants can invoke values to legitimate technological decisions. The same ought not to be possible in the technical phase. Values must not be used as reasons to justify technical judgements.

1.4 Locating the Problem of Extension

Having clarified the meaning of technological decision-making in the SEE framework, it can now be asked how the problem of extension relates to this form of decision-making. More specifically, it can be asked how the problem of extension relates to the two 'phases' of technological decision-making?

The 'problem of extension' does *not* apply to the political phase of technological decision-making. As Fischer (2009: 146) has pointed out, the "classical defining question of the public domain (or of the political phase) is 'that of what to do,' which goals and values to pursue." He continues:

> This concern operates independently of propositional knowledge – or knowledge of any sort – about the technology [or scientific issue]. The normative question here is basically about the way of life and the impact of the technology [or the application of scientific knowledge] on it.

Fischer claims that no 'knowledge of any sort' is needed to address questions of preference. Epistemic qualifications are not relevant for deciding preferential questions. It does not matter whether someone has a university degree or no education at all – both may 'know' (or sometimes not) what they want to do and how they want to live. It is in such a context that some advocates of public participation argue that just anyone can be involved in technological decision-making. Wynne (2003: 411), for example, has made the following statement:

> To the extent that public meanings and the imposition of problematic versions of these by powerful scientific bodies are the issue, then the proper participants [in technological decision-making processes] are in principle every democratic citizen and not specific sub-populations qualified by dint of specialist experience-based knowledge.

Since SEE agrees with the sentiments expressed by Fischer and Wynne in relation to the 'political phase' of technological decision-making, there is no 'problem of extension' in this specific context. In other words, no reasonable case can or should be made for the restriction of public participation within the SEE framework when it comes to the political aspects of technological decision-making, at least not on epistemological grounds.[19]

The problem of extension is therefore *exclusively* related to the technical phase of technological decision-making. In light of these clarifications, Collins and Evans' (2007: 10) formulation of the problem of extension can be slightly modified:

> To what extent, under which circumstances, and according to which rationale should 'the public' or 'publics' be able to actively participate in *technical* decision-making, which is only a part of broader technological decision-making?

The formulation of the problem of extension can be even further simplified. In its present form, Collins and Evans ask three questions: one about the extent of participation, one about the circumstances and one about the rationale for boundary-drawing. The second question has effectively been answered in the preceding paragraphs: participation should only be limited with regard to technical decision-making, not with regard to technological decision-making. The first question is redundant since the extent of participation clearly depends on the 'rationale' that is used. One could, for example, use the rationale 'eye colour.' Given that there are more people with brown eyes than people with green eyes, the extent of participation is greater when the former is used and smaller when the latter is used. The problem of extension can finally be captured in the following formulation:

> According to which rationale should 'the public' or 'publics' be able to actively participate in *technical* decision-making, which is only a part of broader technological decision-making?

The problem of extension can be characterised as a 'boundary problem' related to determining inclusion and exclusion with regard to technical decision-making.

[19] SEE is relatively neutral with regard to non-epistemological arguments in favour of restricting public participation in the political phase as this is simply none of its business. They are, after all, political arguments in the wider sense which might be accepted or rejected in a political debate. Collins *et al.* (2010) have stated their own preference for political decision-making that involves informed debate among all those who are affected.

What is therefore needed is a demarcation criterion that allows identifying those who possess relevant 'knowledge' or 'qualification' to answer propositional questions of a scientific or technical nature.[20] The criterion proposed by SEE is 'technical expertise.'[21]

1.5 A Realist Concept of Expertise

The problem of extension is about deciding who should and who should not participate in the technical phase of technological decision-making. Collins and Evans' solution to this problem appears to be old-fashioned: they argue that only those with relevant *technical* or *scientific expertise* should be allowed to actively participate within the technical phase. This seems to hark back to technocratic approaches in only accredited scientists were fit to deal with scientific and technical matters.

Before it can be clarified what exactly 'scientific expertise' means, it has to be set out how 'expertise' has to be understood in the context of SEE. Collins and Evans (2002, 2007) provide a unique and innovative concept of 'expertise' that, in contrast to other concepts used in STS, elucidates what expertise actually is (Evans and Collins 2007). Expertise within the SEE framework is linked to tacit knowledge. Tacit knowledge can be understood as knowledge that is not or cannot be made explicit (Collins 2010a). What this link between expertise and tacit knowledge means will be explored in the next section.

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[20] An important clarification has to be made: this does not mean that non-experts are categorically excluded from the technical phase. They can, of course, contribute in various ways to improve the understanding of the relevant technical issues where this makes sense. A huge body of literature, focusing on the interaction of lay-persons and experts in technical decision-making processes, has emerged over recent decades (*e.g.* Krimsky 1984, Peterson 1984, Fiorino 1989, 1990, Laird 1993, Irwin 1995, Joss and Durant 1995, Fischer 2000, 2009, Rowe and Frewer 2000, De Jong and Mentzel 2001a, Kleinman 2001, Bulkeley and Mol 2003, Liberatore and Funtowicz 2003a, Maasen and Weingart 2005a, Stilgoe, Irwin and Jones 2006, Bijker, Bal and Hendriks 2009, Callon, Lascoume and Barthe 2009). Within the bounds of the SEE framework, lay persons can legitimately 'participate in the technical phase by monitoring whether experts adhere to due process, by providing 'public feed-back', by providing vital data and information that might improve technical decision-making. What lay-persons are categorically excluded from is making final technical judgements, *e.g.* formulating answers to propositional questions.

[21] The exact meaning of the term technical expertise is worked out over the next three sections. Although it has been pointed out before, it is worth emphasising that 'technical expertise' in the context of SEE cannot be translated as 'knowledge of scientists and technologists.'

1.6 Understanding, Tacit Knowledge, and Expertise

According to Collins and Evans (2002, 2007), to be an expert on something means to 'know what one is talking about', which necessarily means that one has acquired particular bodies of tacit knowledge.[22] One important aspect of this perspective on expertise is that one can only acquire particular tacit knowledge through particular kinds of experience and socialisation (Collins and Evans 2007: 6).[23] Tacit knowledge can be defined as knowledge "which has not or cannot be made explicit" (Collins 2010a: 85).[24] The importance afforded to tacit knowledge does not mean that explicit knowledge, notably the knowledge that can be articulated or written down, is no longer important. Explicit knowledge of facts remains a significant characteristic of experts. It is, however, the presence of tacit knowledge that enables actors to 'deeply understand' something. For example, the media frequently report complaints from employers about the decline in the education of young people in the UK. This comes despite continuous year-on-year improvements in A-level results. The complaint is that pupils simply learn facts in order to pass highly standardised exams. While this practice leads to good results in the exams, the mere learning of facts does not enable pupils to apply those facts in contexts that do not resemble the neatly structured exam scripts. Thus, while the possession of explicit factual knowledge might correspond with the view of experts as highly knowledgeable actors, it is only in conjunction with tacit knowledge that actors become able to know what they are doing and what they are talking about.

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[22] Being an expert also means to 'know what one is doing.'

[23] To have experience means to 'genuinely understand' something in the context of SEE. The phrase 'genuine understanding' must, however, not be taken to mean that experts have some sort of epistemologically privileged access to reality. Experts can understand things better than others in the sense that they have spent considerable time thinking about an aspect of the world, but there is no guarantee that they understand things 'correctly' or 'in accordance with the truth' and so on. The link between understanding and tacit knowledge is highly important. Brewer (1998), for example, also defines expertise as a specific ability to 'understand', but does not make the link between understanding and tacit knowledge. As far as I am aware, only Krimsky (1984) implicates tacit knowledge in his understanding of scientific expertise within the STS literature on technological decision-making.

[24] Collins (2010a) suggests that three different types of tacit knowledge can be analytically distinguished: relational, somatic and collective tacit knowledge. "Collective tacit knowledge turns on the nature of the social, somatic tacit knowledge turns on the nature of the body, but relational tacit knowledge is just a matter of how particular people relate to each other..." (Collins 2010a: 86). Collins shows, however, that in practice usually all three types are combined. For the purposes of this work, this analytic distinction is not really important.

An important implication of SEE's focus on tacit knowledge is that this allows expertise to be conceptualised as both 'real' and 'social.' It is 'real' because experts under SEE have to possess certain tacit knowledge. While tacit knowledge itself cannot be directly observed, it manifests itself in the abilities of actors. For example, without tacit knowledge in gravitational wave detection, it is practically impossible to pass as an actor in a language-based imitation game, in which one has to talk as if one were an expert (see Collins, Evans, Ribeiro *et al.* 2006, Giles 2006, Boyce 2009, Evans and Collins 2010). Without the ability to talk or to act in a way that indicates to others that an actor knows what she is talking about or is doing, there is no point in calling someone an expert in the context of SEE.[25]

The concept of expertise under SEE is also social. According to Collins and Evans (2007: 7), bodies of tacit knowledge are 'developed' and 'maintained' in social groups. Tacit knowledge is not the property of an individual. This leads to a second reason for the social nature of expertise, which is related to the way in which individuals can acquire tacit knowledge.

> Mastering a tacit knowledge-laden specialism to a high level of expertise, whether it is car-driving or physics, ought, then, to be like learning a natural language – something attained by interactive immersion in the way of life of the culture rather than by extended study of dictionaries or grammars or their equivalents. (Collins and Evans 2007: 23)

The only way for an individual to tap into these 'reservoirs' of tacit knowledge is to actively immerse her- or himself into the social group that possess certain kinds of tacit knowledge. For example, to acquire tacit knowledge that is specific to virologists, one has to actively immerse oneself into the community of virologists, which means one has to either perform virologist-specific practices (which one can only perform properly under instructions) or one has to talk to them about things that virologists do. It should be evident by now that reading textbooks on virology is insufficient to pick up the community-specific tacit knowledge: since

[25] The importance of language is stressed by Collins (2010b, see also Selinger, Dreyfus and Collins 2007). It has to be pointed out that not all people who are able to do something expertly are able to talk about it in a way that reflects their expertness. To express oneself adequately requires 'interactive ability' (Collins and Evans 2007).

tacit knowledge cannot be made explicit, it gets 'lost' when instructions are written down or when they are audio-recorded.[26]

Another important implication of linking expertise to tacit knowledge is related to the distribution of expertise (see Evans 2008). Collins and Evans (2002, 2007) distinguish explicitly between 'ubiquitous expertises' and 'specialist expertises.'[27] 'Ubiquitous expertises' are widely distributed; to master them, it is sufficient to be a member of a society. Specialist expertises, in contrast, are related to 'domains of knowledge' which form around particular activities and are associated with particular subgroups of larger collectives.

The possibility of 'ubiquitous expertise' is provided by the theory of expertise proposed by SEE, because tacit knowledge is involved in every meaningful or intentional human practice. Whether we walk on a busy pavement, use a pocket calculator, utter a sentence in natural language, make a bed, prepare for a date – all these activities involve tacit knowledge. Some skills such as, for example, the ability to speak English fluently in the UK are so widely distributed that an actor simply has to grow up in the UK in order to master the skill.[28] In

[26] This is a marked difference to nominal and relational conceptions of expertise. Nominal concepts of expertise are solely concerned with the possession of explicit knowledge, which makes the existence of isolated and individual experts possible. Such a conception treats expertise as real, but misses the social dimension. Without tacit knowledge there is no reason as to why knowledge cannot simply be transferred between isolated individuals by means of written text. One individual writes down what she knows and another individual can then read the text and gain knowledge. Relational theories of expertise, in contrast, emphasise the social nature of expertise, but cannot treat it as real and substantive. As pointed out above, the relational perspective on expertise that expert status is attributed to an actor by other actors. This attribution depends, for example, on the position of actors in larger networks or on power relations between actors. According to this view, the knowledge that an actor possesses is entirely irrelevant for the attribution of expert status.

[27] The distinction between ubiquitous and specialist expertise has mainly heuristic value. The boundary between the two is fluid and fuzzy and depends on the frame of reference. For example, the ability to speak English fluently is 'ubiquitous' if only the population of the UK is considered. In the context of talking about the entire human population, speaking English is an ability of a certain sub-population. Similarly, while writing can be considered to be a ubiquitous skill in present-day Britain, only a few hundred years ago, writing was considered to be skill mastered only by some in the UK. Moreover, in some parts of the world, the ability to write is still not ubiquitous.

[28] Collins and Evans (2007) draw attention to the failure of producing reliable automated translation devices to emphasise the fact that natural language speaking is not a trivial achievement.

other words, there is no need for an actor to immerse herself deliberately into a specific subpopulation to learn English.[29]

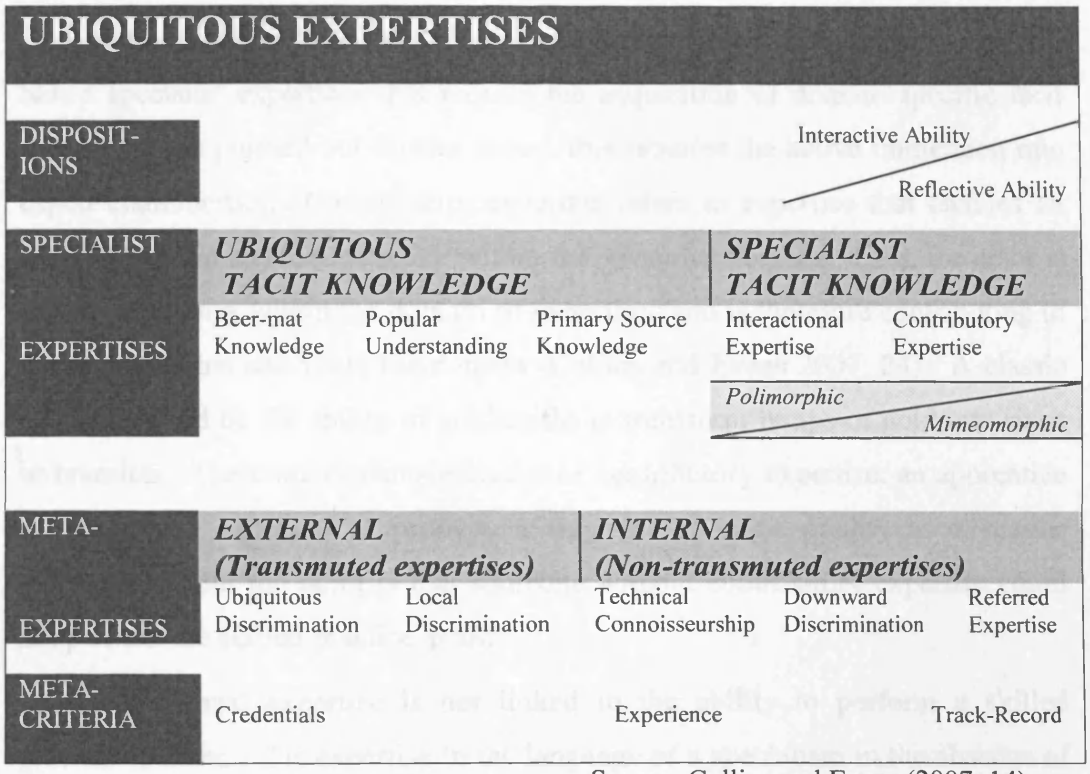
While some expertises can be regarded as ubiquitous in certain settings, others are not. For example, it is not enough to be socialised in a Western society to interpret the raw data provided by a large-scale clinical trial in an acceptable way, to perform a body-domination sadomasochistic practice in a way that pleases someone who is prepared to pay for this 'service' or breed sheep in a way to produce lamb meat that meets EU standards. The tacit knowledge required for this set of activities is developed and maintained by certain specialised groups within a wider society. Most people do not usually interact with members of such groups (at least not in their capacity as members of this specialist group) and are therefore unable to acquire the tacit knowledge related to those activities. This explains why certain forms of expertise are not ubiquitous.

1.7 The 'Periodic Table of Expertises' (PTE)

So far, a number of different concepts have been loosely introduced. Terms such as 'ubiquitous expertises,' 'specialist expertises,' 'ubiquitous tacit knowledge,' and 'specialist' or 'domain-specific tacit knowledge' have been mentioned. Collins and Evans (2007) have taken these and even more concepts and created a classification scheme of various expertises that has been summarised in the so-called Periodic Table of Expertises (PTE).

[29] This is, of course different with regard of learning to speak English with, say, a Scouse accent. While this accent might be ubiquitous in and around Liverpool, one cannot learn to speak in such a way if one grows up in Yorkshire unless one interacts with Scousers or listens to them speaking on TV or radio.

Figure 1: The Periodic Table of Expertises



Source: Collins and Evans (2007: 14)

The Periodic Table of Expertises has been extensively described and explained elsewhere (Collins and Evans 2007, Evans and Collins 2007, Evans and Plows 2007). Here, not every aspect of the table is discussed in detail; rather the focus is on explaining those concepts that are relevant for the purposes of this thesis.

The darkly shaded fields represent different forms of expertises. The top row, 'ubiquitous expertises' summarises all those skills and abilities that enable humans to participate in everyday-life. Those skills and abilities involve tacit knowledge, but the tacit knowledge can in principle be picked up by any member of society in everyday contexts.

Specialist expertises are expertises that relate to specific domains of knowledge such as physics, astrology or sheep-farming. The first three types, beer-mat knowledge, popular understanding and Primary Source Knowledge, can be described as 'levels of knowledge' rather than as expertises. This is because the tacit knowledge needed to acquire those levels of knowledge is ubiquitous. To gain, say, the level of popular understanding about cancer research, actors have to

master such ubiquitous expertises as the ability to buy a book or a science journal in a shop and to read it.

'Interactional expertise' and 'contributory expertise', in contrast, are full blown specialist expertises that require the acquisition of domain specific tacit knowledge. As pointed out further above, this requires the active immersion into expert communities. Contributory expertise refers to expertise that enables an actor to perform a skilled practice within the specialist domain. Thus, the actor is able "to *do* things within the domain of expertise" and is therefore contributing to the activities that constitute the domain (Collins and Evans 2007: 24). A classic example would be the ability of goldsmiths to transform lumps of gold into rings or bracelets. There are obviously grades of contributory expertise: an apprentice would not be expected to produce a ring of the same quality as a master craftsman.[30] But the point is that someone without contributory expertise could not perform the skilled practice at all.

Interactional expertise is not linked to the ability to perform a skilled practice. Rather, it "is expertise in the language of a specialism in the absence of expertise in its practice" (Collins and Evans 2007: 28). Interactional expertise refers to the ability to talk as if one were a full-blown 'contributory expert' without actually being able to perform a skilled practice of a specialist domain. A good example of an interactional expert is a successful ethnographer. Ideally, an ethnographer 'does not go native', but on return from an extended period immersed into the culture under observation the ethnographer should be able to give an account of the culture as if it is her own (see also Collins 1984).

Collins and Evans (2007) point out that the relationship between interactional and contributory expertise is transitive. To have contributory expertise is always to have interactional expertise, even though the latter might be just latent. But an actor can have interactional expertise without having contributory expertise. And while contributory expertise can be passed on through relations that resemble apprenticeships, interactional expertise cannot be

[30] See the five stage model of expertise acquisition developed by Dreyfus and Dreyfus (1986). The stages are: (1) novice, (2) advanced beginner, (3) competence, (4) proficiency, and (5) expertise.

passed on in the same way – it has to be acquired by an actor through interaction with contributory experts.

The 'meta-expertises' row describes five types of expertises that might be used by actors to make judgements about technical judgements and specialist expertises.[31] The first two types of meta-expertise are classified as being 'external' or 'transmuted.' This is because they use social criteria or considerations to produce technical discrimination. For example, the demeanour of an expert might be considered to be untrustworthy and a technical claim made by this expert might therefore be rejected. While ubiquitous discrimination rests on social knowledge that is ubiquitous, local discrimination rests on social knowledge that is acquired through social or spatial proximity. For example, one might believe an expert because one knows her as a neighbour or one might distrust security pronouncements made by experts working for a nuclear power station if one lives close by and has experienced the discrepancy between statements and reality over years.

The remaining three types of meta-expertise are classified as 'internal' or 'non-transmuted.' In contrast to external meta-expertises, internal judgements about expertise are "based on possessing one level or another of the expertise being judged" (Collins and Evans 2007: 15). Internal types of meta-expertise have so far been relatively neglected by research efforts and are therefore not well understood. The most straightforward type of internal meta-expertise is downward discrimination. Downward discrimination occurs when someone who is asked to make a judgement about a technical claim is 'more' expert than the claim-maker. This, according to Collins and Evans leads to reliable and authoritative internal judgements about expertise. Collins and Sanders (2007) have started to explore the meaning of 'referred expertise.' The idea behind referred expertise is that someone who has acquired specialist expertise in one domain might be able to transfer this expertise into a different domain and apply it

[31] This apparently does not include judgements about expertise made by those who possess interactional or contributory expertise in the domain under consideration as this is part of the 'normal' activities within a specialist domain.

there to inform technical judgements.[32] Technical connoisseurship, within the context of SEE, is largely unexplored. It resembles the expertise of art critics or wine experts. In analogy to art critics, the application of technical connoisseurship is only possible in specialist domains governed by relatively stable and well-established conventions.[33]

1.8 Demarcating 'Technical Knowledge' from Other Domain-Specific Knowledge

As the foregoing illustrates, the term 'expert' is not synonymous with 'accredited scientist.' Under the definition of SEE practicing scientists remain experts *par excellence.* Scientific fields and disciplines are archetypical 'domains of knowledge' as they usually focus on particular issues and problems that are explored by employing a range of methods that are deemed to be acceptable by the small set of practitioners. Given that scientists spend usually a lot of time thinking and discussing about specific issues and problems in a particular way, the scientists' discourse is usually removed from public visibility (Miller 2001). The category of 'expert' is, however, not exhausted by reference to scientists. There are countless actors who have acquired tacit knowledge related to one or another non-scientific domain of knowledge. For example, there are experts in plumbing, typewriting, race-car driving, poem-writing, mobile phone using, bread-making, astrology, palm-reading and so on.

Out of the many different variations of specialist or domain-specific knowledge, it is 'scientific' or 'technical' knowledge that is chosen to be crucial for solving the problem of extension. As mentioned above, technological decision-making is policy-making that depends on technical or scientific knowledge. Knowledge in palm-reading or poem-writing is undoubtedly

[32] One unanswered question is what exactly is transferred: technical knowledge or social knowledge? Moreover, given that expertise is referred from another domain, referred expertise is not internal in the strict sense of the word.

[33] It remains unclear, however, whether and if so, how, the expertise of technical connoisseurs is related to that of contributory experts to a specialist domain. Art critics, for example, don't have to be able to write or produce a play to judge it. Moreover, art critics seem to form their own community and appear to develop standards of judgement that seem to be independent of artists' judgements (*e.g.* Cote 2010).

knowledge related to particular domains, but it is entirely irrelevant when it comes to solving the problem of extension that is related to the technical phase of technological decision-making. It is therefore necessary to demarcate technical knowledge from other forms of specialist knowledge. For this purpose, two distinctive concepts, 'formative intentions' and 'family resemblance', will be discussed in this section as they can function as demarcation criteria to separate technical expertises from other instances of specialist expertise.

'Scientific' or 'technical' knowledge, as it is understood here, is linked to so-called 'formative intentions.' What are formative intentions? Formative intentions are intentions that relate to social groups and not just particular individuals[34]:

> Formative intentions are the intentions that are available to actors within a form of life, and partly constitute that form of life, rather than being the intention of any particular individual at any particular time and place; they are 'intention types.' Formative intentions are public because they are the property of the collective rather than the private property of the individual: they are available for inspection by anyone who shares the form of life which they help to constitute (Collins and Evans 2007: 116)

Formative intentions are supposed to bring about certain results that are recognised as such by those who share a form of life.[35] The meaning of formative intentions can be illustrated by using an example reproduced in Collins and Evans (2007). They argue that it is acceptable to dance a Tango in contemporary Britain to win a prize in a dancing competition, but one cannot dance a Tango to bring on rain (see also Collins 1983).

The idea of formative intentions can then be applied to distinguish different societal domains from each other. For the purpose of this thesis it is sufficient to concentrate on Collins and Evans (2007) distinction between science and politics as it corresponds to the distinction between technical and political phase.

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[34] It is very difficult to establish the intentions of a particular individual. For example, courts of law are often confronted with the question of intentionality and it requires a lot of forensic work by the police and other institutions to claim that someone did something with a particular intent (Collins and Kusch 1998). Social sciences are neither particularly interested in nor well equipped to deal with the intentions of individuals. Collins (2004a) has called this the 'anti-forensic principle.' For social sciences, the 'unit of interest' is 'social group', 'community', 'culture' or 'form of life.'

[35] Rob Evans points out that C. Wright Mills (1940) uses the concept of 'vocabularies of motives' to make a similar point.

One difference in formative intentions is related to what Collins and Evans (2007: 120-121) call the 'locus of legitimate interpretation.' Collins and Evans construct a spectrum that connects producers and consumers of knowledge or, more generally, 'output.' A variety of actors can be located on this spectrum running from 'authors' on the producer pole to the 'public' on the consumer pole. In between, closer to the authors, is a 'peer group' and closer to the public are 'critics.' Different practices such as science or politics can then be distinguished with regard to which actors are regarded as legitimate participants in interpreting the work of authors in a particular culture or form-of-life.

> In politics it is normal and appropriate for public opinion to be important in reaching conclusions; in science it is different, if not in practice at least in legitimate intention. (Collins and Evans 2007: 125)

Collins and Evans argue that in (esoteric) sciences, the locus of legitimate interpretation should remain close to the authors, which means that only members of the immediate peer group should be involved in interpreting the work of an author.[36] In contrast, it is normal and acceptable in politics that the public (or its representatives) can offer legitimate interpretations of policies or regulations.

Another difference between science and politics is related to the role in which preferences or values should enter in the respective practices. Collins and Evans (2007: 126) claim that despite STS having shown that politics and values inevitably enter scientific judgements, the formative intention of scientists has to be to resist and to suppress those inevitable extra-scientific influences as much as possible:

> We can, then, demarcate science from politics, not by looking at the content of scientific knowledge but by looking at the contrasting formative intentions of scientists and politicians. ... [S]ocial studies of science may have shown that politics and other mundane influences are *intrinsic* to scientific knowledge but, like interpretative ambiguity, they should never be *extrinsic*. Such influences must be resisted within any activity that we are to call a science. Those who engage in the social studies of science already know this and that is why, while they proclaim on the one hand that science is invested with politics, they insist on the other hand that the testing of drugs is `unduly influenced' by the power of the drug companies, or that studies of the effects of smoking are `distorted' by powerful tobacco interests, or that genetics in the Soviet Union was `damaged,' rather than `energised' by the political backing given to the ideas of Trofim Lysenko.

[36] Collins and Evans (2002) have distinguished between different forms of sciences. One of those forms is called 'public-use technologies' in which public opinion plays a legitimate role.

Thus, while sciences are not value-free, actors who intend to produce scientific knowledge have to ensure that values do not directly influence their knowledge claims. In contrast, it is part of the formative intentions of politics that values or preferences influence political judgements directly (see also Douglas 2009).

Discussions by both Douglas (2009) and Maasen and Weingart (2005b) intersect with such ideas. For Douglas, the value of science arises out of its aspiration to produce 'reliable knowledge.' In contrast, activities such as politics, sport or art do not share this aspiration. Maasen and Weingart (2005b) make a similar point, only more bluntly. By drawing on 'system theory', they suggest seeing the difference between science and policy-making "as one between two differentiated sub-systems with fundamentally different codes of operation":

> While science, as a subsystem, primarily adheres to the code of 'truth,' politics is primarily guided by the code of 'power'. In a nutshell, this claim postulates that, ultimately, science should produce truth, whereas political decisions should safeguard power. (Maasen and Weingart 2005b: 4)

While many within STS are acutely aware that science is struggling to provide reliable knowledge, let alone true knowledge, it is undeniable that this remains or should remain a major aspiration (or formative intention) of science, even though it might never be fulfilled.

From the foregoing, a number of formative intentions can be extracted which might serve as a guide in recognising 'scientific expertise':

1. Authority of interpretation should lie with producers of knowledge
2. Values must not directly influence the interpretation of data
3. Aspiration of claim-makers has to be to produce reliable and true knowledge

It can reasonably be expected that knowledge produced by scientists is based on 'technical expertise.' There are, however, exceptions since there is always the possibility that scientists lie and cheat (*e.g.* Rampton and Stauber 2002, Michaels 2008). Thus, claims made by scientists that violate these criteria cannot be regarded as expressions of 'scientific expertise.' For example, Michaels (2008) describes how some scientists signed letters containing technical arguments challenging the belief that smoking tobacco increases the chances of developing lung cancer which were written by PR companies on behalf of tobacco companies.

The aim was to create the impression that an expert challenged scientific knowledge (smoking tobacco is carcinogenic) on the basis of her or his technical understanding and not because the challenged belief was hurting the business interest of tobacco companies. The fact that PR companies wrote the technical documents violates the first criterion. The fact that this was all paid for by tobacco companies indicates that business interests directly influenced the claims made in the documents. Given that tobacco companies attempted to disguise their influence by paying scientists for their signature also strongly suggests that they lacked the aspiration to produce true or reliable knowledge.

At the same time, non-scientists can make technical claims that are continuous with scientific and technical knowledge. Some aspects of the case of sheep farmers in the Cumbrian hills, researched by Brian Wynne (1989a, 1992, 1996), are instructive here.[37] Wynne's description centres on the changing and contradictory risk pronouncements of the government in the immediate aftermath of the Chernobyl disaster. The problem was associated with a cloud that brought radioactively contaminated rain to Cumbria, including those areas where sheep designated for meat production were grazing. One aspect of the 'fall-out' between government and government-scientists on the one side and sheep farmers on the other was related to the question of how long it would take before the Caesium, which the radioactive rain had brought to Cumbria and which threatened the marketability of lamb meat, would decay. At one point, government experts suggested testing experimentally whether sheep could graze in the valleys instead of on the top of hills as they hypothesised that the contamination of the grass in the valleys would be lower. The experiments were, however, a complete failure. Sheep-farmers, who are experts in sheep breeding, predicted from the outset that the experiments were bound to fail. One significant problem with the experimental design was that the sheep had to be kept in a fenced area to allow close monitoring. Sheep-farmers knew that the sheep, which were used to free roaming, would respond badly to the restrictions of their movement. Had they

[37] Note that Wynne's overall interpretation of the case is seemingly incompatible with the SEE analysis (see Collins and Evans 2002, 2003, Wynne 2003). It is also important to note that Wynne is not interested in isolating certain technical aspects of this case study. He appears to be more concerned with questions of risk assessment, credibility and identities that are complex and multi-faceted. The following account is not an attempt to provide an exact representation of what Wynne intends to say; rather, it picks out one very narrow aspect of the overall case study.

been involved in the design of the experiments in their role as experts on sheep, the scientific experiments might have actually produced useful results.

The sheep-farmers' knowledge of sheep behaviour, while not scientific in the institutional sense, conforms with the formative intentions attributed to science.[38] The only difference between the sheep farmers and, say, university-based experts in animal husbandry is that the former lacked formal university education. Their expertise is, however, continuous. Wynne (1989a) refers to the farmers as 'specialists' with 'informal knowledge.' According to the theory of expertise developed by Collins and Evans (2002, 2007), sheep farmers can be considered experience-based contributory experts on matters of husbandry and related aspects that are routinely dealt with by them. What sheep farmers and scientists have in common is that their expertise is partial – scientists understand what characteristics an experiment should have for its results to be considered credible by peers, while Cumbrian sheep farmers understand how sheep behave in the local context.

The concept of formative intentions provides a means to distinguish technical expertises from other expertises associated with other domains independently of the credentials associated with the claim-maker. As shown, the sheep farmers' professional knowledge is continuous with the knowledge of an animal scientist who works on sheep husbandry. The sheep farmers' expertise thus meets the standards of 'technicality' derived from the formative intentions associated with science.

One further problem remains: an astrologer, for example, might claim that a particular constellation of planets has resulted in the evolution of the latest swine flu virus.[39] Despite its structural resemblance with scientific claims, which provide causal explanations of aspect of the world, it is ubiquitous knowledge in modern Western societies that astrology is no longer regarded as a part of science proper; rather, it is usually classified as a 'pseudo-science.' What is needed is

[38] It might be said that sheep farmers' expertise about sheep is continuous with the expertise that, say, academics working on sheep in a University department of agricultural studies have.

[39] The self-proclaimed 'medical astrologer' Eileen Nauman makes this claim in her blog entry on 28 April 2009. See <URL: http://medicalastrologybyeileennauman.blogspot.com/2009/04/medical-astrology-looks-at-swine-flu.html>

therefore another demarcation criterion to distinguish science and pseudo-sciences.

To demarcate sciences from pseudo-sciences, Collins and Evans (2007) use Wittgenstein's family resemblance concept. Wittgenstein notes, for example, that one is usually able to recognise a practice as a 'game,' despite one's inability to exhaustively define the concept of game (Wittgenstein 2001[1953]). For example, both chess and 'throwing a ball against the wall and catch it when it bounces back' can be considered being games. Yet, it is very difficult to find characteristics that both games share. One is a board game played by two individuals, which also involves the idea of winning, while the other is a ball game that can be played alone without anyone being considered a winner. Wittgenstein suggests that both practices are indirectly connected through a series of overlaps which they share with a broad range of games.

On the basis of Wittgenstein's insights, Collins and Evans postulate the 'family resemblance rule' to distinguish between sciences and pseudo-sciences. They point out, however, that the idea of family resemblance on its own does not solve the demarcation problem. This is because as any other rule, the family resemblance rule does not contain the rules for its application. One has still to make judgement as to what counts as a 'similarity', a 'relevant similarity', and a 'dissimilarity' and so on. These judgements can only be made with reference to a form-of-life or a culture. In Western societies, astrology is usually classified as a pseudo-science, because it lacks any institutional overlap with sciences and its practitioners do not show any ambition to create such an overlap, which must be part of the formative intentions of science.[40] There is no university department of astrology, no university jobs and no recognised peer-reviewed journal and so on.[41]

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[40] It might be argued that sheep-farming also usually lacks institutional overlap and that its practitioners – sheep farmers – usually do not show any intention to create such an overlap. The overlap can, however, also be created by scientists working on animal husbandry, which seems to be the case. In contrast, astronomers do not show any interest of incorporating astrological knowledge into their body of astronomical knowledge.

[41] This does not mean that astrology does not play any role in decision-making in modern, predominantly scientific cultures, although the position taken here is that it ought not to have any effect on technological decision-making. An example for astrology's practical influence on certain decision-making processes is the selection policy of the former manager of the French national football team, Raymond Domenech. Reports in the media suggest that his selection policy was not necessarily based on the ability of players or how they fit into a particular system, but under which star sign they were born. It has been suggested that he would never pick a 'Scorpio' as he did not

The difference between 'sciences' and 'pseudo-sciences' can therefore be expressed in terms of 'discontinuity.' While shifts or even 'revolutions' in thinking and theorising are common in science, the intentional stance of scientists who suggest new ways of looking at issues should be to preserve as much as possible regarding previous findings, institutions, methods and so on (*e.g.* Kuhn 1962, Fleck 1979[1935]). Astrology in contemporary Western societies lacks any overlap with sciences in terms of personnel, institutions and methods and so on and usually shows no intent of creating such an overlap.

Having separated scientific or technical knowledge from other forms of knowledge, it can then be associated with the types of expertises set out in the specialist expertises row of the PTE. By doing this, technical experts can be distinguished from lay-people who are perhaps, to varying degrees, 'technically literate.' Those actors who have acquired explicit technical knowledge, for example through reading popular scientific literature or even scientific papers, but who have not acquired domain-specific tacit knowledge through immersion into an expert community, will still be regarded as lay people in relation to technical knowledge. In contrast, those who have acquired both, explicit scientific knowledge as well as specialist tacit knowledge can be regarded as scientific experts. In other words, those with interactional and/or contributory expertises in technical and scientific domains are *technical experts*. In contrast, those with no technical knowledge or technical knowledge that amounts only to beer-mat knowledge, popular understanding or primary source-knowledge can still be defined as *lay-persons*. Making this distinction explicit is vital as the concept of 'technical expertise' provides the SEE-specific solution for the problem of extension.

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trust players of this particular star sign. Being a 'Scorpio' has apparently cost Robert Pires his international career in 2004, when Domenech took over the French team as the player has never been picked ever since. Despite guiding France to the final of the 2006 World Cup, his reign ended in controversy after France was eliminated in the group stages of the last World Cup in South Africa.

1.9 Conclusions

This chapter has introduced, explained and clarified a range of basic concepts that are required to understand the problem of extension as well as the SEE-specific solution to this problem. The problem of extension is the challenge for social analysts to provide a criterion according to which participation with regard to policy-relevant technical decision-making can be regulated. The criterion to solve the problem of extension proposed by SEE is 'technical expertise.'

First, the concept of technological decision-making, the subject matter of the 3W model, has been defined. Technological decision-making processes are constituted by two complementary 'phases.' The technical phase is defined as a decision-making space in which policy-relevant *technical decisions* are made. Only relevant experts should be able to actively participate in answering propositional questions of a technical or scientific nature. The political phase, in contrast, is defined as the decision-making space in which *technological decisions* are made. Technological decisions are partly constituted by its dependence on technical decisions or scientific advice.

Second, the meaning of the terms expertise in general and technical expertise in particular have been outlined. Experts are understood to be those actors who know what they are talking about and/or who know what they are doing. Within the context of SEE, expertise is linked with tacit knowledge. Tacit knowledge is a type of knowledge that cannot be made explicit, but which plays an important role in people's understanding of things. Without tacit knowledge, it would be impossible to successfully apply explicit knowledge to new situations. It has been pointed out that this link makes some expertises 'ubiquitous' due to the fact that tacit knowledge is involved in any intentional human action.

Third, given that technical expertise is based on scientific or technical knowledge, it is necessary to separate technical knowledge from other forms of knowledge. It has been shown that this can be done by using the idea of formative intentions. In the case of science, specific formative intentions are, for example, the aspirations to produce true knowledge and to let values influence interpretations as little as possible. Moreover, astrological knowledge about the provenance of the swine flu virus, which might be structurally mistaken for

scientific knowledge, can further be distinguished from scientific knowledge by checking whether they show an institutional overlap with science or any intent of creating such an overlap.

Fourth, according to the Periodic Table of Expertises, five types of technical knowledge can be distinguished. The three lowest levels cannot be regarded as amounting to 'scientific expertise' as actors have not acquired domain-specific specialist tacit knowledge. Only those actors who have acquired this type of tacit knowledge in conjunction with explicit technical knowledge can be classified as technical experts. Thus, only those with interactional and/or contributory technical expertises ought to be allowed to actively participate in the technical phase of technological decision-making according to the 3W model. This also means, in turn, that the 3W model does not prescribe who should and who should not participate in the political phase. In principle, every citizen can be regarded as a legitimate participant in contributor to the political aspects of technological decision-making.

An aspect that has not yet been entirely and sufficiently clarified is related to the issue of what exactly counts as a *propositional question of technical or scientific nature*, which those participating in the technical phase are supposed to answer. It is clear that the class of such propositional questions contains direct questions about the content of science: Is AZT safe? Does sinking an oil rig destroy marine life? Does second-hand smoke increase cancer risk? In line with Collins and Evans' argumentation, it seems plausible that only those with technical expertise should answer such direct technical questions. In chapter 6, it will be shown in passing that President Mbeki could not be regarded as a scientific expert and that he should have been excluded from intervening in the technical phase.

What has yet to be explored is whether questions that only indirectly depend on technical or scientific judgements should also be subject of the problem of extension. This is highly relevant in the context of the case study, because Thabo Mbeki maintained that he apparently never made a direct judgement about AZT. He claimed that he only informed the public that the drug was the subject of a scientific controversy. This judgement of the state of the scientific discourse

about the safety of AZT had relevance for the policy-process as this judgement provided the government with a justification to postpone technological decision-making about PMTCT. It becomes therefore interesting as well as important to explore whether participation in answering policy-process-relevant *propositional questions that are only indirectly of technical or scientific nature* should be restricted. The expertise involved in answering such questions has been described as meta-expertises in the PTE.

In terms of the PTE it might be stated that Collins and Evans have solved the problem of extension with regard to the specialist expertise row; the ambition in this thesis is to solve the problem of extension with respect to the meta-expertise row. Chapter 6 will explore whether just anyone can make policy-process-relevant credibility judgements which bear on the question whether a specific scientific discourse is characterised by controversy or consensus. Chapter 7 will explore whether restrictions in participation should apply to judgements that gauge the authenticity or inauthenticity of scientific controversies that are invoked in the context of technological decision-making processes.

2. Technocracy, Technological Populism, and the Problem of Legitimate Participation

2.1 Introduction

In the previous chapter, the problem of extension has been introduced as a 'boundary problem.' The problem is to find a suitable rationale to determine who should be included in and excluded from *technical decision-making*, which is conceptualised as small, but important part of *technological decision-making*. It has been shown have shown that SEE proposes 'scientific' or 'technical expertise' as the demarcation criterion to solve the problem of extension. While the term is an 'old' one, its meaning has been redefined by a substantive theory of expertise that links expertise to tacit knowledge.

Collins and Evans (2002, 2007) have justified the proposition of SEE as a 'new, third wave' of science studies by arguing that Wave 1 is inadequate for dealing with the problem of extension, which is caused by applying Wave 2 insights to technological decision-making. In particular, the inherently relativist and anti-essentialist stance of Wave 2 with regard to expertise makes it impossible for social analysts to use the concept normatively to inform an a priori distinction between experts and non-experts. When the Wave 2 conceptualisation of expertise is applied to the problem of technological decision-making, it creates, in theory at least, the problem of extension, because analysts' are no longer in a position to determine who is an expert and who is not.[42]

[42] It is important to emphasise 'in theory' in this sentence, because in practice, most approaches to technological decision-making that rely on insights of relativist science studies are inconsistent in the sense that they still recognise practices such as 'expert technical debate' or 'expert risk assessment' (Wynne 2007, Millstone 2009). The question, that can be asked from a SEE point of view is, how do authors who recognise such distinctive practice within larger decision-making processes involving experts actually recognise who an expert is? It appears that this question has so far been ignored. This is partly understandable as the overarching concern of those using relativist insights to reform technological decision-making appears to be on increasing public participation in the political aspects of technological decision-making. The problem of ignoring the problem of recognising expertise is that it might reify nominal concepts of expertise that underpin technocratic approaches as some conception of expertise has to be used to sustain such approaches.

Wave 1 is inadequate to deal with the problem of extension, because its specific conception of 'scientific expertise', which is used as the demarcation criterion, does a poor 'demarcation job.' The problem is that a nominal concept of expertise is used, which simply associates specialist understanding with certain formal educational credentials. While such a nominal concept of expertise affords a solution to the boundary problem, it can be shown that this might lead to the exclusion and/or inclusion of the wrong actors.

The chapter is organised in the following way: the first section deals with technocratic approaches to technological decision-making, which are compatible with Wave 1 conceptions of scientific knowledge and expertise. It outlines the assumptions that underpin such approaches and also shows that they can, in principle, deal with the problem of extension by using 'scientific expertise' as a demarcation criterion. It is, however, shown that using a nominal concept of expertise leads to the creation of wrongful boundaries between experts and non-experts.

The second section will focus an approach to technological decision-making, which is called 'technological populism.' Technological populism is compatible with Wave 2 conceptions of scientific knowledge and expertise. Under technological populism, the technocratic demarcation criterion of scientific expertise is thoroughly deconstructed, but, in contrast to 3W model, it is not replaced or re-conceptualised. This leads to the creation of the problem of extension.

A third section briefly revisits the initial debate about SEE that occurred in the pages of Social Studies of Science in 2003. Critics of SEE have claimed that Wave 3 is antidemocratic and represents a return to technocracy. By revisiting the critique of SEE, it can be shown that this interpretation is based on number of misunderstandings. It can be shown that the apparent gulf between Wave 3 and Wave 2 is far narrower than critics have made it out to be.

2.2 Against Wrongful Boundaries: Technocracy and the Problem of Extension

Wave 1, often referred to as 'sociology of science', was hardly interested in questions that concerned the nature of scientific knowledge or expertise. In their review of the field, Ben-David and Sullivan (1975: 203) characterise the discipline and its research questions in the following way:

> Sociology of science deals with the social conditions and effects of science, and with the social structures and processes of scientific activity. Science is a cultural tradition, preserved and transmitted from generation to generation partly because it is valued in its own right, and partly because of its wide technological applications. Its most distinguishing characteristic is that the primary purpose of its cultivators, the scientists, is to change the tradition through discoveries. This bears some similarity to the purpose of modern artists and writers. But innovations in art and literature are accompanied by dissension and conflict, because there are no explicit criteria and accepted procedures to determine whether an innovation is an improvement or deterioration of existing tradition. Although scientific criteria and procedures are neither perfectly unequivocal nor entirely stable, they are still far superior to criteria used in the evaluation of other cultural products. The relatively objective, consensual evaluation of discoveries makes science an extreme case of institutionally regulated cultural change.
> Sociologists of science have concentrated on this characteristic of science as a tradition and as an institution. The questions they deal with are: How did this unique tradition of modern science emerge and become institutionalized? How is it maintained and controlled? How is research organized? What determines changes in scientific organization, and how are these changes related to research?

This represents a very optimistic view on science as an activity that 'discovers' knowledge objectively and consensually. The questions asked by Wave 1 reflect this view of 'unproblematic' scientific knowledge as they focus mainly understanding the success of science by studying its historic emergence and current institutional structure.

Under Wave 1, sociology has no business in researching scientific knowledge. The main reason for this is that a positivist view of scientific knowledge prevailed. For example, Karl Mannheim (1985[1929]), one of the credited founders of sociology of science, declared that the sociological inquiry must not deal with the content of sciences. For Mannheim, there was simply nothing to study 'sociologically' as scientific knowledge is a true and objective representation of external reality, which does not require any social mediation. To be sure, it is accepted that extra-scientific aspects influence the success of science. Merton (1938), for example, has shown that science cannot thrive under political regimes that curb the freedom of scientists. Merton maintains, however, that such

the social context does not affect scientific knowledge itself. While authoritarian regimes might lead to a reduction in the rate of scientific discoveries, scientific knowledge itself is universal and entirely independent of social influences.

Even scientists, who discover scientific knowledge, are supposed to have no personal influence on scientific knowledge. They are depicted as merely neutral and 'non-invasive' executors of scientific methods, whose personalities, convictions, values and beliefs must have no influence on the discovered scientific knowledge. While positivists have been at times been aware that such a narrative was in conflict with empirical observations, concepts such as 'context of discovery' and 'context of justification' or 'epistemic values' have been introduced as 'repair tools' to explain away certain anomalies (*e.g.* Longino 1990, Douglas 2009).[43]

Such a positivist description of science provides a congenial narrative to justify the use of scientific knowledge for political action (*e.g.* Lane 1966, Krimsky 1984, Ezrahi 1971, Collingridge and Reeve 1986, Wynne 1989b, Fischer 1990, Jasanoff 1990, Millstone 2009). Science is not only omnipotent when it comes to solving all sorts of problems or puzzles, but the solutions it produces are also regarded as valid (or true), neutral, consensual and optimal. If science more or less replaces politics, the advantages appear to be considerable: political conflicts are eradicated, mistakes are avoided and only the best possible solutions are implemented.

