The Dilemma of National History

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Knowledge in Ferment

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Dilemmas in Science, Scholarship and Society

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Leiden University Press

Cover design: Kok Korpershoek, Amsterdam, The Netherlands
Lay-out: ProGrafici, Goes, The Netherlands
Translation advice: Marilyn Hedges
Editorial assistance: Natasja Schellaars
www.leiden.edu

ISBN 9789087280178
NUR 130/740

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Preface

Dilemmas, fundamental controversies, basic oppositions between methods and approaches, occur in all fields of science and scholarship. Often dilemmas arise at the interface where science and society meet, or whenever several sciences or disciplines clash. The paradox of dilemmas is that although one might prefer to do without them, they are nevertheless indispensable. Without dilemmas progress in science and scholarship would be unthinkable. New paradigms come into existence and compete with the old for acceptance. Thus, by inciting researchers to make new efforts and pose new questions, dilemmas reveal new insights and sustain the ferment of knowledge.

As the Rector Magnificus of Leiden University for six years, from 2001 to 2007, Professor Douwe Breimer devoted his great talents and his best endeavours to developing and improving teaching and research inside and outside Leiden. As Professor of Pharmacology in Leiden from 1975, of Pharmacology and Pharmacotherapy from 1981, Breimer was the architect of, first, the Center for Biopharmaceutical Sciences (1983), then the Center for Human Drug Research (1987) and finally the research school, the Leiden/Amsterdam Center for Drug Research (1992). In 1984 he became Dean of the Faculty of Mathematics and Natural Sciences. Breimer's meritorious services to scientific research and to the organisation and development of science have been recognised in the seven honorary doctorates which he has received from universities all

over the world. But as Rector Magnificus, Douwe Breimer has been much more than the champion of the natural and life sciences, for he has also upheld Leiden's pre-eminence in the humanities, jurisprudence and the social and behavioural sciences. As a scientist, an administrator and especially as Rector Magnificus Breimer has been accustomed to act with circumspection, but also with decisive vigour. He has always shown himself to be one of the *esprits préparés* of Louis Pasteur's dictum, 'Le hasard ne favorise que les esprits préparés', a saying very dear to his heart. But he is also the embodiment of a proverb in his own mother-tongue, Frisian, 'Sizzen is neat, mar dwaen is in ding' (talk is nothing, but doing is something). He always was, and is, a man with style.

During his rectorship Douwe Breimer has enjoyed the deep respect and warm sympathy of the whole University. The University continues to regard him with pride and admiration. On his retirement as Rector Magnificus his friends and colleagues wished to demonstrate their gratitude by offering him this volume of studies. They have chosen as its theme 'Knowledge in ferment: dilemmas in science, scholarship and society'. In the word 'ferment' one may detect an allusion to a phenomenon in Breimer's own field of study; but it also refers to the catalytic role that dilemmas play in the development of science and scholarship. Colleagues from all Faculties and many departments of the University have contributed with enthusiasm to this volume. Authors and editors offer it to Douwe Breimer as a tribute of their gratitude, respect and friendship.

Leiden, 8 February 2007

Adriaan in 't Groen
Henk Jan de Jonge
Eduard Klasen
Hilje Papma
Piet van Slooten
Editors

To Douwe Breimer

on the occasion of his retirement as Rector Magnificus
of Leiden University
after a six-year term of office (2001-2007).
During these years he has inspired the University through
the example of his exceptional scientific achievements and his ideal
of the university
as promoter of welfare, well-being and culture.

He has exercised his office with unflagging energy, uncontested authority,
a rigorous insistence on the highest academic standards,
the wisdom of his judgement and experience,
his profound humanity
and grand style.

Introduction: Dilemmas in science: what, why, and how

James W. McAllister

Dilemmas are important turning points in the development of science and in the experience of scientists. They probably create more perplexity and even distress for scientists than any other predicament. If only for this reason, it is worthwhile attempting to understand what dilemmas are, why they occur, and how they can be resolved. In this introduction, I develop a concept of scientific dilemmas as conflicts between the epistemic values acknowledged by scientists, for which no wholly satisfactory resolution is available. This means that scientific dilemmas resemble moral dilemmas in important respects, and differ from scientific controversies. After having explained what features of science make it possible for scientific dilemmas to arise, I give an example of scientific dilemma in the development of quantum physics. I conclude by arguing that the practical resolution of scientific dilemmas often depends on the emotional responses of scientists to the epistemic values in conflict.

Controversies and Dilemmas

In order to clarify the nature of scientific dilemmas, it is useful to review in what respects they differ from scientific controversies and in what regards they resemble better-known dilemmas in other spheres. By doing so, we will be able to judge what is common to dilemmas of all kinds and what makes a dilemma specifically scientific.

A scientific controversy is a dispute arising from a difficult choice

between incompatible beliefs. During a controversy, adherents of each of the alternative beliefs adduce arguments and empirical evidence in its support. In due course, the arguments and evidence for all but one of the conflicting beliefs come to be regarded as mistaken or as having been interpreted incorrectly, while the remaining belief is accepted as correct. Occasionally, the belief that is eventually adopted is not one that sparked the controversy, but was formulated at a later stage, perhaps in the effort to combine insights of different competing beliefs. Nonetheless, at the closure of a controversy, one belief is accepted as correct while beliefs incompatible with it are rejected.

Controversies are a routine occurrence in science. Scientific controversies may hinge on various issues: what the empirical data indicate, which of two theories is superior, what the explanation of an effect is, and so on. A famous controversy occurred in cosmology between adherents of the big-bang and the steady-state theories of the universe from around 1940 to 1965. The big-bang theory prevailed in this controversy, largely on the strength of logical considerations as well as of the great expansion of the empirical basis of cosmology. Another controversy occurred in palaeontology between those who believed that the mass extinctions at the end of the Cretaceous period were due to an asteroid impact and those who attributed them to volcanic eruptions. It appears that this controversy, which started around 1980, has now been resolved in favour of the asteroid hypothesis.[1]

Dilemmas, by contrast, are cases of conflicting obligations. A person caught in a dilemma has an obligation to perform two or more actions that are incompatible. In a dilemma, a person seems condemned to fall short of what is required: irrespective of the choice that he or she makes, some obligation will be violated. Whereas controversies generate divisions between different participants, dilemmas give rise to intellectual and emotional tensions within each participant.

The concept of dilemma occurs prominently in two areas of scholarly inquiry: game theory and moral philosophy. The prisoner's dilemma in game theory is in many respects the archetype. It arises in a two-player game in which each player must decide whether to cooperate with the other player or to defect from the implied pact between them. The optimal outcome for the two players combined would be attained if both players cooperated. However, irrespective of the other player's actions, each player can improve his or her own outcome by defecting. The

dilemma arises because it is not possible to conform both to the dictates of individual rationality, which suggest that a player defect, and to those of group rationality, which suggest that he or she cooperate. Each possible decision is good in some respects but poor in others – a point made in the technical language of game theory by the statement that the Pareto optimum and the Nash equilibrium do not coincide.[2]

The dilemmas discussed in moral philosophy involve conflicts between moral requirements. Some moral dilemmas are caused by the impossibility of discharging distinct obligations. An example is a situation in which telling the truth conflicts with preventing harm. Other moral dilemmas arise because contingent features of the world do not allow us to discharge a single precept consistently or universally. For example, saving the life of one person may, because of contingent circumstances, necessitate letting another person die.

Some past philosophers thought that moral dilemmas were impossible, or even that the concept of moral dilemma was unintelligible. Immanuel Kant held that his moral monism prevented moral commitments from conflicting, for example, and utilitarian thinkers such as John Stuart Mill believed that any apparent dilemma between courses of action could be resolved by a more general application of the principle of utility. Most present-day philosophers, by contrast, accept that moral dilemmas occur. Dilemmas have their root in the structure of our moral systems. If our moral codes were axiomatic systems, in which each precept were a consequence, specification or application of a more general precept, any apparent clash between two precepts on one level could be resolved by consulting the precept from which they both followed. Instead, our moral codes do not form stable hierarchies, so there is no principled way to resolve all conflicts.