The mode of decision-making that this ideology has informed is known as 'technocracy.' Fischer (1990: 17) defines technocracy as "a system of governance in which technically trained experts rule by virtue of their specialized knowledge and position in dominant political and economic institutions."[44] Technocracy came to be an influential idea and practice in the decades after World War II,

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[43] It is often observed that certain scientists or laboratory workers have a 'special knack' for producing results, while others fail. My mother, for example, used to be a laboratory technician who was 'famed' for her skills in DNA sequencing. By invoking a distinction between a context of discovery and a context of justification, positivists can explain away the obvious influence that personal characteristics of scientists or laboratory workers play. While the context of discovery might be 'messy', this does not matter as long as the knowledge can be justified in a way that deletes all social influences.

[44] Although the term 'technocracy' has first been used in 1919 in America the idea to give political power to 'knowledge elites' dates at least back to the 17th century and is associated with thinkers such Francis Bacon, Henry Saint-Simon and Auguste Comte (Fischer 1990: 17).

when a range of influential thinkers proclaimed that knowledge had surpassed the classic factors of (capitalist) production such as land, capital or labour in importance and that Western societies have become 'information societies' (see Fischer 1990, 2000). Emphasising the importance of knowledge for societal progress, the argument was advanced that 'scientifically and technically trained elites' or experts should be granted far greater political responsibility than it had been the case before the war (Price 1965, Lakoff 1977, Jasanoff 1990, Douglas 2009).

The term technocracy is used in two different senses in the wider STS literature and it is important to be aware of the different meanings (*e.g.* Krimsky 1984, Fiorino 1990, Jasanoff 1990, Millstone 2009). What might be called Technocracy 1 is based on a radical interpretation of positivism. Under Technocracy 1, science replaces politics and accredited scientists take the place of political decision-makers. Technocracy 2 is informed by a more moderate interpretation of positivism.[45] The spheres of science and politics are recognised as being separate and scientists and policy-makers play different roles in technological decision-making. Whenever the term 'technocracy' is used without any further qualification in this thesis, it refers to Technocracy 2.

Under a technocratic framework, scientifically trained experts are afforded a central role in decision-making. Analogous to scientists' involvement in the 'discovery' of scientific knowledge, the involvement of experts in the policy process is portrayed as entirely unproblematic. Experts, by relying on scientific methods, simply 'discover' the best possible policy solutions.

Given that only experts are allowed to participate in the technical component of technological decision-making, it is vital to understand how experts and expertise are defined. Despite the importance of the concept of 'scientific expertise' for technocracy, the nature of expertise often remains opaque in technocratic accounts. What these accounts provide is usually a description of the characteristics of experts. This is what is called a nominal concept of expertise. For example, Kantrowitz (1967), who proposes a science court to provide

[45] Millstone uses the term technocracy for Technocracy 1 and 'decisionism' or 'decisionist model of policy-making' for Technocracy 2.

politicians with consensual scientific knowledge, implicitly equates scientific expertise with the 'knowledge of accredited scientists.' Lane (1966), who argues that all political decisions should be determined by scientific knowledge, does not use the term expert, but instead talks about 'professionals.'

> Now we turn to the application of this [scientific and technical] knowledge to public policy. The people who make this application are, in the first instance the professionals, organized in their own associations, governing and staffing institutions devised to develop and teach the new knowledge and apply it to current problems. (Lane 1966: 657)

Expertise can thus be understood as the knowledge of those who have acquired the knowledge through membership in particular institutions or associations.

Brint (1990), who argues that it is not difficult to distinguish experts from lay people, uses the term 'professional expert':

> Professional experts participate in the political process as experts only by virtue of an assertion of trained, "knowledge-based" authority. This is not true of lay people. (Brint 1990: 364)

Scientific expertise is a form of professional knowledge that is grounded in training or education within scientific institutions. According to Nowotny (2003: 155), a similar definition of scientific expertise has been proposed by John Ziman (1978), who understands it simply as "reliable knowledge, produced within the relevant peer group of scientists."

In general, as Fischer claims in *Technocracy and the Politics of Expertise* (1990: 18), expert knowledge in technocratic frameworks refers "primarily to trained expertise in the 'applied sciences,' particularly engineering, applied mathematics and computer sciences, economics, and the managerially and policy-oriented social sciences."

What all these definitions have in common is that they do not elucidate the nature of expertise, *e.g.* they do not reveal what expertise actually is. Instead, the focus is mainly on what one needs to do or on the condition to become or be recognised as an expert. The emphasis is therefore on formal training and education. Experts can thus be recognised through formal credentials that certify the acquisition of knowledge and skills through formal education and training. These definitions, then, equate expertise with 'knowledge coming from certified scientific and technical experts', *e.g.* those who have received formal training and

education in scientific or technical areas (Nowotny 2003: 152). Expertise, defined in this way, is easy to detect: credentials and formal certificates can serve as recognisable markers of expertise.

Formally, technocracy offers a similar rationale or criterion as SEE to regulate participation with respect to the technical aspects of technological decision-making: only 'scientific experts' are allowed to participate in technical decision-making. The problem of technocracy is that the nominal concept of expertise leads to flawed boundary drawing. The problem is that 'expertise' as understood by positivist science studies and proponents of technocracy is not an accurate indicator for having relevant experience with respect to technical or scientific issues.[46]

Collins and Evans (2002, 2007) have used contemporary research provided by science studies to show that the positivist 'expertise' criterion draws the boundary between legitimate and illegitimate participation in the wrong place. Such research shows, on the one hand, that there are actors who do not possess formal credentials, but who nonetheless 'know what they are talking about.' On the other hand and often at the same time, such research also demonstrates that the possession of formal credentials is no guarantee for genuine understanding of certain issues. In particular, Collins and Evans point to STS case studies that show that actors who are classified as lay-persons according to the nominal understanding of 'expertise' might 'know what they are talking about.' These case studies show, for example, that self-educated AIDS activists were able to contribute constructively to the design of clinical trials (Epstein 1995); that sheep farmers were excluded from developing scientific models that deal with radioactive contamination of soil as well from designing experiments to measure

[46] To be sure, positivist science studies and technocracy can be criticised on much more than an overly simplistic concept of expertise. For example, STS research has shown that the development of sciences is intricately linked with political developments outside the realms of science (*e.g.* Fox Keller 1985, Shapin and Shaffer 1985, Ezrahi 1990, Shapin 1994, 1996). By concentrating on the actual practice of knowledge production, STS case studies show in detail that scientific interpretations and choices are permeated with extra-scientific considerations and values (*e.g.* Shapin 1979, Collins 1981a, Knorr-Cetina 1981, 1999). Moreover, as Nelkin (1971, 1975) and others have repeatedly shown, scientific knowledge that enters the political domain becomes inevitably politicised (Hammond *et al.* 1983, Collingridge and Reeve 1986, Weingart 1999, Maasen and Weingart 2005b). Given that it is the problem of extension that is of specific interest here, I'll concentrate on a critique of the rationale to determine the extent of participation in technical decision-making used by technocratic approaches.

radioactive contamination in sheep despite their deep understanding of local conditions and sheep behaviour (Wynne 1989a); that farm workers were excluded from assessing the risk of a pesticide despite their great experience of using the chemical compound on farms (Irwin 1995); that a sociologist of science was accused of producing junk-science, despite his expertise about the expertise of 'fingerprint identification' (Lynch and Cole 2005, Cole 2009).[47]

What these findings suggest is that experience in what is usually regarded to be technical and scientific matters is not necessarily linked with the possession of formal credentials. This also suggests that the technocratic understanding of technical expertise is incapable of recognising such experience-based or 'experiential' expertise which reflects genuine and deep understanding (De Jong and Mentzel 2001b, Collins and Evans 2002, 2007). It is this non-recognition of valid forms of understanding that renders positivist and technocratic solutions to the problem of extension inadequate. The exclusive attribution of expertise to accredited scientists has also contributed to what Nowotny (2003) has called the transgression of expert knowledge. Nowotny argues that scientists qua their status as experts often comment on issues that is beyond their area of immediate experience.

2.3 The Need for Boundaries: Against Technological Populism

From the early 1970s onwards, the positivist sociology of science has been slowly but steadily pushed aside by approaches that subscribe to different forms of relativists methodologies (*e.g.* Collins 1983, Zuckerman 1989, Cozzens and Woodhouse 1995). Such methodologies are commonly based on varying interpretations of the principles of symmetry and impartiality, which put social analysts in a position to treat certain aspects of the world as equivalent (Bloor 1976).

Symmetry and impartiality explicitly represent two pillars of the so-called 'strong programme' of the sociology of scientific knowledge (SSK) (*e.g.* Bloor 1976, Barnes and Bloor 1982). The strong programme outlines the

[47] Most of these case studies are discussed in Collins and Evans (2002, 2007) and Evans and Collins (2007). There is no need to discuss them in detail again here.

methodological foundation on which relativist studies of knowledge, in particular scientific knowledge, are based. The principle of symmetry holds that the same causes or factors explain all scientific beliefs, whether they are regarded as true or false. The principle of impartiality holds that the social analyst should remain neutral with respect to truth and falsity or rationality and irrationality of beliefs.[48]

Barnes and Bloor (1982: 22) argue that all 'relativist doctrines' subscribe to two basic elements: first, "the observation that beliefs on a certain topic vary" and second, "the conviction that which one of these beliefs is found in a given context depends on, or is relative to, the circumstances of the users." The characterisation of a third element, the so-called 'equivalence postulate', depends on what form of relativism theorists wish to describe. Barnes and Bloor (1982: 23) opt to focus on 'credibility':

> Our equivalence postulate is that all beliefs are on par with one another with respect to the causes of their credibility. It is not that all beliefs are equally true or equally false, but that regardless of truth and falsity the fact of their credibility is to be seen as equally problematic.

The task of the social analyst is to causally explain why some beliefs on a certain topic are treated as more credible than others. The interesting point is that there is nothing inherent in the beliefs itself which can explain the differences in credibility. A belief that is regarded as credible in location A can be regarded as devoid of any credibility in location B. Adopting a relativist perspective with respect to scientific knowledge, defined as accepted (or credible) belief, enables social analysts to locate the causes that brings people in different locations or times to regard different beliefs on the same topic as credible or not. Related to scientific beliefs, it can be explained, by invoking extra-scientific causes, why different scientists accept or reject different beliefs about the same natural phenomenon.

While the stated ambition of the strong programme is to make the sociological study of science 'scientific', critics have pointed out that its relativist perspective contributes to the devaluation of science and scientific knowledge (Ashmore 1989, Hacking 1999, Brown 2001, Labinger and Collins 2001). By

[48] The other two principles that complete the strong programme are 'causality' and 'reflexivity.' The causality-principle urges analysts to provide causal explanations, while the reflexivity-principle states that analysts should use the same methodology to explain their own beliefs.

assuming that the essential difference between accepted and rejected scientific knowledge is caused by varying attributions of credibility and not, as positivists would have it, by 'nature' itself, undermines the notion of the superiority of scientific knowledge as a true reflection of external reality.

While Bloor and Barnes's 'optimistic scientism' with respect to the social study of knowledge and science has not found many followers, the relativist perspective which is grounded in the principles of symmetry and impartiality has been widely adopted (Ashmore 1989). For example, Collins' (1981a,b) 'Empirical Programme of Relativism' (EPOR), which encourages micro-studies of scientific controversies, has contributed to the 'deconstruction' of scientific knowledge as a special form of knowledge. This is because such case studies show that the closure of scientific debates, and therefore the acceptance of particular claims as true scientific knowledge, can be only explained with recourse to extra-scientific reasons. Most EPOR inspired case studies first demonstrate the possibility of 'interpretative flexibility', by showing how scientific methods, such as experiments, are in themselves not enough to determine *the* correct interpretation of scientific data (*e.g.* Collins 1981a). Given that interpretative flexibility makes scientific controversies potentially endless, analysts then explain how it is that scientific controversies do end. While the actual 'closure mechanisms' might vary from case to case, they can commonly be characterised as representing non-cognitive or extra-scientific factors.

EPOR contributes to the deconstruction of a positivist perspective of scientific knowledge by demonstrating that scientists do not make decisions about acceptance and rejection of contested knowledge on the basis 'scientific evidence.' Rather, mundane social considerations such as 'nationality of the experimenter', 'reputation of the experimenter', 'reputation of the institution an experimenter works in' and so on are used to decide which claim to believe (Collins 2004a, see also Shapin 1995). Such an analysis not only undermines the traditional view that scientific knowledge is purely based on proven facts and therefore superior to other forms of knowledge; it also shows that the positivist belief that personalities, convictions and values of scientific experts do not affect scientific knowledge is a 'myth' (Collingridge and Reeve 1986).

The deconstruction of traditional positivist beliefs can be regarded as a side-effect of the Strong Programme and EPOR, which focus primarily on explaining – causally or not – what counts as accepted knowledge. Others within science studies appear to regard deconstruction as an end itself for science studies and are, in the words of one commentator, 'anti-explanatory' (Durant 2010). According to Durant, influential science studies scholars such as Latour, Jasanoff and Wynne have, to varying degrees, moved away from a worldview where systems or elements are sufficiently independent from each other to allow explanations; that is, in which one element can be explained by another. Rather, a relational perspective is embraced where everything is connected and dualities such as nature v culture, science v politics, expertise v laity and so on collapse entirely (*e.g.* Latour 1987, 1999a,b, 2005, Jasanoff 1990, 2003a, Wynne 2003, 2007, 2008).[49] Within such a perspective, the deconstruction of boundaries becomes an end in itself. How this works can be shown with regard to the category 'expertise.'

In her critical reply to Collins and Evans' (2002) original proposal of SEE, Jasanoff argues that 'expertise' can only be treated as an attribute that is afforded to some actors by other actors as a result of certain contingent social processes. She illustrates her argument with recourse to a case study about lawsuits concerning potentially harmful effects of silicone gel implants (Jasanoff 2002). Claims that silicone gel implants might have harmful effects were not only contested, they were also relatively unexplored when they entered the court setting:

> It is hard to see how [Collins and Evans's] conceptual system would have helped the courts to find the right experts when they began their task. There was no cognizable body of 'breast implant science' at this stage in the proceedings. Agnosticism was the only responsible position to start with. Gradually, as the

[49] For example, Latour has repeatedly stressed that the principle of symmetry proposed by Bloor does not go far enough. Instead of simply postulating symmetry with regard to credibility of beliefs, Latour proposes super-symmetry between nature and culture and humans and non-humans (*e.g.* 1987, 1999a,b, 2005). Impartiality becomes agnosticism and instead of assuming an a priori difference between things belonging to 'nature' and things belonging to 'culture', Latour regards such categorisations as outcomes of complex processes within extensive networks linking everything in the world. Accordingly, he explores the processes that make some things 'cultural' and other things 'natural.' Description appears to be the only way in which those subscribing to relational world views can depict the emergence of certain more or less stable categories out of a 'primordial soup' in which everything is connected. 'Following the actors', *e.g.* describing their relations to other entities, is the supposed job of science study scholars.

> cases grew in volume and salience, some witnesses took on the character of acknowledged experts, simply by appearing in more than one lawsuit and thus being accredited by the performative rituals of the law. The nature of claimed expertise changed as the first epidemiological results began to be published (showing no causal connection between implants and typical connective tissue disease); it changed again when a federal court consolidated thousands of claims into a single, enormous case of multidistrict litigation and appointed a scientific panel to advise it." (Jasanoff 2003a: 396)

Jasanoff argues that a realist or essentialist concept of expertise provided by SEE, which defines expertise a priori, would have been of no use in the context of this case as her analysis shows how the attribution of expertise shifted during and after the initial court case. On the basis of these observations she argues that agnosticism with regard to the question of expertise is the '*only* responsible position' for science studies scholars to take. Accordingly, Jasanoff maintains that science study scholars should restrict themselves to describing retrospectively how expertise is attributed:

> [W[hat emerges as most deserving of analysis by our field is how particular claims and attributions of expertise come into being and are sustained, and what the implications are for truth and justice; the intellectually gripping problem is not how to demarcate expert from lay knowledge or science from politics (though reflexive attempts to make such demarcations should be taken seriously). Such demarcations will keep being produced in any case, in the everyday work of scientists, citizens and institutions of governance. Showing what is at stake in the making of such boundaries is another matter. That is a fitting place for critical science studies scholarship. (Jasanoff 2003a: 398)

Jasanoff does not deny that actors routinely distinguish between science and politics or between experts and lay persons. For 'critical science studies', as defined by Jasanoff, doing such boundary work is not 'intellectually gripping' given that boundaries are always produced by someone. The only fitting task for science studies is to point out, presumably time and again, what the consequences of making such distinction are.[50]

Jasanoff's prescription that science studies must avoid prescriptions appears to dovetail with Durant's (2010: 190) observation that relational approaches regard 'understanding' as the only legitimate end of STS analysis.[51] As Collins

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[50] With regard to silicone gel implants, Jasanoff (2003a) shows, for example, how the certification of authoritative expertise on the matter transformed the identities of women from individual sufferers into 'statistical victims' (see also Jasanoff 2002).

[51] As Collins points out, Weber's idea of sociological analysis was only partly based on 'understanding.' For Weber, understanding was not an end in itself, but a means to provide

(2008) has shown, concentrating exclusively on understanding – instead of regarding it as a first step towards 'explaining' – only requires analysts to describe how actors make sense of the world. The social analyst can remain entirely agnostic and does not need to develop 'analyst's categories.' The identity of STS promoted by Jasanoff and other relational theorists is characterised by analysis that is descriptive and retrospective in nature.

If the insights of relational STS approaches are used to inform the thinking about technological decision-making, as it is done on a regular basis by theorists such as Jasanoff and Wynne, the resulting models of technological decision-making can be described as belonging to 'technological populism.' The term has been introduced by Collins and Evans (2007) as an 'umbrella term' to mark a certain class of approaches to technological decision-making.

The most significant characteristic of technological populism is the absence of any boundaries with regard to decision-making rights. It describes extremely inclusive modes of technological decision-making which, in principle, treat all participants as equals. Under technological populism, technical expertise is not a prerequisite to make policy-relevant technical claims (Collins *et al.* 2010). In other words, actors without any experience are not only able to make legitimate claims on technical matters. What is more is that their chances of influencing technological decisions do not depend on the content or epistemic merit of their claims but *solely* on non-epistemic factors such as their political influence or the characteristics of the political system that structures decision-making. Relational approaches do not and cannot provide any rationales or criteria that can be used to inform the prescriptive drawing of boundaries. Its strength is deconstruction of rationales and criteria, not their re-construction. This leads inevitably to models of decision-making that do not include any epistemic distinctions between actors.

This is, however, only partly the case as contradictions between methodological assumptions and political prescriptions can be observed regularly. It can be argued that such contradictions represent a struggle between methodological commitments and common-sense. For example, Wynne (2007:

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explanations that were based on understanding. The aim of understanding is to explore how actors understand and make sense of the world.

108) states that 'public issues involving technical expertise' are predominantly matters for citizens to resolve, but then concedes that "where appropriate it should be informed by [specialist technical expertise]." Thus, it appears that Wynne recognises a separate or autonomous role for technical experts. For his suggestion to make sense, however, he has to be able to recognise specialist technical expertise, which suggests that he has to have a demarcation criterion. Unfortunately, Wynne fails to make this criterion or rationale transparent.

This contradiction of relational approaches becomes even more apparent when scholars draw up explicit models to describe technological decision-making. For example, Millstone's (2009) model, which is explicitly based on relativist insights of science studies, identifies a separate, though well connected, element of technological decision-making process that he calls 'expert risk assessment', which is supposed to contain 'mostly scientific considerations.' What Millstone also fails to provide, however, is a clear rationale for determining who should be allowed to legitimately participate in expert risk assessment.

It can only be speculated, as the details remain hidden, that scholars such as Wynne and Millstone actually rely on nominal definitions of expertise, which also inform technocratic models. While they push for increased public participation, the technical element of technological decision-making, *e.g.* the technical phase, remains firmly in the hand of those with formal expert credentials. If such a proposal is compared to SEE, it becomes clear that SEE is actually far more progressive. SEE also enables the extension of public participation with regard to the political aspects of technological decision-making. In addition, SEE redistributes expertise in societies: sheep farmers, farm workers and AIDS activists can be recognised as technical experts and granted access to the technical phase. This remains impossible in the context of policy-models such as proposed by Millstone and Wynne.

One of the most prominent proposals that reflects Wave 2 insights (and avoids the contradictions just mentioned) is Funtowicz and Ravetz's (1993) post-normal science model. With regard to the role of scientific experts in technological decision-making under post-normal conditions, Funtowicz and Ravetz (1993: 751) make the following claim:

> Only a dialogue between all sides, in which scientific expertise takes its place at the table with local and environmental concerns, can achieve creative solutions to such problems, which can then be implemented and enforced.

According to Funtowicz and Ravetz, scientific expertise represents just one valid viewpoint in post-normal decision-making. Other concerns and perspectives are equally valid.

While Funtowicz and Ravetz present 'post-normal' decision-making as just one of three different policy-relevant 'puzzle-solving' modes, which only applies under conditions of extreme uncertainty and/or extremely high decision-stakes, such subtleties appear to have been lost in subsequent years.[52] Evans and Collins (2007: 612-613), for example, have made the following observation:

> Although Funtowicz and Ravetz clearly intend such arrangements to apply only when either the uncertainty or the stakes associated with a decision are particularly high ... the STS perspective generalizes it to controversy more generally. As expertise is contested, so uncertainty is increased, and as concerns about the dangerous precedent that may be set are articulated, so a controversy can be made to move from the arena of professional consultancy to that of post-normal science. In this sense, the stakes and uncertainty implicated in a controversy are part of what the protagonists are trying to establish.

According to Evans and Collins, radical STS scholars are able to claim that any contested issue involves high uncertainties and/or high stakes. If such claims are accepted, post-normal technological decision-making becomes the 'normal' mode of technological decision-making that flows from a radical relational framework.[53]

Moreover, technological populism is the only form of technological decision-making that can escape the radical critique of relational approaches. As Jasanoff (2003a) points out, the main intellectual problem that grips critical STS scholars is to warn about the consequences of boundary-drawing by showing what is at stake. Jasanoff's 'critical STS scholars' are able to show that any boundary

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[52] The other two forms are 'applied science' and 'professional consultancy.'

[53] It is interesting to note that Funtowicz appears to be involved in the hidden radicalisation of the original 'post-normal science' proposal. Funtowicz, in collaboration with Liberatore (Liberatore and Funtowicz 2003b), discuss a number of 'democratic' technological decision-making models. They call the most radical model 'extended participation model', which appears to be very similar to the post-normal mode of decision-making proposed by Funtowicz and Ravetz 10 years earlier. Interestingly, Liberatore and Funtowicz do not mention extremely high uncertainty or decision-stakes in their discussion of the model: "Last but not least, the extended participation model argues that science is a crucial but not exclusive form of relevant knowledge, citizens are at the same time (while to different degrees) users, critics and producers of knowledge. A plurality of perspectives is considered as enhancing both procedural legitimacy (through inclusiveness) and quality of knowledge (through extended peer review)" (Liberatore and Funtowicz 2003b: 149).

produces certain social, political, ethical, economic (and so on) consequences. Thus, any technological decision-making model that includes some sort of boundary can be criticised for creating certain social consequences. This mode of critique makes it, however, impossible for 'critical STS scholars' to distinguish between boundaries that produce more or less desirable consequences. Moreover, a tacit assumption of those favouring such an approach appears to be that the lack of participatory boundaries produces no undesirable consequences.[54] Thus, the only technological decision-making mode that escapes the symmetrical critique of relational approaches is technological populism given that the approach eradicates all boundaries.

While this failure to provide a critique of technological populism is consistent with assumptions of relational approaches, it is of little value from a political point of view. This can be illustrated with the help of a thought experiment: Collins usually refers to an encounter he had with South African anthropologists in Boulder, Colorado, that motivated him to develop a realist theory of expertise. The anthropologists simply asked Collins how he, as a relativist STS scholar, would respond to claims by certain men in South Africa that having sex with virgin cures AIDS. This refers to a practice known as 'virgin cleansing' (*e.g.* Leclerc-Madlala 2002). Imagine now that political decision-makers hear about this and believe that the practice of virgin cleansing might be useful to reduce HIV/AIDS prevalence in South African men. This might lead to the creation of laws that encourage the practice of virgin cleansing, *e.g.* virgins might receive payments if they agree to sex with a HIV infected man. On the basis of Jasanoff's 'critical STS approach', no critique is warranted.[55] The decision to allow and embrace virgin cleansing as a solution to HIV/AIDS in

[54] This assumption is explicitly expressed by scholars such as Nelkin (1984), Fiorino (1990) Jasanoff (2003a) and Nowotny (2003). It will be discussed in the next section of this chapter in more detail.

[55] Of course, I am not suggesting that Jasanoff or anyone else would find such a decision acceptable. What this example highlights is that theories of technological decision-making that are informed by the relational understanding of scientific knowledge are simply not capable of providing a critique that is internal to its theory. This fictional course of events can be criticised on moral grounds, but these are not part of the relational conception of technological decision-making. It also begs the question of consistency: if this particular decision-making process can be criticised on moral grounds, then every decision-making process can be criticised in such a way. If not, does this not mean that the analyst brings her or his personal convictions to bear onto the analysis?

South Africa was not influenced by any boundaries between experts and non-experts. The decision was taken democratically and no one has been privileged in the decision-making process on epistemic grounds.

In sum, relational theories, if used to inform technological decision-making, lead to technological populism as this is the only form of technological decision-making that does not impose any boundaries. Everyone can actively participate in any aspect of a technological decision. Moreover, even if relativism is not turned into a political resource, it does not enable its proponents to criticise manifestations of technological populism. Since technological populism means universal participation due to the absence of any boundaries, there is nothing for critical relativists to criticise.

The problem of extension only becomes relevant if one does not want to live in a society where technological decisions can be made in a technological populist way. This is a political choice. To avoid technological populism and to be able to criticise it requires a response to the problem of extension. This means a rationale or criterion has to be put forward which helps to determine the extent of legitimate participation in technical decision-making. Relational approaches in STS, by concentrating on description and understanding, are unable to provide social analysts or anyone else with such a criterion and rationale. While such approaches might be methodologically sound and consistent and while they surely provide important insights, political action in general and technological decision-making in particular must not be based on relational thinking if one does not want to live in a society where anyone can make policy-relevant technical judgements no matter whether s/he does or does not know what s/he is talking about.

2.4 Understanding Misunderstanding: Reviving a Critical Debate

Given that Collins and Evans original paper (2002) was explicitly positioned as a 'discussion paper' by the editor of *Social Studies of Science*, the subsequent debate did not shed much light on the usefulness and validity of SEE arguments. Despite Collins and Evans' explicit statement that Wave 3 is continuous with Wave 2, the invited respondents interpreted the proposition of Wave 3 as an attack

on Wave 2 (Jasanoff 2003a, Rip 2003 and Wynne 2003). In particular, Jasanoff appears to believe that SEE pursues an anti-democratic agenda, while Wynne argued that Wave 3 marked a return to technocracy.

The exposition of the SEE-based 3W model in this thesis has not produced any indications that such an interpretation is justifiable. In fact, it has been pointed out that the 3W model differs markedly from technocracy. Moreover, by arguing that no restrictions in participation ought to apply to the political phase of technological decision-making, it is difficult to see how the 3W model could be anti-democratic. By revisiting the debate, it can be shown that the interpretation of the critics of SEE is predominantly based on a misunderstanding of the concerns of SEE. The purpose of Wave 3 is not to replace Wave 2, but to complement it to enable STS scholars to make normative contributions to technological decision-making processes, which might hopefully lead to better technological decisions.

As discussed earlier, the 3W model is informed by the belief that a refusal to draw any boundaries within technological decision-making, which supports the unlimited extension of public participation in technical decision-making, can lead to undesirable consequences. In other words, SEE believes that the radical democratisation of technological decision-making does not inevitably lead to good policies. It is this belief that directs Collins and Evans towards proposing a realist theory of expertise, which provides analysts' with a rationale to limit participation in technical decision-making, which is regarded as a small, but important aspect of technological decision-making.

The critics of SEE have, to varying degrees, failed to engage with this concern. Rip (2003) believes at least that the 'problem of extension' might really be a problem in the sense that more participation might indeed not always lead to 'good' policies. He then, however, claims that the 'problem of extension' is part of a larger problem:

> [It] is part of the dynamics of closure, of inclusion and exclusion, of interactions and opening-up, of arrangements that emerge and may be more or less productive. There may well be good reasons for having core-sets: social closure can have epistemic advantages. What such good reasons could be is a broader question than the problem of extension, and requires what I call socio-epistemic history of science. (Rip 2003: 423)

While it might be indeed the case that the problem of extension bears upon a larger socio-epistemic history of science, Rip fails to further engage with the specific point raised by the problem of extension. As shown, Collins and Evans frame the problem of extension as a practical political problem in the context of technological decision-making that, if left unaddressed, leads to or condones technological populism. While a socio-economic history of science might lead to interesting findings, it is difficult to see how such a research project can deal with the specific problem as set out by Collins and Evans.

Wynne (2003: 402) mentions the problem of extension, but instead of addressing the point, he asserts that the whole approach proposed by Collins and Evans is based on a "mistaken ... understanding of the 'problem of legitimacy'." Wynne argues specifically:

> Their implicit assumption is that the problem of public legitimacy for science is rooted in the way that people with authentic but unrecognized expertise are denied access to expert deliberations about such (propositional) questions as those stated earlier (for example, 'is UK beef safe?'). Their aim is therefore to have this different kind of practical and experience-based expertise recognized and admitted. I suggest instead that this multidimensional legitimacy problem is more about the institutional neglect of issues of public meaning, and the presumptive imposition of such meanings (and identities) on those publics and the public domain. Strikingly, with its exclusive basis in propositional issues, this same omission bedevils the Third Wave too. (Wynne 2003: 402)

Wynne explains the problem of extension away by incorporating it into an all-encompassing 'problem of legitimacy.' It is obvious from the quote, and also from his later responses to SEE, that he believes that the SEE framework reduces technological issues to technical issues (see also Wynne 2007, 2008). If this were the case, Wynne would have a point as this would lead to a technocratic mode of decision-making. It has been shown in the previous chapter, however, that Wynne's assertion does not reflect what is set out in the 'Third Wave' paper or in subsequent publications. Wynne's interpretation of SEE appears to be based on overlooking the explicit distinction between a technical phase and a political phase of technological decision-making.[56]

Jasanoff (2003a) also acknowledges Collins and Evans' concern relating to the problem of extension. She explicitly mentions that their proposal holds that

[56] Fischer's (2009) exposition of SEE suffers exactly from the same misinterpretation.

too much public involvement might have negative effects on policies. Again, however, Collins and Evans' concern is explained away, this time as a 'misunderstanding' of the "purpose of participation in contemporary democratic societies" (Jasanoff 2003a: 391). Jasanoff presents five arguments that, in her view, justify greater public involvement in 'technically grounded decision-making':

> First, it is worth remembering that the presumption in democratic societies is that all decisions should be as far as possible public; it is the exceptions that require justification. The American Freedom of Information Act, a crowning achievement of law in the Vietnam era, reflects just this sensibility. Second, public engagement is needed in order to test and contest the framing of the issues that experts are asked to resolve. Without such critical supervision, experts have often found themselves offering irrelevant advice on wrong or misguided questions. Third, as we have seen, expertise is constituted within institutions, and powerful institutions can perpetuate unjust and unfounded ways of looking at the world unless they are continually put before the gaze of laypersons who will declare when the emperor has no clothes. Fourth, participation is an instrument for holding expertise to cultural standards for establishing reliable public knowledge, standards that constitute a culture's distinctive civic epistemology. Finally, and this is a point that C&E [Collins and Evans] do seem to recognize at some level, participation can serve to disseminate closely held expertise more broadly, producing enhanced civic capacity and deeper, more reflective responses to modernity.

With regard to the fifth point, Jasanoff already recognises that there is no contradiction between her position and the position of the 3W model informed by SEE. It can be shown, however, that the four other points raised by Jasanoff's are also not in conflict with Wave 3.

First, Collins and Evans argue that addressing propositional questions of a technical and scientific nature constitutes an exception, because dealing with them requires specialist tacit knowledge that is not widely distributed within a society. To emphasise the exceptional status, Collins and Evans construct a narrow technical phase, in which participation is reserved for those with special understanding and skills. Apart from answering such propositional questions of a technical and scientific nature, all other aspects of technological decision-making are public.

Second, as will be shown in more detail in the next chapter, framing is defined by the 3W model as an activity that falls into the political phase. As

pointed out in the previous chapter, no participatory restrictions apply to activities of the political phase.

Third, given that the 3W model relies on a normative theory of expertise, political decision-makers are not forced to accept any attribution of expertise made by powerful institutions. In fact, such a normative theory, when utilised by members of the public, allows marginalised groups to contest attributions of expertise invoked by powerful actors.[57] Moreover, the normative theory of expertise might actually bring about the recognition of actors as experts who lack powerful institutional backing and formal credentials.

Fourth, actors participating in the political phase have ample opportunity to influence and scrutinise expert deliberations in various ways. Within the 3W framework, experts are only autonomous when it comes to technical decision-making and even here, the recognition of more heterogeneous experts afforded by SEE's theory of expertise could lead to more debate and scrutiny. I will show in the next chapter that these technical decisions do not determine policy.

Collins and Evans have explicitly pointed out that Wave 3 is built on an understanding of the nature of science that is informed by research done under Wave 2. The discussion in this section shows that there is actually no fundamental disagreement as the interpretation of Wave 3 by critics is based on a range of misunderstandings. Of course, there are differences between Wave 2 and Wave 3. Wave 2 is based on a relativist methodology. While it enables STS scholars to gain interesting insights into the nature of science such, it is less fruitful when applied to the context of technological decision-making. Wave 3 is a realist approach. As such it is less useful to gain new insights into the nature of science, but it enables STS scholars to use their specific expertise about the nature of science to make normative contributions to technological decision-making processes. Instead of being regarded as antagonistic, Wave 2 and Wave 3 ought to be regarded as complementary.

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[57] In a presentation at the 4S conference in Rotterdam in 2008, Kyle Powys Whyte has shown how the normative theory of expertise can help indigenous groups in North America to counter the interests of large corporation that want to exploit certain natural resources in their home lands. By insisting on environmental assessments conducted by 'proper' experts, they are able to contest the environmental impact assessments made by experts who work for the corporations.

2.5 Conclusions

In this chapter, it has been shown that to solve the problem of extension a Wave 3-compatible approach to technological decision-making, the 3W model, is needed. This is because approaches to technological decision-making that are compatible with Wave 1 and Wave 2 of science studies respectively are either inadequate or part of the problem of extension.

It has been shown that technocracy, which is compatible with a conception of science held by Wave 1, uses the demarcation criterion of 'scientific expertise' to deal with the problem of extension. It has, however, also been shown in the context of positivism, scientific expertise is understood as the knowledge of accredited scientists. The use of such a nominal conception of scientific expertise has been shown to lead to flawed solutions of the problem of extension as experienced non-scientists are excluded, while often too many scientists are included.

It has also been shown that the relativism that underpins Wave 2 creates the problem of extension. Relativist science studies are thus part of the problem, which renders it incapable of solving it. While Wave 2 has led to a better understanding of the nature of science and scientific knowledge, its particular methodological stance makes it unsuitable to contribute normatively to technological decision-making. If relativism is used to inform the conception of such decision-making processes, it leads to technological populism.

Wave 3 and the 3W model are built on the insights of Wave 2, which makes both approaches continuous. It is, however, the realism that underpins the SEE-specific conception of expertise that affords the use of some of these insights to inform technological decision-making without leading to technological populism. Both approaches complement each other. While the essentialism of the 3W model is useful to help analysts' to distinguish between legitimate and illegitimate participation in technical decision-making processes, it makes the critique and deconstruction of boundaries between experts and non-experts – one of the purposes of Wave 2 – impossible.

3. The Minimal Default Position

3.1 Introduction

In the previous two chapters I have outlined what the problem of extension is and why science studies needed a 'third wave' to deal with it. Solving the problem of extension is, however, only a first step in the development of the 3W model of technological decision-making that intends to be practically useful. To solve the problem of extension, it is vital to separate technical and political aspects of technological issues. Separation is vital to allow those with relevant technical expertises to deal with propositional questions of technical nature autonomously. The problem is that once a technical answer has been produced it has to feed back into the political phase. The way in which SEE defines and conceptualises technological decision-making puts limits on the way in which technical decisions can feed into technological decision-making processes. If one assumes, for example, that technical decisions exhaust the technological decision-making process, that is, technical decisions are technological decisions, the separation between technical and political aspects of technological decision-making is redundant. If that were the case it would be enough to declare any consensual claims produced by the technical experts to be 'technological policy.' At the same time, the separation between technical and political phase is equally superfluous if it is assumed that those in the political phase are in a position to ignore the outcomes produced in the technical phase. If that were the case, why go to such lengths in solving the problem of extension as well as ensuring that the 'right' actors are identified as experts, only then to ignore what the experts say?

To respond to such issues, Collins and Evans (2002, 2007) and Evans and Plows (2007) have laid out some vital groundwork. In particular, Collins and Evans (2002) have outlined the basis for the analytical distinction between political and technical phase, which has been introduced in chapter 1. Evans and Plows (2007) have specified the functions of both phases in technological decision-making processes, which will be introduced below. In this chapter, the

emerging 3W model has to be aligned with two fundamental principles, which are relevant for thinking about the relationship between technical and political phase as they rule out certain potential options.

First, Collins and Evans (2002) insist that only experts, as defined by SEE, have the right to make technical judgements. They emphasise that this always applies, no matter whether technical knowledge on which such judgements are based are regarded as certain or uncertain. This principle guarantees the autonomy of experts when it comes to answering propositional questions.

Second, Collins and Evans (2007: 138) also insist that policy-makers operating in the political phase must retain freedom of political choice. This principle has been restated in a recent paper by Collins *et al.* (2010: 193), which asserts that "political judgments always trump scientific judgments in technological decision-making processes." The second principle effectively means that the output of the technical phase – expert advice – must not determine the choice of policies.

The purpose of this chapter is to develop a prescriptive position which sets out how expert advice generated in the technical phase should impact on the political phase in which final technological decisions are made. I will outline how the technical and the political elements can be integrated into a hybrid concept of technological decision-making that affords overall decision-making power to the political phase, without making expert advice redundant.

3.2 Relationship between technical and political phase

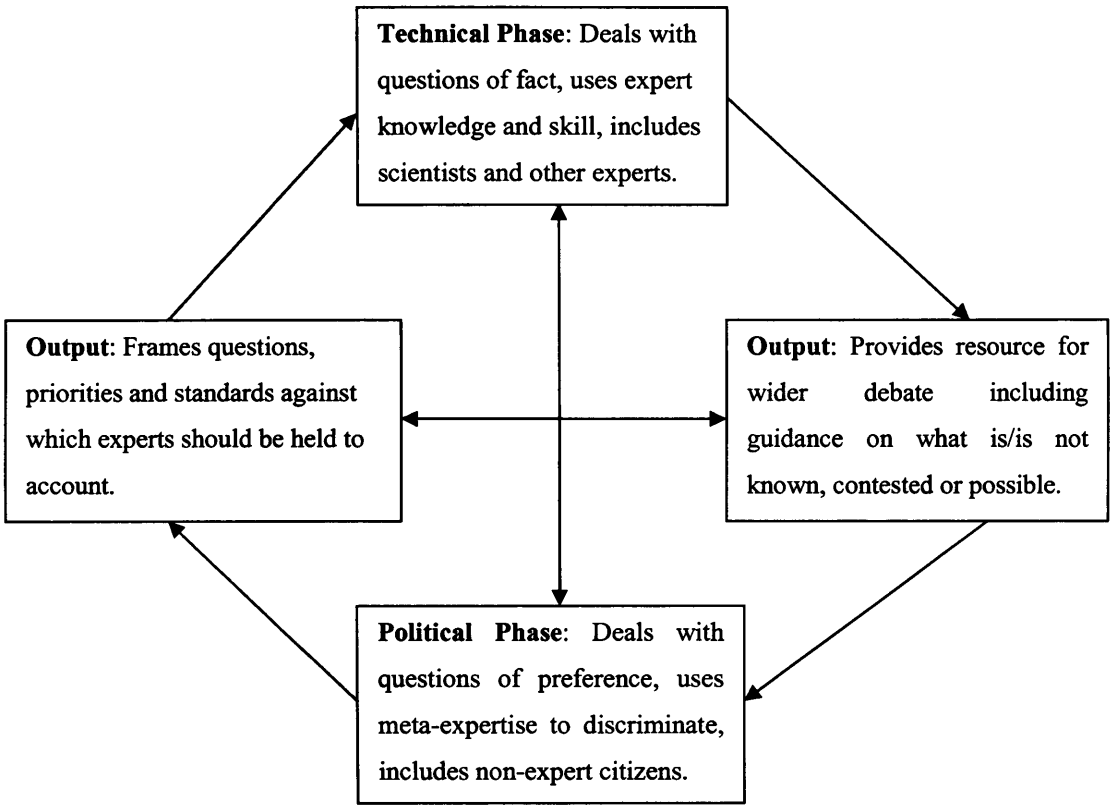
The question this chapter focuses upon is how the two phases of technological decision-making can be reintegrated in such a way that public input in technological decisions is maximised without compromising the quality of technical knowledge that is provided by the technical phase. Contributions to the 3W model have, so far, only superficially dealt with this question. As pointed out, Collins and Evans (2002, 2007) have stated that the technical phase must not dominate or determine the political phase. Important as such a position is, it does not explain how technical judgements are supposed to feed into political

judgements. Evans and Plows (2007) have been more specific. They point out that the two phases complement each other:

> Rather than thinking of two separate decisions, taken at two separate times by two separate groups, distinguishing between the technical and political phases highlights the two different, but to some extent complementary, approaches that need to be utilized if the relationship between science and society is to be one in which influence flows in both directions. (Evans and Plows 2007: 834)

They have tried to capture the relationship between the two phases in a model. Their attempt is depicted in Figure 2:

Figure 2: Relationships between technical and political phase



Source: Evans and Plows (2007: 835)

The complementary relationship between political and technical phase can be described as dialectical or circular. The output of one phase has repercussions on the other phase and *vice versa*. Since a description of the relationship has to start somewhere, it will start with activities in the political phase.

According to Evans and Plows (2007) a number of activities in the political phase might influence or shape activities in the technical phase. One is creating the rules and regulations that govern the overall process of technological decision-

making. This might include the setting of procedural rules for the technical phase, the definition of research priorities as well as the agreement on standards by which the experts are held accountable. Another one is the framing of the technological issues, which has implications for which aspects of an issue are referred to the technical phase.[58] Another important activity of the political phase is to agree on the narrow framing of the propositional questions that only experts are supposed to answer in the technical phase. Of course, this framing process requires input from experts as they might be more aware of what an answerable question might be, but the input from non-experts is important and desirable at this stage (*e.g.* Wynne 1989a, 2003, 2008, Guston 2001, Stilgoe *et al.* 2006).

The activities in the technical phase, in contrast, are very narrow as they do not go beyond providing answers to the propositional questions. The answers provided by experts might either feed into a technological decision-making or might make it necessary for those participating in the political phase to reformulate the question or to formulate new propositional questions. While Evans and Plows (2007) make important clarifications, they still do not set out how exactly answers to propositional questions should enter the political phase. What does it exactly mean to refer to expert knowledge as "a relevant factor" to be taken into account by the political phase (Evans and Plows 2007: 834)?

3.3 Minimal Default Position

The proposition that is made here will be called the 'Minimal Default Position' (MDP). After introducing it in more detail, the significance of the label will become clearer. The basic proposition is this: if experts provide a consensual answer to a propositional question with a sufficient degree of certainty, this

[58] In principle, the technical phase has to be regarded as being optional or latent. It only becomes relevant if the framing process identifies propositional questions that need to be answered. For example, the question whether to build a nuclear power station might be perceived as being a technological decision, because it involves scientific and technical aspects. But one can imagine an admittedly unlikely case in which all the participants of the political phase agree that no new nuclear power station should be built, say because they prefer decentralised energy solutions. In such a case propositional questions such as 'is nuclear power safe?', 'is it cost-effective?' and so on, do not arise and there is no need for expert input. As Collins *et al.* (2010) have pointed out, in a society in which science plays an important role, it would be desirable if as many propositional questions as possible would be formulated by citizens.

answer should, as a minimal requirement, have a constraining effect on the decision-making in the political phase.

There are a number of phrases such as 'consensual answer', 'sufficient degree of certainty' and 'constraining effect' in this proposition that require clarification before its full meaning becomes visible. The most important phrase in the minimal default position proposal is 'constraining effect.' The word 'constraining' is *not* meant in the sense that expert advice limits the policy-choices of those involved in the political phase of technological decision-making. The policy-choices itself remain unaffected by expert advice under the minimal default position. For example, even if experts unanimously agree that 'AZT is safe', not providing AZT for PMTCT remains a legitimate policy-choice just as providing the drug free or for a fee or postponing its introduction and so on remain legitimate policy-choices.

Instead, it is the *justifications* that policy-makers use to publicly legitimise their policy-choices that are to be constrained by consensual expert advice. It is worth noting that a particular policy-choice can be justified by a potentially infinite number of reasons. The minimal default position suggests that a sufficiently robust expert consensus on a propositional question takes away one or very few of the potentially infinite justifications from policy-makers. It is in this sense that policy-makers are 'constrained' by consensual expert advice.

For example, imagine that experts (in the wide sense of SEE, which might include farm workers) are fairly certain that a particular pesticide is safe for users as well as for consumers of agricultural products. According to the MDP, this expert assessment itself must not necessarily impact on the policy-choices of decision-makers. It might be decided that farmers can legitimately use the substance, but it is also feasible that legislation is approved, which forbids the use of the substance. Whatever policy-choice is elected, policy-makers must not misrepresent the expert assessment. If policy-makers legislate against the use of the pesticide, they must not justify their policy-choice by claiming that experts are uncertain about the issue of safety or that experts have even judged that the substance is too unsafe. Instead, they can justify such a decision by stating that despite the fact that experts have found the pesticide to be safe, they have decided

not to allow the use of the substance because they aspire to establish a purely organic agriculture [59]

Another key phrase in the proposal relates to the 'consensual' nature of expert advice. The minimal default position *has to* prescribe that experts provide policy-makers with consensual answers to propositional questions. This is due to a core prescription of SEE, which states that only those with relevant expertises should be allowed to actively participate in the technical phase which deals with propositional questions (Collins and Evans 2002, 2007, Collins *et al.* 2010). This prescription makes it inevitable that, within the SEE framework, those participating in the technical phase have to make a technical decision, one way or another.

This is a normative point which remains unaffected by empirical findings which suggest that experts often fail to agree on technical issues that are relevant for technological decision-making (*e.g.* Nelkin 1971, 1975, 1984 Collingridge and Reeve 1986, Jasanoff 1990).[60] It is not difficult to invent rules and institutional mechanisms that force those participating in the technical phase into a final decision that resembles a consensus. For example, Kantrowitz's (1967) idea of a science court represents an institutional mechanism which forces a consensus in the form of a final judgement. It is also possible to create rules that require expert

[59] While I will discuss the issue in the conclusion in more detail, it should be pointed out here that such a proposal makes it especially important that policy-choices are subjected to a proper debate, preferably in all-inclusive, democratically governed arenas. Collins *et al.* (2010) have stated that SEE prefers a political system that puts special emphasis on deliberation. Theorists such as Bohmann (1996) and Dryzek (2000) and many others have emphasised the importance of deliberation for democratic decision-making. While SEE has so far not taken a great interest in the political aspects of technological decision-making, it should not be difficult to work out in greater detail how decision-making in the political phase can be structured to meet certain democratic requirements. This is, however, not the subject of this thesis. Collins *et al.* (2010) expect a reply to their paper by a number of scholars in the pages of *Critical Policy Studies*, which will hopefully lead to a lasting and fruitful debate.

[60] It needs to be remembered here that technical judgements in the technical phase do not constitute scientific judgements as such but judgements for a practical purpose, *e.g.* judgements that are policy-relevant. Scientific judgements are about 'truth', at least in aspiration. Policy-relevant judgements are about the 'best course of action given contemporary knowledge.' For example, Andrew Wakefield, who started the controversy about the safety of MMR vaccines subscribed to the scientific judgement that the MMR vaccine might be causally implicated in the development of autism in children. In 1998, his judgement for practical purposes about the use for MMR should have differed from his scientific judgement. This is because there simply was not enough evidence to support his claim that MMR might be harmful. As he and his co-authors stated quite clearly in the infamous Lancet paper, more research was needed to verify their 'scientific' suspicion that the MMR vaccine was not safe enough (Wakefield *et al.* 1998).

committees or other institutions that generate expert advice to put forward a singular position with regard to a technical judgement. For example, decisions can be made with a simple majority and in the case of parity the chairperson is given the decisive vote.

While forcing experts to provide 'consensual answers' is possible, it is impossible to force experts to be 'certain' about them.[61] Thus, experts might agree on a propositional problem, but they might be very uncertain about the correctness of their agreement. In a philosophical sense, as exemplified by the problem of induction, it might be stated that experts can never be certain. In practice, experts, like anybody else, can distinguish between different levels of certainty. For example, in 2010 experts are very certain that HIV causes AIDS. This was not necessarily the case in 1984 or 1985 when only a handful of virological and epidemiological studies supported the view that AIDS was caused by a virus. While most experts supported the HIV theory, the certainty of this belief was low and it might have taken only a couple of studies in the mid-1980s to shed doubt on the HIV theory. Since then numerous other pieces of evidence from many different disciplines seem to have confirmed the virus theory (*e.g.* Epstein 1996, Iliffe 2006, Whiteside 2008, National Institute of Allergy and Infectious Diseases 2010). It would require a massive research effort today to shed any doubt at all on the HIV paradigm, let alone to overturn it.[62]

To distinguish between different levels of certainty, a scale has to be constructed that enables experts to rate the certainty of the knowledge that informs a technical consensus. Scales of certainty are widely used in various contexts and it is not critical for the purpose set out here to create a new scale.[63] What is

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[61] Again, the comments related to 'certainty' are normative and are thus unaffected by empirical findings that suggest that experts often overstate the certainty of their claims or fail to make uncertainties transparent (*e.g.* Wynne 1989a, Rampton and Stauber 2002). In a response to such tendencies, Jasanoff (2003b) explicitly demands 'epistemic humility' from experts (see also Nowotny 2003 and Stilgoe *et al.* 2006). In the long run, however, misrepresenting the certainty or uncertainty of scientific facts and issues undermines the credibility of scientific advice in particular and of science in general.

[62] Collins (2004a) introduces the metaphors 'dam of certainty' and 'reservoir of doubt' to illustrate how certain individual facts are part of a larger network of facts.

[63] Scales of certainty are, for example, used in the context of regulating drugs. The Food and Drug Administration (FDA), for example, operates with a scale of five categories. The imitation games, that can be used to experimentally measure interactional expertise, use a four category scale. Bijker, Hendriks and Bal (2009) distinguish four types of uncertainty.

crucial is that those involved in the political phase of technological decisions choose a particular scale of certainty in the beginning of such decision-making processes. They also have to make a decision regarding a cut-off point, which effectively defines which consensuses count as 'certain' and which consensuses count as 'uncertain.'[64] While those consensuses that are deemed to be 'certain' trigger the minimal default position, those which are deemed to be uncertain should have different consequences.[65] Given that the relative high degree of uncertainty effectively means that experts do not know, technological decisions, if they have to be taken, should not be justified with any reference to technical knowledge by policy-makers. For example, if experts agree that genetically modified organisms (GMOs) are safe for human consumption, but are too uncertain about it, a decision to introduce GMOs must not be justified with the claim that they are safe for human consumption. Likewise, a negative decision regarding their introduction must not be justified by claiming that they are unsafe.

In sum, the minimal default position ensures that policy-makers cannot ignore 'certain consensual expert advice.' Such advice does, however, only affect the *justification* of policy-choices. The proposal is designed in a way, however, that allows policy-makers to retain maximum freedom with regard to final policy-choices. I call it 'Minimal Default Position' because it outlines the minimal requirements for those participating in the political phase with regard of taking expert advice into account. Given that policy-makers retain maximum freedom of choice, they might simply decide to delegate political power to those in the technical phase. This is common practice, for example, with regard to licensing of drugs. Thus, expert judgements will be translated directly into final policies. While this would formally resemble a technocratic mode of technological decision-making, this is not forced upon political actors by the underlying

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[64] The cut-off point in the context of technological decision-making might be defined on a case-by-case basis or a universal cut-off point might be defined. In any event, defining a cut-off point has to be done in the political phase of technological decision-making before a technical decision is made.

[65] It has to be pointed out that these specific prescriptions are theoretically motivated. Technical experts have to provide a consensus to retain control over making technical judgements. Given that 'uncertain consensuses' should have no impact on justifying policy-choices, it also practically feasible to allow experts to 'agree to disagree', if they are too uncertain about knowledge to agree on a singular position. Technical experts could provide 'split reports' to policy-makers that sets out different positions. In practice, this would have the same effect as 'uncertain consensuses.'

decision-making model informed by SEE; giving up legitimate political power would be a political decision.

3.4 What's Different? Comparing the Minimal Default Position

Analogous to the discussion about the problem of extension, the way in which SEE relates technical expert judgements to political judgements in the context of technological decision-making can again be fruitfully compared with technocratic and technological populist approaches in this chapter. This time, the question is not about the extent of legitimate participation in the technical phase, but about the construction of the relationship between technical and political components. The comparative analysis is guided by two specific questions that bring the differences between the three models to the fore:

1. Are policy-makers able to choose policies that go against consensual expert advice?
2. Are policy-makers able to justify policy-choices in ways that contradict consensual expert advice?

With regard to the Minimal Default Position (MDP), the first question can be answered with a 'yes,' while the second can be answered with a 'no.' The proposal grants maximal decision-making freedom to those in the political phase by ensuring that expert advice does not affect actual political choices. At the same time, the MDP does not allow those participating in the political phase to justify policy-choices entirely independent of consensual expert advice. As shown, consensual expert advice always impacts on the justification of policy-choices, although it does so in different ways depending on the certainty threshold. If expert advice is certain, justifications used by policy-makers must not contradict the advice offered by experts. If expert advice is too uncertain according to agreed standards, however, it should not be used as a justification for policy-choices in any case.

The aim of the following two sections is to provide answers to the questions of 'choice' and 'justification' in the context of technocracy and technological populism.

3.5 Technocracy and Political Decision-Making

The approach that has been introduced as Technocracy 2 assumes a role for scientifically untrained politicians in technological decision-making processes.[66] The division of labour between experts and politicians is based on what Krimsky (1984) has called 'separability principle.' This principle holds that a clear distinction can be made between a policy component of a technological decision, which deals with value determinations and a technical or scientific component that deals with facts only.

Given that technical and political components are separated, technocratic models face a similar challenge as the 3W model: technical judgements have to feed into political decisions. Despite the structural similarities between technocracy and SEE, there are significant differences between the two models that lead to differences in the way technical judgements are related to political judgements. In particular, decisionist models subscribe to a dichotomy between facts and values. It is believed that expert advice takes the form of true and politically neutral factual knowledge, which therefore represents the best possible solution to a policy-problem, politics is limited to 'value determinations':

> According to the separability principle, the policy component of the issue contains all the value determinations. Are the risks worth the benefits? Is the decision just? What ought the distribution of risks and benefits be? What social resources should be allocated? (Krimsky 1984: 247)

This gives policy-makers some autonomy and influence on final technological decisions. These value determinations can, however, only be applied to the factual expert judgements they receive. In this sense, policy-makers are constrained with regard to the choices of policies. According to Fischer (1990: 18), within an ideal-type technocratic society those who make political decisions "would justify themselves by appeal only to technical expertise grounded in scientific forms of knowledge." In other words, while policy-makers might add some 'value determinations' to the technical advice they receive, the fundamental parameters of technological decisions are set by experts (Krimsky 1984).

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[66] The two questions are meaningless in the context of Technocracy 1, because scientific experts are the policy-makers.

Traditionally, technocratic models suggest that experts present policy-makers with a technical consensus or a single best solution to a policy-problem. Kantrowitz's (1967) model of a science court represents such an approach. The science court proposal is interesting as it has a similar starting point as the 3W model. The problem that occupies Kantrowitz is how good technological decision can be made when the policy-relevant technical knowledge is still controversial within the scientific community. Kantrowitz's way of tackling this problem is proposing a science court which is staffed by scientists and technical experts. The science court is conceived as a quasi-judicial institution that allows the production of consensual technical judgements or expert advice. Active scientists fulfil the role of advocates, while it is the task of mature scientists with diverse scientific backgrounds to act as judges.[67] The consensual expert advice generated by the science court then feeds into the political decision.

It is telling that Kantrowitz does no more than hint as to how exactly consensual science court judgements should feed into technological decision-making. A deterministic relationship between scientific knowledge and political decision-making appears to be obvious and unproblematic to him. For example, at one point Kantrowitz (1967: 763) makes the following claim:

> It has occasionally been maintained that scientific and non-scientific components of a mixed decision are generally inseparable. It is, of course, true that a final political decision cannot be separated from scientific information on which it must be based.

There is no attempt to justify why policy decisions 'must' be based on scientific information. Given that Kantrowitz regards scientific knowledge as true and

[67] Interestingly, Kantrowitz is aware of a fundamental problem: if scientists stop working as scientists, they lose their expertise as the scientific field develops further. His solution is to claim that 'scientific expertise' can be maintained through the activity of judging itself: "if a mature scientist is deeply involved in finding the truth between the claims and counterclaims of sophisticated advocates, his education will be continuously improved by the advocates and his thinking will be continuously stretched in the effort to reach wise judgments" (Kantrowitz 1967: 764). This supposed solution can be critiqued by pointing to the short-comings of the nominal concept of expertise used by Kantrowitz. For Kantrowitz, scientific expertise appears to be a monolithic thing. Taking into account that judges should come from a different scientific background than advocates, he essentially argues that a nuclear physicist can maintain her expertise by judging cases in molecular biology. From a SEE point of view, this does not make sense. With SEE, one might argue that a nuclear physicist might over time acquire interactional expertise in molecular biology if s/he is involved in enough cases, but the expertise in nuclear physics will be inevitably diminished. This is because the nuclear physicist who becomes judge at a science court loses contact with her or his field and has no longer access to the tacit knowledge of the domain.

objective, it can be presumed that he could simply not think of any objection with regard to the political influence of scientific knowledge. Another hint by Kantrowitz appears to confirm the presumption. Kantrowitz (1967: 764) justifies his demand that judgements issued by science courts should be published with the following statement:

> [I]t would provide the whole political community with a statement of scientific facts as currently seen by unbiased judges after a process in which opposing points of view have been heard and cross-examined. Hopefully, these opinions would acquire sufficient presumptive validity to provide an improved base on which political decisions could be reached.

It appears to be the case that under the model proposed by Kantrowitz, scientific facts are regarded as the firm and valid basis upon which policy-decisions can be taken. The scientific advice effectively determines policy-choices and thus provides ready-made justifications.

In contemporary literature, another type of policy model that is based upon decisionist assumptions can be found. This modified model departs from the assumption of a 'single best solution'; instead, policy-makers are provided with a range of options by scientific experts. An example of such an approach is implicit in Pielke, Jr.'s (2007) conceptualisation of scientists as 'honest brokers of policy alternatives.'[68]

According to Pielke, Jr. (2007) scientifically untrained policy-makers are responsible for making decision as they are accountable to the electorate. Scientists, who take on the role of 'honest brokers,' therefore are not supposed to interfere into actual decision-making processes. Nonetheless, they play a key role as it falls to them to provide policy-makers with a comprehensive and 'honest' overview of options and choices with regard to technological issues. Presented with a range of feasible choices, policy-makers can then narrow them down according to their political preferences and interests.

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[68] The role of the 'honest broker' is just one out of four ideal type-like roles for experts in the political decision-making process. Apart from the honest broker, Pielke, Jr. (2007) also discusses roles such as 'the pure scientist', 'the science arbiter', and 'the issue advocate.' The pure scientist provides policy-makers simply with fundamental scientific information and stays entirely clear of any decision making. The 'science arbiter' functions as a neutral adviser and is supposed to answer all those technical questions that policy-makers might come up with, but again does not get involved in the decision-making process. 'Issue advocates' are experts who have a particular political solution in mind and try to convince policy-makers to adapt it by means of technical arguments. Pielke, Jr., however, commends the role of the honest broker to scientists.

While this model suggests that policy-makers have some choice, these choices themselves are predetermined by experts. There is nothing in Pielke Jr.'s proposal that suggests that policy-makers can come up with their own ideas if they do not agree with any of the choices presented to them. Again, the technocratic pattern with regard to the questions of choice and justification shines through: expert advice ultimately determines both policy choices and their justifications, which leaves no room for disagreement between experts and policy-makers with regard to choices or justifications.

3.6 Technological Populism and Technological Decisions

As pointed out in the previous chapter, technological populism is a decision-making mode that does not involve meaningful boundaries. Technological decision-making is conceptualised as a domain of actors with equal rights; 'technical experts' as far as they are recognised have the same influence on technological policy-making as any other actor no matter what the issue is. Any form of technological decision-making that affords equal decision-making rights to experts and non-experts with respect to technical decision-making is treated as a manifestation of technological populism.

The crucial point is that technological populism removes the problem, which the minimal default position tries to address. Given that no meaningful demarcations affect technological decision-making under technological populism, there is no separate expert advice that has to be integrated into a broader decision-making process. Instead, technological decision-making is regarded as an inclusive and shared process where, to paraphrase Funtowicz and Ravetz (1993), all actors take their seat on the same decision-making 'table.' Such a design of technological decision-making does not and cannot guarantee that the knowledge of nominal experts, as far as they are recognised as such, has any influence on technological decisions. The diverse 'collective of policy-makers' has no obligation to attribute any more weight to the views of experts than they have with regard to the views of any other legitimate participant. It seems that radically inclusive forms of technological decision-making are governed by democratic norms that afford the same influence to any participant. Thus, unless an expert

can convince enough participants of technological decision-making process that her or his view on a technical issue is correct, the majority might simply ignore the 'expert advice.' This means that there is no deterministic relationship between expert advice and policy-choices. Policy-makers are free to justify their choices in whichever way they agree on, which might include justifications that contradict expert advice.

In their introduction to a special issue on 'democratising expertise', Liberatore and Funtowicz (2003b) discuss a range of contemporary technological decision-making models in STS. They distinguish between a 'modern model', a 'demarcation model' a 'precautionary model', a 'framing model' as well as an 'extended peer review model.' Only the last of the five mentioned models, the 'extended peer review model', can be classified as representing technological populism.[69] The model does not explicitly distinguish between technical and political decision-components. Moreover, it advocates a 'pluralist position' with regard to knowledge inputs. In contrast to technocratic models in which expert knowledge determines policy-making, the extended peer review model grants a multitude of actors representing different knowledges the opportunity to influence technological decisions:

> [S]cience is a crucial but not exclusive form of relevant knowledge, citizens are at the same time (while to different degrees) users, critics and producers of knowledge. A plurality of perspectives is considered as enhancing both procedural legitimacy (through inclusiveness) and quality of knowledge (through extended peer review) (Liberatore and Funtowicz 2003b: 149).

Since no distinction between technical and political aspects of technological decisions is made, all actors – experts and citizens alike – share the same decision-

[69] Liberatore and Funtowicz (2003b: 148-149) summarise the first four models as follows: "We identify here several conceptual models of interaction between science, expertise and policy. According to the modern model, scientific facts, seen as unproblematic, determine correct policy. An example of such model is the idea that 'truth speaks to power'. In the precautionary model it is acknowledged that scientific uncertainty involves the need to consider other elements and types of knowledge, for instance, whether the possibility of irreversibility of health, ecological, or other processes needs to be dealt with by means of precautionary measures while better knowledge is achieved, or in case uncertainties persist. In the demarcation model, policy-making is the responsibility of democratic institutions, and science and expertise provide the facts to underpin decisions. Such demarcation protects science and expertise from political interference, and accountability is the exclusive concern, and asset, of policy makers. The framing model suggests that, in policy debates, problems for scientific investigation and for expert advice are set by stakeholders, thus the evidence produced by science and expertise is shaped by policy commitments."

making space. Scientific experts possess just one form of relevant knowledge that can influence technological decision-making. While Liberatore and Funtowicz (2003b) do not explicitly mention how technological decisions are taken, it can be inferred from the similar 'post-normal science' approach that inclusive deliberations amongst all actors should lead to consensual decision-making (Funtowicz and Ravetz 1993).

Liberatore and Funtowicz (2003b), who seem to prefer the extended peer review model over the other models for its democratic potential, describe scientific knowledge as 'crucial but not exclusive form of relevant knowledge.' Their overall concern is with the accountability and quality of technical knowledge. While they do not actively advocate technological populism, the extended peer review model is susceptible to it. One problem with the approach is that it is hard to see how it deals with significant disagreements over knowledge issues as is the case in the South African PMTCT controversy. With everyone sharing the same 'decision-space' and no one granted any obvious authority on factual matters, it is hard to see why those with political power could not simply reject the supposedly 'crucial' scientific advice of certified experts and make technological decisions on the basis of alternative considerations such as concerns over identity or lay concerns over side effects of antiretroviral drugs. Thus, while the motives of those advocating the extended peer review model might be 'progressive' or 'good', the 'side-effects' of conflating technical and political aspects of technological decisions and extending participation without recourse to a (modified) concept of expertise can be grave. There is nothing in such a model that compels policy-makers not to ignore or contradict expert advice as the latter is just one of many, possibly competing forms of legitimate knowledge. This also means that there is nothing in such model that would compel policy-makers to act upon expert advice. In sum, a decision-making model that represents technological populism allows policy-makers, in principle at least, to ignore consensual expert advice and to opt for privileging other forms of knowledge. This also means that consensual expert advice cannot determine policy-choices.

3.7 Conclusions

The questions that have guided the comparison between the Minimal Default Position, Technocracy and Technological Populism are the following:

1. Are policy-makers able to choose policies independent of consensual expert advice?
2. Are policy-makers able to justify policy-choices in ways that contradict consensual expert advice?

The discussion in the previous two sections makes it possible to summarise the respective answers to the questions of choice and justification in Table 1:

Table 1: Questions of Choice and Justification

	Question of 'Choice'	Question of 'Justification'
Minimal Default Position	YES	NO
Technocracy	NO	NO
Technological Populism	YES	YES

Table 1 shows that the three different approaches to technological decision-making structure the relationship between expert advice and final policy-making in different ways. Under technocracy, policy-makers have very little autonomy and political influence. Expert advice more or less determines the policy choices. Policy-makers might make some value determinations but they cannot change the factual basis that informs policy-choices. Given that policy-choices are determined by expert advice, the latter automatically provides policy-makers with justifications for their choices.

With regard to the two aspects focussed on, technological populism represents the polar opposite of technocracy. Policy-makers are completely autonomous in relation to expert advice. Whether such advice is taken into account or not is context dependent and cannot be predicted. Similarly, policy-makers' justifications for their policy-choices might be consistent with expert advice, but they might not be, which means that contradicting expert advice on technical matters is a possibility.

The exact relationship between expert advice and final policy-making has not previously been worked out in the context of the 3W model. Two important

prescriptions of SEE structure the task: first, experts must be in charge of technical decision-making, which is understood as small but important part of technological decision-making. This conforms to a central cultural preference that underpins SEE:

> There are those who would not accept that scientists have any special rights even in these esoteric matters, but here we must simply state our starting point that, as members of the scientific community broadly conceived, and contributors to Western scientific society, 'we think they do'. (Collins and Evans 2002: 243)

Second, political judgements trump technical judgements with regard to final political decisions. This expresses a political preference for technological decision-making that is organised in non-technocratic ways. The solution offered by the MDP is to link expert advice exclusively to justifications used by policy-makers to account for their policy-choices, which are unconstrained by expert advice.

The MDP ensures that experts provide policy-makers with the best possible advice that can be obtained. Firstly, experts are forced to agree on the best possible advice given contemporary information. This can be done by introducing certain rules or institutional mechanisms such as obligatory votes or majority decisions in expert committees or by organising experts in institutions such as a 'science court' which ensures that a final verdict on technical aspects is produced. Second, experts can make individual certainty judgements. I have pointed out that expert advice should impact differently on public justifications for policy-choices depending on the certainty attributed to an expert consensus. If, for example, experts agree that AZT is safe for the purpose of PMTCT and are certain about it, then policy-makers cannot justify what could otherwise be a legitimate decision to not provide the drug to those who need it by claiming that it is unsafe. If experts agree that another drug is safe, but they are very uncertain about it, then policy-makers should refrain from using expert advice for justificatory purposes.

As with any formal rules, they can be circumvented. While the rules of the MDP can be, in principle, implemented in any political system, they require a certain political culture or environment to function in practice. As Collins *et al.* (2010) have pointed out, a proposal such as the MDP requires a political culture or environment that values and supports inclusive and transparent debate about

technological policy-choices *before* technological decisions are made. This entails, for example, that as many technical propositional questions as possible are asked and referred to the technical phase. This also means that both expert advice and policy-makers' justifications for policy-choices have to be made public so that citizens are not only informed about the state of the technical discourse, but that they are also able to check whether policy-makers misrepresent expert advice.

4 The Context of the South African Policy-Making About PMTCT

4.1 Introduction

Before the actual South African case study is set out, some contextual information is provided to allow readers to situate the case and to understand some of its particular features, such as the extent of the problem of MTCT in South Africa, the potential options open to policy-makers as well as the dominant roles of President Thabo Mbeki and Minister of Health Tshabalala-Msimang.

The chapter is organised in the following way. The first section provides a short history of HIV/AIDS policy-making in South Africa until 1999. The purpose is to provide some basic understanding of how the epidemic developed and how policy-makers acted. As will be shown, direct treatment of HIV/AIDS was not considered to be an option until 1997 when a range of developments contributed to make antiretroviral treatment at least a topic for political debate.

The next section focuses explicitly on the issue of paediatric HIV/AIDS and MTCT. More epidemiological background is provided to explain why MTCT became an important issue in the late 1990s. As will be shown, this is mainly linked to the rapid increase of HIV infections amongst women of childbearing age during the mid-1990s.[70]

In the next section possible technical responses to the problem of MTCT are discussed. It will be shown that short-term use of antiretroviral drugs represented the only viable option for the South African government if it wanted to address the problem of MTCT directly.[71]

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[70] The prevalence of a disease does not automatically lead to a response by policy-makers as can be seen with regard to the lacklustre response to Malaria or Tuberculosis in many countries. It is clear that MTCT has more than an epidemiological dimension to it. MTCT raises moral, social, and economic concerns that have led a variety of actors to take the issue seriously. It is, however, doubtful that MTCT would have received the attention that it has received in South Africa if there were only a few hundred cases per year. From this point of view, the epidemiological underpinnings of the issue are worth pointing out.

[71] As will also be shown, there are also indirect ways to respond to MTCT, such as reducing the number of unwanted pregnancies or reducing the prevalence of HIV/AIDS in the adult population.

The final section focuses on the distribution of political power in South Africa. It is intended to explain why the analysis of the controversy focuses so narrowly on Thabo Mbeki. Given that South Africa is a democracy the narrow focus on Mbeki can indeed be questioned. As will be shown, however, the position of Mbeki as Head of State, leader of the government and leader of the dominant political party enables him to 'impress his will' on the political discourse.

4.2 Aspects of South Africa's AIDS Policy until 1999

AIDS policy in South Africa has been controversial to varying degrees ever since the first HIV infection of a South African citizen was detected in 1982.[72] A substantial literature exists which deals with the history and developments of AIDS policy in South Africa from 1982 onwards.[73] Here, a few paragraphs are provided that summarise the policy efforts related to the treatment of HIV/AIDS between early 1997, when the government put its faith into an unproven compound named Virodene and late 1999, when President Mbeki made his controversial remarks about the safety of AZT.

Before the focus turns to policy-making, a brief description of important features of the South African AIDS epidemic is provided.

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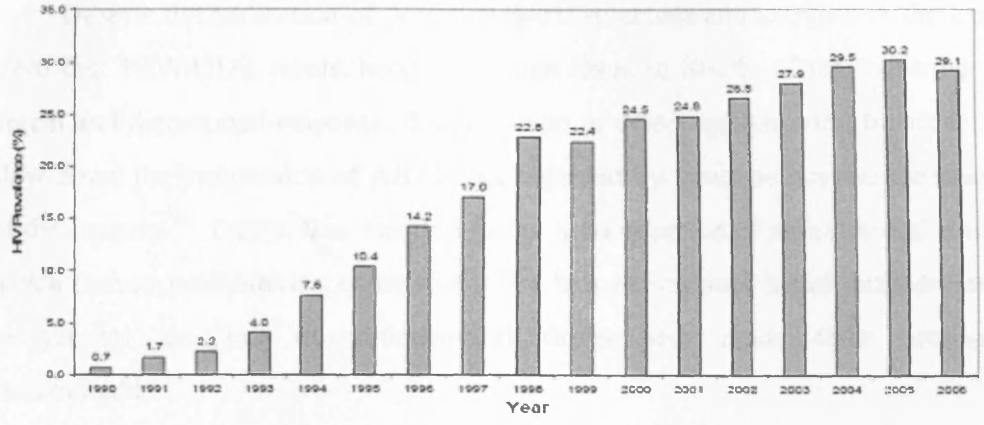
[72] Of course, the name HIV was not used in 1982. Until 1986, when the term HIV was adopted, the virus was known as HTLV-III (human T-lymphotropic virus) or LAV (lymphadenopathy-associated virus).

[73] For general overviews stretching back to 1982, see for example Van der Vliet (2001), Weinel (2005b) and Nattrass (2007). See Grundlingh (2001) and Christie (1991) for assessments of apartheid-era policy responses to HIV/AIDS. Most authors, however, concentrate on AIDS policy in post-apartheid South Africa: Schneider (1998, 2002), Marais (2000), Garbus (2003), Mbali (2003) and Coovadia and Coovadia (2008) provide useful overviews. Quinlan and Willan (2005) and Nattrass (2006a,b) focus specifically on the policies that deal with the provision of antiretroviral drugs. Comparative perspectives of South Africa's policy development with other countries are provided by Gauri and Lieberman (2004), Parkhurst and Lush (2004), Nattrass (2006c), Von Soest, Calcagnotto and Weinel (2006) and Youde (2007). Strode and Barrett Grant (2004) analyse the institutional response to the South African HIV/AIDS epidemic. Schneider and Stein (2001) look specifically at the level of implementation as contrasted to the level of policy formulation. Diverse analyses aimed at understanding and explaining South Africa's policy response, especially the period of AIDS denial after 1999, are attempted by Mbali (2002), Schneider and Fassin (2002), Heywood (2003), Nattrass (2004) Butler (2005), Van Rijn (2006), Youde (2006), Myburgh (2007, 2009), Weinel (2007), Wang (2008). Both Sheckels (2004) and Paroske (2006) analyse the rhetoric of Mbeki's AIDS policy.

Figure 3: HIV prevalence rate among antenatal clinic attendees (1990-2006)



Source: Department of Health (2000)

Until 2002, when a comprehensive household survey on the issue of HIV/AIDS was conducted for the first time, the best available data about HIV/AIDS in South Africa were obtained in state-run antenatal clinics. Figure 3 shows the estimated prevalence of HIV infections amongst pregnant women visiting public sector antenatal clinics. These figures show the dramatic arrival of HIV/AIDS in South Africa during the 1990s. While the infection rate stood at less than 1% in 1990, when the survey was first conducted, prevalence rates amongst antenatal clinic attendees has virtually exploded in subsequent years. In 1999, when the controversy about the safety of AZT started, the prevalence rate was 22.4%, while it appeared to have peaked in 2005, the only year to date at which the 30% mark has been exceeded.

These figures must not be confused with the HIV prevalence amongst the total population of South Africa, which is significantly lower. It maps, however, the general trend in South Africa. HIV/AIDS was virtually non-existent in the early 1990. A decade later, the country was believed to have the biggest HIV/AIDS epidemic in the World. In 1999, estimates suggest that between 3.5 and 4.5 million South Africans were living with HIV/AIDS, which means that about 10% of the total population were infected. For the same year, it was estimated that possibly as many as 500,000 new infections would fuel the

epidemic (Marais 2000, UNAIDS 2000, Grimwood *et al.* 2000, Dorrington *et al.* 2002, 2004, 2006).[74]

Despite the realisation of policy-makers, scientists and activists in the early 1990 that HIV/AIDS would become a huge issue in South Africa requiring an urgent and determined response, the provision of direct antiretroviral treatment to slow down the progression of AIDS was regarded by many as beyond the means of the country.[75] Partly, this was due to the market prices of antiretroviral drugs, which proved prohibitively expensive. The lack of required health infrastructure to support the use of antiretroviral drugs also made their provision unsustainable.[76]

During the second half of the 1990s, when the epidemic was taking shape, its size was becoming visible and the prospect of witnessing millions of AIDS deaths in the not so distant future became a reality, the issue of treatment slowly started to appear on the political agenda. Three seemingly separate developments are indicative for this trend.

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[74] The epidemic has entered a 'mature phase' in recent years, which means that prevalence levels have stabilised (Doherty *et al.* 2006, Iliffe 2006). This trend does not, however, signal a success in the fight against HIV/AIDS. Maturation of South African epidemic is mainly due to a dramatic rise in deaths amongst people living with HIV/AIDS. The stability of the epidemic is therefore due to the balance between new infections and deaths attributable to HIV/AIDS. To see a significant decline in the prevalence rates of HIV/AIDS in South Africa, the government not only needs to address the issue of new infections, but also has to ensure that those who already live with HIV/AIDS have access to treatment that has become more effective and affordable over recent years. While the signs at the end of the first decade of the 21st century are encouraging as a firm commitment to the provision of treatment is in place for a number of years and implementation, although slower than planned, is progressing, the South African government had, for many years, a rather ambiguous position with regard to treatment of HIV/AIDS.

[75] The adoption of a six-year *National AIDS Plan* in 1994 is a strong indicator for such a widespread consensus (*e.g.* Schneider 1998, Marais 2000, Van der Vliet 2001). Work on the strategy that informed the plan started in late 1991 and despite the political turmoil that threatened peace in South Africa, the National Plan was developed in co-operative fashion and relied on the input of a diverse set of actors. According to Marais (2000:12), "the Plan was not managed by a government department, but by a coalition of forces largely outside government." While the plan was comprehensive, the provision of antiretroviral drugs was not considered to be a viable option.

[76] The cost argument made only sense before the background of a commitment to acquire antiretroviral drugs on the global market dominated by those pharmaceutical companies that possessed the patents of the drugs. In 1999, a year-long supply of the, by then, outdated mono-therapy using AZT cost about $10,000, which was unaffordable for the vast majority of South Africans, many of who lived in dire economic poverty. The example of Brazil, however, demonstrates other strategies were also possible. Brazil introduced universal long-term treatment for HIV/AIDS free of cost to all those who required it in 1996. This was possible due to the use of generic drugs, *e.g.* cheap copies of drugs that were protected by patents. The health infrastructure in many parts of South Africa, especially in the former Bantustans, barely enabled the provision of basic services. The provision of antiretroviral drugs requires not only the presence of sufficiently qualified personnel, but also additional services and equipments such as HIV testing, counselling, refrigerators to store the drugs, as well as laboratories to monitor patients.

First, as will be shown in detail in chapter 5, discussions with regard to the drug-based prevention of mother-to-child transmission started in 1997. The use of antiretroviral drugs for the prevention of MTCT was regarded by many – certainly by AIDS activists, perhaps less so by government officials – as a first step towards the universal long-term treatment of HIV/AIDS.[77]

Second, in February 1998, the Pharmaceutical Manufacturers Association of South Africa (PMA) sued the South African government backed by the US based Pharmaceutical Research and Manufacturers of America (PhRMA) for violating property rights protected under the Agreement on Trade Related Aspects of Intellectual Property Rights (TRIPS). The lawsuit aimed at the Medicines and Related Substances Control Amendment Act, which was signed by Nelson Mandela in December 1997 (Kühl 2002:76). The 'Act' gave the Minister of Health the power to allow both compulsory licensing and parallel importation of patented medicines.[78] The fear of pharmaceutical producers – and the hope of AIDS activists – was that the government would use these powers to enable the state to acquire antiretroviral drugs for a fraction of the then market price.[79]

Third, the government under the leadership of Deputy President Thabo Mbeki became embroiled in a stand-off with South Africa's Medicines Control Council (MCC) over a compound called Virodene, which was claimed to be a cure for AIDS by its developers (see Myburgh 2007 and 2009, Geffen 2010). The MCC, which is by law responsible to evaluate the safety and effectiveness of substances that claim to be medicines, reviewed the drug and found that there was

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[77] While AIDS activists and health professionals were seeing this as a positive development, there have been suggestions that the government opposed drug-based PMTCT precisely for this reason as they might have feared the financial implications coming with a commitment to anti-retroviral treatment (*e.g.* Nattrass 2003, 2007).

[78] "Compulsory licensing is generally defined as the granting of a license to a third-party by a government to use a patent without the permission of the patent holder" (Kühl 2002:17). "Parallel importation refers to the imports of goods purchased in a foreign market by an independent third party and later resold in the domestic market where much lower prices compete with the prices charged by authorized distributors" (Kühl 2002:18).

[79] Under large national and international pressure and with bleak chances to win the case, the PMA dropped the charge later in 1998. The pharmaceutical industry brought the case again in 2001, but was eventually defeated in court. The fears or hopes associated with the case, namely that this would enable the South African government to provide antiretroviral treatment for all who needed it, were, however, unjustified. The Minister of Health made clear that the government would not use the powers given to it by the Act to secure preferential prices for antiretroviral drugs. While this stance was criticised by AIDS activists, it was generally in line with the neo-liberal position of the South African government under Thabo Mbeki.

no sufficient evidence to support the claims of the developers. In contrast, members of the cabinet, led by Mbeki, publicly and enthusiastically endorsed the drug after receiving a presentation by the developers of Virodene.[80] Instead of leaving the matter to the MCC, Mbeki and other politicians were apparently convinced that Virodene was effective. It appears that their judgement was partly influenced by the fact that Virodene was developed in South Africa by South African researchers. They attempted to pressure the MCC into granting a licence to Virodene or at least allowing it on the market as a 'mercy treatment' until its worth was properly proven, but the leadership of the MCC refused to buckle. Several reviews always brought the same result: there was no evidence that Virodene was safe and effective. While Mbeki, publicly at least, had eventually to stop demanding the provision of Virodene in 1998, he did not take this defeat lightly and was the driving force behind a subsequent 'reform' of the MCC. Almost the entire experienced leadership of the MCC was replaced with administrators who were supposed to be more sympathetic to the demands of ANC leaders. Amongst the long-term effects of this 'reform' is the increased waiting time for the approval of new medicines as well as the proliferation of quackery in South Africa (Geffen 2010).

4.3 MTCT in South Africa

HIV/AIDS is usually associated with adults, since the most common route for the transmission of HIV is sexual intercourse. HIV/AIDS, however, is also affecting children directly. Estimates by UNAIDS at the turn of the Millennium have suggested that about 5 million children worldwide have been infected since the beginning of the epidemic in the early 1980s. Moreover, estimates also suggest that about 600,000 children were infected annually in the late 1990s.

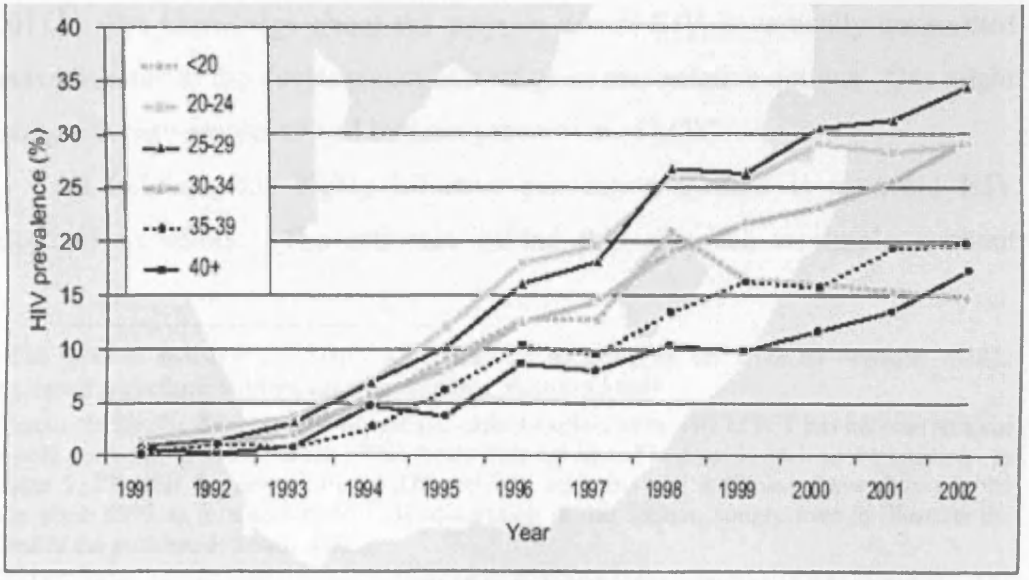
South Africa has witnessed a sharp rise in paediatric HIV infections during the 1990s. While such infections were very rare in the early 1990s, the country

[80] In fact, the MCC only heard about Virodene due to the press coverage about the presentation which the Virodene developers were allowed to give at a Cabinet meeting in early 1997. The 'clinical trial' which was supposed to provide evidence of Virodene's safety and efficacy was therefore illegal as notification of the MCC is compulsory. Moreover, the Virodene researchers gave Virodene straight away to humans without any preliminary testing of toxicity and safety. Their trial design was also poor as no control groups existed.

accounted for about 10% of the global paediatric HIV cases 10 years later, since estimates suggested that between 60,000 and 70,000 children would acquire HIV annually in the late 1990s (Wilkinson, Floyd and Gilks 2000, Marseille *et al.* 1999). The vast majority of paediatric HIV infections were caused by mother-to-child transmission of HIV, which explains the sharp rise in HIV infections in young children. This is because the prevalence of paediatric HIV is directly linked to the prevalence of HIV in pregnant women. Medical research suggests that without any measures to reduce the risk of mother-to-child transmission of HIV between 15% and 35% of children born to HIV infected mothers will acquire the virus (UNAIDS 1999). The bigger the general HIV/AIDS epidemic is in a country, the greater the incidence of paediatric HIV.

As shown above, South Africa was affected by the biggest HIV/AIDS epidemic in the world. Especially problematic was that amongst the population women were the most severely and disproportionately affected by HIV/AIDS in South Africa. According to the figures of UNAIDS, women accounted for about 56% of adult HIV infections in South Africa in 1999. Especially relevant for the problem of MTCT is, that the great majority of those women living with HIV/AIDS were of 'reproductive age.' A more detailed representation of the data obtained by the survey of antenatal clinic attendees demonstrates this.

Figure 4: HIV prevalence among pregnant women at antenatal clinics, by age group, 1990-2002



Source: Department of Health (2003)

Figure 4 shows that by 1999, apart from those antenatal clinic attendees aged 40 or older, HIV prevalence rates were at least above 15% and, in the cases of women aged between 20 and 29 years of age, as high as 25%.

One statistically visible effect of the increase in MTCT in South Africa has been a rise in child mortality between 1990 and 1998. While UNICEF estimates that 56 out of 1,000 born children die under the age of 5, this figure has increased to 83 out of 1,000 just 8 years later. The latest UNICEF figures indicate that in 2008, the rate has declined compared to 1998, but is still higher than in 1990 as it stands at 67 out of 1,000 (Bodibe 2010).[81]

The figures presented in this section show that MTCT was a considerable problem.[82] As the estimates showed, it was likely that more than 60,000 children in South Africa would be annually infected if nothing was done to counter the problem. Given that South African HIV/AIDS epidemic did not show any signs of maturation or decline in the late 1990s, it was foreseeable that this figure would further rise over subsequent years.

4.4 Technical responses to MTCT – The state of treatment

Medical and scientific research indicates that HIV can be transmitted from mothers to babies at three different stages: (1) during (mainly late) pregnancy; (2) during labour and delivery; and (3) during breast-feeding (Newell 1998, Bulterys 2001).[83] The knowledge about the ways in which HIV is vertically transmitted has contributed to the development of a range of preventative options. One might distinguish between direct and indirect prevention of MTCT.

An indirect, but highly effective prevention method is to avoid HIV infections in adults. The rationale behind this approach is simple: without

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[81] The specific statistics for South Africa can be accessed on the UNICEF website: <URL: http://www.unicef.org/infobycountry/southafrica_statistics.html>

[82] Just to clarify, the figures alone do not and cannot explain as to why MTCT has become an issue for policy-makers in South Africa. This thesis does not intend to provide such an explanation. In chapter 5, I'll treat the interest that AIDS activists, scientists and politicians have shown in the issue since 1997 as a 'social fact.' The discussion in this section simply tries to illustrate the extent of the problem in South Africa.

[83] MTCT rates are generally lower in countries in which breast-feeding is not common. The majority of South African babies have been and still are breast-fed, which means that South Africa's 'natural' MTCT rate is believed to be 25% or higher.

infected mothers, no transmission to newborns can take place. In principle, the transmission of HIV from one human to another is entirely avoidable. In practice, however, despite great global efforts, new HIV infections still occur. Efforts to prevent new HIV infections should be part of any broad strategy against MTCT, but as long as such infections take place, the concrete risk of vertical HIV transmission remains.

Since the possibility of the occurrence of MTCT has been recognised in the 1980s, a small range of direct options to prevent MTCT has been developed. By the late 1990s, these options were Caesarean section, the use of formula-feed to replace breast-milk, long-term use of antiretroviral treatment for the pregnant women in advanced stages of HIV infections and the short-term use of antiretroviral drugs. Usually used in combination, these technological interventions have helped to reduce MTCT rates significantly in most Western societies, pushing the risk of MTCT below 2% (Fowler *et al.* 2003, 2007). Given that most Western societies have well-funded public health systems that provide health care to virtually all citizens, the transferability of those technical solutions to a country such as South Africa, where many people struggle to access any public health care infrastructure was not a given.

Caesarean section reduces the risks of MTCT at the stage of delivery. It is more or less routinely used in the West in combination with other interventions. In South Africa, however, it was not and still has not been regarded as a broadly applicable option due to its costs and demands with respect to available medical staff, facilities and equipment (Treatment Action Campaign 2001, Pattinson 2000: 51).

Formula feed has been considered and used as an option for South Africa, but benefits and risks need to be balanced carefully. The benefits obviously refer to a reduction of paediatric HIV infections. The risk of MTCT through breastfeeding was only roughly quantifiable in 1998. According to a review by Newell (1998: 833), observational studies suggest a doubling of the overall MTCT rate through breastfeeding, while other limited evidence suggests an increase of the transmission risk between 7% and 22% compared with non-breast-feeding populations. While the costs of milk formula are significant – Wilkinson *et al.* (2000) estimate the costs for the provision of a 4 month course to be around

240 Rand (about $35) – formula feeding is generally considered to be cost-effective. The preventive impact of formula feed is therefore potentially substantial. The main risk of using formula feed is the potential contamination with unsuitable water. The provision of clean water is essential, but was not readily available in some parts of South Africa in the late 1990s, especially not in informal urban settlements, where the highest concentration of HIV infections is found. While the use of formula feed is feasible in principle, considerable investment into the provision of sanitary infrastructure is required (Wilkinson *et al.* 2000). Moreover, Söderlund *et al.* (1999) argue that formula feeding might be counterproductive in areas with high infant mortality or where the beneficial effects of breastfeeding outweigh the risk of MTCT. Another risk is that formula feeding might come to be seen as a 'marker' for HIV positive status of a mother, especially in predominantly breastfeeding societies (Söderlund *et al.* 1999). Given the stigma that is attached to HIV/AIDS in some parts of South Africa, this might lead to discrimination and ostracism.

A third option is the long-term treatment of adults in the form of HAART (Highly Active Antiretroviral Treatment) at an advanced stage of HIV infections, which also includes pregnant women. In general, the risk of HIV transmission is directly related to the amount of HIV in a human body (also referred to as 'viral load'). The higher the viral load the higher the chances of HIV transmission through various routes, including vertical transmission from mothers to newborns. Long-term antiretroviral treatment is designed to inhibit the replication of HIV in human bodies and thus leads to a decrease of the viral load. HAART, once started, is a life-long form of treatment. As pointed out above, prices for this type of treatment would have been prohibitively high at around 1999 and were therefore not considered to be an option – even by vocal South African AIDS activists.

This leaves only one more option, namely the short-term use of antiretroviral drugs. The advantage of the short-term use of drugs is obvious – it is much cheaper and easier to administer. A trial in the United States of America in 1994 tested the suitability of AZT, a nucleoside analogue reverse transcriptase inhibitor (NRTI) and the first drug ever to be registered as a treatment for

HIV/AIDS, for its potential to reduce the risk of MTCT. [84] The pregnant women who were assigned to the active arm of the double-blind randomised trial received multiple daily doses of AZT before and during delivery for several weeks. Newborn were also treated with multiple daily doses for six weeks. Connor *et al.* (1994) reported that of the 363 newborns whose HIV status was known, 180 were born to mothers in the active arm of the study and 183 were born to mothers who received placebos. 18 months after being born the infection ratio amongst children born to mothers in the AZT group was calculated to be 8.3%, while the infection ratio in the placebo arm was 25.5%. This suggests that the relative effectiveness of AZT compared to placebo is 67.5%.[85]

While the trial led to the implementation of AZT-based PMTCT programmes in Western countries, African and Asian countries with high HIV prevalence rates could not afford such programmes. The reason was that the original treatment protocol involved multiple daily dosages of AZT, which proved to be overly expensive and also too complex to administer in resource-weak settings.[86] PMTCT programmes only became a realistic option for developing countries from early 1998 onwards when the results of a trial in Thailand were announced (Centers for Disease Control and Prevention 1998, see also Shaffer *et al.* 1999). Pregnant women received tablets of AZT or placebo twice daily from 36 weeks' gestation and every 3 hours from the onset of labour to delivery. Newborns did not receive any treatment and women were asked to refrain from breastfeeding and to use formula feed instead (Shaffer *et al.* 1999). The researchers concluded that the regimen proved to be safe and also effective as a 50% relative reduction of MTCT risk compared to the placebo group was found. Subsequent trials in West Africa, which allowed for breastfeeding, found that HIV

[84] A variety of antiretroviral drugs exist and work differently; they are classified according to the mechanism at work. Reverse transcriptase inhibitors work by inhibiting the synthesis of DNA by reverse transcriptase. Reverse transcriptase is an enzyme that allows the transcription of single-stranded ribonucleic acid (RNA) into double-stranded deoxyribonucleic acid (DNA). Without this process, HIV cannot integrate its genetic information into the host-cell chromosome (*e.g.* Nattrass 2008:18-21).

[85] In chapter five the meaning of 'relative effectiveness' is discussed and contrasted with the concept of 'absolute effectiveness.'

[86] The first clinical trial to test for safety and efficacy of AZT, ACTG 076, used a protocol that was simply unsuitable for developing countries for two reasons (Connor *et al.* 1994). Firstly, pregnant women and newborns received multiple daily doses of liquid AZT for several weeks which made it prohibitively expensive. Second, the protocol is very complex and requires a health infrastructure that was and still is beyond the capacity of developing countries.

transmissions could still be reduced by about 37% and 38% respectively (see Mofenson 1999 for an overview, see Dabis *et al.* 1999 and Wiktor *et al.* 1999 for details).