Any practical resolution of a moral dilemma, because it involves violating an ethical principle, leaves a moral residue, consisting typically of sentiments of regret, remorse, or guilt. Citing this phenomenon, Bernard Williams has argued that, in important respects, moral dilemmas resemble conflicts of desires more than conflicts of beliefs. When we accept just one of two conflicting beliefs, we reject the other belief wholeheartedly. There is no regret at losing a belief that we now regard as false. By contrast, when we act on one of two conflicting desires, the overridden desire is not rejected: it manifests itself in regret for what we miss. Our experience of moral dilemmas shares this feature.[3]

The Rise of Scientific Dilemmas

Among the domains in which moral dilemmas arise are scientific research and the application of scientific techniques. Biomedical science and health care are an example. When experiments are conducted with human or animal subjects, conflicts can arise between the interests of the subjects, the experimenters, science as an ideal institution, people with certain illnesses, the wider public, and other stakeholders. One of the most widely cited ethical frameworks in health care is that of Tom L. Beauchamp and James F. Childress, which centres on the four principles of respect for autonomy, beneficence, non-maleficence, and justice. Because they acknowledge a plurality of principles, Beauchamp and Childress implicitly allow that moral dilemmas occur. They therefore rightly emphasise the need to exercise individual judgement in health care decisions.[4]

Moral dilemmas also arise, of course, in the management of scientific institutions. For example, Max Planck worked to reach a succession of uncomfortable accommodations between political forces, scientific values, and his own world view during his stewardship of German science in the 1930s.[5]

However, alongside moral dilemmas such as these, scientists also face dilemmas of a different kind: specifically scientific dilemmas, caused by conflicts between epistemic values. Science acknowledges a system of values that is similar in structure to our moral codes. The practice of science is frequently described as having an overarching goal. This goal is usually formulated in terms of attaining truth, empirical adequacy, or practical applicability. In practice, however, scientists acknowledge a variety of epistemic values. These include predictive accuracy, completeness, consistency, simplicity, objectivity, intelligibility, tractability, metaphysical acceptability, and aesthetic merit. It is plausible to assume that many of these epistemic values are functionally related in some way to the goal of science. For example, if we formulate the goal of science as the attainment of truth, it sounds plausible to say that the value of logical consistency follows from that goal. In fact, however, the relation between the goal of science and individual epistemic values acknowledged by scientists is never precisely determined. Instead, the various epistemic values take on a life of their own: scientists attach importance to them irrespective of their relation to the goal of science and of their mutual

compatibility. In consequence, the system of epistemic values acknowledged by scientists, like our moral codes, contain a plurality of values that do not form a stable hierarchy.

In many cases, no conflict occurs between the epistemic values acknowledged by scientists: the pursuit of intelligibility may coincide with the pursuit of predictive accuracy. In other cases, however, these values pull in different directions: predictive accuracy may conflict with simplicity, for example, or simplicity with intelligibility. This is the origin of specifically scientific dilemmas.[6]

Whereas the obligations on scientists in a scientific dilemma arise from epistemic rather than from moral values, its structure parallels that of a moral dilemma: a scientist has an intellectual obligation to apply two or more methodological precepts that are incompatible. Any practical resolution of a scientific dilemma thus involves violating a methodological precept that scientists acknowledge as applying to them.

Scientific dilemmas have the following features, which are counterparts of aspects of moral dilemmas. First, scientific dilemmas, unlike mere controversies, do not consist primarily of a disagreement about matters of fact. Instead, they centre on methodological tensions. A scientific dilemma arises from the impossibility of simultaneously obeying all the rules of scientific procedure that are acknowledged to apply in a given situation. It is likely that different practical resolutions of a scientific dilemma will lead scientists also to adopt different beliefs about matters of fact, but that is not the crucial aspect of a scientific dilemma.

Second, participants in a scientific dilemma acknowledge that any possible practical resolution of it has disadvantages. These disadvantages stem from the fact that some applicable methodological precept is violated. This means that a scientific dilemma, like a moral dilemma but unlike a scientific controversy, admits no resolution that is satisfactory in all respects.

Third, because of this, a practical resolution of a scientific dilemma leaves a residue analogous to that which follows from moral dilemmas. The residue in scientific dilemmas usually falls short of the anguish that we experience in some moral cases: rather, it takes the form of epistemic dissatisfaction or a sense of lack of understanding. We may take the existence of this residue as indicating that an episode has constituted a scientific dilemma rather than merely a controversy: a scientist who has resolved a controversy may experience nothing other than self-

congratulation at having ascertained the facts, but the resolution of a true scientific dilemma does not afford such unalloyed satisfaction.

The idea that a scientific advance on one dimension may be accompanied by a retreat on another dimension was put forward by Thomas S. Kuhn. He suggested that scientific revolutions invariably involve losses in conceptual performance in some areas as well as gains in other areas – a phenomenon that later authors have dubbed 'Kuhn loss'. An instance of Kuhn loss creates a dilemma for the scientists involved in it, since any choice between the old and the new paradigms incurs some cost. Let us turn for an example to the transition from the Cartesian to the Newtonian account of the solar system. Newtonian celestial mechanics was superior to its Cartesian predecessor in many respects. However, it involved a loss of explanatory power in regard to certain features of the solar system. The Cartesian account was able to explain with ease the facts that the orbits of the planets lie in approximately the same plane, and that the planets orbit the sun in the same direction: these facts are consequences of the origin of the solar system in a vast vortex. By contrast, the Newtonian account regarded these facts as arbitrary initial conditions of the physical system, for which no explanation could be offered within the theory. A seventeenth-century natural philosopher who weighed this loss of explanatory power against the advantages of the Newtonian theory would have experienced a dilemma in theory choice.[7]

Dilemmas in Quantum Physics

If the concept of scientific dilemma has validity, then in order to identify examples of scientific dilemmas we must search for scientific advances that involved the violation of an acknowledged methodological precept for the sake of conforming to another precept.

The development of quantum theory in the 1920s and 1930s is a rich source of examples. Quantum theory was created in the attempt to account for experimental findings about some phenomena that had proved inexplicable to classical physical theories: black-body radiation, the photoelectric effect, and the absorption and emission spectra of atoms. However, quantum theory soon showed itself to be quite different from the theories of classical physics: it was indeterministic, portrayed the world in some respects as discontinuous, and did not lend itself to consistent visualisation. These features of the theory led to a dilemma

between the precept of accounting for empirical data and the precept of maintaining the style of theorising that had proved so successful in classical physics.

A particular manifestation of this dilemma pertained to the nature of the fundamental constituents of the physical world. Some experiments appeared to confirm both that electrons were particles and that electromagnetic radiation, including light, was a particle phenomenon; other experiments seemed to indicate both that electromagnetic radiation was a wave phenomenon and that electrons behaved as waves. A few experiments, such as the two-slit diffraction experiment, even provided support for both conclusions simultaneously. These outcomes meant that a conflict arose between three methodological precepts that had been in harmony in classical physics: the precept of formulating theories that have internal logical consistency, the precept of formulating theories that accord with empirical data, and the precept of visualising submicroscopic phenomena in classical terms. Physicists discovered that obeying any two of these precepts entailed violating the third. This predicament was often described by early quantum physicists as the wave/particle dilemma.[8]

On a more general level, the dilemma presented by quantum phenomena took the form of a conflict between the precepts of formulating theories that were deterministic and that provided visualisations of phenomena in classical terms on the one hand, and the precept of choosing the theories that best accorded with empirical data on the other. This dilemma split the physics community into two. One group, including Niels Bohr and Werner Heisenberg, judged the loss of determinism and visualisation a price worth paying for an empirically successful theory of subatomic phenomena. The other group, which included Albert Einstein and Erwin Schrödinger, acknowledged the empirical success of quantum theory but found the theory unacceptable in virtue of its violation of the classical precepts of theorising. Schrödinger was repelled especially by the abstractness of quantum theory: he strove to find a visualisation of the Schrödinger equation in classical terms, but it quickly emerged that none could be provided. By contrast, Einstein was displeased by the theory's indeterminism: for him the beauty of the world would be marred if God decided occurrences on the cast of a die.

Both groups could claim to be acting with justification. Bohr and Heisenberg followed well-established epistemic values in embracing the best-performing theory of subatomic phenomena available. But the other

group, too, could claim empirical support: the lesson of classical physics seemed to them that adherence to the principles of determinism and visualisation is crucial in the long term for scientific progress.

However, both groups also perceived the disadvantages of the practical resolution of the dilemma that they advocated. Bohr, Heisenberg, and other members of their group were not left unperturbed at the indeterminism and lack of visualisation of quantum theory: many of them found it unsettling not to be able to trust their classical intuitions in developing and applying physical theory. For instance, Bohr wrote that acceptance of quantum theory was made possible 'only by a conscious resignation of our usual demands for visualisation and causality'.[9] Einstein, Schrödinger, and like-minded physicists, for their part, recognised the disadvantage of rejecting the empirically best-performing theory of submicroscopic phenomena available. For example, Einstein accepted that quantum theory had demonstrated good empirical adequacy even as he resolved on balance to reject the theory. In the following passage, he formulates his objection in terms of an alleged incompleteness of quantum theory:

Experiments on interference made with particle rays have given a brilliant proof that the wave character of phenomena of motion as assumed by the theory does, really, correspond to the facts. In addition to this, the theory succeeded, easily, in demonstrating the statistical laws of the transition of a system from one quantum condition to another under the action of external forces, which, from the standpoint of classical mechanics, appears as a miracle .... In spite of this, however, I believe that the theory is apt to beguile us into error in our search for a uniform basis for physics, because, in my belief, it is an incomplete representation of real things .... The incompleteness of the representation is the outcome of the statistical nature ... of the laws.[10]

The participants thus acknowledged that none of the conceivable practical resolutions of the situation was ideal in all respects, and accepted the residue of epistemic dissatisfaction that would unavoidably follow. This acknowledgement and acceptance mark this case as a scientific dilemma rather than a simple controversy.