During 1999, another even more suitable option for PMTCT in developing countries emerged. In the middle of that year, the results of a clinical trial in Uganda were made public that involved a new drug called Nevirapine (National Institute of Allergy and Infectious Disease 1999, Guay *et al.* 1999).[87] The trial compared a short-course AZT treatment that was similar to the one tested in Thailand with a single-dose Nevirapine treatment. In the Nevirapine arm of the study, pregnant women received one dose during labour and the newborns received one even smaller dose within 72 hours of birth. In contrast to the study conducted in Thailand, all women were allowed to breastfeed. The study showed that Nevirapine was 50% more effective when compared to AZT. Another strong advantage of Nevirapine was that the single-dose protocol made it even more suitable for resource-poor settings often found in developing countries than a short-course AZT treatment as no sophisticated medical infrastructure was required. Moreover, since pregnant women only received the drug once during labour, there was no need for them to visit hospitals or doctors a few days before delivery. This is a significant advantage in remote rural areas, where health care facilities can be quite a distance away from patients.[88]

As will be shown in the next chapter, by 1997 the consensus among a diverse set of stakeholders, ranging from government officials to AIDS activists, was that the short-term use of antiretroviral drugs represented the only viable option to reduce the risk of MTCT in South Africa. In addition, it was also broadly agreed that formula milk should be used wherever this is feasible, depending on the availability of clean drinking water. While many stakeholders were able to express their preferences of how to address the problem of MTCT, which happened to coalesce around the short-term use of antiretroviral drugs, the South African government possessed the final decision-making power. As the

[87] Nevirapine belongs to the class of non-nucleoside reverse transcriptase inhibitors (NNRTI). Like AZT, it targets the reverse transcriptase enzyme, but it binds it in a different way than AZT does.

[88] For a vivid description of treatment efforts in remote rural communities, see Jonny Steinberg's (2008) excellent book 'Three-Letter Plague.'

next section demonstrates, governmental decision-making power in South Africa was extremely concentrated.

4.5 Political Power in South Africa

Formally, South Africa is a democracy. Its new, post-Apartheid era constitution was widely hailed as one of the most progressive in the world. The constitution prescribes a separation of the powers of executive, legislative and judicial branches of the state. The federal structure of the state – nine provinces have their own governments and can send delegates to the second chamber of parliament – is supposed to add an additional layer of checks and balances. Political power is to be contested amongst a wide spectrum of parties in regularly held elections on national, provincial and local level.

Despite the existence of the formal democratic safeguards, politics in South Africa has been largely dominated by the African National Congress (ANC) since 1994, when it emerged as the dominant political force of the post-Apartheid era. The ANC, then under the leadership of Nelson Mandela, won the first free elections in 1994 by a landslide and also won majorities in 6 out of 9 provinces. By then, the opposition was already weak and fractured - the strongest opposition party, the National Party (NP), received just 20% of the votes. The political dominance of the ANC increased even further in 1999 when the second national and provincial elections were held. The June 1999 elections saw the ANC, then under the leadership of Thabo Mbeki, missing out on a two-third majority by less than half a percentage point as it secured 66.32% of the vote. The party also increased its provincial dominance, leading seven out of nine provincial governments. The weakness of the opposition became even more pronounced; the strongest opposition party received less than 10% of the votes.

Thus, while South Africa is formally a modern democracy with a constitutionally enshrined division of power between executive, legislative and judiciary, the unchallenged dominance of the ANC turns it *de facto* into a state that is controlled by a single party. Given that the opposition was practically unable to hold the ANC to account, aspects of 'inner-party' democracy became more important. In this respect, centralistic tendencies within the ANC that have

been linked to Mbeki's rise to power within the ANC have stirred debate amongst political observers whether democracy in South Africa has come under threat.[89] Thabo Mbeki was one of the leaders of the ANC and its close ally, the South African Communist Party (SACP), during the 1970s and 1980s when he lived in exile. Mandela appointed him as Deputy President in 1994 and he kept this post until 1999 when he became President of South Africa. Two years earlier, in 1997, Mbeki had already taken over the leadership of the ANC from Mandela. At the same time as becoming leader of the ANC, he was also appointed to lead the National Executive Committee (NEC). Thus, by 1997, Mbeki was already the most powerful individual within the ANC and the government, given that Mandela had practically handed over the daily business of government to Mbeki.

The ANC, with its Marxist inclinations and decades spent in exile, has traditionally been an organisation that favoured strong leadership and hierarchies. From 1997 onwards, Mbeki used his elevated position in the party and later in government to concentrate political power even further in his own hands. Two separate, but similar developments are worth mentioning in some detail.[90] First,

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[89] The unchallenged electoral dominance of the ANC as well as the centralising tendencies displayed under the leadership of Thabo Mbeki led political scientists, journalists, politicians and observers of the political developments in South Africa to debate whether this represents a danger to the newly established democracy in South Africa. Southall (2005) distinguishes three broad positions:

First, there are those who believe that the democracy in South Africa is under acute threat and that an era of 'elective dictatorship' is close. These observers point, among other things, to the inability of minorities and opposition parties to influence the political agenda, to the placement of important issues above public debate by the government, to the deliberate blurring of boundaries between state and dominant party and the erosion of inner-party democracy within the ANC.

Second, there are those who deny the characterisation of South Africa as a 'dominant party state'. They point out that South Africa remains a democracy despite the ANC's 'natural majority' and that 'checks and balances' are in place that protect democracy.

Third, there are those who occupy the middle ground and who argue for a "weak version of the dominant party thesis". While they acknowledge political dominance of the ANC, they point to inner-ANC debates about policies and to the circumstances of South Africa as a country in transition that faces enormous social, economic and political challenges. While the former aspect might mitigate the electoral dominance of the ANC, the latter might justify a certain degree of centralistic control for the ruling elite.

If the case of decision-making about PMTCT in South Africa is looked at in isolation, it supports the first position which questions the democratic credentials of the post-apartheid regime. This does not mean, however, that policy-making on the whole can be classified as being generally conducted in undemocratic fashion.

[90] These two developments can only give an indication of the centralising tendencies under Mbeki's leadership. Another instance of centralisation is the control of the national government over the government of provinces, which for the greater part works through financial means. Another example is the undermining of institutions such as Parliamentary committees which are

the delegates at the 1997 national conference in Mafikeng voted for far reaching changes of the party constitution which made the National Executive Committee (NEC), already the most powerful body in the party chaired by the current ANC leader, and several of its even more exclusive sub-committees even more powerful. The new constitution placed "all ANC structures including national, provincial and local government caucuses under the supervision and direction of the National Executive Committee" (Giliomee, Myburgh and Schlemmer 2001: 172, see also Southall 1998, 2005, Habib and Taylor 2001, Lodge 2003).

This meant effectively that the NEC had complete control over the party. For example, the NEC was given control over the party committee that decides who gets on electoral lists in which position. This is an effective tool to punish unruly party members as they might find themselves excluded from or very low on those lists and thus miss out on parliamentary seats (Kulzer 2000, Giliomee, Myburgh and Schlemmer 2001, Lodge 2003). The NEC was granted control over the deployment committee, which decides which party member gets available posts within party, government but also civil service. This allows the leadership to bring trusted and loyal members into key positions or to demote critical party members. This is especially critical with respect to the parliamentary caucus. The leadership is able to render – to some extent – the parliamentary committee system ineffective by threatening overly critical ANC committee members with sanctions (Southall 1998, Feinberg 2007). Moreover, the NEC is able to frame policies and party programmes. Delegates to meetings such as national conferences might be able to vote, but they are not free to decide on what they vote.

The second push for centralisation coincided with Mbeki's election as President of South Africa. When he became President of South Africa, Mbeki transformed the 'Office of the Presidency', which employed just a handful of people under Mandela, into "the central node of all policy-formulation and co-ordination, and making directors-general, the most senior civil servants, contractually accountable to himself rather than to their individual ministers"

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supposed to control the government, but are largely unable to perform this task (*e.g.* Feinstein 2007).

(Gevisser 2007: 714).[91] This meant that Mbeki had effectively control over all government departments and was able to influence the formulation of all government policies if he wished to do so.

Taken together, by 1999 Mbeki had more or less secured personal control of the ANC, the Cabinet and the government as a whole. One significant side effect of the concentration of political power in the Presidency was that public critique of policies approved by Mbeki put the political or professional careers of those who issued critical remarks at risk (Kulzer 2000, Lodge 2004, Gumede 2005, Gevisser 2007, Feinstein 2007, Myburgh 2009, Govender 2009, Geffen 2010).[92]

It is the enormous concentration of political power in the hands of one individual that made it possible for 'the South African government' to challenge established paradigms with regard to HIV/AIDS. It is worth quoting at length from a recent book by one of leaders of the influential Treatment Action Campaign, Nathan Geffen, in which he explains how Mbeki's 'deluded' views on AIDS could prevail for several years within the government, but also within the wider society:

> As president of the ANC and by far its most powerful member, Mbeki was able to impress his personal positions on the organisation. Despite an essentially democratic structure – branches and sectors elect their leaders, who in turn elect the organisation's leadership at provincial and national level – the ANC has much within its culture that is anti-democratic and renders it vulnerable to and easily manipulated by the personal views of its strongest leaders. Mbeki's public statements on ARV's [antiretroviral drugs] and Aids ... created an understanding within the party that opposing Mbeki's position on Aids would be politically costly. To make matters worse, the proportional voting system, by means of which all ANC MPs are beholden not to their constituencies but to their party bosses, creates an additional disincentive to speak out against poor leadership decisions. Indeed, when ANC MP Pregs Govender spoke out against the state's policies, she resigned shortly thereafter from Parliament. Barbara Hogan was also marginalised by Mbeki when she spoke out.
>
> The ANC together with its allies liberated South Africa from apartheid. It is recognised and admired as the liberator by about two-thirds of the voting population. ... [M]ost South Africans certainly look to the ANC for direction and leadership. ... [W]hat follows from this is that the president of the ANC has

[91] Gevisser (2007: 714) continues to make the power of Mbeki over policy formulation clear: "Frank Chikane [the Director-General of the Presidency and member of the NEC] wielded far greater responsibility than his predecessor Jakes Gerwel: he oversaw all policy formulation in cabinet, rather than merely managing the body. And Joel Netshitenzhe [head of the Government Communication and Information System and also a member of the NEC] was now given the responsibility for a new five-unit Policy Co-ordination and Advisory Service (PCAS), set up to vet and co-ordinate all proposed legislation."

[92] The treatment of the pre-1998 administration of the MCC, briefly described above, is a case in point.

> immense influence over the party's position and that the ANC has immense influence over South African society. Consequently, Mbeki's views on Aids, as well as the views of people acting on behalf of Mbeki or perceived to be acting on his behalf, carried enormous weight. (Geffen 2010: 193-194)

The quote sums up how one person's ideas can have large societal ramifications even though the person is acting in a supposedly democratic society. This is not to suggest that the leadership of the ANC in general or Mbeki in particular were simply able to do whatever they wanted. As I have shown elsewhere with respect to the so-called AIDS causation controversy, which developed directly out of the AZT controversy, Mbeki did not have total control over policy issues (Weinel 2005a). In that case, combined pressure from within the ANC, the media, scientists and AIDS activists forced Mbeki to retreat publicly from questioning the causal role of HIV after about 8 months.[93]

Moreover, despite the popular perception of Africa and specifically South Africa as epicentres of HIV/AIDS, the issue is not as dominant in South Africa as Europeans might think. Mbeki's ANC managed to increase its share in the national and provincial elections in 2004 despite all the controversies it caused with regard to the issue of HIV/AIDS. While HIV/AIDS is an important issue, other issues such as economic development, the reduction of crime or the provision of housing, clean water and basic health care infrastructures are usually perceived as more important by South Africans, both politicians and the public.

4.6 Conclusion

This chapter has provided vital information about the context in which the technological decision-making about the use of AZT for the prevention of mother-to-child transmission (PMTCT). As the following chapter illustrates, the prevention of MTCT became a public political issue in 1997. While the information provided in this chapter cannot exactly explain why this has been the case, the extent of the problem of MTCT, the available options to prevent it as well as the distribution of decision-making power have been highlighted here.

[93] It is, however, nonetheless astonishing that Mbeki could publicly hold the belief that HIV might not be the cause of AIDS for such a period of time given the widespread acceptance of the causal role attributed to HIV in South Africa and the rest of the world at that time.

It has been shown that the incidence of MTCT is directly linked to the general HIV/AIDS epidemic. Given that women were especially affected by the rise in prevalence of HIV in South Africa, the consequence was a sharp increase of paediatric HIV infections during the 1990. Children were affected to such an extent by HIV, that South Africa is one of only a few countries in the world in which child mortality rates have increased over the last 20 years.

MTCT is to some extent preventable. In Western countries it has almost been eradicated. Insofar as a number of preventive options exist, the foregoing discussion has shown that not all of those options were applicable in the context of South Africa. A high prevalence rate, relative shortage of public funding as well as a weak medical infrastructure in large parts of the country meant that the short-term use of antiretroviral drugs presented the best preventative option. In areas with clean drinking water, the use of formula feed presented an additional option.

The next chapter illustrates that while a broad range of stakeholders were involved in establishing the prevention of MTCT as a political issue, the power to make final decisions about it rested solely within the government. As the present chapter explored, the reality of South African politics in the late 1990s meant that particular individuals within the ANC led government, most notably Thabo Mbeki and his close circle of trusted allies, were in a position to influence any decision-making process if they wished to do so.

5. Testing the MDP: Technological Decision-Making About PMTCT in South Africa

5.1 Introduction

The aim of this chapter is to analyse the relationship between the technical and the political phase in technological decision-making about the use of antiretroviral drugs to prevent MTCT in South Africa. While the beginning of the technological decision-making process can be dated back to mid-1997, it is more difficult to determine its exact duration. One might argue that the process is still not complete given that antiretroviral drugs for PMTCT are still not available to all pregnant women living with HIV/AIDS in South Africa. By contrast, it could also be advanced that the process ended in July 2002, when the Constitutional Court upheld a ruling by a High Court in Pretoria, which forced the government to roll-out a country-wide PMTCT programme. An alternative point is 17 April 2002, when the South African Cabinet released a statement that announced the government's decision to make Nevirapine available for PMTCT to all those who needed it (Government Communication and Information Service 2002).

For the purposes of this study, the date that is regarded as the most significant is August 2000. While initially sympathetic to drug-based PMTCT, the government categorically refused to provide pregnant women living with HIV/AIDS with antiretroviral drugs to reduce the risk of MTCT between October 1998 and August 2000. Although the government only promised to test Nevirapine in 18 provincial pilot projects, the decision taken in August 2000 signalled a fundamental shift in the stance of the government, which proved to be irreversible (Heywood 2003). Another reason for choosing August 2000 is that the focus of this thesis is on the debate about the safety of AZT. When the government chose the cheaper and easier to administer Nevirapine as the only option for PMTCT in South Africa, AZT and the debates around it lost their political relevance.

For the purpose of this chapter, the technological decision-making process can be divided into four stages or episodes. The initial stage, lasting from July 1997 to October 1998, was characterised by almost perfect consensus within both technical and political phase. PMTCT was framed as a political issue and although different actors framed the issue differently, all stakeholders agreed that short-term usage of antiretroviral drugs offered the best possible solution to the problem of MTCT. A new and shorter protocol for the use of AZT for PMTCT, which became available in early 1998, was universally recognised as offering a safe and effective intervention and concerted efforts were made to implement pilot projects in various provinces as quickly as possible.

During the second stage of the technological decision-making process, which lasted from October 1998 to October 1999, consensus was lost in the political phase. While most non-governmental stakeholders insisted on the speedy implementation of PMTCT initiatives, government officials withdrew their support by arguing that a country-wide PMTCT programme could not be funded. Interestingly, however, no disagreement with regard to the safety or efficacy of AZT was publicly detectable at the time.

The financial controversy remained unresolved when the policy process entered its third stage. With actors in the political phase in disagreement, President Mbeki added a layer of controversy to the technical phase by claiming in late October 1999 that AZT was the subject of an ongoing scientific debate about safety. Mbeki instructed the Minister of Health to investigate the scientific controversy and to find out where the 'truth lies.' Despite numerous attempts to resolve the controversy by enlisting scientific experts, the prevailing view within the administration was that AZT was not safe enough. The alleged scientific controversy about the safety of AZT lost its relevance in August 2000, when the government switched its focus on Nevirapine as the drug of choice to prevent MTCT.

Choosing Nevirapine as the preferred drug to address the issue of MTCT in August 2000 signalled the beginning of the fourth and as yet unfinished stage of the technological decision-making process. In principle, the government changed its stance on PMTCT as it decided finally to allow the use of antiretroviral drugs

to reduce the risk of MTCT in the public health system. This decision, however, did not mean that all political conflicts about the issue receded. The difference was now that these were conflicts about the scale of PMTCT programmes in South Africa and no longer debates about how the issue of MTCT could be addressed.

The description of the technological decision-making process reveals that it proceeded at a relatively slow pace especially after October 1998, when provincial PMTCT pilot projects were one funding decision away from being implemented. It took a further two years before the government entertained the idea of introducing new pilot projects. To explore the technological decision-making process between mid-1997 and August 2000 from a normative point of view, this chapter returns to the concept of the minimal default position (MDP), developed in chapter 3. MDP is used here as an analytic resource to investigate whether the misrepresentation of consensual expert advice – or deviations from the MDP – directly contributed to the slow pace of technological decision-making.

5.2 PMTCT as an issue and the formation of a consensus: July 1997-October 1998

As shown in chapter 4, the potential for antiretroviral drugs to reduce the risk of mother-to-child transmission had first been established in 1994. Clinical trial PACTG 076 indicated that the risk of transmission could be halved. This technical solution to the problem of MTCT was almost immediately integrated into the treatment repertoire in Western countries (*e.g.* Centers for Disease Control and Prevention 1994, Mofenson 1999). The use of AZT in combination with other elements such as Caesarean section and formula feeding, proved to be successful as it helped to reduce vertical transmission rates from about 25% before 1994 to below 2% a few years later (Fowler *et al.* 2003, 2007).

Politicians, civil servants, medical staff, scientists and AIDS activists in South Africa were aware of these developments with regard to preventative treatment, but there was a broad consensus that the protocol used in PACTG 076 was unsuitable for implementation on a large scale in a country that struggled to provide basic health care to many of its about 40 million inhabitants (Heywood

2003, Martin 1999). One scientist described in an interview how experts, who were advising the Department of Health, were seeing the situation at around 1997:

> [T]his is the situation: most mothers come to us when they are in 28 weeks of gestation, in fact a fair amount of mothers only come at delivery, so [PACTG] 076 is not working in our setting, we understand this and we appreciate it, this was all the perinatal response and what we are doing at the moment is the PETRA trial, the Thai trial and there is the HIVNET 082 and these trials are more focussed around the intra-partum period and this is more appropriate for what will work in our setting. So there is no point in implementing 076 when we don't get women in at 10 or 11 weeks at gestation.

The hope was new clinical trials would establish new shorter and simpler treatment protocols suitable for countries with weaker health infrastructures. Indeed, by 1996 new clinical trials in search for an effective short course regime were underway in a range of Asian and African countries such as Thailand, Ivory Coast, Burkina Faso, South Africa and Tanzania (*e.g.* Guay 1999, Shaffer *et al.* 1999, Wiktor *et al.* 1999, The Petra Study Team 2002).

Despite the lack of feasible technical solutions to the problem of MTCT, in 1997 a range of non-governmental actors started to put pressure on the government to make the benefits of existing and expected medical interventions available to South Africans dependent on the public health sector. Mark Heywood (2003: 280) from the AIDS Law Projects recalls that,

> ...as early as 1997, organisations such as the AIDS Law Project (ALP) at the Centre for Applied Legal Studies [at Witwatersrand University, Johannesburg], the AIDS Consortium and the Perinatal HIV Research Unit at the University of the Witwatersrand began a period of sustained lobbying of the Minister and the Department of Health to develop a policy and programme to prevent MTCT. The objective was to pressure the government to implement the 'steps to be taken to prevent perinatal transmission of HIV' listed in the 1994 National AIDS Plan.

The first step to make MTCT an issue that policy-makers could not ignore was taken in July 1997, when the *AIDS Law Project* convened a seminar on 'The Rights of Pregnant Women in the Context of HIV.' In this broader context, the provision of PMTCT as a rights issue was discussed. Apart from representatives of non-governmental organisations such as the *AIDS Law Project* and the *AIDS Consortium*, officials from the HIV/AIDS and STD Directorate of the National Department of Health and the Gauteng Provincial Directorate were in attendance. The participants of the seminar formed a committee whose task it was to draw up a 'Code of Best Practice on Pregnancy and HIV', which was eventually published in late 1997 (Treatment Action Campaign 2000, Heywood 2003). It listed the

rights of pregnant women and also detailed procedures to be followed when HIV antibody tests were conducted during pregnancies. The document also emphasised the right for information about treatment even though it was accepted that without shorter treatment protocols antiretroviral drugs could not yet be used as an option to reduce the risk of MTCT.

By mid-November 1997, a broad consensus had been formed by governmental as well as non-governmental actors that antiretroviral drugs represented the best option to address the issue of MTCT. This consensus was reached during a conference on 'Affordable Options for the Prevention of Mother-to-Child Transmission of HIV-1: From Research to Clinical Care', which was co-organised by the National AIDS Programme, which was part of the National Department of Health and the Perinatal HIV Research Unit. A draft report on the results of the conference issued by the Department of Health included a section that indicated the areas in which a broad consensus existed between the stakeholders and contained the following three points[94]:

> All pregnant women should have VCT [voluntary counselling and testing];
> Short course ARV using AZT or AZT and 3TC should be offered;
> Pilot studies need to be done (cited in Mthathi 2001: paragraph 187)

The first point reiterated a necessary condition for any successful reaction to the growing problem of MTCT. Without knowing one's HIV status and without being informed about options and choices, any interventions were bound to remain inadequate. The two other points, however, signalled that the political response to MTCT would concentrate on the provision of antiretroviral drugs.

Progress in technological decision-making, however, depended on progress with regard to the development of a short course antiretroviral therapy that suited the South African context. The technical breakthrough came in early 1998. On

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[94] The draft report also revealed that the government was under concrete legal pressure to address the problem of MTCT in a meaningful and substantial way. It contained a quote from Mark Heywood, who represented the AIDS Law Project and who indicated that the government faced the prospect of legal action in 1997 over the lack of medical and therapeutic interventions to prevent MTCT: "The AIDS Law Project has been approached by someone in the Johannesburg area to consider a civil action against a public hospital on behalf of a child born with HIV on the grounds that measures did and do exist that could have reduced the risk of HIV transmission to that child. It is being discussed, but it's also being held [back] because of initiatives such as this conference and the commitment of the Department of Health to look at issues such as the provision of short course AZT if it works and simpler strategies such as subsidized milk formula" (cited in Mthathi 2001: paragraph 188). It is unclear, however, to which degree the government was driven by the fear of legal action.

19 February 1998, the Centers for Disease Control and Prevention (1998) in the USA issued a statement that a short course protocol based on AZT had been successfully tested in Thailand.[95] Within hours, the ANC-led provincial government of Gauteng started to lay the political groundwork for a provincial PMTCT pilot scheme. It immediately formed the so-called Gauteng Task Team, whose main duty was to plan the implementation of pilot projects in the Province to provide short-course AZT treatment to pregnant women. The Gauteng Task Team comprised civil servants as well as scientific experts. Two months later, in April 1998, 17 hospitals in Gauteng, which had the potential to function as pilot sites, where short-listed. A month later, on 18 May, five hospitals were finally selected from that list. By August 1998, all five sites had appointed pilot site managers and essential preparations such as storing of AZT, training of staff and testing of protocols were under way. By September 1998, the first of the five pilot schemes was up and running and provided short-course AZT to pregnant women (Mthathi 2001, Heywood 2003). In other provinces such as KwaZulu Natal and Western Cape, the success of the 'Thailand regime' had also caused provincial governments to take action and efforts to create pilot projects were made there as well.

Normative reflections

The first stage of the technological decision-making process about the provision of PMTCT programmes in South Africa is an example of technological decision-making in a state of complete consensus, both politically and technically. The actors of the technical phase – the South African Medicines Control Council as well as those scientists and doctors involved in establishing the safety and efficacy antiretroviral treatment – agreed that a short-course of AZT was safe as well as effective in reducing the risk of MTCT. This technical knowledge was so uncontested that the provincial government of Gauteng acted without delay once the Centers for Disease Control and Prevention (CDC) confirmed that the clinical trial in Thailand had proved to be successful.

[95] As mentioned in chapter 4, the only problem was that the results of the 'Thailand trial' were obtained by providing formula feed to the enrolled women, while breastfeeding is very common in South Africa.

The political phase was also characterised by a broad consensus. First of all, the main actors in the political phase – politicians, civil servants, scientists, doctors and AIDS activists – agreed that MTCT in South Africa was an issue that needed to be addressed. Moreover, they also agreed that a PMTCT programme based on the use of antiretroviral drugs represented the best option to do so. The political consensus was translated into concrete decisions. It was agreed to test PMTCT programmes in selected hospitals in a number of provinces. While Gauteng took the lead, other provinces such as KwaZulu Natal and Western Cape also developed plans to implement pilot schemes.[96]

From the MDP point of view, the technological decision-making process up to this stage was entirely unproblematic. The conditions set out by the minimalist default position have been fulfilled: political actors neither ignored nor misrepresented consensual expert advice in their justifications for implementing PMTCT pilot schemes. In fact, political actors went beyond the minimal prescriptions set out by the MDP and also based their policy-choices on expert knowledge. Without any frictions between political and technical phase, the technological decision-making process went speedily ahead. In just seven months, Gauteng implemented the technical solution to the problem of MTCT in five hospitals.

5.3 The consensus crumbles: October 1998 – October 1999

In early October 1998, it looked as if significant progress had been made with regard to PMTCT. The pilot schemes in Gauteng were already partially running and other provinces were about to follow suit. Depending on the success of the pilot programmes their expansion appeared to be a question of when, not if. Then, in early October, the technological decision-making process ground almost to a complete halt. A meeting of MinMEC, a body that included the National Minister of Health as well as all her provincial counterparts, took place on 2 October 1998.

[96] Western Cape, the only province outside of direct ANC, is the only province that, in clear contravention to directives issued by the national Department of Health, allowed PMTCT projects to run. The first started delivering services in January 1999 in an impoverished township in Cape Town and has often been invoked as a vivid example that using antiretroviral drugs is safe, effective and possible in South Africa (*e.g.* Heywood 2003, Mbali 2005, Von Soest and Weinel 2007).

Apparently unanimously, the decision was taken not to fund the emerging PMTCT pilot projects (Heywood 2003, Nattrass 2007). Without national or provincial funding, the pilot projects could not begin and the decision effectively signalled the end of the provincial projects to reduce the risk of MTCT.

While no immediate justification for this decision was publicly provided, it emerged over the following days and weeks that the main argument against funding the provincial projects was an apparent lack of financial means.[97] The protocol of the MinMEC meeting, which the Treatment Action Campaign (2000) obtained, shows that shortages in health budgets were cited as the main reason for the decision:

> Based on the cost estimates and the limited health budget available, with the provincial health departments experiencing financial difficulties in providing basic health services, the Health MINMEC took a decision on 2 October 1998 not to introduce the AZT regimen at this point of time.

Publicly, the Minister of Health insisted on numerous occasions that there was simply no room in the budget to provide funds for PMTCT (*e.g.* Paton 1998, Marais 2000, Heywood 2003, Nattrass 2007).

The seeming lack of funding made it necessary for the government to look for cheaper means of reducing the risk of MTCT. It opted to focus its resources on prevention of HIV infections in adults as this would lower the risk of MTCT in the long run. It was probably no coincidence that the decision to withdraw the funding for the provincial PMTCT schemes was announced on the same day on which Deputy President Thabo Mbeki launched a R80 million Partnership Against AIDS that focussed entirely on the prevention of new HIV infections (Mbeki 1998, Marais 2000, Van der Vliet 2001).[98]

Those in favour of PMTCT attempted to force the government to rethink its decision to withdraw the funding of the PMTCT pilot projects. Partly, this was

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[97] 'Other' arguments were related to inadequate state of the health infrastructure (Marais 2000), the apparently greater effectiveness of awareness campaigns (Paton 1998), incompatibility with the priorities of the government which claimed to focus its efforts on prevention and not treatment – notwithstanding the fact that treatment to reduce the risk of MTCT is preventative – (Paton 1998, Van der Vliet 2001) and the fact that the Thailand trial did not involve breast-feeding and its success could therefore not necessarily be translated into the South African context (Department of Health 1998).

[98] In particular, the Partnership aimed to bring the people of South Africa together to try to achieve three things: to promote prevention in order to reduce the incidents of HIV infections in the country, to improve care for those who have been already infected and to increase research efforts to speed up the search for a vaccine and a cure for HIV/AIDS.

done by employing 'political' arguments. Some appealed to the government by pointing out that not providing PMTCT puts the life of children at risk. Others argued that not providing PMTCT services violated human rights as well as the constitutionally enshrined right to health care (Geffen 2001, Heywood 2003).[99] Others argued that South Africa was rich enough to afford a country-wide PMTCT programme, which was estimated to have cost about R156 million in 1997 (Wilkinson *et al.* 2000, Nattrass 2003, 2007). To raise such a sum, some have pointed out that the prevention campaign initiated by the government in October 1998 would have gone some way to cover the costs of such a PMTCT programme. Others have suggested that the national and provincial health budgets have been regularly under-spent, which also provided the government with some financial room for manoeuvre (Marais 2000, Nattrass 2007). Others emphasised that the government had no right to plead poverty, because it had ratified an international arms deal in 1998, which committed South Africa to spend several tens of billions of Rand on military equipment (Feinstein 2007).

Some also tried to marshal technical arguments, in particular economic ones, to persuade the government to change its stance. A number of economists had done calculations that demonstrated the cost-effectiveness and total costs of different possible PMTCT programmes (Wilkinson *et al.* 1998, 2000, Marseille *et al.* 1998, 1999).[100] Some researchers, by including additional costs related to paediatric HIV infections such as increased hospitalisation, even argued that a

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[99] Nathan Geffen (2001: 4) from the Treatment Action Campaign expresses this sentiment when he discusses a cost-effectiveness threshold for health interventions defined by the World Bank: "The World Bank recommends that health care interventions in a middle-income country, such as South Africa, costing less than R730 (or US$100) per DALY [Disability-Adjusted Life Year] are worth implementing. This is a very conservative recommendation, as it is not unusual for developed countries to spend tens of thousands dollars per DALY on some interventions. It is arguable that within a human rights framework, a cost of R730 per DALY is unacceptably low."

[100] Cost-effectiveness, in short, "expresses as a ratio the cost of obtaining an additional unit of health outcome" (Graham *et al.* 1998: 125). The most common 'additional unit of health outcome' is DALY or Disability-Adjusted Life Year.[100] The WHO defines DALY as follows: "One DALY can be thought of as one lost year of "healthy" life. The sum of these DALYs across the population, or the burden of disease, can be thought of as a measurement of the gap between current health status and an ideal health situation where the entire population lives to an advanced age, free of disease and disability." 'Cost-effectiveness' is thus usually represented as the costs it takes to gain an additional life year. The judgement whether a given ratio is considered to be 'effective', *e.g.* represents 'value for money', depends either on conventions or comparisons.

country-wide PMTCT programme would have actually saved money (Nattrass 1998, Söderlund *et al.* 1999).[101]

Both political and technical arguments used by proponents of PMTCT programmes had no visible effect on the government, since it upheld its stance that a country-wide PMTCT programme was unaffordable. The affordability argument remained a central to the stance of the government with regard to the provision of antiretroviral drugs for years to come.

Normative reflections

From the MDP point of view, nothing illegitimate had happened between October 1998 and October 1999. The technical arguments had not been contradicted or misrepresented by the government. The government justification for withdrawing the funding for the pilot schemes and thereby distancing itself from an implicit commitment to PMTCT was that a country-wide PMTCT was unaffordable. Government representatives argued that the few resources that were available were better spent on prevention to reduce the number of HIV infections in adults.

While the government concentrated on affordability, technical arguments used by government critics focused on total costs, cost-effectiveness and also cost-saving. These issues have, in principle at least, nothing to do with affordability. Even if one can show that a certain intervention is cost-saving, it does not follow that one can afford it. Affordability is related to the willingness to pay for something, which is in turn related to the priorities and preferences of the payer. Of course, government expenditure easily outstripped the R155 million or so that were needed to cover an AZT based PMTCT programme, but no technical argument can force the government to prioritise this expenditure over any other

[101] On the basis of economic calculations of cost-effectiveness, one member of the Treatment Action Campaign (TAC) tried to construct a normative argument to force the government into accepting the affordability of a country-wide PMTCT programme (Geffen 2001). Geffen uses a World Bank threshold of cost-effectiveness of US$100 per DALY and shows that differently shaped country-wide PMTCT programmes in South Africa would incur costs that fall below the $100 threshold. He then argues that the government is bound by the constitution to provide PMTCT programmes: "Section 27 of the constitution implies that an mtctp [mother-to-child transmission prevention] programme should be implemented by the state if it can be shown to have the available resources. The state's resources can be quantified in terms of the cost and cost-effectiveness of an mtctp programme. There is a consensus in the scientific literature that mtctp will cost a small percentage of the health budget and that such a programme would be cost-effective, probably cost-saving, preventing at least 11,500 HIV infections, but probably substantially more" (Geffen 2001: 9).

expenditure. The question of affordability is political and has therefore to be decided in the political phase. The government was in a strong enough position to ignore the political, moral and technical arguments of its critics.

5.4 Just not safe enough: October 1999 – August 2000

National and provincial elections in April 1999 and the installation of a new government over the subsequent weeks and months put the issue of PMTCT somewhat on the backburner. The ruling African National Congress (ANC) secured a resounding victory on the national and provincial level and former Deputy President Thabo Mbeki became the new President of South Africa. Mbeki removed Dlamini-Zuma from the Health Department – she became the new Minister for Foreign Affairs – and appointed instead Manto Tshabalala-Msimang, who had a long record of working on health issues for the ANC. Those hoping for a reversal of the government's position on PMTCT were initially encouraged by the rhetoric of the new Minister, but the hopes were dashed considerably when Mbeki delivered his inaugural speech to the National Council of Provinces on 28 October 1999. The surprise that Mbeki's speech caused can at least partly be attributed to the fact that the safety of AZT when used for PMTCT had so far not been an explicit issue in the technological decision-making process. As shown above, an expert consensus on the safety of AZT was simply treated as a social fact. While Mbeki shed doubt on this fact, he failed to specify the exact nature of the safety concerns.

The Government's Assessment of AZT

It fell to the new Minister of Health to specify the safety concerns of the government with respect to AZT in an address to the *National Assembly* on 16 November 1999 (Tshabalala-Msimang 1999).[102] In her statement, the Minister

[102] Thabo Mbeki did not make any further public statements on the issue of AZT's safety until February 2000. There is, however, compelling evidence that Mbeki remained the driving force behind the doubts about the safety of the drug. Between November and January, he privately consulted a range of AIDS and AZT critics (Rasnick 2000, Farber 2000, Bialy 2004, Myburgh 2007, Cherry 2009). A conversation between Mbeki and one leading AIDS and AZT critic, David Rasnick, suggests that Tshabalala-Msimang had not subscribed entirely to the critical line on AZT as pursued by Mbeki. There is little doubt among observers that Tshabalala-Msimang only acted

tried to achieve two things: (1) to outline the principles underpinning the government's policy on the provision of medical treatment in general and (2) to explain the government's policy with regard to the treatment of HIV/AIDS by applying the policy principles.[103]

With regard to the first issue, Tshabalala-Msimang referred to guidelines drawn up by her Department that were supposed to govern the provision of treatment. According to those guidelines (Tshabalala-Msimang 1999), medical treatments had to meet the following three requirements to be considered for provision in the public health sector:

- Treatments have to be backed by evidence that shows that the treatment is clinically effective for South Africans;
- Treatments need to have a favourable benefit/risk ratio;
- Treatments need to be affordable.

If a medical intervention failed to meet any one of those requirements, the Department of Health would not sanction its use. The Minister then applied the three criteria to make clear why the government refused to provide AZT for PMTCT.

The Minister stuck to the government position taken up in October 1998 that a PMTCT programme on the basis of a short-course of AZT programmes could not be afforded. She pointed out that an AZT short course would cost around R400 per person and additional costs for formula feed, HIV tests and counselling had to be taken into account as well. The Minister did not provide the total annual costs that an AZT based PMTCT programme would incur, but she made clear that the government did not regard the costs associated with such a programme as affordable.

AZT also failed the effectiveness criterion. The Minister considered the 'absolute effectiveness' of AZT as being too low. Absolute effectiveness in this context is the effectiveness of an antiretroviral drug calculated in relation to all babies born to mothers living with HIV/AIDS. Tshabalala-Msimang assumed that

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on behalf of Mbeki. In subsequent years, however, she became one of the most fervent critics of antiretroviral drugs and earned the nick-name 'Dr No' (*e.g.* Cullinan and Thom 2009).

[103] In her speech, Tshabalala-Msimang tried to explain the government position with a range of different usages of antiretroviral drugs such as long-term treatment and post-exposure prophylaxis, but I concentrate exclusively on PMTCT here.

without any intervention about 25 out of 100 newborns would acquire HIV, which means at the same time that 75 babies will not acquire the virus in the absence of an intervention. She then assumed a relative effectiveness rate of 37% for AZT, which applies only to the 25 babies infected with HIV. This led her to claim that the use of AZT would 'only' save about 8 additional babies out of 100, hence the claim of an effectiveness of 8%.[104]

While the efficacy of AZT represented a relatively well established fact, it was, according to the Minister, counterbalanced by considerable evidence of toxicity:

> [T]here is evidence of the toxicity of AZT that has been published in scientific literature. For example, baby mice that were exposed to AZT through the placenta of their mothers, developed tumours. Over the past two years additional toxicological data has been published in the scientific literature raising similar toxicological concerns regarding MTCT exposure in studies performed in monkeys and in humans. (Tshabalala-Msimang 1999)

This evidence of toxicity had to be taken into account when balancing the risks and the benefits:

> Can we truly justify exposing 75% of the healthy babies to toxic drugs, in order to make sure that an extra 8% overall, do not run the risk of becoming infected, when we have little or no idea of what the long term effects of the drugs on the babies/children will be? We must remember that 5 years is the longest time that we have had so far to study the effects on children who were given AZT as babies. (Tshabalala-Msimang 1999)

By also taking into account that AZT might have some as yet unknown long-term risks, which had not yet been established, the government's overall view regarded the use of AZT as too risky.

Expert Advice on AZT

On the face of it, the apparent uncertainty with regard to the safety of antiretroviral drugs shifted the emphasis from the political to the technical phase of technological decision-making. This brought experts to the forefront of the technological decision-making process about PMTCT: without expert agreement on the safety of antiretroviral drugs, so the government argued, concerns about the

[104] Medical researchers and other proponents of PMTCT usually refer exclusively to 'relative effectiveness', which is the effectiveness of an antiretroviral intervention calculated in relation to the number of babies that would be infected without any intervention. For example, Dabis *et al.* (1999) found that after 6 months the rate of HIV infections in the AZT arm of the study stood at 18.0%, while it was 27.5% in the placebo arm. Hence, the researchers reported that AZT reduces HIV infections by 38% after 6 months.

safety of the drugs and their non-introduction in South Africa were justified. As Mbeki (1999) pointed out in October 1999, as long as South Africa's 'medical authorities' did not know where the truth lay, the provision of AZT would be 'irresponsible.'

The Medical Control Council (MCC) investigates

In response to Mbeki's warnings about AZT, the head of the MCC, Helen Rees, had initiated an investigation of AZT by MCC experts. An interim report was given to the Minister of Health in early November which formed the basis of Tshabalala-Msimang's speech before Parliament on 16 November 1999. The interim report concluded that although some adverse side-effects were associated with AZT the benefits of using AZT for PMTCT by far outweighed the known risks (Cherry 2000b, 2009). Tshabalala-Msimang refused to reconsider the government stance on AZT on the basis of this report, since she insisted that meaningful policy decisions could only be made once a final report, expected in late January had been released.

In early February, Minister Tshabalala-Msimang revealed that she had rejected another version of the MCC report on AZT, which had probably arrived at some point in January 2000, as it apparently lacked focus on the issue of adverse side-effects (Cherry 2000b). The Minister also told the press that she had recently received a final report from the MCC, but that she had only started reading it and could thus not yet comment on its content (Cherry 2000b, Heywood 2003, Nattrass 2007). That was the last the South African public heard about this final MCC report, which was not made public. It has been reported, however, that the MCC report concluded that there was a great deal of consensus on AZT's safety – at least amongst the experts within the MCC (Heywood 2003).

Mike Cherry and colleagues advise President Mbeki

In contrast to the official nature of scientific advice the Minister of Health received from the MCC, President Mbeki personally challenged some of those who criticised his stance on AZT to provide him with their expert opinion. The trigger for this had been an article in *Business Day* on 20 January 2000. Its author, Mike Cherry, then a lecturer in Zoology at Stellenbosch University and a South Africa correspondent for *Nature*, quoted Malegapuru Makgoba, then

president of the South African Medical Research Council, saying "that he [Makgoba] has read nothing in the scientific or medical literature that indicates that AZT should not be provided" (Cherry 2000a). Mbeki reacted to the article by sending both Cherry and Makgoba numerous articles and other documents that he perceived as supporting his stance on AZT.[105]

Mbeki explicitly challenged Cherry to assess a recently published review article by Papadopulos-Eleopulos *et al.* (1999), which alleged that AZT was ineffective, but highly toxic (Cherry 2009: 21). Mbeki assured Cherry that he had no personal stake in the matter and that he "would have no difficulty whatsoever in publicly stating that I was wrong, if anything I have said is proved wrong" (Cherry 2009: 22).

Cherry agreed to evaluate the article, but enlisted the help of a virologist and a pharmacologist. According to the assessment of Cherry and colleagues, the review was seriously flawed, both formally and with regard to its content.[106] As requested, Cherry and his colleagues reported back to Mbeki and offered to meet personally to discuss the matter in more detail. Instead of a reply by Mbeki, they received a rebuttal of their assessment by Papadopulos-Eleopulos and others, who had written the assessed review article. Cherry and colleagues responded by sending a reply to Papadopulos-Eleopulos and colleagues, which prompted another reply (Papadopulos-Eleopulos 2000a, b). Mbeki did not get involved in this exchange.

The Presidential Advisory Panel on AIDS

It transpired in late February 2000 that the Minister of Health was asked by the President to invite a large group of international experts to resolve all uncertainties around HIV/AIDS, which also included explicitly the safety of antiretroviral drugs.[107] The Presidential Advisory Panel on AIDS (henceforward

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[105] Makgoba reportedly referred to these documents as 'rubbish' that did not contain any valuable 'data' (Cohen 2000, see also chapter 6).

[106] See the discussion in chapter 6 for more details.

[107] The terms of references of the expert panel were as follows:

"1. The following questions needed to be addressed in dealing with this issue of the evidence of viral aetiology of AIDS and related concerns about pathogenesis and diagnosis:
a) What causes the immune deficiency that leads to death from AIDS?
b) What is the most efficacious response to this cause or causes?

referred to as 'expert panel') met twice in South Africa; the first time in early May and the second time in early July, just days before the start of the International AIDS Conference in Durban.

Despite the obvious urgency of the matters under consideration and initial promises that a final report would be available by October 2000, an interim report of the panel meeting was only published in March 2001 (Presidential AIDS Advisory Report 2001). What is more, in spite of the publicly expressed intention of the government to seek consensual advice from the expert panel, the recommendations exhibited a sharp divide amongst the experts. One set of experts doubted that HIV was causally implicated in AIDS; rather, they suggested that AIDS was probably caused by extreme poverty, malnourishment or toxic drugs. Consistent with their view on the aetiology of the disease, they claimed that antiretroviral drugs could not be considered to be an effective therapeutic intervention. Another set of experts vehemently disagreed with this position. They insisted that HIV was the cause of AIDS and that antiretroviral drugs, such as AZT, provided a therapeutic option in preventing HIV infections and slowing down the progression of AIDS.

All efforts to establish whether AZT was really safe or not appeared to be wasted when the government decided in August 2000 to ultimately and definitely rule out the future use of AZT for PMTCT. Instead, consultations with South African researchers convinced officials in the Department of Health that Nevirapine was a drug that was even more suitable for the South African context.

Normative Reflections

Of course, the government as the main responsible actor for technological decision-making in South Africa had the right to ensure that AZT was safe enough

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c) Why is HIV/AIDS in sub-Saharan Africa heterosexually transmitted while in the western world it is said to be largely homosexually transmitted?
2. What is the role of therapeutic interventions in the context of developing countries? This should cover therapeutic interventions in the following contexts:
· In patients with AIDS
· In HIV-positive patients
· In the prevention of mother-to-child transmission
· In the prevention of HIV transmission following occupational injury
· In preventing HIV transmission following rape
3. Prevention of HIV/AIDS
a) The discussions above should be underpinned by considerations of the social and economic context, especially poverty and other prevalent co-existing diseases and the infrastructural realities of developing countries." (Presidential Advisory Panel on AIDS 2001: 11-12)

to be used for PMTCT. On the face of it, the government had some reasons, which were presented by Tshabalala-Msimang, to doubt existing expert assessments, notably, that the benefits of using the drug outweighed its risks. Thus, that policy-makers should ask for a re-evaluation of pre-October 1999 expert assessments is not itself problematic. Rather, what is at issue here is that Mbeki (1999) had publicly claimed that there was a scientific controversy about the safety of AZT and that he did not consult experts *before* making such a statement (see chapters 6 and 7).

From the MDP point of view, things started to go seriously wrong, however, in the aftermath of Mbeki's speech. The most serious deviation from the Minimal Default Position occurred when the government first rejected and then simply ignored the view of the MCC. This body was legally authorised to assess the safety and efficacy of drugs in South Africa. While it might be accepted that the government rejects interim reports as incomplete or lacking focus on specific issues, there can be no excuse for ignoring the final report of the MCC which was sent to the Minister of Health in February 2000. At this point, Mbeki and Tshabalala-Msimang should have publicly accepted that the warnings about the excessive toxicity of AZT were, at least in the context of policy-making, unfounded or not sufficient to sustain the claim that using AZT was too risky. Once the final MCC report was delivered, the justification that AZT was too toxic - which was used by Mbeki and Tshabalala-Msimang to delay the introduction of a country-wide PMTCT programme - was no longer legitimate.[108]

One might argue that the other two expert investigations – an informal one led by Cherry and the formal one conducted by the expert panel – both serve to vindicate Mbeki's view given that they clearly show that there was unresolved expert disagreement. This is, however, not necessarily the case. In contrast to the MCC assessment, these two expert encounters were structured in an adversarial way which made expert disagreement the only likely outcome.

In the case involving Cherry and his colleagues' assessment of the Papadopulos-Eleopulos *et al.* review, Mbeki moved the goalposts once he

[108] This does not mean that the government would have been forced to change its policy-position, but it should have changed its public justification. It could have come up with new reasons or it could have just re-emphasised the affordability argument.

received the assessment of the paper. Instead of listening to the advice he had sought, Mbeki simply passed it on to the authors of the review. To expect a consensus emerging from such a debate is unrealistic as both sides were convinced that they were right. No amount of exchanged technical arguments could have forced one or the other side to back down as science study research under Wave 2 has often shown (*e.g.* Collins 1981a, Jasanoff 1990).