In the end, the dilemma was resolved in the way Bohr, Heisenberg, and their group advocated: by abandoning the classical precepts of theorising,

by deciding to base theory choice purely on empirical performance, and by accepting the concomitant loss of determinism and visualisation. For the specific wave/particle dilemma, this resolution entailed abandoning the classical terms of the discussion: the constituents of the physical world are neither classical particles nor waves, but rather radically new entities that may be termed 'quantum particles' or 'wavicles' and that have no precise counterpart in previous theories.

As this narrative shows, practical resolutions have now been found for some of the initial dilemmas of quantum theory. Nonetheless, the idea that quantum phenomena confront us with dilemmas remains a standard trope in present-day expositions of the theory. Physicists use it to remind students that quantum theory inaugurated an unprecedented phase in the development of physical science.[11]

The Role of Emotion in the Resolution of Dilemmas

How do scientists arrive at a practical resolution of a scientific dilemma? The crucial fact about scientific dilemmas is that they arise from a conflict between epistemic values. Any practical resolution of a dilemma violates an epistemic value. If epistemic values formed a stable hierarchy, it would be possible to adjudicate between them by appeal to some higher value. As we have seen, however, the epistemic values that scientists acknowledge do not form a stable hierarchy. Because of this, it is not obvious which epistemic value should be privileged in a dilemma.

The structure of scientific dilemmas means that they are not resolvable by algorithmic reasoning. Take the ideal of a rational calculator as a reference point. A rational calculator is a perfectly rational agent: it makes decisions by following all applicable instructions and taking account of all relevant information. A rational calculator is able to deal with a conflict between precepts only if it is provided with principles for adjudicating between the conflicting precepts. Since a dilemma is a situation in which such principles are absent, a rational calculator would grind to a halt in a true scientific dilemma: it would be unable to arrive at any practical resolution.

This suggests that, to the extent that scientists are able – albeit with a degree of conceptual unease – to reach a practical resolution of scientific dilemmas, their behaviour deviates from that of a rational calculator. It must be, instead, that scientists rely on some faculty to leapfrog the

obstacles that would halt the rational calculator. It is reasonable to conjecture that this faculty is a scientist's emotion system.

Work on emotions over the last twenty years has undermined the view that emotions are merely the antagonists of cognition and rationality. Instead, the view has gained ground that reliance on emotional responses is a necessary condition for making sound inferences and decisions in many circumstances. The contribution of the emotion system is most important for inferences and decisions of a practical nature where the relevant values and information are not complete or not consistent. A rational calculator is not able to reach a conclusion in such cases. A person's emotion system, however, is able to break the deadlock. The emotion system attributes salience to aspects of situations, enabling the decision-maker to overcome the incompleteness or inconsistency of the relevant values and information.

Since dilemmas are characterised by the inconsistency of values, we can expect emotions to play an important role in our reactions to them. There is strong evidence that emotional responses help determine the practical resolution of moral dilemmas. There is now a growing realisation that scientists' emotional responses play a similar role in the resolution of scientific dilemmas. Only by relying on their emotional responses can scientists adjudicate between two conflicting epistemic values when there are grounds for holding to both values and insufficient evidence for disqualifying either of them.[12]

The development of quantum physics in the 1920s and 1930s provides an illustration of the role of emotion. Bohr, Heisenberg, Einstein, and Schrödinger were confronted by the need to decide between epistemic values that were entrenched to a similar degree in the history of science: empirical adequacy, determinism, and visualisation. A rational calculator faced with the competing values would never resolve this dilemma: it was precisely the unavailability of higher principles that sparked it. The scientists involved were thus compelled to base their decisions to a large extent on their emotional responses to epistemic values and to theories.[13]

We thus see the exceptional place of scientific dilemmas in the practice of science: they constitute not only important turning points in the development of a discipline, but also the locus at which emotion necessarily enters scientific work.

Notes

1 On the controversy in cosmology, see Helge Kragh, *Cosmology and Controversy: The Historical Development of Two Theories of the Universe* (Princeton, N.J.: Princeton University Press, 1996); on that in palaeontology, see William Glen, ed., *The Mass-Extinction Debates: How Science Works in a Crisis* (Stanford, Cal.: Stanford University Press, 1994). Broader literature on scientific controversies includes H. Tristram Engelhardt, Jr., and Arthur L. Caplan, eds., *Scientific Controversies: Case Studies in the Resolution and Closure of Disputes in Science and Technology* (Cambridge: Cambridge University Press, 1987); Bruno Latour, *Science in Action: How to Follow Scientists and Engineers Through Society* (Milton Keynes: Open University Press, 1987) p. 21–62; and Peter Machamer, Marcello Pera, and Aristides Baltas, eds., *Scientific Controversies: Philosophical and Historical Perspectives* (New York: Oxford University Press, 2000).

2 On the prisoner's dilemma, see Robert Axelrod, *The Evolution of Cooperation* (New York: Basic Books, 1984).

3 Bernard Williams, 'Ethical Consistency' (1965), reprinted in Bernard Williams, *Problems of the Self: Philosophical Papers 1956–1972* (Cambridge: Cambridge University Press, 1973) p. 166–186. Some classic discussions are reprinted in Christopher W. Gowans, ed., *Moral Dilemmas* (New York: Oxford University Press, 1987). Popular examples are collected in Martin Cohen, *101 Ethical Dilemmas* (London: Routledge, 2003).

4 Hugh LaFollette and Niall Shanks, *Brute Science: Dilemmas of Animal Experimentation* (London: Routledge, 1996); Tom L. Beauchamp and James F. Childress, *Principles of Biomedical Ethics*, 5th ed. (New York: Oxford University Press, 2001).

5 John L. Heilbron, *The Dilemmas of an Upright Man: Max Planck as a Spokesman for German Science* (Berkeley: University of California Press, 1986).

6 The axiology of science is discussed by Larry Laudan, *Science and Values: The Aims of Science and Their Role in Scientific Debate* (Berkeley: University of California Press, 1984). I have treated the relation between empirical and aesthetic values in James W. McAllister, *Beauty and Revolution in Science* (Ithaca, N.Y.: Cornell University Press, 1996).

7 Thomas S. Kuhn, *The Structure of Scientific Revolutions*, 2nd ed. (Chicago, Ill.: University of Chicago Press, 1970) p. 99–100. On the Cartesian theory of the solar system, see E. J. Aiton, *The Vortex Theory of Planetary Motions* (London: Macdonald, 1972).

8 On the wave/particle dilemma, see Mara Beller, *Quantum Dialogue: The Making of a Revolution* (Chicago, Ill.: University of Chicago Press, 1999) p. 227–232.

9 Niels Bohr, *Atomic Theory and the Description of Nature* (Cambridge: Cambridge University Press, 1934) p. 108.

10 Albert Einstein, 'Physics and Reality', translated by Jean Piccard, *Journal of the Franklin Institute,* 221 (1936) p. 349–382, at p. 374; emphasis as in the original. For more discussion of the dilemmas of quantum physics, see McAllister, *Beauty and Revolution in Science* (cit. n. 6) p. 188–201.

11 An example is Andrew Whitaker, *Einstein, Bohr and the Quantum Dilemma: From Quantum Theory to Quantum Information*, 2nd ed. (Cambridge: Cambridge University Press, 2006).

12 Empirical evidence that the emotion system plays an important part in the consideration of moral dilemmas is presented by Joshua D. Greene, R. Brian Somerville, Leigh E. Nystrom, John M. Darley, and Jonathan D. Cohen, 'An fMRI Investigation of Emotional Engagement in Moral Judgment', *Science,* 293 (2001) p. 2105–2108. A wider philosophical discussion of the relation between emotions and moral dilemmas is developed by Patricia S. Greenspan, *Practical Guilt: Moral Dilemmas, Emotions, and Social Norms* (New York: Oxford University Press, 1995). I treat the roles of emotion in scientific practice in James W. McAllister, 'Emotion, Rationality,

and Decision Making in Science', in Petr Hájek, Luis Valdés-Villanueva, and Dag Westerståhl, eds., *Logic, Methodology and Philosophy of Science: Proceedings of the Twelfth International Congress* (London: King's College Publications, 2005) p. 559–576.

13 On the part played by emotion in the development of quantum physics, see Mara Beller, 'The Conceptual and the Anecdotal History of Quantum Mechanics', *Foundations of Physics,* 26 (1996) p. 545–557; Beller, *Quantum Dialogue* (op. cit. n. 8) p. 30–39.

1
Novel drug discovery – serendipity or design?

Meindert Danhof

Modern medicines are indispensable weapons in the war against (serious) disease. Not surprisingly, medicinal products play an integral and important role in our society. Yet, their availability and accessibility are not self-evident. For many serious disorders there are still no effective medicines available. Moreover, for those disorders where effective medicines do exist, loss of effectiveness over time is prevalent, as is amply illustrated by the worldwide development of resistance against anti-infective drugs. Finally, with time new diseases continue to develop. This underscores the need to continuously search for novel drugs and for novel concepts of drug treatment.

Another important problem is the lengthy and costly process of developing a new chemical or biological entity into an effective drug. In this respect development concerns the identification of the optimal dose of a new chemical entity and the demonstration of its efficacy and safety in clinical trials. At present, the development of a single new drug takes on average ten to twelve years, requiring an investment of approximately 1 billion Euros. Moreover, drug development is associated with high failure rates, with attrition as high as 92%. As a result, the cost of new medicines is very high. This may ultimately limit the access to important life-saving drugs to only select and privileged groups in our society or in the worst case may even preclude their development.

The above emphasises the need to advance research in the field of drug discovery and development. Specifically it will be essential to improve the

efficiency of this process both in terms of the number of novel drug molecules to be identified and the cost and duration of the development process. An intriguing question is how to approach this. Will we have to rely on the discovery of novel drugs by serendipity, in the course of biomedical research? Or will it become possible to design new drugs in dedicated investigations?

Diseases in search of drugs

The report *Priority medicines for Europe and the world* of the World Health Organization[1] analyses so-called 'pharmaceutical gaps' which affect society. In this analysis pharmaceutical gaps are identified as those diseases of major public health importance for which medicines either do not exist or are inadequate. The report analyses the total burden of disease in terms of disability adjusted life years (DALY's) using the WHO Global Burden of Disease Database. Data on the clinical efficacy of existing treatments is extracted from the Cochrane Database of Systematic Reviews as the most authoritative source of this information.

The WHO report identifies demographic changes as a major determinant of the expected burden of disease in the years to come. In the western world the population is ageing, with more people – especially women – living beyond 80 years. In the developing countries there is a sharp difference between Africa and other regions. In many African countries infectious disease (in particular HIV and AIDS) has a substantial impact in reducing life expectancy. In other regions, the rapid ageing of the population will lead to an increase not only in the number, but also in the proportion of the elderly in the population. Another trend is the rapid urbanisation taking place around the world. As a result of these trends, over the next thirty years there will be a demographic shift towards rapidly ageing populations in large cities, particularly in Asia, in Latin America and in some African countries. These demographic changes, which are very similar to the changes that we have experienced in the western world, will be marked by a continued increase in non-communicable chronic diseases worldwide.

The WHO analysis identifies 20 major diseases which account for nearly 60% of the global burden of disease. Using a variety of criteria, of which an in-depth discussion is beyond the scope of this article, the following diseases/conditions have been identified as the main disorders

where pharmaceutical gaps prevail: infectious diseases, mental and central nervous system conditions, cardiovascular diseases, (auto) immune disorders and cancer. This has led to the conclusion that innovation in drug discovery and development should focus primarily, albeit not exclusively, on these diseases.

In the WHO report, focus is not only on medicines for the treatment of disease, but ample attention is also paid to the prevention of life-threatening diseases. Specifically, the report identifies a number of preventable high-burden diseases as having pharmaceutical gaps. The most important are the secondary prevention of heart attack and stroke. Although for these diseases prevention is possible, through the use of a combination of effective medicines, in practice, for a variety of reasons, these drugs are not taken. The WHO report proposes to develop fixed-dose combinations as an approach to increase their use.

In addition to identifying priority diseases, the WHO report also reviews improved pharmaceutical delivery mechanisms for existing medicines. Medicines have to be administered to the patient to achieve maximal clinical benefit. The most commonly used delivery mechanisms are tablets or capsules for oral delivery. Over recent decades, a wide range of sustained release devices (such as patches for transdermal drug delivery) have been developed. At present such technologies are heavily under-utilised. Yet these technologies will be critically important to improve treatment in children and the elderly, and other areas where individualised dosing of medicines is imperative, such as patients with impaired liver or kidney functions, or patients with compromised immune systems. Finally, safety concerns may drive the need for targeting potent, but very toxic medicines (i.e. anti-cancer drugs, anti-viral drugs) to the pertinent site of action in the body rather exposing the whole body to the medicine.

The above underlines the wide range of diseases with an imminent need for novel drugs as well as the multi-faceted nature of the development of such drugs and the corresponding pharmaceutical products.

The high days of drug discovery

In his fascinating book *The rise and fall of modern medicine* James le Fanu[2] describes the unprecedented medical achievements and innovations in the

years following the Second World War. The book focuses on twelve decisive moments in medical history, which have been true landmarks in the development of effective treatments for human disease. It is very interesting to see how novel medicines have played a major and often decisive role in the vast majority of these developments. Specifically, four of these twelve decisive moments concern the discoveries of novel drugs *per se*: penicillin for infectious disease, cortisone for inflammatory disorders (i.e. rheumatoid arthritis, asthma), streptomycin for tuberculosis and chlorpromazine for psychiatric disorders (i.e. schizophrenia). Moreover, for the majority of the other breakthroughs, it holds that they have become possible only through the availability of novel drugs. For example, kidney transplantation would have been impossible without the development of cyclosporine as a drug to suppress rejection of the transplant in a controlled manner. The same applies for open-heart surgery, which was made possible by the development of synthetic opiates as anaesthetics, to provide cardiovascular stability. Likewise, the cure of childhood cancer relies entirely on the use of novel anti-cancer drugs. Finally, artificial ventilation of patients in intensive care, the prevention of stroke by the treatment of high blood pressure, the birth of test-tube babies and the treatment of peptic ulcers all depend on the use of novel effective and safe medicines. It is intriguing and remarkable to see how the vast majority of these drugs were discovered largely by serendipity, without much understanding of disease processes.

Drug discovery and development in perspective

At present, novel drugs are almost exclusively discovered and developed by the international pharmaceutical industry. Not surprisingly, over the years the pharmaceutical industry has largely determined the research agenda in drug innovation. In his book *In quest of tomorrow's medicines* Dr. Jürgen Drews,[3] formerly president of global research at Hoffman-La Roche, describes in an enlightening manner the development of the pharmaceutical industry and the pharmaceutical sciences in the nineteenth and twentieth century.

Briefly, the pharmaceutical industry arose from two sources. One is the aniline dye industry which evolved in the wake of the industrial manufacturing of town gas. The other is the traditional apothecary with its focus on medicinal plants.

Town gas was originally obtained from the carbonisation of hard coal. Over the years, the by-product of this process, coal tar, was discovered to contain many compounds which could not only be used as dyes in the textile industry, but which also served as a source of organic compounds for the rapidly developing field of synthetic organic chemistry. As a result, the aniline dye industry became an important source of new chemicals, including drugs. It is of interest to see that many well-established international pharmaceutical companies such as Bayer and Novartis indeed have their roots in the aniline dye industry.

Traditionally, apothecaries were institutions where prescriptions written by physicians were filled. With the rise of chemistry in the nineteenth century, a major shift occurred when the active ingredients from previously known medicinal plants were extracted and used as drugs. Well-known examples are the extraction of morphine from opium, caffeine from the coffee plant and quinine from the cinchona bark. In order to make these new substances available to a wide public, a number of apothecaries developed themselves into 'industrial' apothecaries or in other words, pharmaceutical firms. This led to the establishment of pharmaceutical companies which still exist today, such as Merck, Schering and Boehringer.

The development of chemistry in the nineteenth century was paralleled by the development of experimental pharmacology as a scientific discipline. The hallmark of this research was the systematic description of the effects of drugs on animals and isolated organs. Important theoretical concepts on dosage-effect relationships and other aspects of modern drug research originated directly from this work. It is fair to say that, in addition to the advances in organic chemistry, in particular the development of modern pharmacology has indeed been the cornerstone of modern drug discovery and development. The key factor was the interfacing of chemistry and pharmacology. Specifically, the study of the relationships between chemical structure and the binding of molecules to biological target proteins has been the key to the discovery of many new drugs. It was Sir James Black's discoveries, first of propranolol (which blocked the beta receptors in the heart, thus relieving the symptoms of angina) and then of cimetidine (which blocked the histamine receptor in the gut, thus reducing the amount of gastric secretions and allowing ulcers to heal) that led many to believe that the effects of drugs could be predicted on the basis of their chemical structure.[4,5] Up to the present

day, novel drug molecules are designed on the basis of the study of 'structure-activity relationships'.

The dearth of new drugs

Between 1960 and 1970 a sharp decline in the success of drug innovation occurred. While the number of new drugs marketed was running at around seventy per year throughout the 1960's, by 1970 this number was down to around 30 per year, a trend which continued in the years thereafter. Moreover, many of the new drugs marketed in the last decades were improvements to existing drugs, rather than truly innovative medicinal products. There are a number of explanations for this decline in drug innovation.

The most commonly mentioned explanation for the decline in drug innovation is the tightening of safety regulations in the aftermath of the thalidomide (Softenon®) disaster. As a result, initial toxicity testing of new chemical entities in animals needed to be conducted prior to first administration in man. Moreover, the results of extensive clinical trials in humans, demonstrating efficacy and safety, were mandatory before a drug could be approved for release to the general public. This made the entire process of drug innovation more time-consuming, complicated and expensive. Ultimately this resulted in the current situation where the development of a single new drug takes approximately 10-12 years, involving the investment of on average 1 billion Euros.