The same is true for the expert panel. The composition of the panel was such that it discouraged consensus formation despite the government's rhetoric that it looked for consensual advice. Almost half of the participants were openly denying a causal relationship between HIV and AIDS, which meant that the panel was split in the middle with regard to the most fundamental of questions. By 2000, the HIV theory was so institutionalised in practices and policies around the globe that it was simply not feasible for mainstream experts to compromise on their position. A consensus was therefore only possible if AIDS sceptics had changed their stance, but the prospect of them backing down was negligible as their disagreement with the mainstream about the aetiology of AIDS dated back to the mid-1980s.

These two expert encounters act as a reminder that expert consensus has to be 'achieved' or 'negotiated' (*e.g.* Jasanoff 1990, Bijker *et al.* 2009). In the context of the 3W model, such open-ended adversarial expert debate would not have been permissible. The whole point of the 3W model is to afford timely expert judgements on policy-relevant technical or scientific matters that are still to some degree controversial. The technical phase is not just another arena for scientific disagreement, but it is a place to make technical judgements for practical policy purposes. Rules and institutional mechanisms have to be put in place to ensure that consensus can be achieved.

5.5 Further Developments: August 2000 onwards

By August 2000, the South African government was under considerable national and international pressure to revise its controversial stance on the issue of PMTCT (Heywood 2003, Nattrass 2006b, 2007). Indeed, the Department of Health appeared to put an end on the controversy about the use of antiretroviral drugs for

PMTCT and decided that it would test Nevirapine in 18 pilot projects throughout South Africa – two in each province – for two years. This decision not only made all inquiries into the safety of AZT redundant, it also effectively ended those aspects of the decision-making process that were concerned with the question as to how the issue of MTCT in South Africa should be addressed.

This decision did not, however, signal the end of the technological decision-making process about the provision of PMTCT. For years to come there were political confrontations between the government and non-governmental interest groups that were largely concerned with the 'size' of the country's PMTCT programme and the speed of its implementation. The problem was that the decision to use Nevirapine for PMTCT, which was taken in August 2000, did not mean that suddenly every pregnant women living with HIV/AIDS had access to preventive treatment.

A first hurdle was that Nevirapine had to be licensed for paediatric use by the MCC. This alone took more than half a year and it was not until April 2001 that the 18 pilot projects could officially begin to provide Nevirapine. While AIDS activists suspected that the government was behind the slow registration, there was not much they could do about it (Heywood 2003).

As soon as Nevirapine was licensed by the MCC, AIDS activists challenged the decision by the government to test PMTCT for two years in only 18 pilot projects. They demanded the rapid expansion of the programme so that all pregnant women living with HIV/AIDS could benefit from preventive treatment. The government refused to give in to such demands and insisted on testing the suitability of Nevirapine for two years before a decision about the expansion of such projects could be taken. In response, AIDS activists involved the High Court in Pretoria to force the government to change its position. The High Court ruled in favour of the AIDS activists in December 2001 and ordered the government to roll-out a country-wide PMTCT programme on the basis of Nevirapine. The government appealed, but lost the appeal in March 2002. While the Cabinet took a decision in April 2002 to comply with the High Court ruling and to roll-out a universal PMTCT programme within a year, the government still appealed to the Constitutional Court to overturn the ruling of the High Court. In July 2002, the Constitutional Court dismissed the government appeal, which effectively sealed

the decision to roll-out a country-wide PMTCT programme (for detailed descriptions of the court proceedings see *e.g.* Berger 2002, Heywood 2003, Nattrass 2003, 2007, Cullinan and Thom 2009, Geffen 2010).

Even after the government had been forced to roll-out a country-wide PMTCT in South Africa, progress was slow. According to UNAIDS estimates, about 220,000 women living with HIV/AIDS required access to antiretroviral drugs for PMTCT annually between 2004 and 2007 (UNAIDS 2008). In 2004, only about 33,000 women received medicine for PMTCT, which equates to a coverage rate of about 15%, while in 2007, about 130,000 women received medicine for PMTCT, which equates to a coverage rate of 57%. The latest figures on the UNAIDS website suggest that the coverage rate has been further increased to about 90% in 2009.[109] Recent research commissioned by the South African Health System Trust (Day *et al.* 2010) suggests, however, that there is still much to do, especially in rural areas. Complete coverage of HIV anti-body tests in antenatal clinics has still not been achieved. The Department of Health under the new post-Mbeki administration has launched an Accelerated Plan for PMTCT in 2009 to address the inequality of PMTCT coverage in South Africa.

5.6 Conclusions

The history of this specific decision-making process, which began in mid 1997, suggests that there was a realistic prospect that localised projects using AZT to prevent MTCT could have been up and running from October 1998 onwards. It took, however, another two years before the government decided in principle to use antiretroviral drugs to reduce the risk of MTCT, even though only on a limited scale. The description has shown that during those two years the government used two different justifications for not committing to the provision of PMTCT on the basis of antiretroviral drugs:

1. A lack of financial resources (October 1998 – August 2000)
2. The existence of a scientific controversy about the safety of AZT (October 1999 –August 2000)

[109] See <URL: http://cfs.unaids.org/factsheet.aspx>.

The legitimacy of justifications deployed by the government can be assessed by using the MDP as a benchmark. In doing so, it has to be concluded that only argument 2 is in conflict with the prescription of the MDP, which postulates that those in the political phase must not use justifications for their policy-decisions that contradict or misrepresent expert consensus. In case of argument 2, the government should have, at least from February onwards, accepted that MCC experts could not find any serious disagreement among experts. The government, however, disrespected the judgement by the MCC. On this basis, one can argue that out of the almost 22 months of delay – if it is indeed accepted that numerous PMTCT projects could have been running since October 1998 – six months are directly attributable to misrepresentations of consensual expert advice.

The analysis has produced another interesting finding: throughout these two years of indecision, the government maintained that a country-wide AZT-based PMTCT programme was unaffordable. In the context of the MDP, this represents a legitimate justification for the policy-choice of the government, *e.g.* its refusal to provide antiretroviral drugs for PMTCT. It is a legitimate justification, because affordability is linked to preferences of how to spend public money. This suggests that had the government not insisted on its belief that AZT was unsafe for PMTCT, no criticisms from the 3W model point of view could be levelled against it.

What this shows is that the government's disrespect towards science actually had no prolonged and no decisive *direct* influence on the provision of antiretroviral drugs for PMTCT. This does not affect two incontrovertible facts: first, many paediatric HIV infections and therefore deaths of children could have been prevented had the government decided to go ahead with provincial PMTCT pilot schemes in October 1998. Second, there is sufficient evidence that senior figures within the South African government, including President Thabo Mbeki and Minister of Health Manto Tshabalala-Msimang, were antipathetic towards the use of antiretroviral drugs (*e.g.* Heywood 2003, Nattrass 2003, 2007, 2008, Gevisser 2007, Feinstein 2007, Cullinan and Thom 2009). It shows, however, that the fundamental problems related to the provision of antiretroviral treatment for PMTCT and other indications were of political nature.

The main problem was not that some government figures, for whatever reason, were not in favour of using antiretroviral drugs. The main problem was that very few actors – Mbeki and a probably only a handful of others belonging to the so-called 'inner-circle' of the President – were able to impose their political choice with respect to addressing the issue of MTCT upon the country despite widespread opposition. Without working democratic checks and balances, President Mbeki's influence on the technological decision-making process was almost absolute.

This emphasises a deliberate practical limitation of the MDP. The MDP is designed to bring about the best possible technical advice, but it leaves policy-choices to policy-makers. In contexts, in which a few actors dominate the political phase and are determined to make particular policy-choices, even the best technical advice cannot change this. For the MDP to work best, it requires a democratic context in which policy-choices are subject of open and transparent deliberations and in which policy-makers are prepared to change their preferences in light of 'good' arguments.

6. Domain-Specific Discrimination: studying the problem of extension for meta-expertises

6.1 Introduction

The last chapter has concentrated on the relationship between the technical and political phase in the technological decision-making process related to PMTCT in South Africa. Over the course of this chapter and the next, the problem of extension and questions about the legitimacy of participation in the technical phase come to the fore.

Collins and Evans have effectively solved the problem of extension in the context of the 3W model for those propositional questions directly concerned with technical or scientific judgements such as 'Is it safe to use AZT for PMTCT?' or 'Does smoking cause cancer?' As shown in chapter 1, they have proposed that only those actors that can be classified as relevant 'technical experts' should be allowed to provide policy-relevant answers to such questions. Given the normative nature of this prescription, it cannot be 'proved' through empirical work. All that can be done is to test whether the practical implementation of the prescription does indeed improve technological decision-making.

The next two chapters attempt to solve the problem of extension for a different class of questions, which might be referred to as meta-questions. Meta-questions are understood to be questions, whose answers inherently contain indirect technical judgements. The question on which this chapter focuses is the following: Is a particular scientific fact or issue subject of a scientific controversy or not? The judgement that is required is about the state of a particular scientific discourse. As such it is not directly a judgement about a scientific fact or issue. Inevitably, however, it contains a technical judgement. This is best illustrated with an example: if one assumes a binary structure for this type of judgement, than it follows that one can either say that the safety of AZT is subject of a

scientific controversy or the safety of AZT is subject of a scientific consensus. If one opts for the former, one inevitably assumes that at least two legitimate scientific opinions exist on the issue otherwise it would not make sense to talk about a scientific *controversy*. If one opts for the latter, one inescapably assumes that there is only one legitimate scientific opinion on the issue.

Apart from theoretical ambitions, there is also a practical justification for the interest in solving the problem of extension with respect to meta-questions: it makes a real, policy-relevant difference whether a scientific issue or fact is regarded as the subject of a scientific controversy or not. The whole idea of technocracy appears to be based on this insight as it is assumed that science is able to provide true, *e.g.* consensual, scientific knowledge on which policy decisions can be based (*e.g.* Krimsky 1984, Collingridge and Reeve 1986, Fischer 1990). Students of science policy have observed that consensual scientific knowledge is far more acceptable and easier to use in technological decision-making than controversial knowledge (*e.g.* Mazur 1973, Jasanoff 1990).

The flip side of this is that declaring a scientific fact or issue to be controversial can be strategically used to thwart or delay policy-making efforts. Indeed, there are documented cases that suggest that this strategy has been used in practice (*e.g.* Latour 2004, Michaels 2008, Shwed and Bearman 2010, Oreskes and Conway 2010). For example, Frank Luntz, a Republican pollster, made the rationale behind this stance explicit when he was quoted in an editorial of the New York Times on 15 March 2003:

> 'Should the public come to believe that the scientific issues are settled', he [Luntz] writes, 'their views about global warming will change accordingly. Therefore, you need to continue to make the lack of scientific certainty a primary issue.' (New York Times 2003)

Another example is a quote from an executive working for a tobacco company, which had an interest in delaying anti-smoking legislation:

> Doubt is our product since it is the best means of competing with the 'body of fact' that exists in the minds of the general public. It is also the means of establishing a controversy. (Michaels 2008: 11)

The description of the technological decision-making process about PMTCT in South Africa, presented in the previous chapter, suggests that something similar might have happened in South Africa: Thabo Mbeki appeared to use the apparent

existence of a scientific controversy about the safety of AZT as a justification for the non-action of the government regarding the provision of antiretroviral drugs for PMTCT between October 1999 and August 2000.

The discussion in the previous chapter demonstrates that the MDP cannot deal with such a situation. Even if the rules of the MDP had been accepted by the South African government and even if the government had withdrawn its concerns about the affordability of an AZT-based PMTCT programme, the political decision-making process would still have been held up for almost four months between October 1999 and February 2000. The prescriptions of the MDP applied only after mid February, when the MCC submitted its final report. While it is impossible to know what practical difference a four months delay in passing legislation to approve PMTCT would have made in South Africa, it is highly probable that even the implementation of few localised PMTCT projects four months earlier would have avoided some HIV infections in newborns. The potential consequences related to the invocation of scientific controversies for technological decision-making lend great importance to the policy-relevant judgement of the state of a particular scientific discourse. Given this importance, the question arises who should be allowed to make such a policy-relevant judgement.

This chapter is organised in the following way. The first section explores in more detail what is involved in making judgements about the state of scientific discourses. Insights provided by Wave 2 suggest that such judgements are inescapably linked to judgements of credibility of involved views. They also suggest that technical expertise is not necessarily required to make credibility judgements. It is, however, argued that differences in the quality of credibility judgements can be systematically linked to differences in meta-expertises.

The purpose of the second and the third section is to establish whether the existence of such a link can be established empirically. In the second section, the focus is on Thabo Mbeki, who claimed that the safety of AZT was the subject of a scientific controversy in 1999 and 2000. First, it will be shown that Thabo Mbeki did not possess specialist or referred expertise on the issue of drug safety and that he can be, at best, classified as someone with 'Primary Source Knowledge.' It

will then be analysed on which experiential basis Mbeki made the judgement that a scientific controversy about the safety of AZT was occurring. The analysis suggests that Mbeki, due to his lack of specialist expertise, had to rely exclusively on non-technical or social considerations to inform his judgements that led to him to believe that a scientific controversy about the safety of AZT existed.

The third section concentrates on mainstream scientists with different types of expertises, such as interactional, contributory and referred expertises, on the issue of drug safety. Analogous to the analysis in the previous section, justifications for their shared belief that no controversy about the safety of AZT occurred are analysed to establish on what type of expertise this belief is based. The analysis reveals that those with expertise can, to varying degrees use technical as well as non-technical considerations to inform their judgement regarding the status of the scientific discourse about the safety of AZT in 1999.

The fourth and last section discusses the possibility of a new type of expertise that has hitherto been overlooked by the PTE. This type of expertise will be called 'domain-specific discrimination' (DSD). This potentially new type of expertise has been discovered by comparing the respective social judgements of credibility by Mbeki and scientists. The comparison reveals a qualitative difference in the judgements that can be linked to the different social proximity to the scientific domains dealing with the assessment of AZTs safety.

The results presented in this chapter allow the tentative conclusion that there are good reasons to believe that the quality of a judgement about the state of a particular scientific controversy can be linked to the level of expertise the judge possesses. It is argued that it is possible to put a limit on those who should participate in making policy-relevant judgements about the state of scientific discourses. Specifically, it is argued that those who rely exclusively on ubiquitous discrimination should be excluded from making such judgements as their judgements are of relatively low quality and they do not offer any additional insights compared to those with other forms of meta-expertises. This suggests that it is possible to solve the problem of extension with respect to meta-expertises.

6.2 Credibility and the Judgement of the State of a Scientific Discourse

What exactly is involved in or required to judge the state of a scientific controversy? To illuminate this preliminary question, the example of Thabo Mbeki's invocation of a scientific controversy about the safety of AZT can be used.

In his speech in October 1999, President Mbeki claimed that he had recognised the existence of the disagreement between medical researchers regarding the safety of AZT by reading literature on the Internet. He repeated the claim that the ability to read scientific texts is sufficient for recognising a scientific debate in a letter to Mike Cherry, which also contained a number of articles allegedly demonstrating the excessive toxicity of AZT. Mbeki made the following assertion:

> I made no 'claims' about AZT. I reported to Parliament and the nation on the existence of the views expressed in the enclosed articles. (Cherry 2009: 21)

According to Mbeki, all that is seemingly required to detect expert disagreement is to find mutually exclusive opinions expressed by scientists, *e.g.* people with scientific degrees in natural and medical sciences. The President implicitly claimed that this could be done by reading scientific texts. Such a view only holds, however, if a scientific controversy is understood as 'mere disagreement about a scientific fact or issue between people with more or less relevant scientific degrees.' Put differently, this view only applies if anything other than perfect agreement between people with relevant scientific degrees means that a scientific controversy is occurring.

Such an 'all-or-nothing' view on scientific consensus and controversy does not make much sense as can be shown with the help of another example: it is perfectly acceptable to claim in 2010 that there is no scientific controversy about the fact that HIV is the cause of AIDS. Yet, a person such as Peter Duesberg, a microbiologist and a "giant of retrovirology", to quote one of the scientists interviewed for this study, claims that HIV is not the cause of AIDS. Does this mean that there is a scientific controversy about the cause AIDS in 2010?

Based on an insight established by Wave 2 of science studies, it can be argued that recognising the mere existence of conflicting views of scientists is, on

its own, insufficient to reliably identify the existence of a scientific controversy. Of course, President Mbeki is right by pointing out that a constitutive characteristic of any scientific controversy is the existence of differing views amongst experts (see chapter 7). What he has overlooked, however, is that judging whether two differing views constitute a scientific controversy inescapably involves a judgement of 'credibility.' If one does not believe that technical claims that represent one of two views allegedly constituting a scientific controversy have any credibility, one would simply not talk about a scientific controversy.[110] 'No credibility, no scientific controversy,' to paraphrase Steven Shapin (1995: 257).

Shapin (1995) has argued in a paper about the importance of credibility that it can be assessed in very different ways. It is worth quoting Shapins's considerations at length:

> In principle, there is no limit to the considerations that might be relevant to securing credibility, and, therefore, no limit to the considerations to which analysts of science [or here: analysts of states of scientific discourses] might give attention: The plausibility of the claim; the known reliability of the procedures used to produce the phenomenon, or claim; the directness and multiplicity of testimony; the ability to impute bias to the claimants or to assess risks being taken in making the claim; the personal reputation of the claimants or the reputation of the platform from which they speak; knowledge of the friends and allies of claimants, including their personal reputation and power; calculations of the likely consequences of withholding assent; claimants' class, sex, age, race, religion, or nationality and the characteristics associated with these; claimants' expertise, including means by which that expertise becomes known; the demeanor of claimants and the manner in which claims are delivered; minute aspects of the life-histories of those making them. Again, in principle there is no reason why an inquiry into the grounds of scientific credibility might not find itself concerned with the investment portfolios of individual scientists (did Martin 'Cold Fusion' Fleischman own stock in a palladium mine?) or what they eat in the morning (does a medical researcher warning against the risks of dietary cholesterol turn out to eat a 'full English breakfast' every day?) Any aspect of the scene in which credibility is accomplished may prove to be relevant, and the

[110] The impossibility of neutrality of STS analysts in the context of controversy studies has been pointed out by Scott, Richards and Martin (1990). They argue that by treating some disagreement between scientists as a scientific controversy inevitably leads analysts to embrace the position of the 'underdog' in the debate. While Collins (1991, 1996) has conceded the point that neutrality is impossible to maintain, he does not agree with Scott, Richards and Martin who argue that neutrality ought to be surrendered and that analysts should take up a 'activist' position. Collins argues that neutrality has to remain an ambition or intention of STS analysts. Furthermore, Ashmore (1996) has pointed out that while STS controversy studies lend credibility to the 'losing' side in a scientific controversy, this does not mean that the losers are necessarily underdogs. Tobacco companies that lose the argument about the causal relationship between smoking and cancer risk cannot be deemed to be 'underdogs' in the sense that Scott, Richards and Martin implied.

> relevance of nothing can be ruled out in advance of empirical inquiry. (Shapin 1995: 260-261)

Shapin's account illustrates at least three things: first, there is no such thing as a grand theory of credibility (Shapin 1995: 261). This means that the credibility of scientific claims can hinge upon all sorts of 'aspects of the scene.' For example, a scientific claim can be internally consistent, but its credibility can be undermined by private habits of the claimant.

Second, it follows from the first point that the technical expertise of an actor does not impact on the ability of an actor to make a credibility judgement. This means that, in principle, just about anyone can make potentially relevant credibility judgements. To take up the example of the medical researcher warning about dietary cholesterol: some non-scientist who knows about the researchers eating habits, can make a credibility judgement that is just as relevant as the credibility judgement of a fellow medical researcher, who bases her or his assessment solely on the published articles of the medical researchers.

Third, if one goes through the list of potentially relevant aspects provided by Shapin, there is at least a hint that technical expertise might be useful to assess the credibility of some aspects of claims. To assess whether 'the procedures used to produce the phenomenon, or claim' requires certain knowledge about methods that is not ubiquitous. Arguably a statistician will be better placed to judge the plausibility of a claim based on a clinical trial, than someone who has no knowledge of statistical principles and no experience in conducting such trials.[111] Moreover, scientific expertise might also be helpful to assess the 'plausibility of a claim.' Many scientific claims are counter-commonsensical and appear implausible to the untrained observer (*e.g.* Collins 1993). For example, the claim that replicating experiments does not decisively prove that a new phenomenon exists, which is plausible to almost anyone within STS, has caused consternation amongst scientists and philosophers of science (*e.g.* Collins 1985, Labinger and Collins 2001).

Taken together, it is possible to accept the argument about the unpredictability concerning the potential relevance of all sorts of aspects for

[111] Adam Hedgecoe, who has done research on Research Ethics Committees, has repeatedly made this point during KES seminars at Cardiff University.

assessing credibility and the irrelevance of expertise for the ability to make credibility judgements, without being forced to accept that all credibility judgements are of equal quality. It is in this sense, that expertise might play a role in credibility judgements and therefore in judgements about the states of scientific discourses.

It is therefore hypothesised that a systematic difference in the quality of judgements about states of scientific discourses can be attributed to differences in the expertises of actors. This hypothesis can be substantiated both theoretically as well as empirically. From a theoretical point of view, the classification of expertises, summarised in the Periodic Table of Expertises (PTE), allows associating different ways to assess the credibility of claims with different types of expertises.

On the one hand, the credibility of scientific claims can be judged on the basis of *technical considerations*, which means that judges have to possess technical expertises, *e.g.* interactional and/or contributory expertise. When claims are assessed on the basis of technical considerations, it is the content of technical claims that takes centre stage. This means, for example, that judges establish whether claims are supported by sufficient and acceptable evidence, whether claims are contradicted by accepted scientific facts, whether the methods used to generate claims are sound and so on.

On the other hand, relevant credibility judgements can also be made on the basis of non-technical or *social considerations*, which means that judges do not require any scientific expertise, although different types of meta-expertises such as 'ubiquitous discrimination' or 'local discrimination' are involved.[112] When claims are assessed on the basis of social considerations, the focus is not on the content of the claim, but on a huge range of contextual features. Judges might assess the credibility on the basis of the demeanour of the claimant, the political position of the claimant, the quality of the journal in which a claim is made.

[112] While lay-persons, due to their lack of technical expertise cannot rely on technical considerations in their meta-judgements, the opposite does not follow. As research, for example, by Shapin (1994, 1995) and Collins (1981c, 2004a), shows, experts routinely use social considerations when they assess their peers.

To test whether systematic differences in the quality of credibility judgements and thus ultimately in the quality of judgements of states of particular scientific discourses can be associated with differences in underlying expertises, the analysis in this chapter compares the publicly accessible rationales of Thabo Mbeki and mainstream scientists behind their respective and mutually exclusive judgements of the state of the scientific discourse about the safety of AZT. While Thabo Mbeki characterised the scientific discourse about the safety of AZT as being in a state of controversy in his speech on October 1999, mainstream scientists claimed that the discourse was characterised by broad consensus. By analysing the respective rationales, it should be possible to establish which expertises the different actors use and how these expertises impact on their judgements.

6.3 Thabo Mbeki's Expertises

Mbeki's level of specialist expertise

Given that the level of specialist expertise has been identified as a possibly decisive variable when it comes to judging whether a scientific fact or issue is subject of a scientific controversy, it is important to establish what type of specialist expert Thabo Mbeki was in 1999 and 2000 with respect to the issue of drug safety. This will be done in two steps. First, his biography is scrutinised for indications of formally or informally acquired tacit knowledge in natural sciences in general and disciplines such as pharmacology or toxicology in particular. Second, the available documentary evidence is analysed to reconstruct the duration and the character of the specific process in which Mbeki became acquainted with the issue of AZT's safety.

Mbeki's life is relatively well documented as a number of biographies, both official and unofficial, have been written about him (Gumede 2005, Gevisser 2007, Roberts 2007, Presidency of South Africa 2010). Biographical accounts of Mbeki agree that he is exceptionally intelligent and very well educated. He studied Economics at the University of Sussex and obtained a Master's degree. He also studied Marxism-Leninism at the Lenin Institute in Moscow. There is, however, no hint in his biography that suggests he obtained formal education in

any natural sciences. It is certain that he does not possess any formal or certified expertise in medical sciences or natural sciences such as pharmacology, toxicology or biochemistry and so on that might be relevant for assessing the safety of drugs.

While formal education is not the only route to technical understanding, it can also be stated with confidence that Mbeki did not develop expertise informally on matters related to the safety of drugs before 1994. The reasons is that Mbeki, according to his biography at least, had no real opportunity to pick up tacit knowledge on drug safety due to his life-long involvement in and commitments to the ANC and the struggle for the liberation of South Africa. His work in exile was predominantly directed towards the acquisition of international political support for the armed liberation struggle of the ANC. From the mid 1980s until the mid 1990s – the time when many antiretroviral drugs for the treatment of HIV/AIDS were developed or tested – he was one of the leading figures within the liberation movement who negotiated a peaceful political transition in South Africa, first in exile and then in South Africa (Gevisser 2007).

There is also no evidence to support the suggestion that Mbeki acquired experience in relation to the assessment of the safety of antiretroviral drugs between 1994 and 1999, when he was serving as Deputy President.[113] During this time, Mbeki was responsible for the overall coordination of South Africa's HIV/AIDS policy and might have gained some knowledge about its various aspects.[114]

Another way of exploring Mbeki's technical expertise is to reconstruct when and how he came to believe that the safety of AZT was subject of a scientific controversy. The available evidence suggests that Mbeki started acquainting himself with information about the toxicological profile of AZT by

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[113] This has to remain a speculative claim as President Mbeki did not answer repeated requests for an interview. There is, however, no publicly available evidence which suggests that this speculation is wrong. Of greater note, however, during 2000, Mbeki claimed on several occasion that he is not an expert on the matter and would leave the assessment of drug safety to scientists (*e.g.* Mbeki 2000a, Cherry 2009).

[114] Mbeki's controversial involvement in the 'Virodene affair' suggests that he developed some interest in treatments for HIV/AIDS from 1997 onwards, but at the same time, the episode indicates that his technical knowledge was limited. For example, he did not seem to object to the fact that the Virodene researchers tested the drug straight away on patients and thereby violated basic ethical protocols.

reading popular and scientific documents no earlier than March 1999, just about six months before he went public.

Available evidence suggests that Mbeki first became aware of a different viewpoint on the safety of AZT at some point after March 1999, probably in late April 1999 (Myburgh 2007). Mbeki told South African journalist Alister Sparks that it was an exchange of arguments between Anthony Brink, a South African barrister and self-proclaimed expert on AZT, and Desmond Martin, then the President of the Southern African HIV/AIDS Clinicians Society, that alerted him to the existence of antagonistic positions with regard to AZT's safety (Sparks 2003: 286). Brink (1999) claimed in a contribution to *The Citizen* that AZT was so toxic that it should not even be considered as a drug. Martin (1999) responded by acknowledging that AZT, just like any other drugs, has side effects. Martin was, however, keen to point out that these side-effects were not severe and when balanced with the benefits of reducing the risk of vertical HIV transmission, the involved risks were worth taking. According to Mbeki, Brink contacted him at some point and sent copies of the exchange with Martin, which Mbeki then read, probably in late April or early May 1999.[115]

While Mbeki gave no public indication that he had already developed serious doubts about the safety of AZT by mid 1999, further evidence suggests that Mbeki remained interested in the issues raised by the public exchange between Brink and Martin. One of my respondents told me that Mbeki, at least by August 1999, was in possession of what s/he referred to as the 'Duesberg Dossier', a set of papers authored or co-authored by American AIDS sceptic Peter

[115] James Myburgh, editor of South African website *Politicsweb*, has presented strong evidence which suggests that Mbeki's version of events does not fully reflect reality. Myburgh (2007, 2009) confirms that Mbeki received Brink's writings on AZT, which consist of little more than polemic comments on de-contextualised quotes from scientific papers. Myburgh disputes, however, Mbeki's version of events. According to documentary evidence available to him, Brink never contacted Mbeki directly. Myburgh suggests that Mbeki was made aware of Brink's article by businessman Zigi Visser, who at that time was in close contact with Mbeki as he had enlisted the President's support in developing a drug called Virodene. Visser and his associates claimed that Virodene was a cure for HIV/AIDS. These claims were apparently believed by some influential politicians within the ANC, most prominently among them Thabo Mbeki. Myburgh claims that Mbeki and other high ranking ANC officials backed the development of Virodene with millions of Rands at least until early 2002, although the MCC judged already in late 1998 that there was no evidence to suggest that Virodene was a cure for HIV/AIDS, let alone an antiretroviral treatment. Both Mbeki and Myburgh agree, however, that there was no direct personal contact between Mbeki and Brink or Martin at any point between March 1999 and October 1999.

Duesberg.[116] Duesberg, one of the best-known AIDS sceptics, has advanced what is known as 'drug-AIDS hypothesis.'[117] There is evidence that Mbeki sought personal advice from a group of South African HIV/AIDS experts to gauge the quality and credibility of the scientific papers that constituted the 'Duesberg Dossier.' One of the scientists interviewed for this study remembered the following:

> Interviewee: [...] This issue of AZT is poisonous, where is this coming from? Until I note, a couple of weeks before that [Mbeki's speech in October 1999] a couple of colleagues had been summoned to the President's office, so this was literally two months after he took office and he gave them this document to read and to advise him. [...] they then went to give feedback to the President. And when they gave feedback they tried to put a balance of what is there and scientific uncertainty, which is naturally to HIV/AIDS and how as scientists we deal with uncertainty and either take a Popperian approach which is we wait until we find the black swan or we take the verificationist approach and deal with what we have and evolve our response as the knowledge evolves, but we have to act because it is a public health problem and we are not alone in acting on that, the whole world, it's a global issue...
> MW: Do you know what document that was?
> I: The Duesberg Dossier. You know, Duesberg had this whole collection of documents.

According to my interviewee, the contact between Mbeki and the group of scientists ended quickly because Mbeki realised that the advice given to him was highly critical of the literature he was reading.[118] It was certainly too short for Mbeki to develop any meaningful technical understanding of the matters in

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[116] This 'Duesberg Dossier' might or might not be identical with a dossier on AIDS sent to Mbeki by South African journalist Anita Allen in June or July 1999. Allen, another self-educated AIDS sceptic, told a reporter in 2000 that she had sent Mbeki a dossier containing scientific literature in mid 1999, which, according to her, suggested that HIV was not the real cause of AIDS (Farber 2000).

[117] According to this hypothesis, the real cause of AIDS is not a virus but toxic drugs, including AZT. The 'drug-AIDS hypothesis' has been set out in a range of papers published in a variety of scientific journals, but they are all available at the Internet and therefore easily accessible. Harvey Bialy (2004), another AIDS sceptic, claims in his biography of Peter Duesberg that Mbeki read a joint paper by Duesberg and David Rasnick (1998), another US American AIDS sceptic, which was published in 1998 in *Genetica*. This paper is one amongst many that advances the 'drug-AIDS hypothesis.' It is, however, unclear whether Mbeki read the paper before or after 28 October 1999.

[118] This version of events has been confirmed to me by another person with knowledge of the meeting. Another indicator that Mbeki did not consult with experts on the matter before making his speech is the strong reaction of specialists in the aftermath of the speech (*e.g.*, Sulcas and Randall 1999, Cherry 1999, 2000a, Marais 2000). The MCC, for example, launched a review of AZT in response to Mbeki's claims in parliament as it had not received any previous warnings (Independent Online 1999).

question. Moreover, there is no evidence that Mbeki talked personally to Duesberg or any other AZT critic about this literature before 28 October.[119]

According to investigative research by James Myburgh (2007, 2009), the reason that Mbeki went public on 28 October might be related to a lengthy critical review of AZT, which was sent to the President just a few days before he delivered his controversial speech. The paper was written by a group of AIDS sceptics, known as the 'Perth Group', under the leadership of Eleni Papadopulos-Eleopulos, and eventually appeared in print in the November issue of a journal called *Current Medical Research and Opinion* (Papadopulos-Eleopulos *et al.* 1999). The Papadopulos-Eleopulos *et al.* paper, while mainly focussing on the issue of effectiveness of AZT, also attempts to demonstrate that AZT is mainly transformed into a substance with highly toxic properties when it is metabolised in human bodies. In essence, the authors of the review not only claim that AZT is completely ineffective, but they also suggest that AZT is highly toxic. Taken together, the two claims suggest a very unfavourable benefit/risk ratio.[120] Again, there is no evidence that Mbeki met or at least talked to the authors or someone with technical understanding of the matters about the quality and credibility of the claims made by Papadopulos-Eleopulos *et al.* before 28 October 1999.[121]

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[119] Documentary evidence suggests that Mbeki only met or at least personally talked to AIDS sceptics from November 1999 onwards. He probably first met with Anita Allen in early November to further discuss the evidence against the HIV paradigm (Farber 2000). Minister of Health, Manto Tshabalala-Msimang met Charles Geshekter, a historian and AIDS sceptic in December 1999 (Rasnick 2000). At the same time, the Minister was also introduced to Sam Mhlongho, one of the very few South African AIDS sceptics with an academic background (Rasnick 2000). The first direct contact between Mbeki and American AIDS sceptics close to Duesberg came in January when Mbeki contacted David Rasnick for advice on various issues (Rasnick 2000). According to Harvey Bialy (2004), Mbeki phoned personally both Duesberg and Rasnick in January 2000 to invite them to become members of the Presidential Advisory Panel on AIDS.

[120] A reason to believe that Mbeki was deeply impressed at least with Perth Group paper is the already described encounter between Mbeki and Mike Cherry (see chapter 5). Mbeki, in an interview with the South African Sunday Times, described the paper as 'a lengthy article with millions of references' which had been published in a 'very senior scientific journal' (Robertson, Hartley and Paton 2000).

[121] As indicated in discussion about the encounter between Mbeki and Mike Cherry (see chapter 5), Mbeki was probably in direct contact with Members of the Perth Group, but only after October 1999. Members of the Perth Group were also invited to participate in the Presidential Advisory Panel on AIDS which met in May and July 2000 respectively. They were, however, only able to attend the second meeting in July.

There are also some indications that Mbeki read other documents as well, although the exact nature of this literature is not known.[122] In his speech in October 1999, Mbeki hinted at numerous documents he had accessed on the Internet. That Mbeki had indeed accessed and downloaded material from the Internet before 28 October 1999, was confirmed a few days after Mbeki's speech by a media liaison officer working for the Presidency:

> 'The president [has] got a thick set of documents. He went into many sites, including the World Health Organisation's one. The president goes into the Net all the time,' she said. (Sulcas and Randall 1999)

Unfortunately, it is likely that it will never be fully known which specific documents Mbeki read to inform his view about the scientific debate about the safety of AZT.[123]

Whatever Mbeki read, it seems certain that he did so in isolation and without involving anyone with specialist expertise on the matter. The strongest indication for this comes from Mbeki himself, who explained to a group of international HIV/AIDS experts how he had come to discover the existence of an alleged scientific controversy about the cause of AIDS[124]:

> [...] I faced this difficult problem of reading all these complicated things that you scientists write about, in this language I don't understand. So I ploughed through lots and lots of documentation, with dictionaries all around me in case there were words that seemed difficult to understand. I would phone the Minister of Health [who is a medical doctor] and say, 'Minister, what does this word mean?' And she would explain. I am somewhat embarrassed to say that I discovered that there had been a controversy around these matters for quite some time. I honestly didn't know. I was a bit comforted later when I checked with a number of our Ministers and found that they were as ignorant as I, so I wasn't quite alone. (Mbeki 2000a)

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[122] A key factor that makes it so difficult to find out what Mbeki knew about the science around AZT is that the President did not make any more public comments on AZT until February 2000.

[123] From comments made from February 2000 onwards, it can be deduced that Mbeki read lots of documents, including reports from institutions like the World Health Organization (WHO), the Joint United Nations Programme on HIV/AIDS (UNAIDS) or the Centers for Disease Control and Prevention (CDC). It has also been reported that Mbeki sent Malegapuru Makgoba, the then head of the Medical Research Council, about 1500 pages of documents that were supposed to justify the President's scepticism about the safety of AZT (Cohen 2000). Mike Cherry (2009), zoologist and correspondent for Nature, recall receiving numerous documents downloaded from <URL: http://www.virusmyth.com> from Mbeki. It is, however, unclear, how many of these documents Mbeki read before 28 October 1999. Myburgh suspects that Mbeki only started to read widely on AZT and the aetiology of AIDS after giving his speech to the National Council of Provinces.

[124] The government's questioning of the causal role of HIV in the development of AIDS emerged as a public issue only in late February 2000. To a large degree, critical claims about the dangers of AZT are intertwined with the literature that challenges the HIV paradigm.

This statement makes it very clear that Mbeki read the scientific literature largely on his own. Although he claimed to have drawn on the 'medical expertise' of the Minister of Health, who had obtained a couple of medical degrees in the late 1960s and in the 1970s without ever practicing medicine, the Minister merely fulfilled the role of a dictionary for the 'complicated' medical terminology.

Taking the Periodic Table of Expertises (PTE) as a guide to classify Mbeki's expertise on matters around AZT it has to be concluded that he possessed, at best, Primary Source Knowledge (PSK). According to the PTE an actor should be classified as possessing PSK when s/he has read primary sources, *e.g.* scientific papers, on technical issues without any active immersion into the community of experts that produces the primary sources. This indicates that Mbeki could not draw upon specialist tacit knowledge in understanding the technical arguments made by AZT sceptics.[125] As will become clear in the course of the chapter, the social distance between Mbeki and the expert community which comes with exclusive reliance upon written materials is a significant factor. It is significant because it influences Mbeki's ability to justify his belief that the claims of AZT sceptics had enough credibility to constitute a scientific controversy.

Mbeki's public justifications

It has been established in the previous section (6.2) that anyone can judge the credibility of technical arguments. The difference between those with sufficient levels of specialist expertises, *e.g.* interactional and contributory expertise, and those without them is that the former can base their judgements on technical considerations, *e.g.* they should be able to assess the content of technical arguments. In contrast, lay persons cannot use technical considerations, since they lack the understanding to do so.[126] They have to rely exclusively on non-technical or social considerations to judge formal aspects of technical arguments such as the claim-maker's "demeanour, the internal consistency of their remarks,

[125] Why this is the case will be explained in the next chapter when I outline the limitations of Primary Source Knowledge.

[126] In accordance with the Periodic Table of Expertises, anyone who has not acquired specialist tacit knowledge can be considered a lay person, even though the actor might have acquired specialist knowledge through reading primary sources.

the appropriateness of their social locations and so forth" (Collins and Evans 2007: 15).

The aim of the following analysis of Mbeki's public justifications for his belief that the safety of AZT was the subject of a scientific controversy is to establish on what type of meta-expertise Mbeki' judgement rested.[127] The analysis starts with Mbeki's initial justification for investigating the safety of AZT, which contains a few references to non-technical considerations:

> There also exists a large volume of scientific literature alleging that, among other things, the toxicity of this drug is such that it is in fact a danger to health. ... To understand this matter better, I would urge the Honourable Members of the National Council to access the huge volume of literature on this matter available on the Internet, so that all of us can approach this issue from the same base of information.

It is significant that Mbeki's initial statement on the dangers of AZT did not contain a technical justification. All that the President did was to tell the public that a 'large' or 'huge volume' of scientific literature existed, which alleged that AZT was excessively toxic. As there is no justification as to why this literature should be taken seriously, the implicit claim is that there must be something to those warnings about the toxicity of AZT, because a lot has been written about it.[128]

Mbeki only followed up publicly on his speech in an interview with the Sunday Times in February 2000. By this time, his view that there was literature that warned about AZT had been challenged by a range of scientists and commentators. Mbeki had reacted by sending documents, including the Papadopulos-Eleopulos *et al.* (1999) review paper, to Mike Cherry. In the interview, Mbeki highlighted specifically the value of the Papadopulos-Eleopulos *et al.* paper, which was crucial for his argument:

> The conclusion [of South African scientists] was: 'Therefore we don't know what this President is talking about.' I wrote to the lecturer [Michael Cherry] and said: 'You know, it's possible that you people haven't read any such articles, please

[127] One can, of course, argue that Mbeki really understood the technical arguments and the reason that he did not display this ability is due to the fact that his public audience could not have followed the thought process of the President. While this might be theoretically the case and cannot completely ruled out, the analysis in the previous sections makes such a scenario unlikely.

[128] It remains unclear what is meant by 'large' or 'huge volume' – an outsider might be impressed by 10 articles or 20 articles, while insiders recognise that a discourse might encompass thousands of papers.

> find enclosed an article published in 1999 [Papadopulos-Eleopulos *et al.*] in a very senior scientific journal. A very lengthy article with millions of references, presenting whatever that particular group of scientists thought about that matter.' There you have university people, professors and scientists who haven't read. (Robertson *et al.* 2000)

Again, Mbeki makes no attempt to judge the quality of the literature, in this case the Papadopulos-Eleopulos *et al.* paper, on the basis of technical considerations. Instead, Mbeki's judgement that the article has credibility and merits attention rests entirely on non-technical grounds. The President used quantitative markers such as the length of the article and also its (exaggerated) number of references to support his claim that the paper is important and should be taken seriously. Mbeki also described the journal in which the article appeared as 'very senior' in an attempt to transfer authority from the journal to an individual article.[129]

In another interview in April 2000, Mbeki justified his position of AZT by pointing out that he regarded the authors of the literature he was relying on as trustworthy and credible:

> But we also need in that context to answer the particular questions of toxic effect of this drug [AZT]. If you sit in a position where decisions that you take would have a serious effect on people, you can't ignore a lot of experience around the world which says this drug has these negative effects. (Shenton 2000)

There is, again, no evidence that Mbeki had assessed the merits of the arguments of AZT critics on a technical basis. Rather, he justified his trust in the credibility of the arguments provided by AZT sceptics by pointing to their 'experience.'

Later in the same interview, Mbeki was more specific why he thought that those who warned about AZT had 'a lot of experience':

> Certain things that one thought one knows - HIV equals AIDS equals death. One of the things that became clear, and which was actually rather disturbing, was the fact that there was a view which was being expressed by people whose scientific credentials you can't question. (Shenton 2000)

Mbeki equated experience with the possession of formal credentials. The quote suggests that Mbeki believed certain claims that contradicted mainstream beliefs because the alternative view was expressed by people with 'scientific credentials.' A few weeks after the interview appeared, Mbeki explained in more detail in a

[129] It remains, however, unknown which criteria Mbeki used to arrive at the judgement that *Current Medical Research and Opinion* was indeed such an important journal.

letter to fellow heads of state what kind of credentials AZT and AIDS sceptics possessed:

> The scientists we are supposed to put into scientific quarantine include Nobel Prize Winners, Members of Academies of Science and Emeritus Professors of various disciplines of medicine! (Mbeki 2000b)

To Mbeki the claim-makers had immaculate credentials. Prestigious prizes such as the Nobel Prize or other forms of peer recognition such as membership in national academies of science or high ranking positions within academic institutions which AZT critics had received suggested to Mbeki that the claims of those scientists should and could not simply be ignored.

The previous statement indicates that Mbeki was aware that the authors of the literature that informed his judgement were marginalised figures in the scientific discourse. The credentials of those marginalised figures provided just one reason for Mbeki to ignore calls that warned him about taking their claims seriously. Another reason for Mbeki to take the 'minority view' seriously was related to the way in which those critical scientists had been marginalised.

> We are now being asked to do precisely the same thing that the racist apartheid tyranny we opposed did, because, it is said, there exists a scientific view that is supported by the majority, against which dissent is prohibited. Scientists, in the name of science, are demanding that we should co-operate with them to freeze scientific discourse on HIV-AIDS at the specific point this discourse had reached in the West in 1984. (Mbeki 2000b)

According to Mbeki, AZT and AIDS sceptics have been unjustly pushed to the fringes by the scientific majority. He expresses the view that the reason for the marginalisation is not technical as the rules of scientific debate have apparently been violated. Instead of open debate, which allows different viewpoints to be discussed and tested, the discourse about the causation of AIDS was prematurely closed in 1984. Those who have been marginalised had never the chance to make their position heard. Their marginalisation can therefore not be the result of a fair assessment of their arguments' merits.

When Mbeki finally welcomed the members of the newly created Presidential Advisory Panel on AIDS in May 2000, he again justified the inclusion of marginalised AIDS and AZT sceptics by pointing out that their marginalisation cannot be justified in the first place:

> There is an approach which asks why is this President of South Africa trying to

> give legitimacy to discredited scientists, because after all, all the questions of science concerning this matter had been resolved by the year 1984. I don't know of any science that gets resolved in that manner with a cut-off year beyond which science does not develop any further. It sounds like a biblical absolute truth and I do not imagine that science consists of biblical absolute truths. (Mbeki 2000a)

It is unsurprising that Mbeki even compared the AIDS sceptics, and the AZT critics that battle with scientific dogma, with historic figures such as Galileo and Don Quixote:

> This other point of view, which is quite frightening, this alternative view in a sense has been blacked out. It must not be heard, it must not be seen, that's the demand now. Why is Thabo Mbeki talking to discredited scientists, giving them legitimacy? It's very worrying at this time in the world that any point of view should be prohibited, that's banned, there are heretics that should be burned at the stake. And it's all said in the name of science and health. It can't be right. (Shenton 2000)

> "By resort to the use of the modern magic wand at the disposal of modern propaganda machines, an entire regiment of eminent 'dissident' scientists is wiped out from the public view, leaving a solitary Peter Duesberg alone on the battlefield, insanely tilting at the windmills," he said. (Sulcas 2000)

Mbeki thus dismissed the majority view that HIV is the cause of AIDS and that antiretroviral drugs are therefore a potential solution to deal that has been demonstrated to be safe and effective due to this particular view of the development of the scientific discourse. For Mbeki, experts that did not conform to the quasi-religious views of the majority of scientists were treated as just as credible, or more credible, than those that did. Again, Mbeki justified a technical judgement with reference to social considerations, in this case a very particular version of the history of AIDS research that was mainly promoted by AIDS sceptics and critics of antiretroviral drugs.

Up to this point, the analysis appears to confirm that Mbeki based his judgement that the case presented by AZT critics was credible solely on non-technical considerations. This judgement informed another judgement, namely that a controversy about the safety of the drug was open and ongoing. At least on one occasion, however, Mbeki appeared to use technical arguments to justify his judgement regarding the credibility of the AZT critics' case. An article by John Jeter in the *Washington Post* quotes Mbeki while the President was on a visit to the United States in May 2000:

> This weekend, Mbeki defended his stance by displaying an impressive breadth of scientific knowledge, using terms more common to the head of a university biology department than to a head of state. "When you are dealing with a virus and you... put some drug into the human body, whatever antiviral agent comes into this particular cell, it has to... produce phosphorous particles, which are the things that have an impact on the virus," he said. But "science isn't even agreed upon that question," he continued. "Does such phosphoral relation [sic!] take place?" (Jeter 2000)

Mbeki explained to his audience that AZT only works if it is triphosphorylated within human cells and then claimed that science is not agreed upon whether such phosphorylation takes place. He thereby repeated claims made in the Papadopulos-Eleopulos *et al.* (1999) review paper. Can the fact-like reproduction of technical claims be taken as proof that Mbeki 'knew what he was talking about?' The answer has to be 'no'. The fact that Jeter praises Mbeki's scientific understanding should not be taken on face value. Of course, Jeter was impressed by Mbeki's use of technical terms which he did not understand, but the journalist is not in a good position to judge Mbeki's scientific understanding. The report by Jeter suggests only two things: first, Mbeki has read the Papadopulos-Eleopulos *et al.* paper and second, he was able to use certain technical terms to express a particular claim. What Mbeki's statement does not demonstrate is that the President was able to use the technical knowledge on 'phosphorylation' and apply it in novel contexts.[130] Only this would prove that Mbeki had acquired sufficient specialist tacit knowledge to count as an expert.

Apart from this exception, all the quotes attributed to Mbeki suggest that the President used non-technical (or social) considerations to justify his endorsement of the claims made by AZT critics. Only by taking AZT critics seriously was Mbeki able to declare the existence of a scientific controversy about the safety of AZT. Whether he was aware of it or not, by taking the critics seriously Mbeki also endorsed their technical arguments, while at the same time he inadvertently expressed doubt about the technical claims made by the scientific mainstream.

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[130] The statement does not demonstrate that Mbeki could have performed experiments to measure levels of phosporylation or whether phosporylation is important for other antiretroviral drugs than AZT. Collins and Evans (2007) have made a similar point in relation to 'beer-mat knowledge' about a hologram. The text on a beer-mat provides the reader with a rough description how a hologram is made, but without any further knowledge, this information does not enable the reader to make a hologram or to refine techniques used to produce holograms and so on.

Mbeki's Judgements of the State of the Scientific Discourse

The analysis of Mbeki's justification for taking the claims of AZT sceptics confirms the prediction made above, namely that he had to rely on non-technical or social considerations to assess the scientific literature and to judge whether AZT's safety was subject of a scientific controversy. According to the PTE, Mbeki relied on transmuted meta-expertise, which is a form of expertise that allows non-experts to "use social [or non-technical] discrimination to produce technical discrimination" (Collins and Evans 2007: 15). The PTE distinguishes between two types of transmuted meta-expertises: 'ubiquitous discrimination' and 'local discrimination.' The question that remains to be answered is which type Mbeki relied on.

It is unlikely that Mbeki relied on local discrimination as there are no indications of local or socio-metric proximity to experts or institutions that are involved in determining the safety of antiretroviral drugs or PMTCT. The President did not seem to have been personally or directly affected by side-effects of antiretroviral drugs. Indeed, Mbeki claimed in 2003 that he was not aware of anyone in his family who had died of AIDS or had been infected with HIV (Thom and Cullinan 2004). The analyses of Mbeki's biography has also established that he never worked within natural sciences and that he was never involved in conducting clinical trials or in undertaking toxicological research. It is therefore highly likely that Mbeki was never 'socio-metrically' close to knowledge that was relevant for establishing the safety of AZT.

This leaves ubiquitous social understanding as the only plausible basis for Mbeki's expertise. Collins and Evans (2007: 45-46) characterise this type of meta-expertise in the following way:

> [T]hose with little scientific knowledge can sometimes make what amounts to a *technical* judgment on the basis of their *social* understanding. The judgment turns on whether author of a scientific claim appears to have the appropriate scientific demeanor and/or the appropriate location in the social networks of scientists and/or not too much in a way of a political and financial interest in the claim. Ubiquitous discrimination is what we have all been learning since we could speak and is just a particular application of our regular judgments about friends, acquaintances, neighbours, relations, politicians, salespersons, and strangers, applied to science and scientists within Western scientific society. (Italics in original)

Both the analysis of Mbeki's technical expertise and the analysis of his justifications for taking AZT sceptics seriously yield results that are consistent with the characteristics of ubiquitous discrimination. It has been shown that Mbeki had a technical understanding of the matters around the safety of AZT that was limited to Primary Source Knowledge (PSK). More importantly, it has also been shown that Mbeki's public justifications for his belief that the safety of AZT was the subject of a scientific controversy were solely based on social considerations such as the volume of the sceptical literature, the formal credentials as well as the history of the marginalisation of AZT sceptics.

These credibility judgements informed Mbeki's view that, by October 1999, the safety of AZT was the subject of a scientific controversy. Given that this judgement was obtained exclusively by relying on ubiquitous discrimination, this raises questions about the quality of Mbeki's judgement of the state of the scientific discourse about the safety of AZT.

6.4 Mainstream Experts

Mbeki's judgement that the safety of AZT was the subject of a scientific controversy was not shared by many other actors who were involved in the technological decision-making process. Amongst those who most vehemently disagreed with Mbeki were scientists. Their assessment of the state of the scientific discourse can provide an interesting basis for a comparison with Mbeki's judgement. This is not only because they disagreed with Mbeki, but also because they were able to base their credibility judgements, in addition to social considerations, on technical considerations. The 'mainstream' scientists, whose justifications for the judgement that the safety of AZT was not the subject of a scientific controversy will be analysed in this section, represent a variety of expertises, ranging from contributory over interactional to referred expertises. Almost all of them have been more or less actively involved in the debate that was provoked by Mbeki's critical comments on AZT in October 1999. Thus, the focus of this section is on how technical experts representing the mainstream justify their belief that the safety of AZT was not the subject of a scientific controversy in 1999 and 2000.

On the basis of the analysis in the previous section, three different justificatory strands used by Mbeki can be identified. First, Mbeki explained why the literature he was reading was significant and relevant. Second, Mbeki also tried to explain why he had little choice other than accepting the AZT critics' view. The reason was that they appeared to be full-blown experts. Third, Mbeki explained his preference for their position by pointing to the unjust nature of their marginalisation. These three justificatory strands can be used to structure this section to facilitate the comparison between Mbeki and mainstream experts. Thus, the expert statements are organised in such a way that they address the three broad issues: (1) relevance and significance of literature, (2) expertise of AZT critics, and (3) history of AZT critics' marginalisation.

Relevance and Significance of Literature

While Mbeki was impressed by the sheer quantity of critical publications with regard to the safety of AZT, actors with interactional and/or contributory expertise on the matter assessed this literature differently. For example, Malegapuru Makgoba, immunologist and the Chairman of the South African Medical Research Centre (MRC), told *Science* reporter Jon Cohen what he thought of the literature that Mbeki was relying on:

> In January [2000], Makgoba says Mbeki sent him about 1500 pages of documents that question the so-called 'HIV/AIDS hypothesis'. "It's pure rubbish," says Makgoba. "They never provided any data and, at the same time, they are taking things out of context." He told Mbeki as much in a letter that also offered detailed counterarguments. (Cohen 2000: 590)

Makgoba, unlike Mbeki, did not take the existence of more than a thousand pages of documents as an indicator that its content had any credibility. Instead, Makgoba appeared to be more interested in 'data.' He was also able to recognise that critics of AZT construct negative narratives by using de-contextualised data and quotes from genuine scientific literature.

Mbeki's specific evaluation of the Papadopulos-Eleopulos *et al.* review article on the pharmacology of AZT can also be contrasted with evaluations from actors using other types of meta-expertise. Together with many documents downloaded from the virusmyth.com website, Mbeki sent this article to zoologist and Nature correspondent Mike Cherry in January after the later had published an article in Business Day that was critical of Mbeki's position on AZT. Cherry

(2009: 23), who has no direct expertise in pharmacology, but has experience in writing scientific review articles, made the following observations while reading the article:

> [I]n reading the article, I realised that it did not present original research findings but purported to synthesise and review the findings of workers in the field. The literature, which was already vast, had been highly selectively reviewed. I did a search and found at the time 6472 peer-reviewed articles available on AZT. The authors had ignored almost all of the articles written over the previous five years (in other words, since AZT had been widely used in MTCT) – a quite astonishing omission.

Cherry's assessment concentrates on different factors than Mbeki's and concludes that the Papadopulos-Eleopulos *et al.* article is not a credible publication. Specifically, Cherry mentions the overall character of the publication and points out that the authors themselves have not contributed any substantial research as the publication is a review article. He also notes that the original sources cited in the review are neither contemporaneous nor representative. To Cherry, who, having published review articles in zoology, might be regarded as someone with 'referred expertise' with respect to the formal aspects of scientific reviews, these considerations are relevant and significant for assessing the credibility of the paper in the absence of direct technical understanding.[131] Characteristics such as the length of an article and the number of footnotes, which were highlighted by Mbeki, appear to be of no interest to someone with experience in writing and reading reviews.

Cherry also disagrees with Mbeki's assessment that the journal in which the Papadopulos-Eleopulos *et al.* article appeared was 'very senior.' Cherry states that he has "never even heard of the 'very senior scientific journal *Current Medical Opinion and Research*, to which Mbeki was referring, which was unsurprising as it had a very low impact factor" (Cherry 2009: 23). While the reliability of quantitative measures of quality such as the 'impact factor' is debatable, the fact remains that a scientist such as Cherry, even though his background is in zoology, knows about it and mentions it. Similarly, South

[131] Cherry uses referred expertise since he is able to inform a credibility judgement related to pharmacology through expertise gained in zoology. Note, however, that it is not 'technical expertise' gained in zoology that informs Cherry's judgement. His dismissal of the review is based on formal aspects that are 'non-technical.' How this can be interpreted will be explained in the next section.

African health economist Nicoli Nattrass, whose publication record also points to experience with respect to publishing in scientific domains, turns to the impact factor in her assessment of the credibility of *Current Medical Research and Opinion*:

> "[T]his journal [*Current Medical Opinion and Research*] was cited by other journals a total of 1,148 times in 2004. Compare this to the number of times the top medical science journals get cited: for example, the *New England Journal of Medicine*, 159,498 citations; the *Lancet*, 126,002 citations; and the *Journal of Immunology*, 108,602 citations." (Nattrass 2007: 58)

While it remains unclear on which criterion Mbeki based his assessment of the journal's quality, Cherry and Nattrass base their respective judgements on the 'impact factor' of the journal. On the basis of this measurement, they conclude that *Current Medical Research and Opinion* is a publication on the fringes, even though they might not be able to completely comprehend the content of the paper.

While Cherry's expertise, gained through immersion into the community of zoologists, allowed him to assess the formal aspects of the Papadopulos-Eleopulos *et al.* review, he had to consult with pharmacologist Gary Maartens and virologist Carolyn Williamson to make judgements about the content of the paper[132]:

> [W]e pointed out [in their report to Thabo Mbeki] that since the early nineties AZT has been more commonly used in combination therapy with other drugs. The early AZT monotherapy studies were conducted before the development of the techniques currently available to quantify the virus accurately in humans (in vivo). We drew Mbeki's attention to two recent studies (which, needless to say, had been ignored by the review written by the dissidents) that had clearly documented a decrease in HIV-1 RNA in vivo in association with the administration of AZT alone.

Both Maartens and Williamson have probably contributory, but at least interactional expertise on aspects related to the antiretroviral treatment of HIV/AIDS. It is their particular familiarity with the scientific discourse on the issue at hand that enabled them to refer Mbeki to other studies that potentially refute certain claims made by Papadopulos-Eleopulos *et al.* Based on their technical expertise on the matter, Maartens and Williamson had no reason to believe that the Papadopulos-Eleopulos *et al.* review merited any attention as its

[132] This is in itself a significant point. By consulting with contributory experts, Cherry admits that his expertise is insufficient to assess the paper technically. Mbeki seemingly lacked this kind of epistemic humility as he read the paper on his own.

core claims were outdated and contradicted by more recent and advanced measurements.

While the material presented above is certainly limited in volume, a few provisional conclusions can already be drawn from the analysis. Only some of the justifications for rejecting the claims made by AZT critics are based on technical considerations. For example, Makgoba bemoans the quality of sceptics' data, while Maartens and Williamson reject certain claims because they have been generated on the basis of outdated techniques. In contrast, Cherry and Nattrass, both classified as having probably referred expertise, use non-technical or social considerations to inform their judgement of the credibility of the Papadopulos-Eleopulos *et al.* paper. This is not surprising given that they could only rely on understanding gained in other scientific disciplines. The interesting aspect is, however, that their judgements are *transmuted*, which means they are based on social considerations. For example, they judge that the Papadopulos-Eleopulos *et al.* review has no credibility, because it is published in a fringe journal and because the reviewed literature is incomplete and outdated. These are non-technical considerations and in this sense similar to Mbeki's judgements.

A possibly significant difference between the transmuted judgements of Mbeki on the one side and Cherry and Nattrass on the other side begins to emerge. It appears as if the social proximity to or the familiarity with scientific domains (even if they are not the ones from which technical claims emerge) enables those with referred experts to take non-technical considerations into account that might not be accessible to complete outsiders such as Mbeki. For example, those with experience in scientific domains were not simply impressed by the fact that some belief had been written up and published in a scientific journal. The quality of scientific journals varies and it is a marker of quality and credibility to publish in one of the leading journals in a field. The experts, in contrast to Mbeki, were aware of this and both Cherry and Nattrass compared impact factors to make this very point.

Expertise of AZT Sceptics

Mbeki's assessment of the expertise of AZT sceptics was not based on personal knowledge of them. As shown in detail above, Mbeki had not met any AIDS

sceptic personally before 28 October 1999. It is therefore unsurprising that Mbeki's assessment of their expertise was based on the 'rough-and-ready' criterion of 'formal credentials.'

In contrast, those with scientific expertise appear to be reluctant to accept formal credentials as a sufficient marker of experience and credibility. One senior South African HIV/AIDS researcher made this point quite explicitly with reference to Mbeki, but also to her/himself, during an interview:

> [H]e's [Mbeki] got a Master's in Economics... a master's, you can get a shitty master's and you can get a very good master's, it doesn't make you an expert, not at all. I have a master's in [names a discipline in medicine], it doesn't make me an expert, I can tell you that, I haven't done much for years.

The interviewee points out two things. First, a formal title does not tell anything about the quality of the education that lies behind the credential. A masters acquired at a 'bad' institution is worth less than a masters acquired at a very good university. Second, for formal credentials to be linked to experience and expertise requires continuous input. In other words, without continued learning, expertise can be lost. Formal credentials are static as they are awarded at some specific point in time. Expertise and experience are, however, dynamic as they have to be continuously nurtured.

While the formal credentials of some of the AZT critics are incontrovertible, experts familiar with the ongoing research in the field, look beyond the formal markers such as academic titles and credentials. A general and relatively sweeping view on the 'expertise' of AIDS and AZT sceptics, which is widely shared within the scientific mainstream, has been offered by Seth Kalichman, a social psychologist and editor of *AIDS and Behaviour*. As far as I am aware, Kalichman was not directly involved in the South African AZT debate in 1999 and 2000, but has recently published a book on 'AIDS denialism' (Kalichman 2009a).[133] In a contribution for the *New Humanist*, he characterises AIDS sceptics in the following way:

> AIDS denialists rely on a small band of fake experts, mostly retired academics who proclaim that HIV does not cause AIDS. There is not a single instance of an "expert" offered by AIDS denialism that has ever actually done research on AIDS. In rare examples, denialist experts have a history of credible science only

[133] While Kalichman has published these remarks recently, they were as valid (or invalid) 10 years ago as not much has changed over the last decade.

> to have later gone off the deep end. The most credentialed AIDS denialists are Nobel Laureate Kary Mullis, who developed the PCR [polymerase chain reaction] technology for sequencing the genetic code, and Peter Duesberg, Professor of Biochemistry and Molecular Biology at the University of California-Berkeley and member of the National Academy of Science. Although credentialed, neither is credible. Aside from saying HIV cannot cause AIDS, though he has done no research on AIDS, Mullis has shared his experiences on LSD and encounters with an alien fluorescent raccoon, and Duesberg, who did important work on cancer in his early career, now claims that there is no genetic basis for any cancer. Both have demonstrated an outright disregard for scientific evidence. (Kalichman 2009b)

Kalichman's account focuses on the limits of formal credentials. While Kalichman, whose detailed research on 'AIDS denialism' has probably provided him with interactional expertise on various aspects of HIV/AIDS sciences, accepts that AIDS sceptics can have impressive formal credentials, they have nothing to show for it in terms of research experience related to HIV/AIDS or continuous contact with the scientific community. His account also demonstrates again that a greater degree of personal knowledge, that allows looking past formal credentials and titles, can have a strong effect on the judgement of actors' expertises.

As Kalichman mentions, one of the best known critics of AZT and the HIV paradigm is Peter Duesberg, who is also regarded as one of the founding fathers of retrovirology. One of my interviewees, a scientist who is personally acquainted with Duesberg, told me about a chance encounter with Duesberg in 2000, which also says something about the interviewee's view on Duesberg's expertise:

> [J]ust prior to that [the first meeting of the Presidential Advisory Panel on AIDS in May 2000] I was in a lift in Pretoria, an elevator, with Duesberg. He was staying in this hotel and I was there for some events unrelated to this... I think. [...] maybe he came a day or two before for meetings. I remember this peculiar feeling: here I was a scientists working with the virus, which I was at the time and here was the denialist and a giant in retrovirology: Peter Duesberg. So, that was a funny feeling, I did not greet him, just kept quiet.

The expression of admiration for the achievements of Duesberg is obvious from the reference to Duesberg as 'a giant of retrovirology.' This judgement is, however, confined to the past, since my interview partner also refers to Duesberg's then present identity as 'the denialist.' It is some degree of personal knowledge or at least familiarity with the work of Duesberg's that enables this scientist to paint a nuanced picture of Duesberg's expertise and allowed to differentiate between the 'early, well respected retroviriologist' and the 'later, discredited AIDS denialist.' Interestingly, no reference is made to Duesberg's

formal credentials as 'Professor' and 'member of the American Academy of Science.'

In general, assessing the expertise of actors does not require technical knowledge as this only requires a social judgement. Interestingly, Mbeki, an outsider to science, relied on 'formal credentials' which are the most visible and easily obtainable markers of expertise and credibility. Whether he was aware of limits of 'formal credentials' when they are used as a 'meta-criteria' to asses someone's expertise is unknown. What appears to be clear, however, is that his outsider status did not allow Mbeki to use any meta-criteria for expertise which required greater familiarity with AZT sceptics' work.

The contrast to experts' judgements of the expertise of AZT critics is pronounced. The analysis shows that they do not equate formal credentials with expertise. Experts, it appears, in assessing the expertise of AZT sceptics look beyond credentials and concentrate on meta-criteria such as track-records or research experience. This is, of course, only possible because they are relatively acquainted with AZT sceptics and their respective contributions. While some, such as Kalichman, might acquire this familiarity by doing detailed and targeted research on AZT and AIDS sceptics, it is arguable that contributory experts pick up such information through their routine immersion into the research community.

History of the Marginalisation of AZT sceptics

The previous section illustrates Mbeki's reliance upon a very peculiar history of the establishment of the HIV/AIDS paradigm to justify why he listened to 'experts' that no one else appeared to take seriously. Mbeki invoked a historical narrative according to which all debates around HIV/AIDS ended in 1984. Thus, the marginalisation of AIDS sceptics and subsequently of AZT sceptics was unjustified as they had no opportunity to present their technical arguments.

It has to be pointed out that Mbeki's version of how AIDS and subsequently AZT critics have been marginalised might be rational, in the sense that it provides him with a coherent justification for taking AZT sceptics seriously; it is not in accordance with the historical facts.[134] The two quotes from an interview with a

[134]According to Epstein, who provides a detailed history of the establishment of the HIV paradigm in the first part of *Impure Science* (1996), Mbeki's version of history has little to do with the actual

scientist that will be discussed below are not intended to contest Mbeki's history of HIV/AIDS research, although they show that things were more complex than Mbeki appeared to believe. Instead, what these quotes are mainly intended to demonstrate is the qualitative difference between non-technical considerations invoked by full-blown experts compared to non-technical considerations invoked by non-experts such as Mbeki.

For example, the interviewee was located in the USA at the time of the announcement that a virus was the probable cause of AIDS and followed the debate about who actually discovered the virus very closely; a debate, incidentally, which does not feature at all in Mbeki's history of the discovery of HIV:

> [I was] in US when all these battles [about who discovered the virus] were going on and it took about 3 years or 4 to resolve and so on. It was very interesting, the person who actually discovered HIV gets very little credit, it is a virologist in Montagnier's lab called Francois Barre Sinnoussi and the other person who had been working very closely with Francois was the vaccine person – I'm blanking on his name. He was at the CDC [Centers for Disease Control and Prevention]; he was the one that did the first vaccine trial. I heard from Francois and from the CDC guy that the CDC had uncovered these cases in Africa while there have been these cases in the US and how they were exchanging. And Gallo phones him at the night and shouts at him, because he sent specimens to Montagnier's lab, how could he sent the specimens there, he is the virologist and he will find it [the cause for AIDS]. I have this public knowledge and I have this inside information from that people directly involved at that stage.

Besides the fact that the interviewee claims that it took three or four years for this controversy about who discovered the virus to resolve, the most interesting part of this quote is the last sentence. There the interviewee explains how s/he knows about this. Part of the explanation is that s/he personally knows most of the people involved. Unlike Mbeki, who had to rely on a history of HIV/AIDS that

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events. It would be relatively easy to show on the basis of the material provided by Epstein that the simplistic history presented by AIDS critics and Mbeki is without substance. Alternatively, it is enough to check the list of papers published by Peter Duesberg to see that his first tentative challenge to the HIV paradigm came in 1987. In 1988 he was given the opportunity to publish his views in *Science*, the same journal that published four papers by Robert Gallo in 1984 which are usually regarded as the first published evidence for the HIV theory. With regard to 'science was frozen in 1984' claim, it is interesting to note that a document compiled and regularly updated by the US National Institute of Allergy and Infectious Diseases (2010) entitled "The Evidence that HIV Causes AIDS: An NIAID Fact Sheet", which is usually referred to as the definitive document that contains the scientific evidence that HIV causes AIDS does not mention the public announcement of a viral theory in 1984 and makes no claim to instantaneous discovery. Instead it provides a range of arguments as to why the HIV theory is plausible and underpins this with scientific evidence. Interestingly, hardly any of the scientific papers that support the HIV theory has been published in 1984 (see also Iliffe 2006, Whiteside 2008).

he accessed through reading particular papers which were highly critical of the HIV paradigm, the interviewee was there and was able to ask the people involved for their individual versions of events.

The other passage taken from the same interview explicitly contradicts the version of history represented by Mbeki and AIDS critics. The scientist told me how s/he was introduced to the perspective of Duesberg in 1988. Again, her/his personal involvement is crucial:

> [W]hile I was in the US in 1988 the antibody test had just been developed and there was all this controversy about who discovered HIV, was it Gallo or was it Montagnier? ...studying there [in the USA] I participated in these seminars, so one week we had Montagnier on campus that was at [names an American university], then we had Gallo there, then we also had Duesberg there. And that was the first I heard about this dissident voice, I was very intrigued and in fact in one of my classes the task was that we answered the question: Does HIV cause AIDS?

By studying in the USA in 1988, the interviewee had the chance to meet and discuss with some of the most famous AIDS researchers at that time. It was through the immersion into the academic world that s/he became aware of Duesberg, who was allowed to present his challenge to the HIV paradigm at a prestigious US American university in 1988. Moreover, students were then encouraged to question the HIV paradigm for themselves. This clearly shows that open debate about the causes of AIDS occurred well after the alleged cut-off point in 1984. The direct and personal involvement of this scientist in the developing discourse around HIV/AIDS allows her/him to dismiss the claim that AIDS sceptics were never listened to, which was vital to Mbeki's credibility assessment.

The contrast between Mbeki as an outsider and the insider quoted in this part of the section with regard to the history of HIV/AIDS research is stark. Mbeki invokes a sterile history of HIV/AIDS research that does not provide any details and which suggests that an important and consequential claim such as the AIDS is caused by a virus was immediately accepted by everyone in the field.[135]

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[135] It seems relatively clear that Mbeki got his 'history lessons' from AIDS and AZT sceptics. For example, Peter Duesberg and David Rasnick (1998) write in an article in *Genetica*, which Mbeki had apparently read: "On April 23, 1984, the Secretary of Health & Human Services (HHS), the directors of the CDC and NIH, and the NIH's virus researcher Robert Gallo announced jointly at an international press conference in Washington that a previously unknown virus, since called HIV, was the 'probable cause of AIDS.' The only evidence for this claim was the presence of antibody against the 'AIDS virus' in most, but not all, AIDS patients that Gallo and his collaborators had analyzed. Nevertheless, the 'AIDS virus' was accepted *without delay*, and

In contrast, the little historical 'snippet' provided by the first quote alone is much richer and 'thicker.' It mentions names, tells of rivalries between laboratories and, most importantly, also points out that it took a few years before the question as to who discovered the AIDS-causing virus had been resolved. The second quote destroys the notion that no dissent to the 'dogma', *e.g.* to the belief that HIV was the cause of AIDS, was allowed after 1984.

As recognised already further above, this difference between insider and outsider accounts appears to be related to the relative social proximity of experts to the developments in a specialist domain compared to outsiders. By being socially distanced, Mbeki took a version of history presented by AIDS critics at face value. 'Experienced insiders', in contrast, do not necessarily have to read a historical account as they have lived the history of the field. They 'know' through having 'taken part' and having spoken to other insiders about certain events.

Experts' Judgements of the State of the Scientific Discourse

The analysis of the justifications used by mainstream experts reveals that they use technical as well as social considerations to justify their rejection of technical claims made by AZT sceptics. The bulk of the empirical material presented in this chapter shows how experts use social considerations to inform their judgements that the safety of AZT was not the subject of a scientific controversy. According to the Periodic Table of Expertise (PTE), this indicates that experts, whether they are 'contributory', 'interactional' or 'referred', at least partly rely on social considerations to inform their judgements.

The analysis has brought to the fore an important difference between the accounts of Mbeki and experts. This difference is related to the fact that the Mbeki was socially distanced from the expert community, while the mainstream experts were either part of the expert community or were at least relatively 'close.' The difference in social proximity is significant because it is clearly and systematically reflected in the ways Mbeki and experts respectively justify their

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without review, even before it was published in a scientific journal" (emphasis added). Many similar statements can be found in documents written by AIDS sceptics on <URL: http://www.virusmyth.com>.

differing conclusions about the credibility of the AZT sceptics claims and ultimately about the existence of an ongoing scientific controversy.[136]

Mbeki, as someone who relied exclusively on written material, had no or only very limited social contact with those who were actively involved in the scientific discourse about the safety of AZT and related technical issues. The social distance influenced Mbeki's social understanding and explains a number of obvious 'mistakes.' It explains, for example, why he considered *Current Medical Opinion and Research* as a 'very senior scientific journal', when it is clearly not according to its impact factor. It also explains Mbeki's faith in formal credentials as a reliable marker of experience and credibility, when the poverty of credentials as a marker for experience and credibility can be easily demonstrated and is openly acknowledged by experts. It also explains his reliance on a history of HIV/AIDS research that is out of tune with detailed historical accounts.

While this does not necessarily mean that insiders' judgements based on social understanding are correct, it is safe to argue that experts qua their active immersion into a specialist domain have a more detailed and fulsome understanding of what is going on in an expert community. For example, experts, either through writing them or through reading a lot of them, *know* what a credible review of the literature looks like. They also *know* which journals to take serious and which to ignore. They also *know* that a formal degree does not make one an expert as one of my interview partners has so colourfully demonstrated in a reflexive account. Experts *know* that 1984 was not the year in which the HIV paradigm was established as an absolute and unchallengeable truth, because they were involved in debates about this issue at that time. Experts also *know* that there have been long-standing debates between mainstream scientists and mavericks which have achieved nothing apart from creating anger and frustration, because they or some close colleagues might have debated with sceptics themselves.

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[136] Rob Evans has kindly pointed out that this represents an interesting reversal of the principle of 'distant lends enchantment' (Collins 1985). While normally certainty increases the further away one moves, socially speaking, from the production of scientific knowledge, in this case uncertainty increases. The experts were very certain that AZT sceptics' claims were not only unfounded but wrong. In contrast, Mbeki, by being distanced, actually 'lost' enchantment.

Once the difference in acquaintance with social aspects related to technical domains between Mbeki and experts has been recognised, the explanation for it is rather obvious. SEE postulates that actors to become experts have to acquire domain-specific tacit knowledge through immersion into an expert community. Immersion into an expert community does not, however, only aid the transfer of tacit knowledge of a scientific or technical nature. Through immersion, actors also come to be acquainted with social knowledge about colleagues and institutions. By attending conferences, workshops, lectures, and so on – in general by mixing with colleagues – one learns about the personal demeanour and character of others in the field.

6.5 Domain-Specific Discrimination (DSD) as a New Type of Expertise?

How can the empirical findings presented in this chapter be interpreted in light of the Periodic Table of Expertises? The findings of the previous section can be summarised in three points:

1. those with higher levels of specialist scientific expertise are able to make credibility judgements on the basis of technical considerations (although only a few examples have been presented)
2. those with higher levels of specialist scientific expertise in relevant disciplines are also able to make credibility judgements on the basis of social considerations
3. these social considerations can be related to the experts' immersion into the community that forms around a particular scientific domain

The first point does not represent a challenge to the Periodic Table of Expertise PTE). In fact, observations of this kind are to be expected according to the PTE as it indicates that those with interactional or contributory expertise have enough understanding to make technical judgements on the basis of technical considerations.

Point 2 and 3, in contrast, represent something of an anomaly for the PTE in its current form. The observation that experts make technical judgements on the basis of social considerations that reflect their social proximity to the expert

community might be described as representing a form of 'discrimination' that is based on domain-specific *social* knowledge, which can be referred to as 'domain-specific discrimination' (DSD).

The argument that DSD represents an 'anomaly' is based on the fact that it does not readily fit into the PTE. DSD clearly has some affinity with transmuted meta-expertises as this form of meta-expertise is characterised as making technical judgements on non-technical grounds. In particular, there is apparent similarity between DSD and local discrimination. Local discrimination has been described as making technical judgements on the basis of social considerations which are shaped by socio-metric proximity to an issue. An example used by Collins and Evans (2007) refers to the ability of people who have lived for some time in close proximity to a risk facility such as a nuclear power station to judge safety pronouncements issued by the management of the facility differently to people who do not live close by. Collins and Evans argue that those living close by are in a better position to compare the reality with the safety statements issued by the management. They might, for example, have 'heard it all before', *e.g.* they might have listened to many assurances issued by the management, but are acutely aware that these are just words that have, in the past, never been followed up by deeds.

While there is some affinity, DSD does not fully fit into the transmuted meta-expertise category. The problem is that Collins and Evans (2007) reserve this type of judgement for actors who lack any relevant specialist expertise. In other words, transmuted meta-expertises, *e.g.* ubiquitous discrimination and local discrimination, are associated with lay persons. Now this could, of course, be simply changed. There is, however, a good reason not to do this. As pointed out, DSD appears to be a by-product of immersion into an expert community. Actors who actively immerse themselves in expert communities not only pick up specialist tacit and explicit knowledge about technical aspects that concerns the expert community; through immersion into a community they also pick up tacit and explicit knowledge about social aspects related to the expert community itself. DSD might therefore be described as the 'social' equivalent of technical expertise. DSD is therefore closely related to interactional and contributory expertise. From

this it follows that DSD should be located somewhere in the 'specialist expertises' row of the PTE.

This is supported by another consideration. If one forgets for a moment that DSD appears to be *new* to the Periodic Table of Expertise, one realises that DSD is a rather 'old news' to Wave 2 of science studies. As has been shown further above, Shapin (1995), for example, is fully aware that credibility judgements can be informed by 'all aspects of the scene.' Moreover, Collins and Evans (2007) themselves provide a perfect example of DSD in *Rethinking Expertise*. Footnote 10 on page 50 reads as follows:

> Gravitational wave scientists report that they use the following criteria to judge whether an experiment by another scientist needs to be taken seriously: [1] Faith in experimental capabilities and honesty, based on a previous working partnership; [2] Personality and intelligence of experimenters; [3] Reputation of running a huge lab; [4] Whether or not the scientist worked in industry or academia; [5] Previous history of failures; [6] 'Inside information;' [7] Style and presentation of results; [8] Psychological approach to experiment; [9] Size and prestige of university of origin; [10] Integration into various scientific networks; [11] Nationality.
> The first, second, sixth, eighth and tenth item could be said to be matters of local discrimination with the remaining items matters of ubiquitous discrimination. (Collins and Evans 2007: 50, fn 10 [numbers added by MW])[137]

This describes some findings from Collins' extensive research of physicists' efforts to detect gravitational waves (see Collins 2004a). The experiments conducted by physicists suffer, as any other new experiment, from the so-called experimenter's regress. The problem is the following: to know whether an experiment has uncovered a new phenomenon, one has to know that the experiment has been performed properly, but to know whether the experiment was a proper one, one has to know that its outcome is correct, which is impossible if the outcome is supposed to be a new, yet unknown phenomenon. Collins (1981b) therefore concludes that experimental data alone are insufficient to overcome the 'interpretive flexibility' of experimental data.

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[137] Interestingly, Collins and Evans are at that point inclined to refer to this kind of knowledge as instantiations of transmuted expertises. Once these kinds of criteria are compared to scientific outsiders, the non-ubiquitous character of some of the criteria classified as referring to ubiquitous discrimination becomes more pronounced. While criterion 11 is probably ubiquitous as it is nowadays a matter of seconds spent on the Internet to find out about a scientist's nationality, criterion 7, style and presentation of results, is ubiquitous only to members of a close-knit esoteric community.

In the case to which the footnote discussion refers, one experimenter, Joe Weber, claimed that he had detected gravitational waves and that he had 'data' or 'experimental evidence' to support this claim. Other gravitational wave physicists, however, have interpreted these data very differently. Instead of representing a gravitational wave, the data have been interpreted as 'noise' or some other unrelated disturbance. As the same experimental output can be interpreted in different ways, the experiment as such became a non-decisive factor. The problem the community of gravitational wave physicists faced was which interpretation to believe. What Collins and Evans describe in the footnote quoted above are the controversy-deciding factors that have been recorded by Collins. Not one of them is 'technical.' In fact, Collins (1981c) argues that the debate was decided against Weber due to the successful use of 'rhetorical and presentational devices' which made Weber's claims appear less credible and therefore limited 'interpretative flexibility' to such a degree that the claim 'no gravitational waves have yet been detected' became accepted and consensual knowledge within the domain.

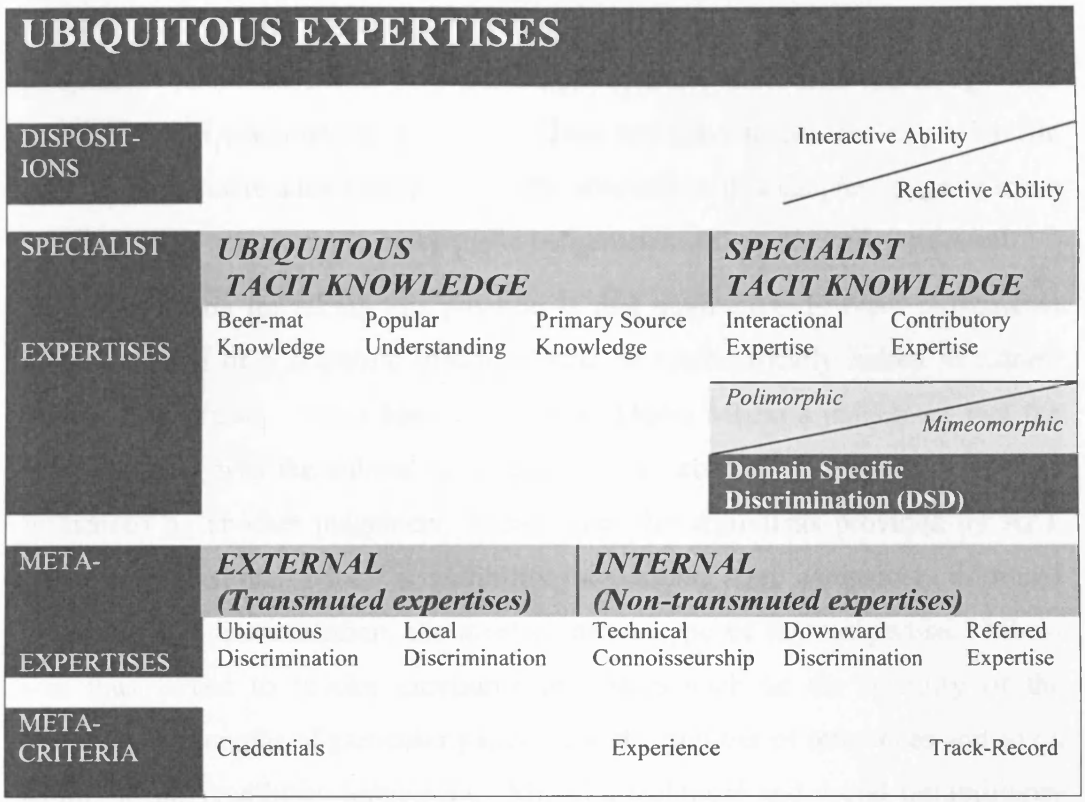
The reasons used by full-blown experts to decide whether to believe a certain technical claim or not are not simply 'social', 'non-cognitive' or 'non-technical.' Apart from the 'nationality' criterion, the use of any of the criteria listed in the footnote in a judgement process requires some degree of intimate knowledge of the expert community and its immediate surroundings.[138]

On the basis of the foregoing discussion, the argument is put forward that DSD represents a distinctive type of transmuted expertise, which is 'parasitic' to contributory, interactional and, to some extent at least, referred expertise. It is therefore suggested to amend the PTE in the following way to make sure that the qualitative difference between DSD and transmuted expertises such as ubiquitous

[138] An imitation game ought to be able to show this. Until now, imitation games are only used to test interactional expertise on technical matters. It might be feasible, however, to widen the scope of imitation games to test whether participants also have domain-specific social understanding. Judges could be instructed to ask questions that try to elucidate how participants would choose between two mutually exclusive technical claims.

and local discrimination, which are associated with lay people, is explicitly represented. An amended version of the PTE is represented here[139]:

Figure 5: Amended Version of the Periodic Table of Expertises



One addition has been made, namely to the 'specialist expertises' row, where DSD adds a social or non-technical dimension to expertises linked to specialist tacit knowledge. Further research has to establish whether DSD can also be associated with forms of internal meta-expertises.[140] It is, for example, feasible that technical connoisseurship also involves DSD as this refers to 'expertise in consumption' or 'expertise related to outcomes.'

[139] This amended version is the result of discussions in a KES seminar in mid 2010. I am grateful for the collective input by those who attended.

[140] Based on the limited evidence presented in the chapter, *e.g.* the examples involving Cherry and Nattrass, one might be inclined to add DSD also to 'referred expertise.' For the moment, this will not be done. Nonetheless, as the example of Cherry's assessment of the Papadopulos-Eleopulos *et al.* paper has shown, referred experts are able to apply their understanding gained in one field fruitfully in another field. I will revisit these cases in the next chapter, when another type of expertise is introduced that might be more fitting for referred experts.

6.6 Conclusions

This chapter has been concerned with the question as to whether the problem of expertise also applies to meta-questions. In less abstract terms, what has been asked is whether just anyone should be allowed to pass policy-process-relevant judgements about the status of a particular scientific discourse. Although it is recognised and acknowledged that one does not need to possess any scientific knowledge to make such a judgement, the analysis in this chapter suggests some restrictions on who ought to make those judgements if they are policy-relevant.

The reason for taking this position is that qualitative different judgements about the state of a scientific discourse can be systematically linked to actors' levels of expertises. It has been shown that Thabo Mbeki's judgement that the safety of AZT was the subject of a scientific controversy in October 1999 was influenced by another judgement, namely that the arguments provided by AZT critics were credible. Mbeki's credibility judgements were exclusively informed by ubiquitous discrimination, the most common type of meta-expertise. Mbeki was thus forced to invoke unreliable indicators such as the quantity of the literature, the lengths of particular papers, and the number of references and so on to inform his credibility judgement. Mbeki's technical and social unfamiliarity with the subject matter and its context can explain the comparatively low quality of his credibility judgements.[141]

In comparison, mainstream scientists' judgement that the safety of AZT was not subject of a scientific controversy in 1999 and 2000 was based on a mix of technical and social considerations, *e.g.* a combination of specialist technical expertises and DSD. Scientific experts were able to rebut certain technical arguments by invoking concerns about underlying methods, the quality of procedures that supported those arguments or more recent research. Experts were also able to base their assessment on a wide variety of social considerations ranging from assessments of the quality of AZT critics' arguments on the basis of where and how they published their arguments to rather detailed assessments of AZT critics' expertise. Scientific experts were able to provide judgements of the

[141] A sign of the relative 'low quality' of Mbeki's credibility judgement is the fact that most of them are not only easily contestable, but also refutable as the accounts provided by the mainstream experts shows.

state of the scientific discourse about the safety of AZT that were of a higher quality than Mbeki's judgement because they were based on specialist expertises as well as on domain-specific discrimination, which are rich in domain-specific tacit knowledge.

The discussion, while limited in scope, has shed some light on the issue of legitimate participation in making policy-relevant judgements of the state of scientific discourses. A number of tentative conclusions can be drawn from it. First, actors whose judgement relies exclusively on ubiquitous discrimination ought to be excluded. The reason is that ubiquitous discrimination is available to anyone, whether they are lay-persons or experts. Ubiquitous discrimination is therefore superseded, not complemented, by higher levels of expertise. The analysis shows that all the information available to President Mbeki were also available to the mainstream experts, but discounted by the latter. Technical experts were aware that critical literature existed, that some AZT critics had scientific degrees and they also knew about historic developments in the field. Everything that Mbeki understood, experts seemed to understand much better. Thus, with regard to those relying exclusively on ubiquitous discrimination, at best, adds nothing new to the process and, at worst, diverts attention away from genuine problems and controversy related to technological decision-making.

Second, the analysis has also presented good reasons to support the inclusion of specialist experts. Their familiarity with the technical issues under consideration as well as their social proximity to the community of experts provides them potentially with rich insights that can give rise to high-quality credibility judgements.[142] This must *not* be taken to mean that only technical experts ought to make credibility judgements. First, this would be in conflict with the findings of Wave 2, which show no specific actor group has a monopoly on information that might prove relevant for the judgement of credibility. Second, credibility judgements of technical experts, while of higher quality, might not

[142] Whether those relying on referred expertise should be included or excluded will be left open here. There is a good reason to exclude them, namely that they are also unlikely to add anything unique to the judgement process if experts are involved. As the discussion in the next chapter will show, they might, however, have access to a further type of expertise (not yet discussed in this work) which might provide them with some special insights.

necessarily be correct. As judgements made by any particular social group, they might be influenced by systematic biases, 'group think' or narrow focus.[143]

Third, while 'local discrimination' has not been discussed, a theoretical case for the inclusion of those relying partly on local discrimination can be made. Shapin (1995) has pointed out that no aspect related to a scientific claim can be ruled out as irrelevant for the assessment of its credibility. Given that local discrimination is based on a specific socio-metric positioning of an actor, which gives her or him a specific vantage point, those relying on local discrimination might add something unique to the assessment of credibility. To take up Shapin's example of Fleischman's fictional stock portfolio, Fleischman's stock-broker can be thought of someone with local discrimination. Information by the stock-broker about the composition of Fleischman's portfolio could indeed prove highly relevant for the judgement of the credibility of claims about the viability of cold fusion.

Taken together, the discussion suggests that, in contrast to making technical judgements, not one singular class of experts ought to be in charge of making policy-process-relevant judgements of states of scientific discourses can be discarded. As with any process that involves a heterogeneous set of actors, this might create an additional problem: it can be expected that there will be disagreement amongst those who make such judgements. This means that

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[143] Steven Shapin's (1979) account of how phrenologists in nineteenth-century Edinburgh described the brain is an excellent example for this. This small community agreed upon a particular interpretation of cerebral features, but Shapin's study can demonstrate that this particular interpretation was shaped by local politics.

There is also an interesting psychological literature on the specificity of risk judgements of experts compared to lay persons, although its findings are not consensual. It appears that many within STS and risk studies have put great emphasis on the findings of Paul Slovic and colleagues (*e.g.* Fischhoff *et al.* 1978, Slovic *et al.* 1979) which suggest that the risk perception of scientific experts is significantly different from lay-persons' risk perception. These findings have been questioned, however, by Sjöberg (2002), who points out that the results by Slovic *et al.* were obtained on an extremely thin empirical basis. Sjöberg claims that research conducted by him and colleagues has not shown any significant difference in the perception of risks among a variety of actors belonging to different social groups.

In some of the interviews conducted with scientists in South Africa, the author has noticed that the scientific practitioners invariably favour the 'collective good' over individual outcomes. They openly acknowledge that prevention of MTCT might, in rare cases, lead to the death of individual children. They emphasise, however, that, as regrettable as such individual tragedies are, a community in its entirety benefits from drug-based PMTCT programme as more lives are saved by preventing paediatric HIV infections. Favouring the 'collective good' appears to be a widespread attitude amongst medical professionals (see Collins and Pinch 2005).

different actors, potentially with the same type of expertise, might judge the state of a given scientific discourse differently. This happens in practice: there are some actors with interactional expertise and DSD on the issue of aetiology of AIDS, who claim that a scientific controversy about the causes of AIDS is active, although the vast majority of experts in the field believe the opposite. How to deal with this sort of disagreement, that is, how to judge the judges' judgement, constitutes the central enquiry of the next chapter.

7. Sociological Discrimination and the Identification of Inauthentic Scientific Controversies

7.1 Introduction

This chapter continues to explore the applicability of the problem of extension to meta-questions. The focus is upon the extent of legitimate participation in answering another meta-question related to the authenticity of scientific controversies that are invoked in technological decision-making processes. The question is whether anyone can judge the authenticity of scientific controversies.

It has been shown in the previous chapter that those relying exclusively on ubiquitous discrimination in making policy-relevant judgements about the state of a scientific discourse ought to be excluded. This does not, however, mean that those actors participating in the political phase are likely to be confronted with a consensus about the state of a scientific discourse. Given this heterogeneity of legitimate participants and potentially relevant criteria to assess credibility of technical claims, disagreement about the state of scientific discourses has to be expected.

In contrast to the previous chapter, which explored how actors with different expertises make credibility judgements, this chapter is prescriptive. It proposes an approach or technique that can be used to judge the authenticity of scientific controversies that are invoked in technological decision-making processes. To make these authenticity judgements as transparent and independent of the personality of judges as possible, four criteria are proposed to guide the demarcation of authentic from inauthentic scientific controversies.[144]

An obvious problem is that providing criteria (or rules) to identify the authenticity of scientific controversies does not guarantee that meta-judges will agree in their authenticity judgements. Taken to its logical conclusion, the path

[144] The 'policy relevance' of authenticity judgements is similar to the one of credibility judgements. As has been pointed out in the Introduction to the previous chapter, it makes a significant difference whether a policy-relevant scientific fact or issue is considered to be the subject of a scientific controversy or not.

chosen here would, in case of disagreement about authenticity judgements, require meta-authenticity-judgements and so on *ad infinitum*. All that can be offered in defence of the chosen approach is a pragmatic response: if judgements about the state of a particular scientific discourse vary, then policy-makers will have to make a choice in any case. In the absence of rules to guide how to make this choice, it is likely that policy-makers will simply choose the judgement that suits their respective political position. Even if meta-judges cannot always be expected to agree, if they do so sometimes on the basis of the proposed criteria, progress will have been made.[145] Another practical advantage of setting out clear criteria as the basis of authenticity judgements is that, in principle, anyone can apply them.

This chapter is organised in the following way: in the next section, a set of four demarcation criteria is introduced that is intended to guide the process of making authenticity judgements. It is emphasised that the criteria *in themselves* are meaningless as they can be interpreted in very different ways. This indicates the need to operationalise the criteria clearly.

The operationalisation of the four criteria is set out in four consecutive sections. Not only will the meaning of the individual criteria be explained and justified, each section will also test the suitability of the criteria with respect to a case study. The South African case of technological decision-making about PMTCT will provide the background for testing the first two criteria. While the third criterion is tested with respect to the so-called UK MMR controversy, the fourth is assessed with respect to the debate as to whether Intelligent Design should be taught in science classes.