Another presumably much more important factor for the decline in drug innovation is the fact that up to the 1970's the process of drug discovery was based on the classical paradigm of synthesising novel chemical compounds which could then be investigated for their potential therapeutic effect. Sooner or later pharmaceutical companies would run out of new chemicals to be tested in this manner. Moreover, drug discovery focused on a limited number of only 500 specific targets.

Meanwhile, progress in molecular biology has opened new avenues to the understanding of the mechanisms of disease. In theory this yields the opportunity to design novel drugs which specifically interact with the disease process or, in other words, correct what is wrong. Yet research from the mid-1970's onwards has not produced many genuinely useful novel drugs. It is important to analyse what may have caused this lack of success.

Multiple interactions between drug molecules and biological systems

Drug effects result from multiple and complex interactions between drug molecules and various components of the biological system. Commonly, two kinds of processes which determine *in vivo* drug action are distinguished. These are referred to as pharmacokinetics and pharmacodynamics.

Pharmacokinetics concerns the processes of the uptake of drug molecules in the body, their distribution into peripheral tissues and their elimination. These processes govern the time course of the drug concentration at the site of action and thereby, indirectly, also the time course of the pharmacological response intensity. In recent years, research has much improved our knowledge of the molecular mechanisms involved in these processes. Although the absorption, distribution and elimination of drug molecules depend on their physicochemical properties (especially the size of the molecule and the lipid solubility), the functionality of transporters and metabolizing enzymes is an important factor. A drug transporter is a protein which specifically functions to transport drug molecules from one side of a biological membrane to the other. In recent years numerous specific drug transporters have been discovered, which may be operative at various levels of the disposition. Specifically, transporters may either enhance or restrict the absorption of drug molecules from the small intestine, their distribution into the brain and their elimination. An important factor is that the functionality of transporters may change over time, causing drug resistance. The most well-known example is multi drug resistance against cancer chemotherapy, which is caused by over-expression of the active efflux transporter P-glycoprotein.[6]

In addition to drug transporters, a wide array of drug metabolising enzymes has also been identified. The role of these enzymes is the breakdown of the often lipophilic drug molecules to facilitate their excretion. Drug metabolising enzymes, which are primarily localised in the small intestine and in the liver, may substantially reduce the amount of drug that ultimately enters the body upon oral administration. Moreover, for most drugs, after absorption these enzymes also determine the rate of elimination from the body.

Pharmacodynamics concerns the mechanisms that determine the

nature, the intensity and the duration of the drug effect. With the advances in molecular biology, research has much improved our understanding of the mechanisms of drug action. Briefly, pharmacodynamics involves the binding of the drug molecule to its biological target, the activation of this target, the transduction into the pharmacological effect and the homeostatic mechanisms which may be operative. The most common targets for drug molecules are receptors. Receptors are specific proteins which, after activation by drug molecules, alter the function of cells. Over the years numerous receptors and receptor subtypes have been identified. Transduction refers to the processes of target activation into a pharmacological response. Typically this involves the activation of a cascade of electrophysiological and/or biochemical events. Transduction may operate at widely different time scales ranging from milliseconds to hours or days. This may explain why drug effects may take a long time to develop, as is illustrated for antidepressants, where the therapeutic effect is not observed until several weeks after the initiation of treatment.

Another important factor that may influence the pharmacological effect is the presence of physiological control mechanisms which may counteract the drug effect. Such counter-regulatory control mechanisms may explain loss of efficacy (i.e. tolerance development) which is often observed in chronic treatment and rebound effects that may occur upon cessation of treatment. Last, but not least, pharmacodynamic interactions must also be considered for drugs which, due to lack of selectivity, act simultaneously on different targets.

The above observations show that drug effects result from the complex interactions of a variety of biological systems and processes. This makes the prediction of the drug effects in man and thereby the design of novel drugs a formidable challenge.

Inter-individual variability: the need for 'tailor-made' pharmacotherapy

The design of novel drugs is further complicated by the wide inter-individual variability that is often observed. This important topic was addressed by Douwe Breimer[7] in his inaugural lecture at Leiden University in 1976 entitled: *Farmacotherapie op maat* (*'Tailor-made Pharmacotherapy'*). In his lecture Breimer demonstrated that the clinical

response to drug administration varies widely among patients. Specifically, drugs may prove inactive in some patients but may be highly active or even toxic in others who are given the same dose. This implies that in practice drug therapy must often be individualised.

In theory, variability in drug response may result from variability in pharmacokinetics, variability in pharmacodynamics or a combination of both. In the meantime it has been convincingly demonstrated that variability in pharmacokinetics contributes substantially. Specifically, the rates at which the processes of drug absorption, distribution and elimination occur differ widely between individuals. As a result, the same dose of a given drug results in widely different concentrations, thus causing wide differences in the response. The observed differences in pharmacokinetics can often be reduced to inter-individual variability in the expression and function of transporters and/or drug metabolising enzymes. Much of this variability is caused by genetic factors. However other factors, such as diseases, differences in age and sex, interactions with other concomitantly taken drugs etc., are often contributory and should therefore also be taken into account.

In contrast to pharmacokinetics, much less is known about inter-individual variability in pharmacodynamics. Yet, beyond reasonable doubt, variability in pharmacodynamics contributes at least equally to the observed inter-individual differences in drug response. It is well established that drug response depends largely on the level of receptor expression in a given target. Receptor expression can vary widely between individuals due to genetic factors. Moreover, receptors are in a dynamic state which means that their expression and function can be either up-regulated or down-regulated, depending on the conditions. In the meantime, there is ample evidence that variability in receptor expression is indeed an important determinant of inter-individual variability in drug response. It seems likely that variability in the efficiency of transduction and homeostatic feedback mechanisms is an equally important factor.

The aforementioned observations show that inter-individual variability is intrinsically associated with drug treatment. This has major implications for the use in clinical practice. In the meantime genetic testing is beginning to be applied clinically to predict treatment response in individual patients. The most well-known examples are the treatment of breast cancer with traztuzumab (Herceptin®) and of chronic myeloid

leukaemia with imatinib (Gleevec®), where genetic testing is used to select patients with cancers that are sensitive to the drug.[8]

Given the enormous clinical implications, inter-individual variability in response has become a key factor in modern drug discovery and development. The prediction of individual variability in drug response contributes an additional major challenge to the rational design of novel drugs.

Tomorrow's drugs: targeted treatment solutions?

In the report *Pharma 2010: The threshold of innovation,* Steve Arlington[9] unfolds his vision on novel drug discovery and development in the years to come. The novel dimension in his proposed approach is the shift in emphasis from pharmacology to disease. Specifically, it is anticipated that future drug discovery and development will be underpinned by an understanding of the mechanism of the disease at the molecular level and as part of the integrated biological system. This redefining of pathology will result in the identification of specific disease subtypes and opens new avenues towards the identification of novel drug targets. Moreover it will also enable the identification of molecules that interact specifically with the disease process.

It is expected that the aforementioned approach will yield novel drugs for the treatment of individual patients with specific disease subtypes, rather than one-size-fits-all blockbuster drugs. This implies that in the future drug treatment will become increasingly individualised. Accordingly, in Steve Arlington's vision, in future the pharmaceutical industry will produce not just drugs, but 'targeted treatment solutions' which include in addition to the drug, diagnostic tests, monitoring devices and a wide range of services to support patients. In principle, such targeted treatment solutions should enable the implementation of highly individualised drug treatment in clinical practice. The recent experience with drugs like Herceptin® and Gleevec® illustrates that for serious, life-threatening disorders with a strong genetic component, this may indeed be a feasible option. Whether this is in general the case remains to be established.

Clearly, the development of targeted treatment solutions, with its emphasis on individualisation, requires innovative approaches to drug development.

Modelling and simulation

In the preceding paragraphs it has been shown that drug effects result from multiple interactions between drug molecules and various components of the biological system. Moreover, it has been demonstrated that there is often a considerable inter-individual variability in drug response. This makes the prediction of drug effects in individual patients a formidable challenge, and indeed complicates the design of novel chemical or biological entities as innovative drugs. As a result, most drugs as we know them today have been discovered largely by serendipity rather than by design. In recent years, however, modelling and simulation of biological systems are increasingly applied in drug research. An intriguing question is to what extent modelling and simulation enable the prediction of drug effects in individual patients. If feasible, this would constitute a major step forward towards the discovery of novel drugs by design and to the implementation of tailor-made pharmacotherapy.

In recent years important progress has been made in the development of mechanistic models describing the functioning of whole body systems, to predict in a strictly quantitative manner drug effects in man. The Leiden-Amsterdam Center for Drug Research at Leiden University has been at the forefront of this research.[10] A pertinent feature of these models of whole body systems is that they contain specific expressions to characterise processes on the causal path between drug administration and response. This includes the pharmacokinetics, the distribution of the drug to target site, the binding to and activation of the target, the transduction pathways and the homeostatic feedback mechanisms. Ultimately, also the interaction of drug effects with the disease processes and disease progression are considered.

As truly mechanistic models of drug action on the whole body may become incredibly complex, their identification constitutes a major challenge. An important question is therefore whether mechanistic models could be reduced so as to contain only expressions for the most essential processes on the causal path, while maintaining their utility for extrapolation and prediction. Meanwhile it has been demonstrated that in many instances parsimonious mechanism-based models do indeed constitute a valuable basis for the prediction of drug effects in man.

It is anticipated that modelling, simulation and high-performance computing will revolutionise the way in which novel medicines are

discovered and developed. Specifically, modelling how drugs act at the sub-cellular level, in organs and ultimately in whole body systems will yield novel concepts of drug treatment. Moreover, such modelling also enables the identification and the design of novel drug molecules. Another important aspect is that, in addition to the aforementioned applications, the prediction of drug effects in man will also enable the design of accurate clinical trials. Meanwhile it has been amply demonstrated that the application of advanced modelling and simulation increases the efficiency of the drug discovery and development process. Last, but not least, modelling and simulation also yield a scientific basis for tailor-made pharmacotherapy, both with regard to the selection of susceptible patients and the individualisation of the dosing. Ultimately this will enable the management of diseases for which no one-size-fits-all treatment solutions are available.

Novel drug discovery: serendipity or design?

The previous paragraphs describe the complexity of novel drug discovery and development. It is shown that drug effects result from the complex interactions of drug molecules with multiple components of the biological system. An important observation is that drug effects may be observed only under very specific conditions. This makes the discovery of new useful drug molecules, which are efficacious, safe and easy to use, look like the proverbial 'search for the needle in the haystack'.

In recent years, however, important progress has been made with the modelling of biological systems as the basis for the prediction of drug effects. Although such models readily become extremely complex and thereby difficult to identify, it has in the meantime been amply shown that reduced, parsimonious models can indeed be used to predict drug effects. This constitutes therefore a basis for the discovery of novel concepts of drug treatment and for the design of novel drug molecules. It is foreseen that in the coming years modelling and simulation will be increasingly applied in rational drug discovery and development. An important factor is that this will also yield an improved understanding of the mechanisms of inter-individual variability in drug effect. Ultimately this may enable the implementation in clinical practice of highly individualised targeted treatment solutions for diseases where easy one-size-fits-all medications are not feasible.

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2
The hunt for therapeutic leads

A strategy to strengthen the position of academic research in the field of identifying therapeutic agents

Herman Overkleeft

The last century has witnessed the rise of well-defined, synthetic therapeutic agents in health care. This is a relatively recent development. Until the nineteenth century, substances with therapeutic value were almost exclusively obtained from natural sources. In those days, compound mixtures in crude plant extracts were most often used and the therapeutic potential of the substances was based on folklore, not on actually known working mechanisms. Not surprisingly, potions and elixirs abounded that at best had a placebo effect, or that today would be labelled as harmful or toxic. However, traditional medicine has met with some remarkable successes, and the notion that (natural) compounds can be used to cure diseases, or to suppress the effects of disease, led to the advent of modern drug research. Medicines against malaria give a nice example.

Chinese herbal medicine (also known as Chinese natural medicine) dates back to more than 6000 years ago and is the richest source of natural medicines. These are founded on the use of substance mixtures called formulae, often assembled from extracts from different sources. The Chinese natural medicine practitioners have known for the last 1000 years that extracts from the leaves of the shrub *Artemisia annua* have a beneficial effect on patients suffering from malaria. It was not until 30 years ago that the active component was identified as the sesquiterpene lactone artemisinin (in China the compound is called qinghaosu) and more recently its mode of action was elucidated.[1]

In Europe, the first natural medicine solution for malaria patients emerged in the seventeenth century, when patients were treated with bark extracts from the South American cinchona tree. The active ingredient, quinine, was not only effective in countering malaria infections, but also appeared to be an effective painkiller and anti-inflammatory agent.[2] In the eighteenth and nineteenth centuries, quinine-containing extracts rapidly became the most important pharmaceutical substance in Europe. So much so, that it became profitable to obtain the pure compound from the cinchona bark to reduce shipping costs from South America to Europe.

In the beginning of the nineteenth century, quinine was obtained in a highly purified form by a group of French researchers. This achievement marks an important step in medicinal practice because it demonstrated the feasibility of using substances containing just one single compound for the treatment of patients. Here, modern medicine started to deviate from traditional medicine such as practised by Chinese herbalists.

From herbalists to modern practice: a one-way road

Traditional medicine and modern medicine are both practised and studied today. It is remarkable that, while modern medicine still finds inspiration in herbal medicine (what, if any, is the active ingredient in a given herb extract?), natural medicine practitioners often refute the scientific achievements of modern medicine. This is unfortunate, since one of the objectives in modern medicine is to pinpoint the exact mode of action of a therapeutic agent, and to provide guidelines with which to predict any potential activity of a given substance. The afore-mentioned anti-malarial agent artemisinin was identified in the course of a study in which no less than 200 herbal potions were investigated that according to Chinese folklore had anti-malarial effect. Only the *Artemisia annua* leaves proved to possess active components; the 199 remaining formulae were completely inactive. It is likely that most of the herbal potions that are for sale in a natural medicine store and that are directed at serious ailments will turn out to be equally ineffective. Natural medicine followers would do well to carry out structure activity studies on their potions that are common practice in modern medicine. This will spare people from becoming seriously ill or dying, weed out all the useless concoctions and possibly point out the few really useful compounds that undoubtedly are hidden in a herbal pharmacy as the needles in a haystack.

The discovery that single compounds such as quinine can be used as medicine coincided with the advent of synthetic organic chemistry in the nineteenth century. Organic chemistry is a science that allows one to prepare or modify chemical substances in the laboratory. In 1829, nine years after the isolation of quinine, French researchers for the first time accomplished the synthesis of a naturally occurring organic compound, namely urea. This achievement revealed that, in theory, any natural product could be prepared in the laboratory. Furthermore, these compounds could potentially be modified in a synthetic fashion in such a way that products not present in nature could be prepared. The importance of this development is well illustrated by perusal of the history of the development of the painkiller, aspirin (acetylsalicylic acid).[3] The ancient Greeks discovered in the fifth century BCE that chewing on willow bark could ease pain and reduce fevers. These remarkable features were largely ignored in Europe until the nineteenth century. At this time, quinine was the predominant painkilling agent in use, but its supply source, the cinchona bark, became scarce. In the search for alternatives, the analgesic and anti-inflammatory effect of willow bark were rediscovered, and in 1828 the organic compound salicylic acid was identified as the active ingredient. This compound became a highly popular analgesic drug and was often prescribed for the treatment of fevers and inflammations. Salicylic acid, however, appeared to have some undesirable properties, amongst which were a bitter taste and, at high doses, stomach bleeding. When Felix Hoffmann, employed by Bayer, was confronted with this, he decided to make some synthetic salicylic acid derivatives. One of these is acetylsalicylic acid, the compound commonly known as aspirin. The year 1897, in which Hoffmann performed his studies, marks the appearance of the first synthetic drug. Aspirin went on to become hugely successful. It is also the first therapeutic that, in the beginning of the twentieth century, appeared on the market in tablet form. As such, the development of aspirin had a major impact on the progress of modern medicine. With its advent came the realisation that therapeutic agents do not necessarily have to originate from nature, but may also be synthesised in a laboratory.

A new strategy is needed

In the twentieth century, the identification and development of

therapeutic agents has evolved to a mature, multidisciplinary science in which many academic researchers are active and that has spawned a multi-billion dollar industry. Enormous progress has been made in understanding the chemical processes that make up life. Much is known about the concerted interactions and reactions that take place within the cell, and how cellular differentiation and intercellular interactions are orchestrated in higher organisms such as human beings. Furthermore, our knowledge of processes which are at the basis of disease, both abnormalities in human biological processes and the molecular basis behind the action of human pathogens, has increased considerably; especially since the identification, by Watson and Crick in 1953, of deoxyribonucleic acid, DNA, as the carrier of genetic information.

At the same time, we have witnessed enormous progress in natural product chemistry, more specifically the isolation and characterisation of biologically active compounds from a large variety of natural sources. Equally important are the advances in synthetic organic chemistry, which enable us to synthesise many of the characterised natural products, but also analogues thereof that are not encountered in nature. Finally, the action of many therapeutic agents, both with respect to their pharmaceutical target and in a broader sense their behaviour in mammalian organisms (issues such as toxicity, bio-availability, stability), has been charted with considerable detail. These issues are studied in the separate scientific field of pharmacology.

Yet, despite all the progress, despite the billions of dollars invested annually in drug research, and despite the legions of scientists active in the sciences related to drug research, only a handful of new compounds reach the clinic on a yearly basis.

The United States is the leading market for therapeutics, and the drug approval and regulations set by the United States-based Federal Drug Agency (FDA) are leading guidelines for drug approval institutions elsewhere in the world. It is thus safe to say that the United States situation accurately reflects the current situation in the rest of the (western) world. As it happens, in the United States only fourteen so-called 'small-molecule drugs' (SMDs) were approved for clinical use in 2002, a number that came down from a total of 44 in 1996. In recent years the so-called 'biologicals', being recombinant proteins or monoclonal antibodies, have been promoted as a new drug category that in the future will replace SMDs as the main class of therapeutics.