The last substantial section discusses whether the specific understanding of the nature of science, which informs the operationalisation of the four criteria, represents another new type of meta-expertise. It is suggested that this is indeed the case as this understanding of the nature of science is the outcome of research attributable to Wave 2 and Wave 3 of science studies.

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[145] Even if this structured approach for assessing the authenticity of scientific controversies does not let to agreement, it might be a helpful way to highlight areas of disagreement between actors. This might make political consensus finding easier.

7.2 Determining Authenticity: Four Criteria

The proposal made here comprises two closely related elements. First, four criteria are introduced that are intended to direct the analysis of scientific controversies by those who want to make a judgement about whether they are open or closed. The criteria are derived from an understanding of scientific controversies as disagreements about scientific facts or issues between members of a community of relevant experts. The four criteria, whose choice will be justified below, are:

(1) 'explicit argument';

(2) 'expertise of claim-maker';

(3) 'constitutive work';

(4) 'conceptual continuity with science'.

The first criterion, 'explicit argument', is to some extent self-explanatory as it is clear that an 'argument' has to be identifiable. The term 'explicit' has been added to make clear that not any form of visible disagreement between scientists can be interpreted as an active scientific controversy.[146]

The second criterion, 'expertise of claim-maker' asserts that those who make claims that challenge a consensus or contribute to one or the other side in a scientific controversy should possess relevant technical expertises. This is, for example, the reason why PR companies, which write articles and letters containing technical claims in an attempt to shed doubt on the scientific consensus that smoking is dangerous on behalf of tobacco companies, pay scientists for their signature (Michaels 2008). If these documents were clearly marked as written by a PR company, no one would take them seriously.

The third criterion, 'constitutive work', is based on the insight that nobody would accept a controversial technical claim that is, say, not based on some sort of

[146] For example, the debate whether HIV causes AIDS has, for all practical purposes been resolved in the mid 1980s. Nonetheless, one can still find the odd paper by AIDS sceptics that claims that this has never been proven (*e.g.* Nattrass 2009, 2010). Disagreement between scientists, that do not constitute scientific controversies, can also be recognised in 'oral' settings. Collins (2004a), for example, describes how Joe Weber, one of the pioneers of gravitational wave detection, time and again tried to convince his colleagues during conferences that his sensitivity calculations for bar detectors were correct, although no one in the community believed Weber. While Weber's claims were repeatedly heard during those conferences, it would be wrong to interpret this as an explicit argument amongst scientists about some fact or issue.

scientific activity. While experts might dream just as much anyone else, one should not take a scientific claim seriously if a scientific expert insists that the content of the claim occurred to her/him in a dream.[147] The purpose of the criterion is to distinguish between 'baseless' claims, *e.g.* speculations, and claims that have at least the chance to be based on more than just 'mere, unsubstantiated belief.'

The fourth criterion, 'conceptual continuity with science' sounds abstract, but the idea behind it is readily intelligible: if a technical claim does not reflect any intention to be part of the domain of science, there is no reason to assume that a *scientific* controversy can be caused by it. For example, the claim in *Genesis* that God created the earth in just 7 days (if the day of rest is included) is regarded by some as a statement of fact, since this is stated in the Bible. This is, however, a claim that belongs to the realm of religion and usually there is no intent to make it continuous with science. Such a claim ought not to have any impact on, say, paleontological archaeology, which, according to literal believers of the Bible, ought not to exist.

Identifying analysis-guiding criteria is, however, only the first step in finding a reliable way to determine the status of a scientific controversy in real-time. The criteria can be interpreted in different ways. It is interesting to note that Thabo Mbeki's justification for invoking a scientific controversy on the basis of his judgement that the claims of AZT critics were credible appealed to at least three of the criteria. Mbeki recognised (1) the importance of disagreeing scientists, (2) the importance of their being real experts, and (3) the importance of basing claims on something substantial. Even more interesting is to note the way in which Mbeki understood or operationalised these constitutive characteristics of a scientific controversy. As pointed out in the previous chapter, (1) the fact that he was able to recognise mutually exclusive views of scientists in the literature was taken as significant by Mbeki. (2) 'Impressive' scientific credentials of AZT critics convinced the President that they represented a proper expert view. (3) The

[147] There are, of course, cultures in which the content of dreams is not regarded as 'baseless', but is taken very seriously. While this might be so, it is clear that 'dreaming' is not an acceptable base for causal claim in a scientific form-of-life as it clashes with the formative intentions of the scientific culture. Scientists in modern societies might get ideas from dreams, but they must not invoke dreams as a justification for certain belief.

fact that they had published scientific articles in journals was interpreted as a proxy for the fact that AZT critics' claims were based on more than mere belief. Thus, Mbeki did not necessarily act unreasonably within the confines of his understanding of scientific controversies in arguing that AZT was subject of a scientific controversy once he had attributed credibility to the arguments of AZT.

The problem with Mbeki's position is that his operationalisation of the criteria was flawed. The previous chapter illustrated the limits and flaws underpinning his belief that any recognisable disagreement between experts can be interpreted as a scientific controversy. With regard to his understanding of expertise, the limits of concentrating on formal credentials have been outlined in the discussion and critique of technocracy in chapter 2. Furthermore, the limits of Mbeki's assumption that 'scientific papers' published in 'scientific journals' have necessarily to contain substantiated claims have been shown in the previous chapter, where scientists demonstrate, for example, that the Papadopulos-Eleopulos *et al.* paper is not what it seems to be to the untrained eye.

Here the understanding of the nature of science that is based on insights provided by Wave 2 and Wave 3 becomes relevant. For example, someone with experience in science studies would not equate the mere existence of critical scientific literature about AZT with an open controversy about the safety of the drug when used as a short term preventive treatment as Mbeki has done. Likewise, someone with an STS background would not regard formal titles and credentials as a suitable indicator for expertise. Unlike Mbeki, s/he would actually look for social proximity to and interactions with members of the scientific community.

7.3 Explicit Argument

The previous chapter illustrated that the majority of relevant experts disagreed with Mbeki's characterisation of the scientific discourse about the safety of AZT. Mbeki claimed that the issue was subject of an open controversy amongst scientists, while mainstream scientists argued that no scientific controversy was taking place. By suggesting a sociologically informed operationalisation of the

'explicit argument' criterion, it is argued that it is possible to make a real-time 'impartial' judgement about the state of a scientific discourse.[148]

As indicated in the previous section, the mere existence of dissenting literature cannot be regarded as a reliable indicator of the existence of an explicit controversy. STS research also shows that concentrating on the peer-reviewed scientific literature alone is still inadequate (Collins and Pinch 1998[1993], Oreskes 2004, Collins 2004a, Shwed and Bearman 2010). This research indicates that peer-reviewed literature can reflect the state of a scientific discourse only when the literature is looked at in a wider context. The contextualisation itself can take two forms.

One way in which a social analyst can contextualise particular pieces of literature is by immersing herself or himself into the oral discourse of an expert community. Collins (2004a) has shown that this can work. For example, he recognised that claims in a certain paper by Joe Weber did not lead to a scientific controversy despite contradicting widely held beliefs in gravitational wave physics.[149] Curious about the lack of debate within the domain-specific journals, Collins discovered by talking to practitioners in the field that Weber's published claims had regularly been rejected during conferences and workshops. Given that Weber was regarded as a maverick and given that his claims had been refuted by

[148] Such a judgement should be 'impartial' or at least as impartial as possible, as it is guided by the criteria and not be personal convictions. To use one's personal convictions to guide meta-judgements is to turn one's political standpoint into a crucial influence on the result. Mike Lynch (2006), for example, appears to use his 'politics' as a yard-stick to assess the legitimacy of the respective involvement of two STS scholars in court cases. Simon Cole's critique of the reliability of finger-print identification is judged to be acceptable, because Cole aligned himself with progressive forces that were in favour of 'civil liberties' and 'combated state power to enlist science as a prosecutor tool (Lynch 2006: 824). Fuller's involvement on behalf of the 'Intelligent Design' movement, in contrast, is deemed to be unacceptable because Fuller aligns himself with fundamentalist Christians (Lynch 2006: 824). It is likely that someone with strong Christian convictions might come to a very different judgement about the acceptability of Fuller's contribution.

[149] Weber claimed that cheap bar detectors were, according his calculations, far more sensitive than laser interferometers, which made bar detectors far more adequate for detecting gravitational waves. Gravitational wave detection using bar detectors would have made research much cheaper than research using laser interferometers. Moreover, continued detection with bar detectors would probably not have lead to a transformation of gravitational wave physics into a 'big science.' Due to their costs, size and complexity, laser interferometers require large scale cooperation between many scientists from different backgrounds. This is not the case with detection based on bar detectors.

leading figures within the field, no one had deemed it to be necessary to respond to his claims in writing.[150]

Other STS research indicates that there is second way in which literature can be used to assess whether a fact or an issue is regarded as controversial or consensual within scientific communities. This research does not involve immersion into an expert community but relies on the analysis of certain quantities of peer-reviewed scientific literature (Collins and Pinch 1998[1993], Oreskes 2004, Shwed and Bearman 2010). The detection of consensus or controversy works on the basis of pattern recognition. In a situation of genuine controversy, a steady output of publications respectively supporting two or more sides in a dispute can be expected. Once a controversial matter has been settled, experts tend to devote their energies to work within an established paradigm. The support for the 'defeated' positions in form of publications tends to fall away.

The lesson for the operationalisation of the criterion explicit argument is that it is possible to concentrate on the literature to establish whether a scientific controversy is open or closed or whether one exists at all. What is necessary, however, is that the dissenting or seemingly controversy-causing literature is assessed in relation to the wider context of the domain under scrutiny. This can be done by means of literature searches and reviews. It can be expected that genuine scientific controversies might leave traces within the published scientific literature.

Concentrating on scientific literature, however, is not the only way to find out whether a fact or an issue is consensual or controversial within an expert community. A specific characteristic of issue under consideration, the safety of AZT, suggests another feasible indicator for determining the status of the scientific discourse about it. This specific characteristic is that the safety of drugs is usually established by specific regulatory institutions, which are legally entitled to make authoritative risk-benefit judgement. Expert committees working on

[150] As Collins (2004a) points out, a written rebuttal of Weber's claims was only published in the early 1990s, almost ten years after Weber's initial claims had been published. The reason for this belated response was that Weber, on the basis of his findings, lobbied politicians to refuse the funding of laser interferometers. Faced with this threat to the funding of new equipment, a refutation of Weber's claims was published to satisfy outsiders, such as politicians, that Weber was wrong.

behalf of such institutions usually review relevant data and literature to inform their judgements about particular drugs (Geffen 2010). Their judgements might be regarded as meta-judgements about the state of scientific discourse with regard to certain facts and issues. While reports of expert committees are not necessarily published – standards of transparency and openness vary from country to country – the granting of a licenses or its rejection can serve as useful indicators for expert agreement or disagreement with regard to the safety of drugs.[151]

The foregoing suggests two potentially fruitful avenues for social analysts who want to determine the publicly visible controversy about the safety of AZT in South Africa in 1999 and 2000 represented a true disagreement between technical and scientific experts. First, I will concentrate on the published literature and contextualise the supposedly critical literature. Second, I focus on meta-judgements of the scientific discourse as embodied in the safety verdicts issued by expert committees on behalf of regulatory agencies such as the South African Medicines Control Council, the US American Food and Drug Administration and the World Health Organization

No visible disagreement within major peer-reviewed publications

As shown in the previous chapter, Mbeki took the mere existence of a supposedly 'huge volume of scientific literature' that warned about the safety of AZT to mean that the issue was the subject of an 'explicit argument' within the scientific community. A sociological operationalisation of the 'explicit argument' criterion suggests, however, that contrarian literature has to be assessed in its wider

[151] This approach has its problems because the historic and contemporary composition of expert panels is influenced by nominal concepts of expertise that usually equate expertise with the knowledge of accredited scientists. Irwin (1995) shows in relation to the case of regulating the use of the 2,4,5-T, that experts in the UK concluded for some time that the substance was safe, only to change their minds later. On that basis, it might be argued that assessments by expert committees are not a useful indicator for 'expert' consensus, especially given that farm workers, who have experience-based expertise on the use of 2,4,5-T, contested the declaration of safety. Irwin, however, also shows that many other regulators around the world had already banned 2,4,5-T, which shows that there was some expert disagreement about the safety of AZT. The short-comings of the indicator that is suggested here might be minimised by looking beyond narrow national contexts and establish whether there is a 'global' agreement.

Another problem, as Jamie Lewis has kindly pointed out, is technical judgements by expert committees are in reality often influenced by political, economic and national concerns. While such empirical concerns are valid, the prescriptive approach here has to assume that values only play an indirect or intrinsic role in technical judgements. Again, national and local biases in expert judgements can be revealed by taking a global perspective.

context. Before a contextualisation can start, it is vital to know how to identify the critical literature. 'Critical literature' can be defined as peer-reviewed literature that explicitly or implicitly contradicts the mainstream belief, which holds that the benefits of using AZT for PMTCT outweigh its risks.

A good starting point for finding the 'critical literature' is to listen to the critics of AZT. Given that critics claim that AZT is poisonous and unacceptably toxic, they should be able to point to literature that contains such claims. One of South Africa's most vocal AZT critics, Anthony Brink, has produced a 'literature review' that solely focuses on literature that highlights the negative side effects of AZT.[152] It should be possible to find literature in Brink's review that challenges the mainstream view that the benefits of using AZT for PMTCT outweigh its risks. Indeed, Brink repeatedly stresses the importance of four papers – Olivero *et al.* 1997a, 1997b, Blanche *et al.* 1999, The Italian Register for HIV Infection in Children 1999 – that apparently support his claim that AZT is unacceptably toxic. A closer look at the original publications, however, reveals that Brink quotes selectively from this set of papers and also fails to mention that the respective authors actually endorse the use of AZT for PMTCT.

For example, with regard to a paper by Olivero *et al.* (1997a), Brink gives the impression that these researchers recommend not to use AZT for PMTC by quoting selectively from the paper:

> Since "AZT is unequivocally a transplacental genotoxin and carcinogen [and] given transplacentally to mice, benzopyrene produced lung and liver tumour multiplicities similar to those observed [with AZT]", the researchers recorded their concern that "the current practice of treating HIV-positive women and their infants with high doses of AZT could increase cancer risk in the drug-exposed children when they reach young adulthood or middle age." (Brink 2001: 43)

Readers, who might not have access to the full text of the publication, are not made aware that the researchers state in the next sentence:

> The remarkable effectiveness of AZT in preventing fetal HIV infection indicates that the immediate need for treatment of a potentially fatal disease should

[152] The term 'review' has to be used very loosely when discussing Brink's publication, because it is problematic in many respects: it is completely one-sided in that it only focuses on risks but not on benefits; the literature cited is highly selective, but no reasons for the selectiveness is provided; it provides quotes from scientific papers that misrepresent the general thrust of the argument; it conflates risk assessments of different usages of AZT, *e.g.* readers are not made aware whether a claim refers to the use of AZT as a long-term mono-therapy or a short-course preventative treatment.

outweigh the potential cancer risk (Olivero *et al.* 1997a: 1607).

The same is true with regard to the Blanche *et al.* (1999) paper that is concerned with mitochondrial toxicity. Brink (2001) only mentions that the paper reports a small number of deaths due to mitochondrial toxicity amongst children who have received antiretroviral drugs, but fails to put these findings in a wider context. He fails to quote the last paragraph of the paper, in which Blanche *et al.* (1999: 1089) contextualise their results:

> We are aware that the suggestion that antiretroviral drugs are toxic raises delicate issues. Prophylaxis of mother-to-child transmission of HIV-1 infection has saved thousands of children from death. The implementation of prophylactic-treatment programmes requires much multidisciplinary effort, and any action that impedes this approach to prevention could lead to a substantial step backwards. We believe our observations are however sufficiently significant to be shared. It is too early to do a risk/benefit analysis. *Our view is that the current recommendations for zidovudine monotherapy prophylaxis should be maintained.* We believe that combinations of molecules that could have cumulative toxic effects on the same cellular target should be avoided. Other nucleoside or combinations of nucleosides may have similar toxic effects. Pregnant women should be informed of the potential effects associated with these treatments during pregnancy. (italics added by M.W.)

Blanche *et al.* (1999) are clearly concerned, but they do not claim to present results that should end the practice of using antiretroviral drugs to reduce the risk of MTCT.[153]

Given the inability of the most fervent South African critic of AZT to find scientific literature that explicitly states or at least endorses the view that the risks of using AZT for PMTCT outweigh its benefits, it seems that the *only* peer-reviewed publication to do so is the aforementioned Papadopulos-Eleopulos *et al.* (1999) review of the pharmacology of AZT. While this paper concentrates on AZT in its function as long-term treatment, it can be inferred from its argumentation that it also recommends against the use of AZT as a short-term preventative treatment. The paper argues that AZT is transformed into highly toxic substances once it enters cells, while lacking antiretroviral activity.

This pits exactly one 'critical paper', a rarely cited review of mainly pre-1994 literature, against hundreds of papers which suggest that the benefits of

[153] The findings of the Blanche *et al.* paper have not been confirmed by other studies which also looked for evidence of mitochondrial toxicity of AZT when used for PMTCT (*e.g.* Mofenson 2000, The Perinatal Safety Review Working Group 2000).

using AZT for PMTCT outweigh its risks.[154] Amongst those 'pro-AZT' papers are four reports of clinical trials that have tested the safety and effectiveness of AZT when used to reduce the risk of MTCT (Connor *et al.* 1994, Shaffer *et al.* 1998, Dabis *et al.* 1999 and Wiktor *et al.* 1999). All four papers report adverse effects noticed during the clinical trials, but the respective authors agree in their conclusion that AZT is 'safe' and 'well-tolerated.' Mofenson (2000: 804) summarises the findings of the four trials in an editorial of The New England Journal of Medicine:

> Data on the short-term safety of prenatal and neonatal exposure to zidovudine are reassuring. In four placebo-controlled trials of perinatal zidovudine prophylaxis, the only toxic effect that was more frequent in infants exposed to zidovudine than in those not exposed was a transient, mild anemia [decrease in normal number of red blood cells] that resolved by 12 weeks of age.

Only the Connor *et al.* study had been conducted early enough to allow for follow-up studies.[155] Culnane *et al.* (1999) followed up on 234 (122 in the AZT group and 112 in the placebo group) uninfected children born to women infected with HIV enrolled in the original PACTG 076 trial. Culnane *et al.* (1999: 151) conclude that "[n]o adverse effects were observed in HIV-uninfected children with in utero and neonatal exposure to zidovudine followed up for as long as 5.6 years." A further follow-up assessment, which was shorter in duration, also concluded that there is no evidence for short-term safety concerns with regard to the use of AZT for PMTCT (Hanson *et al.* 1999).

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[154] A search of PubMed database by using the keywords 'AZT' and 'toxicity' finds 157 publications. The publication types in this search have been limited to: Clinical Trial, Meta-Analysis, Randomized Controlled Trial, Review, Case Reports, Classical Article, Clinical Trial, Phase I, Clinical Trial, Phase II, Clinical Trial, Phase III, Clinical Trial, Phase IV, Comparative Study, Controlled Clinical Trial, Corrected and Republished Article, Government Publications, Journal Article, Multicenter Study, Technical Report, Twin Study, Validation Studies. The search was also limited to a particular publication period between January 1994 and late October 1999. The first extensive reviews of the safety of antiretroviral drugs when use to reduce the risk of MTCT, however, have only been published after 1999 (*e.g.* Bulterys and Fowler 2000, Mofenson 2000, World Health Organization 2000, Brocklehurst 2002, Taylor and Low-Beer 2001, Kourtis 2002, Mofenson and Munderi 2002). If the benefit of hindsight is accepted for a moment, it is interesting to note that none of the reviews conclude that the risks of using AZT for PMTCT outweigh its benefits. While all reviews stress that the long-term safety of the drug when used for PMTCT has not been established, they point out that there is no evidence that suggests that long-term safety issues are likely to arise.

[155] The clinical trials conducted by Dabis *et al.* 1999 and Shaffer *et al.* 1999 include a 6 month follow up. No adverse effects that would support the claim that the risks outweigh the benefits of AZT have been reported. The follow-up period is, however, too short to allow the assessment of the long-term safety of the drug.

In sum, by October 1999 only one peer-reviewed scientific paper, which challenged the widely held and published belief that AZT is safe when used for PMTCT, was available. Given the isolation of the views expressed by Papadopulos-Eleopulos *et al.*, their view should not have formed the basis for expert advice and should have had no impact on technological policy-making in 1999 in South Africa. This does not affect the question as to whether their view was correct or incorrect, but how much political weight should have been given to an isolated and uncorroborated technical claim.

No visible disagreement within regulatory institutions

The expert discourse on the safety of AZT is not only played out in the pages of peer reviewed journals, but is also reflected in reports, guidelines and reviews issued by institutions such as drug regulators, for example Food and Drug Administration (FDA) in the USA or the MCC in South Africa and health-focussed institutions such as the Centers for Disease Control and Prevention in the USA, the MRC in South Africa or the World Health Organization (WHO) and UNAIDS on a global stage. A senior South African scientist emphasised the importance of the FDA and the WHO when it comes to the assessment of drugs:

> MW: Who would you trust on drug regimens in terms of institutions in sciences? Would you always go along when WHO publishes guidelines? What are the gold standards in the field or who is publishing the gold standards?
> I: Right, FDA...
> MW: FDA?
> I: FDA approval and WHO endorsement. I think the WHO expert committee is very good and... good people sitting on there and once there is an endorsement I respect that... I think the FDA in terms of the initial... because you know, let's face facts... I think the majority [of ARVs], if not all, were manufactured or were developed, R&D, in the States, the US... Copies have been made elsewhere, you know, generics in India and wherever, but I think that all of these drugs... so once the FDA has approved, the FDA approval and the WHO endorsement... and then our local MCC I think is a rubber stamping [institution], I think...

The answer given by the interviewee suggests that the most reliable or at least influential assessments of drugs are not contained in individual scientific papers published in journals but are to be found in reports and guidelines issued by the FDA and the WHO. The reason is that these assessments are usually undertaken by a relatively large group of recognised experts, who do not only consider the published scientific evidence, but might also have access to as yet unpublished studies.

The first thing to note is that both FDA and MCC had licensed the use of AZT for PMTCT well before 1999 and never retracted this license at any point. This suggests that experts working on behalf of the two institutions have concluded that AZT was safe enough to be used for PMTCT. As indicated in chapter 5, the MCC, alerted by Mbeki's remarks, conducted a fresh review of the safety of AZT and as far as it is known, did not find any evidence that suggested that its previous judgement on the drug had to be altered. Unrelated to Mbeki's comments, the *Antiretroviral Drugs Advisory Committee* of the FDA met on 4 October 1999 to discuss recent developments in the prevention of MTCT and the safety of antiretroviral drugs formed part of the discussion (Food and Drug Administration 1999). From the transcript of the meeting no disagreement between the participating experts with respect to the benefit/risk judgement on AZT was apparent.[156] The experts noted, however, that toxicological data related to AZT were limited and that the long-term safety of the drug (20 to 30 years) was impossible to assess. Despite the limits in knowledge and data, the meeting did not have any consequences for the licensing of AZT for PMTCT as the FDA continued to endorse AZT.

Other institutions also endorsed the use of AZT for PMTCT. By 1999, AZT had been on the WHO's list of essential drugs. To be included in that list, a drug must be evaluated by experts on behalf of the WHO and safety is a key aspect of the assessment (Gray and Smit 2000, Quick *et al.* 2002).[157] Moreover, in January 2000, a group of experts working on behalf of the WHO published a report that assessed the safety of AZT (World Health Organization 2000). The literature review assesses the gravity of a range of adverse effects associated with the use of AZT such as bone marrow toxicity, mitochondrial toxicity, tumourogenicity and

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[156] Critics such as Brink (2001) like to point out that AZT is classified as a 'pregnancy category C' drug by the FDA. The FDA has five different categories, category A drugs are considered the most safe, while category X drugs are considered to be unacceptable. The C category is the middle one and is defined in the following way: "Safety in human pregnancy has not been determined, animal studies are either positive for fetal risk or have not been conducted, and the drug should not be used unless the potential benefit outweighs the potential risk to the fetus." The very fact that AZT is licensed by the FDA indicates clearly that the agency feels the potential benefits of the drug outweigh its potential risks. It is, however, significant that AZT is still classified as category C in the late 2000s.

[157] The WHO, as a global institution, has great influence on many member states as it offers a way for poorer and less developed countries to access scientific expertise and benefit from expert advice. The endorsement of AZT by the WHO is therefore very significant.

carcinogenicity. Most of the reported and potential adverse effects of AZT are related to the long-term use of AZT as part of a combination therapy or when the drug is used as a mono-therapy. There is very little clear cut evidence of permanent and irreversible adverse effects when AZT is used as a short-term preventative treatment.

The influential *Guidelines for the Use of Antiretroviral Drugs to Prevent MTCT*, which were drawn up by the US Perinatal Safety Review Working Group, also reflected the expert consensus with respect to the benefit/risk assessment of AZT (Centers of Disease Control and Prevention 1994). Updated in February 2000 and containing an extensive review of toxicological data, the guidelines continued to recommend AZT for the prevention of MTCT.

In sum, by 1999 AZT had been reviewed and endorsed by at least four significant expert institutions. While this analysis has only focussed on two national regulators, the FDA and the South African MCC, many other national drug regulators have licensed the use of AZT for PMTCT. Indeed, all four institutions conducted additional reviews or issued new guidelines around the same time of Mbeki's speech in October 1999 and none revised its endorsement of AZT. The available reports and documents suggest that regulators were aware of adverse side-effects and of missing information, but this did not stop them from endorsing the use of AZT for PMTCT.

Interim Conclusions

Taken together, the two indicators chosen to determine whether the safety of AZT when used for PMTCT was regarded as being controversial or consensual within a relevant expert community suggest that there was a broad consensus about the safety of AZT in place in 1999. With regard to the literature, it appears that there was only a single scientific paper, a literature review published in a rather obscure journal, which concluded that AZT did not have any benefits, but posed many risks. Apart from this single paper, no other peer-reviewed article that the present author is aware of endorses such a position.[158]

[158] Neither literature searches in various databases nor asking experts have led me to believe that there is any other scientific paper dealing with AZT, which comes to similar conclusions to Papadopulos-Eleopulos *et al.* (1999). Moreover, if an AZT critic such as Anthony Brink is unable

The existence of this single review paper does not reflect a real disagreement amongst scientific experts. Instead, it suggests that the scientific controversy invoked by Thabo Mbeki can be regarded as an inauthentic scientific controversy, which should not have impacted on technological decision-making.

7.4 Expertise of Claim-Makers

A key insight of STS research that forms a cornerstone of SEE and the 3W model is that formal credentials are neither reliably nor exclusively linked with 'genuine understanding.' In chapter 1, it has been shown that SEE conceptualises expertise very differently compared to the nominal concept utilised by Mbeki. This different understanding underpins a very different operationalisation of the expertise criterion. Within SEE, expertise is linked with tacit knowledge. To become an expert, an actor has to acquire tacit knowledge and the only known way to do this is through direct social contact with those who already posses tacit knowledge. Instead of focussing on formal credentials, SEE takes 'active immersion in a community of experts' to be a far better indicator for the presence of expertise.

The shortcomings of Primary Source Knowledge

Without direct social contact or active immersion into an expert community, the highest level of specialist expertise an actor can reach is, according to the PTE, Primary Source Knowledge. Those depending solely on Primary Source Knowledge (PSK) face a number of problems when attempting to make technical judgements. By reading primary sources in isolation, it is impossible to acquire technical tacit knowledge and, more important, domain-specific social understanding. Those depending on PSK have to make policy-relevant technical judgements on the basis of ubiquitous discrimination. These points will be illustrated with regard to Thabo Mbeki's direct involvement in technical matters following the government's decision to ignore technical advice issued by the MCC on the safety of AZT in February 2000.

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to find any scientific papers that support his own conclusions, namely that AZT is excessively toxic, one can be fairly certain of not having overlooked significant literature.

While it has been argued that Mbeki's sceptical stance towards the safety of AZT in October was not as such incorrect – the problem was that instead of going public after reading scientific texts, he should have at least consulted with the relevant experts of the MCC – Mbeki's continued doubting of AZTs safety after a final report by the MCC had been submitted in February, effectively changed Mbeki's role within the technological debate. After February 2000, by contradicting the assessment of the MCC, Mbeki became an active, although illegitimate, player in the technical phase.

No access to oral discourse

A 'constitutive' problem for those relying solely on PSK is that they cannot access the full scientific discourse.[159] According to Collins and Evans (2007), there is a significant difference between the 'published discourse' and what might be called the 'oral discourse' of a scientific community.

> [I]t can be shown that what is found in the literature, if read by someone with no contact with the core-groups of scientists who actually carry out the research in disputed areas, can give a false impression of the content of the science as well as the level of certainty. Many of the papers in the professional literature are never read so if one wants to gain something even approximating to a rough version of agreed scientific knowledge from published sources one has first to know what to read and what not to read; this requires social contact with the expert community. (Collins and Evans 2007: 22)

Collins and Evans (2007) argue that it is wrong to assume that the written and the oral discourse relating to a specific scientific domain are the same. In fact, they suspect that non-experts, on the basis of exclusively reading scientific sources, might develop 'false impressions' about a science.

Oral and written discourse related to a scientific domain might differ for various reasons. For example, as shown in the previous chapter, Mbeki appeared to claim that he had fully grasped what was going on within sciences relevant for the assessment of AZTs safety by reading published scientific literature with the help of dictionaries and the Minister of Health.[160] In contrast, the literature review

[159] This is a constitutive problem as the lack of social contact with the expert community defines PSK.

[160] It could be argued that holding the belief that reading scientific literature in isolation makes it possible to make sound technical judgements on the basis of the gained technical understanding is in itself a sign that the person lacks basic understanding of how science works. Science tends to be an oral culture in which papers published in journals play only a limited role. Personal encounters, for example during conferences or collaborations between laboratories, play a vital role both in

about the safety of AZT compiled on behalf of the WHO in January 2000 included references to then still unpublished studies concerned with the mitochondrial toxicity of AZT. This was only possible because the experts involved in writing the literature review were aware that these studies existed due to their immersion in the expert community and their access to the oral discourse. Another example of the mistaken belief that reading literature in isolation enables sound technical judgements about scientific issues or facts has been presented by Collins (2004a). Collins has shown how US policy-makers came close to making a flawed funding decision about equipment needed to detect gravitational waves by relying solely on claims published in the scientific literature. While the published literature indicated that bar detectors were even more sensitive than highly expensive laser interferometers, the (oral) consensus in the scientific community was that this was not true (Collins 2004a, see also Weinel 2008).

Even if it is assumed for a moment that oral and published scientific discourse overlap to such an extent that the difference between the two becomes almost irrelevant, someone who is relying solely on Primary Source Knowledge to understand science faces other problems too.

Logistical Problem

A 'logistical' problem which someone with Primary Source Knowledge might face is that the body of technical literature on an issue is so large that it becomes almost impossible to read everything that has been written on the subject. Even if it is assumed that it could be possible to understand a scientific issue genuinely by reading every bit of relevant scientific literature, someone relying on Primary Source Knowledge would, more often than not, face difficulties. While the body of literature related to some esoteric or just emerging topics might still be very small, this was certainly not the case with regard to literature related to AZT. As Cherry (2009) has pointed out, the PubMed database contained more than 6,000

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transferring information and in assessing the value of information that does become published (Collins 2004a, Collins and Pinch 1998[1993]). Access to the oral culture is not available to most outsiders and Mbeki and his supporters within the administration were not integrated into the scientific networks that formed the core set of those dealing with the safety of antiretroviral drugs on a professional basis. Mbeki's sole reliance on reading literature reflects a simplistic understanding of the way that scientific knowledge is made.

scientific papers published on AZT between 1994 and early 2000.[161] While Mbeki's reading burden around the issues of antiretroviral treatment of HIV/AIDS would not have been insignificant, it is doubtful whether he could have read anywhere near 6,000 paper between March and October 1999, a period, in which he became newly elected President of South Africa.

Relevance of Literature

Even if it is assumed that written and oral discourse on a subject are the same and that an outsider has read every piece of literature, this still does not solve all the problems of PSK. This is because the next problem an actor relying solely on PSK faces is to make a range of judgements such as judging the relevance of literature, its relative importance as well as the quality or permissibility of certain technical claims. By virtue of relying on PSK, an outsider cannot inform her or his judgements by technical tacit knowledge or by domain-specific social understanding. As this has been the subject of the previous chapter, some pointers are sufficient to illustrate the problems with certain judgements.

An initial judgement that the outsider has to make is to decide, which parts of the read literature are actually relevant for making a certain technical claim. In the previous chapter, it has been shown that Mbeki regarded the Papadopulos-Eleopulos *et al.* (1999) paper as an important contribution to the scientific debate about balancing the risks and the benefits of using AZT for PMTCT. Mbeki, by virtue of having read the paper in isolation, assessed its relevance on the basis of the lengths of the paper, the number of footnotes and quality of the journal. As shown in the previous chapter, these were not reliable indicators for the relevance of literature.

Relative Importance of Technical Claims

Another problem that outsiders face is related to the assessment of the relative importance of claims made in the literature. Some papers might contain claims which, if accepted as true, would have major implications for an issue. Without sufficient technical understanding, outsiders are in no position to make well-informed judgements about the relative importance of certain assertions.

[161] This means that, on average, 1,000 papers per year are published on the issue or two to three new papers every day.

This can be demonstrated with reference to a particular study that is often cited by AIDS critics to demonstrate the unacceptable toxicity of AZT. The study, authored by Blanche et al (1999) has evaluated the health of more than 1,750 mother-child pairs who received AZT as part of a PMTCT programme. In a letter to critics of his stance on the safety of AZT, reported on in the South African *Daily Star* on 25 March 2000, Mbeki evidently attributes a great deal of importance to the findings of the Blanche *et al.* study:

> The letter [...] quotes five studies that cast doubts on the safety or efficacy of AZT, including a French study [the Blanche *et al.* 1999 study], reported on by The Sunday Independent last year, in which eight children of 1,754 mother-child pairs experienced a condition called mitochondrial dysfunction. "It is clear from your letter that you believe that we should ignore or merely note these findings because of the current 'consensus amongst responsible and authoritative scientific leaders' as well as 'the available evidence'," Mbeki writes. "Undoubtedly, such 'consensus' and 'available evidence' also existed on the use of Thalidomide. Faced with the findings indicated in this letter, I am afraid that my own conscience would not allow that I respond only to the 'consensus' with which you are in agreement" (Sulcas 2000).

Based on his own reading of the research paper, Mbeki seemed to be convinced that the French study indicated dangers of AZT in such a way as to afford a comparison to Thalidomide. He attributed so much importance to the findings presented in this study that he felt entitled to reject an 'expert consensus' and 'available evidence.'

Contrast Mbeki's assessment with that of a senior South African scientist, who I had the chance to interview. He happened to be acquainted with the Blanche *et al.* paper and gave me the following assessment:

> MW: Have you ever come across the article by Blanche et al 1999.
> I: Oh yes, absolutely.
> MW: How do you read an article like that? Do you do a risk-benefit analysis in your head or how do you do it?
> I: That's our daily work. All the time, that's what you do. First of all, you look at the evidence... That's why life is not just finding the truth, but finding the probability. What's the likelihood of it and then you weigh up the balance. If it is a drug which is causing lots of deaths, you can't use it no matter how good it is. But first of all... so we looked at the Blanche paper, it was in the Lancet and there was this probability, but it was a low probability, that's the first thing, a low probability. Second thing, it was not a properly conducted study. They did not set out, a properly conducted study says, OK, we are going to give this drug to a 1000 children or mothers and we are going to follow these guys for say 5 years and we know everything that goes on in these people, that's a proper study, so you know. Then you find out... maybe a small group which had some prior problem, who knows what it is, got this problem so maybe in the future you can

> exclude that group. So it was not that type of study. However, we were all worried, because AZT was a useful drug, so there were all the other big groups, NIH, some European groups, every big group in the world who had data on the use of Nevirapine and AZT looked at it and we had a meeting in the US were they looked at it and tried to check it and it just was not true. Did it happen? The answer is yes. Does it have that effect? The answer is yes. But in terms of the chances of finding the effect it was nowhere near the French experience. And you can pick up another paper, they still find it, but we do not find it anywhere else. And even if we did, think about it for a second, without the drugs the transmission rate in Africa was 35%.

The scientist offers a number of reasons why the results of the Blanche *et al.* paper should be treated with caution. First, the effect that the French researchers found was relatively small; second, the research design was not geared towards providing causal explanations for the detected effects; and third, the research findings were not corroborated by similar research elsewhere. The interviewee also pointed out that even if the findings of the French researchers had been corroborated by other research and even if the effect had been larger, experts would still have recommended the use of antiretroviral drugs on the grounds that more lives would be saved through averted vertical transmission than lost through mitochondrial dysfunction.

The discussion around the Blanche *et al.* (1999) paper shows that the interpretation of scientific papers is not a straightforward matter. Those depending on Primary Source Knowledge cannot base their judgement of importance on specialist (technical) tacit knowledge or on domain-specific understanding as technical experts can. A range of technical considerations impact on the interpretation of the relevance of a particular paper such as knowledge about the quality of the method employed in a paper, knowledge of the findings of similar studies and knowledge of the benefits against which the findings have to be balanced and so on. Moreover, the assessment of technical experts is not only based on published material, but also on impressions gained from direct discussions with other researchers and colleagues. For example, the interviewee mentions a meeting in the US, which provided various research groups with the opportunity to exchange views and interpretations.

Assessment of technical arguments and assumptions

Yet another problem associated with trying to understand technicalities on the basis of solely reading literature is that it is very difficult to assess the quality of

technical arguments. This is not to say that outsiders cannot follow a technical argument; well written scientific papers can be accessible to outsiders even though they might struggle with the technical terminology. Thus, outsiders might be able to assess the coherence and consistency of technical arguments even though they might not fully understand them. The problem is that the plausibility of arguments might hinge upon some technical claims and it is incredibly difficult for outsiders to evaluate the correctness of such technical claims.

A specific claim made in the Papadopulos-Eleopulos *et al.* (1999) paper can be used to illustrate this briefly. As shown in the previous chapter Mbeki was able to follow and to reproduce one of key claims of Papadopulos-Eleopulos *et al.*, namely that AZT had to be triphosphorylated in cells before it could become active and inhibit the replication of a retrovirus such as HIV (Jeter 2000). What Mbeki did not show, however, was the ability to critically assess the argument. It seems that he was only able to take it on face value or on trust. This is to be expected as he had no prior technical knowledge, tacit or explicit, to reading this paper. Consider, for example, the following claim from Papadopulos-Eleopulos *et al.* (1999: 20) paper:

> In their first clinical trial they [a study by Furman *et al.* published in 1986] acknowledged that 'a minimal level for an in vitro antiviral effect' is 'above 1mmol/l' of AZTTP. However, such levels of AZT triphosphorylation are not obtained even under ideal, in vitro conditions, and the level of AZT triphosphorylation in vivo is even lower.

According to Papadopulos-Eleopulos *et al.*, the Furman *et al.* study presents evidence that required levels of triphosphorylation cannot be obtained *in vitro* even under 'ideal conditions.' This is significant for the efficacy because Furman *et al.* apparently also argue that AZT triphosporylation is always lower *in vivo* than *in vitro*. Papadopulos-Eleopulos *et al.* therefore claim that if AZT cannot be triphosphorylated to required levels even under ideal *in vitro* conditions, AZT cannot be effective when it is used in humans.

On its own, an outsider might regard the claim as coherent and consistent. The plausibility of the argument hinges, however, on some technical details such as whether it is really the case that *in vivo* triphosphorylation is always lower when compared to ideal *in vitro* conditions. This specific argument has been disputed by David Back, a Professor for Toxicology at Liverpool University.

During a court case in South Africa in which he served as an expert witness for GlaxoWellcome, Back made the following claim:

> Paragraph 16.2 of the Particulars of Claim [made on the basis of the Papadopulos-Eleopulos *et al.* (1999) paper] describes Furman's *in vitro* conditions as being 'ideal.' The only ideal conditions will be those that reflect as closely as possible the *in vivo* situation. This would require all dNTPs to be present at physiological concentrations and a template primer that reflected the make-up of the viral template. The primary purpose of the Furman study was to look at the selectivity of the inhibitory effect of AZT for viral enzyme reverse transcriptase rather than the human enzyme polymerase α. It was not designed to model the *in vivo* situation and I note that no attempt was made in the Furman paper to assign any *in vivo* significance to the measured IC_{50} value. (Back 2002: 8).

While this discussion does not show who is wrong and who is right, it indicates that a non-expert, who wants to make a technical judgement, has to do exactly this: choose between competing expert claims. It has been shown in the previous chapter that non-experts are able to do this only on the basis of distanced social considerations or 'ubiquitous discrimination.' In contrast, experts are able to use technical considerations, DSD as well as ubiquitous discrimination.

Interim Conclusions

The expertise criterion, operationalised in accordance with SEE, helps to demarcate between those with and those without genuine technical understanding. As has been shown in this section, those with high levels of explicit knowledge gained through reading primary scientific sources are likely to lack tacit knowledge, which is critical for the ability to make technical judgements on technical grounds. On the basis of lacking specialist expertise, Thabo Mbeki's continued claims that AZT was not safe enough even though the MCC had offered a consensus-based judgement that the drug *was* safe for PMTCT, must be regarded as an illegitimate contribution to a technical issue.

It is argued that the obstacles for making technical judgements on the basis of technical considerations for those relying on Primary Source Knowledge are insurmountable. One might differentiate between three types of problems encountered by those relying on PSK. First, there is the 'constitutive problem' that no access to the oral scientific discourse related to an issue is gained. This is in some sense a circular argument as the lack of access to the oral discourse is a defining feature of PSK. Second, there is a logistical problem related to the size

of the literature. This might not apply in all cases as not all scientific issues come with a large body of literature, but it certainly applies to the issue of using AZT as a treatment for HIV/AIDS. Third, there are 'follow-up problems' which are related to the reliance on ubiquitous discrimination to make judgements about the relevance of scientific literature, the importance of technical claims and quality of technical arguments.

It has to be noted that PSK does not always lead to judgements that contradict judgements made by technical experts. Had Mbeki, for example, trusted textbooks instead of 'scientific papers' he found on the Internet, his views on the safety of AZT might have been similar to the views of technical experts.[162] Nonetheless, such an outcome would have been based on chance and not on experience and genuine understanding. The case study shows that PSK is unreliable and therefore not suited for making policy-relevant judgements.

7.5 Constitutive Work and the UK MMR Controversy

The reasoning behind the use of 'constitutive work' as a criterion is that its absence indicates that the opinions expressed are based on speculation. Speculation should have no impact on technological decision-making processes. The term 'constitutive work' draws on the idea of a 'constitutive forum', a concept developed by Collins and Pinch (1979). They define a *constitutive forum* as an abstract 'space' which comprises "scientific theorising, and experiment ... [with or without] corresponding publication and criticism in the learned journals and, perhaps, in the formal conference setting" (Collins and Pinch 1979: 239-240). The abstract space of the constitutive forum contains a set of activities – constitutive work – that constitute science as a distinctive *form of life*.[163] It is

[162] Nicky Priaulx has kindly pointed out that accessing sources on the Internet as such is not problematic. The present author, for example, has accessed many of the sources used for this thesis on the Internet, including most of the articles published in scientific journals. The problem is to know which sites to trust and which not to take seriously. Given Mbeki's unfamiliarity with the scientific domains concerned with the safety of antiretroviral drugs, the President's credibility judgements were based on ubiquitous discrimination and therefore relatively unreliable.

[163] This contrasts with the 'contingent forum' which is the abstract space of activities carried out by scientists which are not generally considered legitimate contributions to scientific knowledge building such as chatting in coffee rooms, gossiping and so forth. Sociologists of scientific knowledge have shown that there is no epistemological distinction between the two *fora* but the

these activities that yield what will be referred to as constitutive work. They can take, however, many different forms, including experiments, theory- or model-based predictions, extrapolations and systematic and controlled observations. Constitutive work must therefore not be mistaken for 'empirically generated evidence' as this is just one of its manifestations.

The operationalisation of the criterion as such is relatively easy: judges have to analyse the basis upon which a technical claim rests and assess whether a technical claim is supported by anything amounting to constitutive work. A technical claim, for example, that is justified by pointing to a passage in Bible or the Quran does not fulfil the criterion. Likewise, technical claims that are justified with phrases such as 'This is the case, because I am an expert' or 'This is the case, because I say so' also fails to meet the criterion. It is, however, far more difficult for an outsider to apply the criterion when technical claims are justified with seemingly technical arguments.[164]

The following brief case study shows that the criterion can nonetheless do some important work even in cases where experts use technical arguments to justify technical claims. The case under consideration is the so-called UK MMR controversy (*e.g.* Leach *et al.* 2005, Boyce 2007, Anderberg *et al.* 2008, Goldacre 2008). The MMR controversy was largely restricted to the UK, began in 1998 and lasted for several years. This account focuses on the initial public claim by Dr Andrew Wakefield that the MMR vaccine could cause autism.

The UK MMR Vaccination Controversy

The MMR vaccine is an immunisation shot against measles, mumps and rubella and contains three live attenuated viruses. A first MMR injection is generally administered to children around 13 months of age and a second dose is given at the age of four or five years. While the MMR vaccine has been widely used for

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sociological distinction remains clear and must remain clear if science is continued to be recognised as a distinctive activity (Collins and Evans 2007).

[164] As the discussion about the Blanche *et al.* paper, which contains evidence about mitochondrial toxicity of AZT, has shown, the cited expert played down the significance of this paper partly by pointing out that the study was not designed to exclusively detect mitochondrial toxicity. To recognise such a 'weakness' in the design of a study requires specialist expertise. As long as it can be shown that the criterion of constitutive work is useful at least in some cases, however, progress will have been made.

many years all over the world, it became the subject of a 'health scare' in the late 1990s in the United Kingdom.

In 1998, Dr. Andrew Wakefield, an adult gastroenterologist at London's Royal Free Hospital, reached a public audience when he claimed that the use of MMR is risky since it might cause autism in children. He made these claims during a press conference intended to publicise research undertaken by a team of scientists at London's *Royal Free Hospital*, the results of which had been published in February 1998 in *The Lancet*. Wakefield also repeated his claims during a video news release authorised by the Hospital (Boyce 2007:4). In particular, Wakefield argued that inoculating children with three live viruses at the same time might overwhelm their immune system and lead to adverse events. For Wakefield the safer alternative was to inoculate children with three single vaccines spaced over time to avoid overwhelming their immune systems

While Wakefield mentioned that his opinion about the risks of the combined MMR vaccine might not be shared by the co-authors of the study published in *The Lancet*, he did not explicitly clarify that the published study did not support his claims (Wakefield *et al.* 1998, Boyce 2007). The paper suggests that the research team led by Wakefield might have discovered a new syndrome. This syndrome involved chronic enterocolitis and regressive developmental disorder, with autism being one of the manifestations of the latter. This assertion was based on the fact that parents of 8 out the 12 children reported an onset of behavioural changes in their children – which were mostly diagnosed as autism – within 2 weeks after the children were inoculated with MMR vaccine. Based on this correlation reported by parents, the authors of the *Lancet* paper express the *suspicion* that the MMR vaccine might be an "environmental trigger" (Wakefield *et al.* 1998: 637).[165]

Crucially, the Lancet paper stated quite clearly that no causal explanation or explanations for the reported behavioural changes could yet be provided. The relevant passages in the paper are very carefully formulated to make sure that the

[165] This suspicion represents just a minor point as the main purpose of the article was to describe what had identified to be a new syndrome. At this stage, it was far from clear that the observations of the Wakefield group really justified the construction of a new syndrome as an accompanying piece in the Lancet argued (see Chen and DeStefano 1998).

authors do not overstate their case. For example, the following statement is made in the discussion section of the paper:

> We did not prove an association between measles, mumps and rubella vaccine and the syndrome described. Virological studies are underway that may help to resolve his issue (Wakefield *et al.* 1998: 641).