However, with a total of six approved biologicals in 2002, a steady number if one looks at the five preceding years, it is clear that this class of compounds will not become the predominant source from which new therapeutics will emerge, at least not in the near future. Obviously the total of new therapeutics that is developed annually is not sufficient if we wish to maintain and improve current life standards. In the western world we are faced with numerous diseases (cancers, cardiovascular diseases), the occurrence of which will increase with our average lifetime. Second and third world countries are struggling with an altogether different set of diseases; infectious diseases such as TBC and HIV, that have progressed to become, or already are, of an epidemic nature. Perhaps the largest threat comes from the increasing occurrence of multi-drug resistant (MDR) bacterial strains. Up until today, outbreaks of MDR bacterial infections have been restricted to confined areas such as hospitals or nursing homes. Here, the number of victims that succumb to MDR bacterial infections is rising steadily. However, these numbers will be as nothing compared to what will happen if an MDR bacterial strain is released into society at large. The seriousness of the situation is demonstrated by the recent emergence in South Africa of what was named the eXtreme multi-Drug Resistant (XDR) M. tuberculosis strain, which to date has claimed many victims. It is clear that the number of therapeutic agents that are identified annually needs to be increased. In order to achieve this we need to change our strategies for finding new drugs.

A complicated and costly affair

What makes the development of new drugs so complicated? On paper the process looks rather simple. The molecular basis behind a given disease is determined and the biological factor or factors that contribute to the onset of the disease are charted. Then, substances that block or reverse the mode of action of these factors are acquired. If the nature of the disease is caused by some unwanted activity, then a blocker of this activity is designed, and if the disease is characterised by an impeded biological process, then one can look for some kind of replacement of this activity. Once the potential therapeutic is defined, it is further studied and modified so that it may reach its target in the body, is not toxic and displays little or no cross-reactivity to other biological targets and is cleared from the body in a regular fashion without being transformed into toxic metabolites along the way.

Obviously drug development in real life is rather more complicated than what is sketched above, and there are several issues that contribute to this. One reason for the relative lack of success in bringing new drugs to the clinic may be the simple fact that we have found most of the relatively simple cures for those diseases that are comparatively easy to cure. Indeed, most (but not all) diseases are caused by more than one factor and it is often rather difficult to pinpoint the malfunctioning biological factor that needs to be repaired in order to counter the disease most effectively. A case in point is diabetes mellitus type 2, a metabolic disorder that is characterised by insulin resistance and hyperglycaemia. Many factors contribute to the onset of this, as yet incurable, disease. These include genetic dispositions (some people run a higher risk of contracting the disease) and behavioural factors. The increased occurrence in the western world is closely linked to obesity and thus to our dietary habits. The many known contributory factors have invited studies towards the development of about as many interfering strategies. Several therapeutic agents that are the result of these studies, such as Rosiglitazone[4] (Avandia, enhancing insulin sensitivity) and Miglitol[5] (blocking glucose uptake in the gastrointestinal tract), have reached the clinic. The known therapeutics suppress the progress of the disease but do not cure it, and it is expected that the development of a truly effective cure is only feasible after pinpointing the exact nature of the complex and interrelated pathways that are behind the disease.

Perhaps paradoxically, our progress in life sciences interferes to some extent with drug development, not so much with the initial stages but with bringing candidates for clinical application to a successful conclusion. With our increased knowledge, we are much better able to identify possible drawbacks. For instance: looking at what we know today about the possible side effects of aspirin (gastro-intestinal bleeding, headaches, dizziness, liver damage, renal failure), it seems highly unlikely that this drug would be introduced in the clinic as easily and on such a scale as was the case in the beginning of the twentieth century.

The high and increasing number of safety criteria a new drug has to adhere to is thus a major limiting factor. Human patients are to be treated with the new drug and undesirable side effects should be limited. There are, of course, differences in the toxicity profile or the number of side effects that are permissible for individual drugs. The more acute or life-threatening the disease, the less stringent safety regulations may be.

Recently, the peptide boronic acid Velcade was approved as a last-resort drug for patients suffering from multiple myeloma, also known as Kahler's disease, who have a life expectancy of only a few months.[6] Obviously, drugs such as Velcade, whose numerous side effects include an enhanced risk of contracting infectious diseases, would never be considered for the treatment of, say, hay-fever.

On the whole, pharmaceutical companies wish to remain safe rather than sorry in their efforts to introduce a new compound to the clinic. First, because of the huge costs: the average cost of the introduction of a new drug is estimated at half a billion dollars, a large part of which is caused by safety regulations. Recent liability claims such as the one that led to the withdrawal of Vioxx (Rofecoxib)[7] by Merck in 2004 also contribute to this reticent behaviour. On its introduction in 1999 as a drug for the treatment of arthritis and chronic or acute pain, the drug netted over 2.5 billion dollars annually, and millions of patients were prescribed Vioxx. In 2004 concerns about elevated risk of heart failure led Merck to decide to withdraw the drug from the market and the liability claims were such that the future of the company was threatened.

Academic versus industrial research

Broadly speaking there are three parties directly involved in drug research. Apart from the medical doctors that treat the patients and provide information on the onset and progress of diseases, these are the academic community and the pharmaceutical industry. Government institutions are indirectly involved in a monitoring capacity, for legislation and in mediating between the three parties.

Research and development are responsibilities shared by academic research institutions and the pharmaceutical industry. In this, the pharmaceutical companies do most of the actual drug development. Universities and University Medical Centres contribute in defining drug targets and providing general knowledge on the diseases, in the development of methodology to create and identify potential lead structures that may be transformed into actual drugs, and in fundamental life science research aimed at unravelling processes in health and disease. Furthermore, the researchers employed by pharmaceutical industries are trained at the academic institutions. Contemporary pharmaceutical research is of a highly multidisciplinary nature and researchers active in

this field are required to possess a general overview of the field and at the same time need to specialise in a particular area of research. Universities try to cater to the demands from the pharmaceutical industry by offering broad life science educating programmes. The Faculty of Mathematical and Natural Sciences at Leiden University brings both Life Sciences and Technology (together with Delft Technical University) and Biopharmaceutical Sciences, the former being of a fundamental nature and the latter more applied.

In academia the opinion abounds that conservatism in the pharmaceutical industry is largely to blame for the relative lack of success in identifying new drugs. Pharmaceutical companies select their targets based on economic potential, with the result that the focus in their drug development programmes is on first world diseases only, and only diseases from which many patients are suffering. If academia had access to the same research budgets, such is the reasoning, cures for third world diseases and orphan diseases (diseases from which only a few patients suffer) would rapidly be discovered.

It is true that academic research budgets have been much smaller in the past decades than those of the commercial parties. This situation, however, has recently started to change. For instance, the Bill & Melinda Gates Foundation controls over 60 billion dollars, a large part of which is to be dedicated to research and development projects aimed at therapeutics for third world infectious diseases such as tuberculosis. It remains to be seen how successful academia will be in transforming such budgets into actual therapeutics.

Pharmaceutical companies are, out of economic necessity, conservative in their target diseases. They are, after all, commercial activities and do require a return on investment. Once a drug development programme is identified, pharmaceutical companies often operate in a manner that is far from conservative. They are amongst the most popular employers, and recruit from the top graduates and PhDs in all sciences. These companies' scientific level is therefore at least on a par with most academic institutions. Their research facilities are often (much) larger and better equipped and bring together many disciplines. Finally, out of the economic necessity of making a profit, pharmaceutical companies go to great lengths to make the most of their research activities. Promising therapeutic leads that fail to become an actual drug for an originally

targeted disease but to which much time and effort have been devoted are often evaluated for other applications. Within the context of a capitalist society we cannot expect pharmaceutical companies to change their strategy. Neither can we expect them to improve much on their performance. Any increase in the number of therapeutics and the nature of the diseases they are aimed at therefore needs to be realised from efforts in the public domain: the academic institutions. Academic researchers can freely choose the diseases to work on and the strategy to pursue this, and can select the class of compounds that may be developed into therapeutic agents at will. In achieving this, academia would do well to learn from strategies pursued in the pharmaceutical industry.

Finding therapeutic targets and therapeutic leads

Room for academic research in drug development can be roughly divided in two directions. These are 1) target identification, or the determination of the biological function(s) that cause disease and can be targeted with pharmaceutical agents and 2) lead identification, or the discovery of compounds that can be developed into actual drugs.

In recent years target finding has received considerable attention in academic research, whereas lead finding is largely unexplored. For instance, in the last decade we have witnessed the elucidation of the human genetic code and that of many human pathogens. Upon completion of the first genome projects the hopes were high that the resulting information would rapidly lead to the identification of the genetic factors that cause disease or are risk factors for specific ailments. While the data amassed from the genome projects have definitely aided in finding some new drug targets and provide a wealth of information for academic researchers to study, the actual impact on drug development to date is somewhat disappointing. Often, scientists state that it takes time to investigate the data from the human genome project, which in fact is a euphemism for saying that we do not know yet how to interpret it.