And the last paragraph of the paper reads:

> We have identified a chronic enterocolitis in children that may be related to neuropsychiatric dysfunction. In most cases, onset of symptoms was after measles, mumps, and rubella immunisation. Further investigations are needed to examine this syndrome and its possible relation to this vaccine (Wakefield *et al.* 1998: 641).

Given that these two passages in the paper make it quite clear that any talk about causal relationships would be premature, it seems that Wakefield's comments about the safety of the MMR vaccine are not backed up by constitutive work that is designed to establish a causal relationship between MMR vaccine and the syndrome.

One problem with Wakefield's claim about the risks of MMR vaccine is that it conflates 'association', the term used in the paper, and 'causation'. While it is relatively common knowledge that correlation and causation are not the same, it follows that association, which refers to an even less established relationship between two phenomena than correlation, should also not conflated with causation. A second problem is that the association reported in the published paper is based on an extremely small sample of 12 children. Given that in the late 1990 more that 500,000 children were annually inoculated with the MMR vaccine in the United Kingdom and given that autism spectrum disorders (ASD) can be diagnosed in children as young as 6 months (although the average age of diagnosis is about 3.1 years), some degree of correlation between diagnosis of ASD and MMR vaccination is statistically to be expected (Mandell et al. 2005). A third problem is that the reported correlation between vaccination and behavioural changes in children were entirely based on reports by parents. As an accompanying comment on the Wakefield paper in the same issue of the Lancet observed, determining the exact onset of behavioural changes in small children is not an easy task (Chen and DeStefano 1998). Nonetheless, Wakefield's claims are

very much dependent on the diagnosis of the exact point in time at which behavioural changes occur.

Interim Conclusions

Wakefield's claims caused a publicly visible controversy as parts of the British media took the alleged MMR-autism link seriously (Boyce 2007). Some parents whose children were affected by autism also took Wakefield's claims seriously and started lobbying for the withdrawal of the triple MMR vaccine. The 'MMR causes autism' debate, however, fails the test of constitutive work – in 1998, Wakefield's claims were not yet backed up by any constitutive work. The UK MMR debate in 1998 (but also at any subsequent stage for that matter) has therefore to be classified as an inauthentic scientific controversy.

It is important to note the limits of the claims made here. All that is argued is that in 1998, when Wakefield begun to make public comments about a potential link between the MMR vaccine and autism, these claims were entirely unsubstantiated and that policy-makers were justified not to take them serious. This does not mean that scientific research about aspects of the safety of MMR should stop or that MMR has been conclusively proven to be safe. It might indeed be the case that specific population groups react adversely to the vaccine or that other forms of inoculation prove to be safer than a triple-jab. The concerns of members of the public could, to some extent at least, have some influence on the direction of future scientific research.[166]

7.6 Conceptual Continuity with Science and Intelligent Design

The fourth and last criterion proposed here – 'conceptual continuity with science' – is informed by Collins' and Evans' (2007) attempt to demarcate science from pseudo-science (see chapter 1). To demarcate science from pseudo-science, Collins and Evans (2007: 128) have suggested the so-called 'family resemblance rule':

[166] This is a difficult point as there is a scarcity of resources available for research, which might lead to conflicts about priorities among members of the public. For example, while some parents might argue that funding should be provided for more research on the potential link between MMR and autism with regard to particular population groups, other members of the public might argue that the funds needed to do this research might be better spent elsewhere.

> Except where specific new findings demand a break, the intentional stance of science must be to maintain continuity as far as possible with the existing science.

The 'intentional stance' refers to the idea of 'formative intentions', which was introduced in chapter 1. Unlike the intentions of individuals, formative intentions are 'public' and the 'property of collectives' (Collins and Evans 2007: 116). Although the 'family resemblance rule' puts emphasis on continuity, it does not mean that science has to keep every piece of knowledge or that it cannot develop quickly and go through paradigm changes:

> [S]cientists pushing forward in the new direction have the intention to change as little as possible consistent with their new theories and findings. They do not want to overthrow the scientific method, nor the greater body of scientific findings, nor the major social institutions of science, nor the existing data of science. They do not want to become outsiders. Paradigm revolutionaries aim to persuade the same scientists to think and act in a new way within their existing institutions, preserving as much as possible of what already exists and their links with it. (Collins and Evans 2007: 130)

Quick changes and even 'revolutions' happen, but the important aspect is that the intentional stance of scientists is to preserve as much of the previous sciences as possible.

The operationalisation of the criterion should therefore focus on whether claims that are supposed to challenge accepted scientific beliefs conform to the formative intention of retaining as much as possible of continuity. How this can be done will be illustrated by analysing a 'controversial' claim made by proponents of Intelligent Design (ID). Supporters of ID claim that their 'theory of creation', which invokes an unspecified supernatural entity, represents a genuine scientific alternative to the Darwinian view on evolution. For this reason, they claim that ID should be part of the science curriculum in American schools.[167]

[167] I do not agree with Nathan Geffen (2005) who argues that AIDS sceptics are doing 'pseudo-science.' The reason for this is that they usually do not try to break with basic scientific methodologies, methods or institutions. A couple of AIDS sceptics have proposed alternative theories of AIDS causation, which use established methodologies and methods. They have also, initially at least, tried to publish those alternative theories in peer-reviewed journals. Their theories might be regarded as wrong and potentially dangerous when taken seriously by policy-makers and there are good reasons to believe that some of them at times, show a lack of integrity (Nattrass 2010). They are not, however, pseudo-science.

Can ID be part of 'Western science'?

Let it be assumed that ID makes novel claims and is not just a rebranded version of creationism as some critics argue. According to its supporters, ID is a serious scientific challenge to the established evolution theory as it apparently identifies anomalies and provides alternative explanations for various phenomena. They therefore claim that a genuine scientific controversy between proponents of the theory of evolution and proponents of ID exists or at least should exist.

With regard to the three previously introduced criteria, ID supporters have a case. Given that ID proponents brand their claims as new, the explicit argument criterion does not really apply as it might be argued that there has simply been no opportunity for an explicit debate. Some proponents of ID, such as one of the main witnesses of the defendants in the United States District Court case of *Kitzmiller v Dover Area School District* heard in Dover, Pennsylvania, Dr. Michael Behe, can be classified possessing contributory expertise in relevant domains such as biology and biochemistry.[168] ID supporters can also point to constitutive work that allegedly supports their claims.[169]

Despite potentially passing or evading the three demarcation criteria proposed earlier in this chapter, the challenge of the theory of evolution by proponents of ID should not be mistaken for a genuine scientific controversy. This is because ID fails to maintain a conceptual continuity with science in at least two important respects. First, while science is generally regarded as an open-ended activity – every solution will bring other problems to the fore – ID 'science' is finite. Once the ID proponents have shown that, for example, complex organisms in nature are the result of the work of a designer or deity of some sort, its 'science' stops. As Collins and Evans (2007: 130) point out, proponents of ID "want science to become much more compatible with something beyond science – the idea of a divine intelligence." This would change the scientific form of life in a radical way:

> "Were they [proponents of ID] to succeed, the methods of science would change in that texts of obscure origin and the revealed certainties of faith would play a

[168] 400 F. Supp. 2d 707 (M.D. Pa. 2005). The full hearing transcripts, and the final judgment can be accessed at <URL: http://www.talkorigins.org/faqs/dover/kitzmiller_v_dover.html>.

[169] The *Discovery Institute* provides a list with peer reviewed articles that apparently support ID. The list can be accessed at <URL: http://www.discovery.org/a/2640>.

> much larger role in the gathering of knowledge and the assessment of its value (Collins and Evans 2007: 130).

In this sense, there is no intention to make ID fit with the existing body of science as a form of life.

Second and closely related to the first point, ID relies on the idea of some degree of 'supernatural' intervention. While some proponents of ID try to avoid talk of 'God' or 'The Creator', concepts such as 'guided mutation' make it clear enough that some supernatural entity has to play some role in the evolution of organisms. This makes ID claims essentially irrefutable and impossible to falsify (see also Ruse 1982). In his final remarks, the judge in *Kitzmiller* summarised this Popperian notion when he ruled against the scientific status of ID:

> There's a reason that science does not consider the supernatural. It has no way of measuring or testing supernatural activity. As Professor Behe testified, you can never rule out intelligent design.
> Defendants' comparisons to the big bang or Newton's work make no sense, for those, as with many scientific propositions, we may have at one time attributed natural phenomena to supernatural or divine action before working out the natural explanations that fall under the heading "science."
> Intelligent design is moving in the opposite direction, replacing a well-developed natural explanation for the development of biological life with a supernatural one which it has no evidence to support.

Interim Conclusion

It is argued that the claims put forward by the ID movement lack the intention to fit into the existing body of science. If supernatural entities can be invoked in scientific explanations, it would not only radically change scientific practices – empirical research might become redundant as one can always invoke supernatural causes – but also change the formative intentions of science. It is therefore argued that claims motivated by ID are not 'scientific claims', even though proponents of ID try to argue this. It follows that the claims of ID cannot represent a 'scientific' challenge to certain aspects of biological evolution theory, even though ID and evolution theory might be mutually exclusive and contradictory.

This does not mean that people should not believe in ID explanations of evolution. All that is argued is that religious beliefs must not be disguised as 'scientific' beliefs to bring about certain political consequences such as a decision to teach ID alongside evolutionary theory in science classes in schools. Policy-

makers might decide that ID should indeed be taught in science classes, but this cannot be justified on the basis that ID is 'scientific.'

7.7 Sociological Discrimination as a new type of expertise

At the beginning of this chapter, it was argued that the operationalisation of the criteria matters. This raises the question of whether the knowledge that informs the specific operationalisation of the four criteria in this chapter might indicate the existence of an identifiable type of expertise. It has been mentioned above that the operationalisation of the four criteria is informed by knowledge that is derived from STS in general and SEE in particular.

The application of STS methodologies and theories to instances of science under Wave 2 has resulted in a particular *understanding of the nature of science.* It is argued here that it is this particular understanding of the nature of science that constitutes a specific type of transmuted meta-expertise, when it is used to inform science-related judgements such as judgements of the authenticity of scientific controversies or, which is also possible, of the credibility of particular scientific claims.[170]

Some elements of 'STS-informed understanding of the nature of science' or 'sociological discrimination', as it will be referred to hereafter, have already been mentioned in the context of the operationalisation of the four criteria. As discussed, not any form of disagreement between scientists about a fact or issue represents a scientific controversy, 'formal credentials' is an impoverished indicator for expertise and scientific claims should be supported by some sort of 'constitutive work.' Other general insights about the nature of science, which might be more or less relevant for thinking about the relationship between science and policy, have been summarised in the literature.

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[170] This might not be recognised as 'expertise' by those committed to Wave 2. It is arguable, however, that one has to apply some sort of understanding of the nature of science if one is engaged in case study research under Wave 2. Researching case studies requires involves drawing boundaries between relevant and irrelevant events, actors, and claims and so on. Moreover, a concept such as 'closure of a scientific controversy', which is central to Wave 2 controversy studies, requires social analysts to make judgements about relevant and irrelevant claims (*e.g.* Collins 1981a, Martin and Richards 1995). This has to be the case as it is almost always possible to find someone who thinks that a controversy is still open – when social analysts declare that a scientific controversy has been closed, they judge that those actors' claims can be ignored.

Important insights provided by STS are that science is intrinsically political and value laden (*e.g.* Krimsky 1984, Jasanoff 1990, Douglas 2009), that the acceptance of scientific knowledge hinges critically upon credibility (Barnes and Bloor 1982, Shapin 1995), that scientific knowledge becomes politicised as soon as it enters the political realm (*e.g.* Nelkin 1971, 1975, Collingridge and Reeve 1986, Jasanoff 1990, Weingart 1999), that scientific consensus building can last decades and is generally slower than political decision-making (*e.g.* Collins and Evans 2007), and so on.

The fact that sociological discrimination is a transmuted meta-expertise that is constituted by an understanding of the nature of science based on STS insights must not be taken to mean that sociological discrimination will always remain exclusive to STS scholars. The concept of the 'double hermeneutic' can be invoked to explain why this is unlikely. The idea of the double hermeneutic is often invoked as a justification for the argument that social and natural science are fundamentally different. The idea of double hermeneutics refers to the idea that domain-specific concepts used by social analysts and 'everyday' concepts used lay actors mutually influence each other.[171] This means that social science concepts (or analyst's categories) are influenced by the way in which actors use these concepts (or actor's categories) in everyday life, while, at the same time, actor's categories are influenced by analyst categories. For example, a concept such as 'social class' that appeared in 'sociological' discourse a few hundred years ago, has been taken up and is applied by lay people in everyday life. This lay use of ideas of social class might influence social sciences, when sociologists create theories of social class on the basis of empirical studies that analyse how lay people use such a concept.

According to the idea of the double hermeneutic, it is to be expected that non-sociologists have become aware of the post-positivist understanding of the nature of science and apply this understanding in their everyday life. Indeed, while some 'lay people' might have come to similar conclusions as STS scholars

[171] Anthony Giddens defines the concept in the following terms: "The intersection of two frames of meaning as a logically necessary part of social science, the meaningful social world as constituted by lay actors and the metalanguages invented by social scientists; there is a constant 'slippage' from one to the other involved in the practice of the social sciences" (Giddens 1984:374).

about the nature of science due to their personal experiences, the 'wisdom' of sociologists is widely accessible in modern societies. Lay people might attend public lectures by STS scholars or they may read features written by STS scholars in general science journals such as *Nature* and *Science* or popular magazines such as *New Scientist* and *Scientific American*. They can also read popular STS books written for a wider audience such as the *Golem* series (Collins and Pinch 1998[1993], 1998, 2005). And, of course, a wide range of blogs are available on the Internet that are dedicated to the development of a better understanding of the nature of science such as Ben Goldacre's 'bad science' blog.[172]

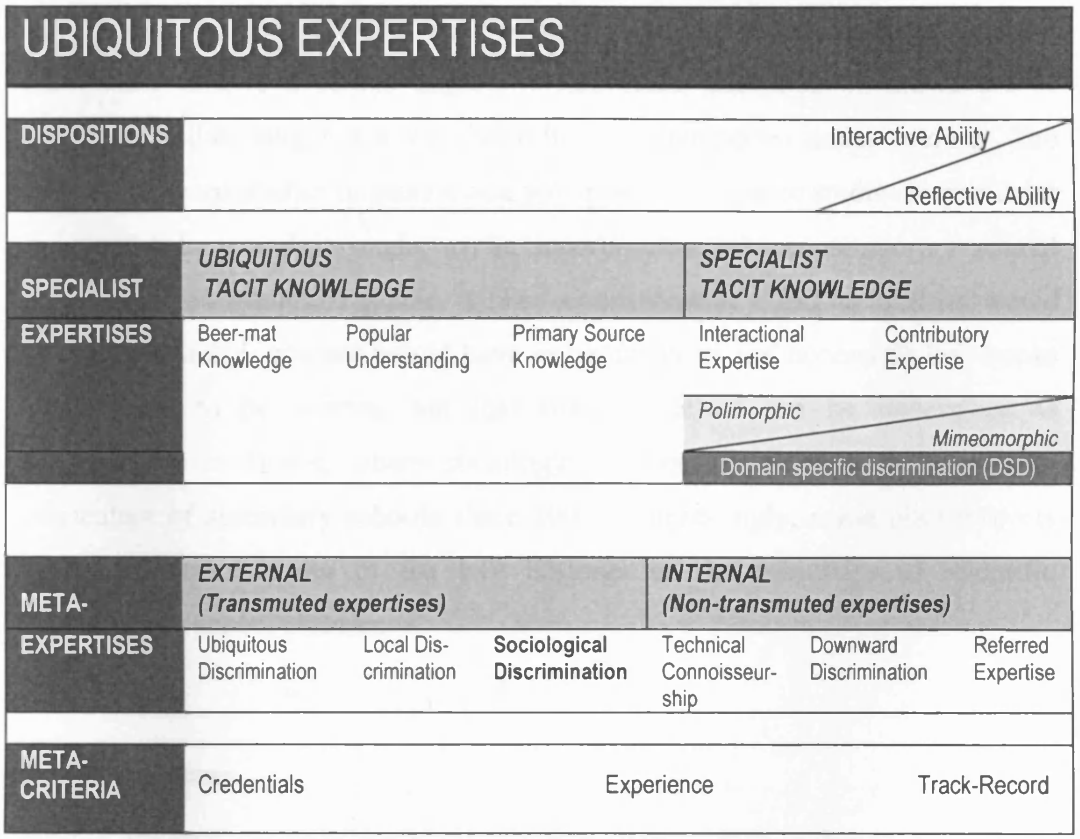
Despite the potential of the popularisation of an STS-informed understanding of the nature of science, it is arguable that such an understanding is still not widely distributed within modern societies. This claim is mainly based on a general impression and not on conclusive empirical evidence. An argument to support this claim is that post-positivist science studies have a relatively short history and has therefore had far less time to disseminate its findings than positivist science studies and philosophy of science. Despite its growth in recent years, STS is still a distinctively niche discipline. The claim can also be supported with the help of a few observations. It is telling, for example, that the concept of 'evidence-based policy-making' that was heavily promoted when New Labour came to power in 1997 has hardly received any critique apart from a few academics, despite its obvious roots in a positivist understanding of science (*e.g.* Stilgoe *et al.* 2006, Fischer 2009). And while the attack of scientists and sociologists with positivist inclinations on relativist STS, which has become known as the 'science wars', can be regarded as proof that relativist STS has come a long way since the 1970s, it also shows that most scientists and public commentators regarded post-positivist STS as a regrettable and dispensable expression of post-modernism (*e.g.* Brown 2001, Labinger and Collins 2001). The case study presented also shows that Mbeki as well as most of his critics actually shared a positivist view of the nature of science. Thabo Mbeki, despite

[172] One might say that the main difference between social researchers with a professional interest in understanding the nature of science and lay people is not one of epistemic capacity; rather, it is one of opportunity. Social researchers are in an arguably much better position than most as the acquisition of a general understanding of the nature of science comes as a 'by-product' of their daily work.

his doubts concerning actual scientific knowledge around the benefits and risks of using AZT for PMTCT, nevertheless exhibited a decidedly positivist view of science; take for example his claim that the 'truth' about the safety of AZT could be established, or indeed, his claim that if experts only considered the fact they would agree on matters of fact (*e.g.* Mbeki 1999, Mbeki 2000a, b). Critics of Mbeki repeatedly pointed out that only science could provide true facts and that, as a matter of fact, AZT had been proved to be safe (*e.g.* Cohen 2000, Makgoba 2000, Nature 2000).

If it is accepted that the STS-based understanding of the nature of science is still relatively unrecognised in the wider societal context, it follows that sociological discrimination has to be distinguished from ubiquitous discrimination. Although sociological discrimination is most likely to be a sub-type of local discrimination as it reflects a certain socio-metric vantage point on the nature of science, sociological discrimination is added as a separate form of transmuted meta-expertise to the PTE until more sub-types of local discrimination are recognised. A new version of the PTE, which includes sociological discrimination, is represented in Figure 6:

Figure 6: A new version of the Table of Expertises



It ought to be possible, in principle at least, to make what is called sociological discrimination today part of the ubiquitous discrimination of tomorrow. In contrast to earlier and largely ill-fated attempts to educate the wider public about the *content of science* under the banner of 'public understanding of science', an educational effort needs to concentrate on relaying insights about the *nature of science*.[173] Given the societal usefulness of sociological discrimination, a case can be made for a new programme that leads to the 'public understanding of the nature of science' (PUNS), which is informed by Wave 2 and Wave 3 of science studies.[174]

[173] The short-comings of the Public Understanding of Science and its association with the deficit model are well-established (*e.g.* Irwin and Wynne 1996, Miller 2001, Burns, O'Connor and Stocklmayer 2003, Sturgis and Allum 2004).

[174] This is also known as the 'public understanding of the process of science', which is part of Jon Miller's (1998) concept of 'civic scientific literacy.' Kutrovátz (2010) has recently used another interesting term, namely 'social intelligence about science', to express a similar point.

This can and possibly ought to start at school level. At the moment, it seems that children are predominantly taught specific content of science, *e.g.* they learn domain-specific specific knowledge in biology, chemistry, physics and so on. While this, to a certain degree, is important, education on the nature of science should be taught in a way that reflects contemporary understanding. The output of science studies in general and post-positivist science studies in particular is relevant here and it ought to be incorporated into contemporary school curricula. This would, of course, require a considerable effort as teachers would have to be trained, courses would have to be designed and accessible text books would have to be written, but that such an effort can be undertaken is demonstrated in Brazil, where sociology has been a mandatory part of the curriculum of secondary schools since 2007. Interestingly, some pilot projects specifically teach some of the key findings of the sociology of scientific knowledge to school children.[175]

7.8 Conclusions

In this chapter, an approach has been introduced which makes it possible to exercise judgements about the authenticity of scientific controversies that might be invoked in the course of particular technological decision-making processes. The approach consists of a set of four criteria that are designed to guide actors through the judgement process. The hope is that the reliance upon these criteria makes the judgement process as transparent and reliable as possible.

The set of criteria proposed here are derived from a ubiquitous understanding of key characteristics of scientific controversies. The four proposed criteria are 'explicit argument', 'expertise of claim-maker', 'constitutive work' and 'conceptual continuity with science.' It has been emphasised that these criteria can be interpreted and understood in very different ways. It has been suggested that operationalising the four criteria in accordance with an understanding of the nature of science that is compatible with Wave 2 and Wave 3

[175] Thanks to Rodrigo Ribeiro for information on this issue, that formed the subject-matter of his presentation at the 4S conference in Washington in 2009.

might prove to be fruitful. The respective operationalisations of the four criteria are summarised in Table 3:

Table 3: Overview of demarcation criteria

Purpose	Criteria	Operationalisation
To examine 'established' claims	Explicit argument	Analyse seemingly 'controversial claims' in larger context of published and /or oral discourse of a domain
To examine all claims	Expertise of claim-maker	Analyse social proximity of claim-maker to expert community
	Constitutive work	Analyse whether claims are supported by activities or output that can be reasonably classified as belonging to the 'constitutive forum'
	Conceptual continuity with science	Analyse whether claims are continuous with the formative intentions of science

The four criteria can be divided into two groups depending on what purpose they serve. The first criterion, 'explicit argument', is the main rationale to establish whether an allegedly long-standing scientific controversy is authentic or not. In contrast, the three remaining criteria can also be used to analyse the authenticity of apparently new scientific controversies as in such cases there might not yet have been time for explicit arguments amongst scientific experts to occur.[176]

If an alleged scientific controversy is found wanting with respect to any one of those criteria, it should be classified as an inauthentic scientific controversy. For example, the alleged scientific controversy about the safety of AZT, which affected technological decision-making between October 1999 and August 2000,

[176] An interesting consequence of taking the four demarcation criteria serious and using them to judge the authenticity of scientific controversies is that this 'creates' the category pairs of genuine scientific controversy v inauthentic scientific controversy as well as genuine scientific consensus v inauthentic scientific consensus. Further research is needed to test the suitability of the existing four criteria. The small number of case studies that have been used here to illustrate the potential usefulness of the criteria are admittedly 'extreme cases', in the sense that they appear to be very clear-cut. This has, however, been necessary to develop and test the criteria. It can be expected, however, that in many cases the criteria might not work and the analysis is too ambiguous to afford clear-cut judgements about the status of a scientific discourse. Further research might also find additional criteria, which would lead to the extension of 'sociological discrimination' beyond the four current criteria.

can be classified as an inauthentic scientific controversy because there was no explicit argument among members of the relevant expert community.

Recognising inauthentic scientific controversies and ensuring that they do not affect technological decision-making has significant practical value. The South African case study indicates that there was at least some chance that PMTCT might have been introduced earlier, if President Mbeki had been unable to influence the technological decision-making process by claiming that the safety of AZT was the subject of a scientific controversy.[177] As well as the potential saving of lives and reduction of suffering there is also a political imperative that makes the recognition of inauthentic scientific controversies important. Actors who try to use inauthentic scientific controversies effectively camouflage political arguments as scientific arguments. De-politicising matters that are political, as happens when certain issues are declared to be matters for expert inquiry and therefore beyond the lay public's grasp, denies citizens a proper political debate.[178]

The findings presented in this chapter suggest that the problem of extension does not necessarily apply to the authenticity question related to scientific controversies. While one might think that only those with sociological discrimination ought to make these judgements, such a conclusion is not warranted. The reason for this is that sociological discrimination is intrinsically transferable and can, through education, be popularised and thus made available to anyone. For example, someone without sociological discrimination who uses the four criteria as set out in this chapter inevitably learns the few parts of

[177] In chapter 5, it has been shown that the government maintained between October 1998 and August 2000 that a PMTCT programme on the basis of AZT was unaffordable. It is therefore not guaranteed that exposing Mbeki's claims about the safety of AZT as an attempt to invoke an inauthentic scientific controversy would have made any practical difference. It might have been the case, however, that the government was under considerable public pressure to introduce AZT-based PMTCT projects in South Africa, as indicated by Mbeki in his speech on 28 October 1999 and that the inauthentic scientific controversy about the safety of AZT provided it with urgently required 'breathing space.' This is, however, pure conjecture.

[178] This is the same point that underpins the Minimal Default Position. With regard to the MDP, it has been argued that policy-makers should not justify their policy-choices by invoking technical judgements that are not supported by expert consensus. The same point also applies to meta-judgements, which only indirectly involve technical judgements. Influencing technological decision-making by invoking an inauthentic scientific controversy has to be regarded as illegitimate as invoking a technical justification for a policy-choice that contradicts expert advice.

sociological discrimination that are embodied in the specifically operationalised criteria. Of course, this transfer of sociological discrimination is very limited.

Conclusion

In this thesis, two main contributions to the development of the SEE-based 3W model have been made. First, the model itself has been clarified and extended in such a way to make it possible to solve the problem of integration. Second, it has been explored whether and how far participation ought to be restricted with respect to judgements that involve meta-expertises. The final task is to synthesise the findings generated by the application of the 3W model to the South African case study and feed them back into the model and Wave 3.

Model Building

The 3W model is the application of the theory of expertise that is at the heart of the Studies of Expertise and Experience (SEE) research programme to the practice of technological decision-making. A number of important elements of such a model have been introduced by others over the last eight years or so. In this thesis, these previous contributions have been summarised and, where necessary, clarified.

An initial task in this respect has been to clarify the meaning of technological decision-making. Collins and Evans (2002) initial contribution used, at times, the terms *technical* and *technological decision-making* synonymously, despite the fact that these terms relate to different things. It was clarified in chapter 1 that, in the context of SEE, *technological decision-making* refers to two distinct practices. On the one hand, it refers to a specific form of policy-making which depends to a significant degree on scientific or technical knowledge. It has been pointed out that such a concept is well established in science studies and science policy literature. On the other hand, technological decision-making reflects a specific feature of the 3W model as it also refers to decision-making in what Collins and Evans have called the *political phase.* One of the tasks of actors participating in the political phase, besides framing,

deliberating, standard setting and so on, is to make policy decisions that incorporate scientific and technical knowledge.

Technical decision-making, in contrast, refers to a practice that is solely concerned with answering propositional questions of a scientific or technical nature that takes place in the *technical phase*. Crucially, the separation of technical and political phase is not based on the fact-value-dichotomy, but on differences related to actors, questions and values.

An aspect of the 3W model that to date has been overlooked relates to this separation of the technical and political phases, which creates what has been called the problem of integration. Although Evans and Plows (2007) have established that the distinctive phases of technological decision-making form a complementary relationship with regard to their respective functions, they have not addressed the question how technical judgements, generated in the technical phase, ought to impact on the political phase. This conceptual gap has been filled by the Minimal Default Position (MDP), which sets out how the dominance of the political phase in terms of technological decision-making can be squared with the autonomy of the technical phase in terms of making technical judgements.

The MDP suggests that consensual scientific advice, *e.g.* the answers to propositional questions exclusively addressed in the technical phase, should only constrain policy-makers participating in the political phase in their choice of justifications for technological decisions. Put differently, policy-makers retain full control over policy-choices, but while they can choose policies that do not follow from consensual technical advice, they cannot use justifications that contradict the consensual technical or scientific advice. Issuing consensual technical or scientific advice is a necessity in the context of the 3W model as it is otherwise impossible to retain the power for making policy-relevant technical judgements in the technical phase. It has been argued that consensus can be enforced through a variety of institutional mechanisms. In contrast, it has also been pointed out that varying degrees of certainty that different actors attach to the judgement should be respected. While there are a variety of ways to attribute degrees of certainty, the important aspect of certainty judgements in the context of

the MDP is that judgements that are too uncertain, according to a prearranged standard, should not be used as justifications for any policy-choices.[179]

In line with the prescriptions of the 3W model, 'science studies experts' such as the proponents of the 3W model are not in a position to impose their model on the practice of technological decision-making in polities; this has to remain the prerogative of the public in general. Social analysts can, however, test the virtue of the MDP as an analytical tool in an attempt to highlight the practical usefulness of the 3W model. The normative analysis in chapter 5 has shown that the Mbeki administration acted in contravention of the MDP in the context of the technological decision-making process about the prevention of mother-to-child transmission (PMTCT) on the basis of antiretroviral drugs between February 2000 and August 2000. According to the MDP, the South African government was not entitled to justify the non-provision of AZT for PMTCT by mid-February by citing doubts about the safety of AZT, because the Medicines Control Council (MCC) advised that the drug was safe for the purpose of PMTCT.

As such, the analysis is in accord with the widespread opinion that the sceptical stance of the government contributed to the unnecessary infection of children in South Africa with HIV. It argues that the *best* decision would have been to administer the drug. The analysis also shows, however, that the government's disrespect for science was more a symptom than a cause for controversial decision-making on PMTCT that, according to various estimates, cost the lives of tens of thousands of children. It was a symptom for unchecked and unaccountable decision-making power concentrated in the hands of a powerful few despite South Africa's undisputed status as a democratic state (see chapter 4).

The MDP as part of the 3W model provides social analysts with a tool for constructive critique of technological decision-making. As such it can help to critically monitor such decision-making processes. It enables critical observers to point out when justifications for policy-choices misrepresent or ignore consensual expert advice. What it cannot do, however, is to guarantee better policy-decisions

[179] As pointed out in chapter 3, if experts find a technical position too uncertain, there is no practical need for a consensus. A more flexible position is possible that allows experts to 'agree to disagree' and to issue different sets of recommendations.

as making policy-choices is the exclusive right of publicly accountable policy-makers.

The Problem of Extension

One of the main motivations behind the proposition of the 3W model is to provide an approach that allows making real-time contributions to technological decision-making under conditions of scientific uncertainty. Under technocratic models associated with Wave 1, only accredited scientists are entitled to make judgements about scientific and technical issues due to their presumed privileged access to reality and the resulting epistemological superiority of scientific knowledge. Under technological populism associated with Wave 2, anyone can, in principle, participate in making policy-relevant technical judgements. This is, partly at least, justified by pointing out that the distinction between scientists and lay-persons, on which the technocratic solution hinges, is not justifiable on epistemological grounds. Scientific knowledge is not inherently 'truer' than other forms of knowledge and privileging the input of scientists in the context of technological decision-making is unwarranted.

The approach of Wave 2 would only be acceptable in practice if the radical extension of public participation in all aspects of technological decision-making invariably leads to better or more desirable policy-outcomes. Empirical research, however, has shown that this is not always the case, at least as far as extended public participation in *technical decision-making* is concerned. In this respect, Collins and Evans usually invoke the example of the UK MMR controversy. This shows that the moral panic created by non-experts such as concerned parents and journalists about alleged side-effects led to a significant fall in vaccination rates in the UK and therefore endangered children, despite the lack of any tangible evidence that would support the notion of MMR's danger (*e.g.* Collins and Evans 2002, 2007, Collins 2007b, Collins 2007c, Evans and Collins 2007).

The South African case of technological decision-making about PMTCT provides another example of the way non-experts, in this case the higher echelons of the government, making policy-relevant technical judgements could have

devastating affects. According to recent estimates the non-provision of AZT for PMTCT, which was partly justified by government figures with claims that the safety of AZT was uncertain and subject of a scientific controversy, tens of thousands of children were infected with HIV through preventable MTCT (Chigwedere 2008, Nattrass 2008). Of course, it is not the task of this thesis to verify these claims, only to reveal what it might sometime mean not to make the *best* decision.

Wave 2's inability to provide a demarcation criterion to distinguish between desirable or legitimate and undesirable or illegitimate participation creates the 'problem of extension.' Collins and Evans (2002, 2007), in the context of the 3W model, have suggested a solution of this problem with respect to propositional questions of a technical or scientific nature. According to their proposal, only those with *relevant technical expertise* should be involved in addressing such policy-relevant questions. This solution does not pretend to lead to correct policy-relevant technical judgements, but it is claimed that leaving them to technical experts is likely to generate the best possible advice.

Under Wave 3, technical expertise is linked with experience and domain-specific tacit knowledge. This has two important implications, which differentiate Wave 3 from Wave 1: first, the possession of explicit technical knowledge is a necessary condition to be considered a technical expert, but it is not, on its own, sufficient. The 'difference-maker' is tacit knowledge, which enables actors to apply the explicit knowledge in varying and new circumstances.[180] Second, accredited scientists are not the only actors that can be classified as technical experts. Actors without any recognised formal education can be experts with regard to certain aspects of the world about which they have experience. For example, sheep farmers ought to be treated as technical experts with respect to issues of husbandry, while certain farm workers with experience in handling pesticides should be regarded as experience-based technical experts with respect to certain safety-related aspects of pesticide use.

[180] Another implication is that one cannot become an expert on the basis of exclusive reliance on book-based knowledge. The reason for this is that only known way to acquire tacit knowledge is to actively immerse oneself in a community of actors that already possesses it or is about to develop it.

Given that the problem of extension has been effectively solved with regard to policy-relevant technical questions, the focus of chapters 6 and 7 has been on exploring whether the problem of extension also applies to other types of policy-relevant questions.[181] In particular, the focus has been on so-called 'meta-questions.' Meta-questions have been characterised as propositional questions which are only indirectly or implicitly of technical or scientific nature. One example, of a meta-question, which was the focus of chapter 6, is the question about the state of a scientific discourse. The question is whether a technical discourse related to a particular fact or issue attracts consensus or controversy within the relevant expert community. While the question is clearly about the state of the relationship between those involved in the technical discourse, it has been shown that answering such a question inescapably involves credibility judgements related to technical matters. Another meta-question, explored in chapter 7, concerns the authenticity of a scientific controversy that is invoked in technological decision-making processes.

That answers to meta-questions can be highly relevant for technological decision-making has been illustrated with reference to the South African case study. It has been shown, for example, that Mbeki, correctly, claimed that all he did between 28 October 1999 and mid-February 2000 was to alert the public to a disagreement between experts about the safety of AZT which he detected while reading literature on the Internet. Mbeki's warning about the state of the scientific discourse about the safety of AZT had, however, concrete policy-implications insofar as it served as an additional justification for the government not to provide AZT for PMTCT.

The focus on meta-questions in chapters 6 and 7 has generated important insights. It has been established that the problem of extension can, to varying degrees, also be 'applied' to this type of question. This is an important finding because it opens up new avenues for research. It has been shown that the boundaries between legitimate and illegitimate participation require different demarcation criteria for technical questions and meta-questions respectively.

[181] While not the focus of this thesis, it has been established in chapter 6 that President Mbeki did not possess the relevant technical expertise on AZT to be regarded as a legitimate participant in making direct technical judgements about the safety of the drug.

While the problem of extensions can be universally solved for technical questions by using technical expertise as the demarcation criterion, both chapters 6 and 7 demonstrate that different meta-questions require different demarcation criteria.

The comparative analysis in chapter 6 suggests that the boundary between legitimate and illegitimate participation with respect to the meta-question about the state of a scientific discourse should be drawn just above the level of ubiquitous discrimination, the most common form of transmuted meta-expertises. Those relying exclusively on ubiquitous discrimination should not participate in making policy-relevant judgements about the state of a scientific discourse on the ground that they are socially too distanced from the issues under consideration, which leads to credibility judgements of a low quality. This also means that anyone else, whether non-expert relying on local discrimination or technical expert relying on specialist expertise might legitimately contribute to answering policy-relevant meta-questions concerned with the state of a scientific discourse.

The prescriptive analysis provided in chapter 7 has established that, in principle, anyone might legitimately answer meta-questions concerned with the authenticity of an invoked scientific controversy. A criteria-based approach has been suggested that affords authenticity judgements on the basis of non-technical considerations. The interpretation of the four suggested criteria is informed by a Wave 2- and Wave 3-compatible understanding of the nature of science, which includes a particular understanding of the nature of scientific controversies. It is the fact that the four criteria embody 'sociological discrimination', as the STS-specific understanding of the nature of science has been called, that makes it possible, in principle, for anyone to make legitimate authenticity judgements.

Further research is required to identify policy-relevant meta-questions and to explore where the line between permissible and impermissible participation should be drawn. The forays into this area made in chapters 6 and 7 can only be regarded as a first and tentative contribution.

Implications for the Periodic Table of Expertises

The application of the 3W model to inform the exploration of participatory restrictions with respect to meta-questions has led to rather unexpected discoveries. These discoveries have not only implications for the 3W model – which has to be extended to cover propositional meta-questions – but also for Wave 3. Specifically, the discovery of two 'new' types of expertises makes amendments to the Periodic Table of Expertises necessary and creates new research opportunities in the context of Wave 3.

The indirect or implicit technical element contained in the meta-question concerned with the state of a scientific discourse, explored in chapter 6, has been identified as a 'credibility judgement.' If one claims that a scientific discourse is controversial, one inescapably attributes credibility to at least two contrarian technical positions. If one, in contrast, claims that a scientific discourse illustrates consensus, one suggests implicitly that only one valid technical position exists. Wave 2 research suggests that relevant contributions to credibility judgements can be based on both technical and social considerations. This insight has been taken up and translated into the terminology of Wave 3; as such, it means that credibility judgements can be performed by technical experts and non-experts alike.

The classification of expertises provided by the PTE has made it possible, however, to explore whether any systematic differences in quality between the credibility judgements of different types of experts and non-experts can be observed. In chapter 6, the credibility judgements of a non-expert with ubiquitous discrimination, Thabo Mbeki, and of technical experts, 'mainstream' scientists, have been compared with respect to technical claims made by AZT critics. It has been found that these judgements differ significantly and that the difference can be attributed to differences in expertises.

In the context of this exploration, a well-known but, in the context of the PTE, overlooked, type of expertise has been (re-)discovered. It has been established that technical experts were able to assess the credibility of AZT critics' claims by using a combination of technical and social considerations. These social considerations available to experts, which have been summarised as 'domain-specific discrimination' (DSD), have not yet been accounted for in the

PTE. Formally, DSD might be mistaken for a type of transmuted meta-expertise. It is argued, however, that DSD is a constitutive part of specialist expertise. As a 'social dimension' of specialist expertises it also involves the acquisition of domain-specific tacit knowledge related to social aspects. It has been argued that technical experts acquire this in the same way as they acquire tacit knowledge related to technical aspects, namely by being immersed in a community of experts. The greater relative quality of the credibility judgements by experts compared to complete outsiders can be explained with the greater social proximity of the former to the issues claims under consideration.

Another new element of the PTE has been discovered in chapter 7. 'Sociological discrimination' constitutes an overlooked form of transmuted meta-expertise, although as a strand of self-conscious enquiry, has been anticipated by Collins and Evans (2007: 144) as social analysts' 'expertise about expertise.' Sociological expertise 'about expertise' is, however, just one instantiation of sociological discrimination. It raises a broad range of considerations which have been summarily referred to as 'understanding the nature of science.'

It has been argued that this specific understanding of the nature of science is the result of doing STS research under Wave 2 and Wave 3 and it must not be mistaken for the expertise required to do STS research. Like the findings of contemporary STS research, sociological discrimination is not widely distributed in modern societies, despite the importance of science and technology for everyday life. It has been argued, however, that this is not an unchangeable and inevitable state of affairs. Rather, sociological discrimination is transferrable and, depending on the educational effort, it could in principle become the 'new ubiquitous discrimination.'

The inherent transferability of sociological discrimination is demonstrated in chapter 7 when it informs the operationalisation of four criteria that have been proposed to guide authenticity judgements related to scientific controversies. Analogous to credibility judgements, authenticity judgements are at the heart of a meta-question concerned with the reality of scientific controversies invoked in the context of technological decision-making. The policy implications of this type of meta-question are evident in the South African case study given that the

invocation of a scientific controversy impeded the technical decision-making process for several months.

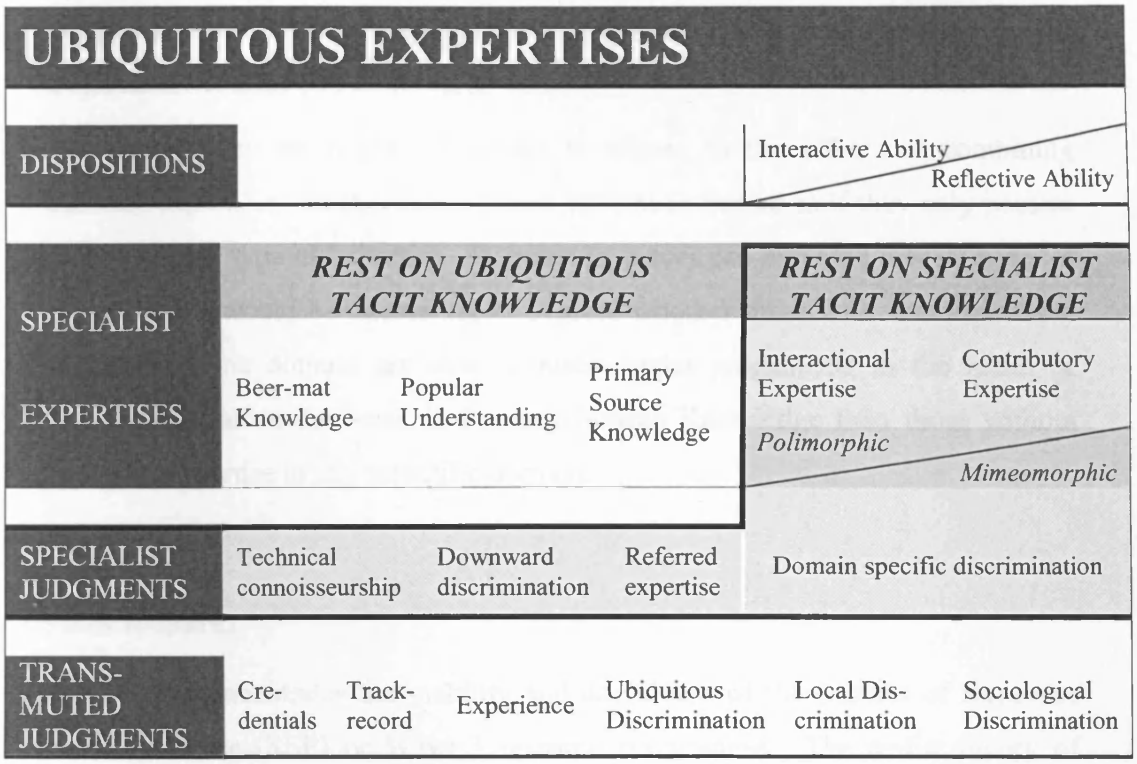
The application of just one of the four criteria, which embodies sociological discrimination, to the South African case study leads to the conclusion that the alleged controversy about the safety of AZT was not a genuine scientific controversy; there was no disagreement between members of community of relevant experts. As such this alleged scientific controversy should not have impacted upon the technological decision-making process.

It is argued that, in principle, anyone, including members of the South African government, could have used the specifically operationalised criteria to make an authenticity judgement. The operationalisation of the criteria, however, is critical. It has been shown that Thabo Mbeki's approach is rendered understandable by reference to some of the criteria; the problem however, was that he drew the 'wrong' conclusions about the authenticity of the alleged scientific controversy because he operationalised the criteria in accordance with Wave 1 insights. It might be argued that if President Mbeki had been less inclined towards Wave 1 understanding of science, the whole confrontation about the safety of AZT and the subsequent debate about the causes of AIDS could have been avoided.[182] Whether this had also led to an earlier introduction of drug-based PMTCT projects is, however, questionable given the governments public concerns about the affordability of AZT.

The two discoveries can be integrated into a reworked version of the Periodic Table of Expertises that is presented in Figure 7:

[182] This also mean that the millions of Rand spent on the Presidential Advisory Panel on AIDS could have been used for a more productive purpose.

Figure 7: Final Version of Modified Periodic Table of Expertises



Beyond adding domain-specific discrimination (DSD) and sociological discrimination, two significant modifications have been made. First, the original meta-expertise row has been split into two rows, one referring to specialist judgements and the other to transmuted judgements. The specialist judgement row contains what has been formerly classified as non-transmuted meta-expertises, such as technical connoisseurship, downward discrimination and referred expertise. This row is now connected with the part of the specialist expertises row that contains interactional and contributory as well as the related DSD, because all those forms of expertises rest on some form of specialist tacit knowledge. The transmuted judgements row unifies the former transmuted part of the meta-expertise row as well as the meta-criteria. What transmuted judgements have in common is that they do not rest on the possession of specialist tacit knowledge.

It can be predicted that this modified version of the PTE will not be the last. Many of its elements are still only derived on the basis of theoretical considerations. Expertises such as 'referred expertise', 'technical

connoisseurship' as well as 'local discrimination' have yet to be investigated in depth. This thesis has shown that when types of expertises are applied to case studies, discoveries of new forms and types of expertises as well as the need for major modifications cannot be ruled out.

Another as yet neglected aspect is related to the effects of combining different expertises. In this thesis actors have been treated as if they only possess one or another type of expertise. In practice, actors can combine several types of expertises. It would be interesting to explore whether those with some specialist expertise in one domain are able to make better judgements in the realm of another domain on the basis of Primary Source Knowledge than those without specialist expertise in any scientific domain.

Final Remarks

The thesis demonstrates the viability and usefulness of the Studies of Expertise and Experience (SEE) or Wave 3 research programme. The realist theory of expertise, that provides the core of Wave 3, has allowed for a number of important insights. With regard to the South African case study, it provides an important resource for constructive critique in cases where scientific advice is disrespected. The critique is not only constructive, because it is possible to point out how technological policy-making can be improved; Wave 3-based critique also offers the chance to improve the public understanding of the nature of science. Unfortunately, in the case of South Africa's controversial HIV/AIDS politics, this opportunity was missed given that the debate was dominated by a Wave 1 understanding of science. Truth, certainty, credentials, incontrovertible evidence and many other terms that reflect positivisms dominated the political discourse.

This does not mean a return to technocracy and giving up on aspirations to democratise decision-making. The thesis lends credence to the view that a balance has to be struck between the power of policy-makers and scientific experts in technological decision-making processes. History has already shown that too much power for scientific experts does not necessarily lead to societal well-being. The South African case study serves, however, as a powerful

reminder that the opposite of technocracy, technological populism, is also capable of producing catastrophic outcomes. Wave 3 and the 3W model make proposals as to how to achieve a better balance between scientific experts and policy-makers and between the technical and political phase.

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