In science, not having an answer is not so much a problem as an opportunity, and molecular biologists and geneticists together have created a whole new field of research that they have termed genomics. This research area may not have yielded a great deal of new insight either, but has led to an enormous windfall in terms of research funding worldwide. Attracted to such unprecedented goldmines for fundamental

academic research, scientists with a different background argued that not so much the genetic information from the genome as the information embedded in the gene products that result from these, the proteins, or even the products of protein action, the metabolites, are the key to understanding the differences in health and disease. As a result, biochemistry is now loosely redefined as proteomics, and analytical chemistry as metabolomics, with rewards in terms of funding opportunities comparable to that of genomics research as a result. Informatics researchers in turn decided that reliable solutions could be generated only when genomics, proteomics and metabolomics data are pooled and analysed with the appropriate software. Witness the birth of the field of systems biology, most likely the most diffuse area of research within the life sciences to date.

The point here is not so much that the research that is pursued under one of these new themes is useless. Many excellent researchers are funded through one of the dedicated research programmes outlined above. In the Netherlands, many fundamental research projects are financed through genomics-related programmes. There are, however, a number of dangers associated with name branding, both for drug-related research and for academic research in general. First, politicians and decision-makers may at some point notice that, rather than providing general solutions, the conclusion of each genomics-type research programme coincides with the recommendation to instigate yet another 5-year programme that makes the same promises, but through a fundamentally altered approach. Second, genomics-type research programmes are often large and involve numerous research institutes. They tend to be bureaucratic, and in the process small innovative projects are stifled. Third, although scientists in general have a chameleonic nature and are quite capable of making their research interests fit within a larger scheme, the genomics-type programmes do favour research directed at obtaining much data in one strike (so-called high-throughput research) over in-depth studies aimed at obtaining detailed insight in a single isolated process.

The importance of finding pharmaceutical leads is highly underestimated in academic research. A pharmaceutical lead can be defined as a compound that has a desirable effect on the selected target. It may, for instance, be an enzyme inhibitor or have an agonistic or antagonistic effect on a target receptor that should be activated or inactivated, respectively. The structure of a therapeutic lead serves as a

starting point for the generation of analogues. Such analogues are often required to generate compounds with an improved biological effect (more active), or improved biological availability; compounds which are furthermore not toxic or do not produce toxic metabolites and have a suitable and predictable clearance rate. It should be realised that the instances in which the final drug was present in the original screen of a set of compounds are extremely rare. Current state-of-the-art drug research dictates that it is not possible to design, on paper, a therapeutic agent for a given pharmaceutical target, synthesise the compound and find it to be an effective modulator. Further, it is almost impossible to decide a priori whether a given compound that does have the desired biological effect but has an undesirable pharmacological profile (too toxic, too unstable, does not reach its target) can be developed to an actual drug. Behind every drug are many compounds, sometimes numbering in the thousands, which have been evaluated, either during the lead finding stage or during ensuing optimisation studies.

Discovery by chance

Historically the finding of therapeutic agents for use in human medicine is characterised by a high trial and error nature, and the situation today is not much different. The notion that we need more than one therapeutic lead per disease for further development, and that we cannot a priori predict whether a compound will turn out to be such a lead by exerting its effect on the projected biological target, implies that we need many compounds. The larger the number of compounds a so-called compound library encompasses, and the larger the structural and functional diversity within the compound library, the better the chance of finding a therapeutic lead. In fact, herbal medicine is nothing more than trying many compounds from various natural sources. The malaria drugs artemisinin and quinine and the analgesic salicylic acid were discovered by chance after millennia of experimentation. During the twentieth century, therapeutic research was professionalised and institutionalised, but the chance factor remained. The following six examples from modern medicine practice serve to illustrate this point.

In the years preceding World War II researchers at Bayer, then part of IG Farben, accidentally found that one of the dyes that were produced within the company provided protection against a series of bacterial

infections. The compound was marketed as Protonsil and became the first successful synthetic drug for the clinical treatment of bacterial infections. French researchers later found that Protonsil itself is not an active drug, but is hydrolysed in the body to produce the actual bactericidal substance, Sulfanilamide.[8] Protonsil thus is not only the first effective therapeutic agent to battle bacterial infections, but also comprises the first pro-drug (an inactive compound that is transformed into the actual drug by a specific biological process within the body of the patient) that was discovered. The discovery of Sulfanilamide, which coincided with the outbreak of World War II and the resulting increase in the number of patients suffering from bacterial infections, spawned extensive research into related compounds. Thousands of Sulfanilamide analogues were prepared and evaluated worldwide. This led to the class of sulfa drugs that are still used in the treatment of specific bacterial infections.

The discovery of penicillin by Alexander Fleming in 1928 is a textbook example of serendipity in science. Fleming noted that bacterial outgrowth around cultured *Penicillium* moulds was inhibited, and concluded that the mould actively prohibited bacterial growth by the excretion of a bactericidal substance.[8] He named this substance penicillin and the structure of penicillin was elucidated in 1940. It became the most important therapeutic for the treatment of infections caused by gram-positive bacteria, and served as a lead for many structural analogues with improved pharmacological properties. Penicillins and related compounds such as the cephalosporins are still in use today as antibiotics. The working mechanism of penicillin was later determined and basically entails blockage of bacterial cell wall biosynthesis. Targeting biochemical pathways which are essential for human pathogens but which we do not possess has proven to be a useful strategy in antibiotics research. Next to penicillin, the discovery of the mode of action of Sulfanilamide has stimulated pharmaceutical research to focus on such pathogen essential pathways. Sulfanilamide inhibits bacterial folate biosynthesis, which is essential for the biosynthesis of one of the four DNA building blocks, thymidine. Humans acquire folate from food sources and do not need to synthesise the compound.

The anti-tumour agent Cisplatin was discovered by mistake some 50 years ago, when researchers under the supervision of Barnett Rosenberg performed electrolysis experiments on bacteria.[9] In their experiments they used platinum electrodes together with aqueous solutions containing

bacteria, and noted that these grew out to 300 times their normal size, but did not divide. They found cisplatin to be the agent responsible for this mitosis blockage. Cisplatin appeared to be formed by partial oxidation of one of the platinum electrodes followed by formation of the complex. Its anti-mitosis effect made cisplatin an obvious candidate for chemotherapy and is nowadays used for the treatment of several tumours, including testicular cancer.

Of a more recent date are the discoveries of Viagra[10] (treatment of erectile dysfunction), Zavesca[11] (treatment of a lysosomal storage disorder called Gaucher disease) and Gleevec[12] (treatment of chronic myelogenous leukaemia). These drugs are the result of directed development programmes in the pharmaceutical industry and are the fruit of the current state-of-the-art pharmaceutical research. Still, all three are characterised by some coincidental factors. The active component in Viagra, Sildenafil, stems from a programme within Pfizer aimed at finding a treatment for hypertension and angina pectoris. During phase one clinical trials, the compound appeared to induce penile erections and today the compound is one of the billion dollars per year blockbusters as a drug to temporarily revert impotency. The active component in Zavesca, N-butyldeoxynojirimycin, and related compounds were previously investigated by several pharmaceutical companies, amongst which Organon and Bayer, as therapeutic leads for the treatment of viral infections, in particular HIV. In procreation, viruses make use of biological processes in host organisms. The idea was that blockage of one of these, specifically the action of an alpha-glucosidase that resides in the endoplasmic reticulum, could interfere with viral reproduction without causing much harm to the host. The alpha-glucosidase is a hydrolytic enzyme that removes glucose residues from various substrates and N-butyldeoxynojirimycin inhibits the action of this enzyme. The effect of inhibiting alpha-glucosidase appeared somewhat disappointing and the project was abandoned. The coincidence here is that the same compound turned out to be an efficient inhibitor of a related, but distinct, enzyme, namely glucosylceramide synthase. Inhibition of this enzyme in turn lowers the glycolipid levels that cause Gaucher disease. Oxford Glycoscience capitalised upon this finding by developing Zavesca for the treatment of Gaucher patients. In their development project, they benefited from the legwork done by others in determining toxicity and bioavailability of N-butyldeoxynojirimycin. Finally, the therapeutic lead

that is at the basis of the development of Gleevec (Imatinib), comes from a research programme that did not have chronic myelogenous leukaemia (CML) as its primary goal. Gleevec is a tyrosine kinase inhibitor and tyrosine kinases form a large family of enzymes that are implicated in different tumours. CML was not the first target Novartis researchers were aiming for, since the number of CML patients is rather small. However, at the request of CML researchers, the CML specific tyrosine kinase (known as the BCR-ABL gene product) was included in the panel of tyrosine kinases assayed in the initial screens. The development of Gleevec is regarded as the pinnacle in the search for medicines to treat cancer patients and is often used to demonstrate our progress in rational drug design. However, at the basis of the development of Gleevec there also lies an element of chance: in this particular case the researchers that advocated the inclusion of the CML related tyrosine kinase in the original screen. Note that two of the three diseases mentioned here, namely Gaucher disease and CML, are in the first instance not very attractive targets. Gaucher disease is a so-called orphan disease, with about 10 to 15 thousand patients worldwide, and neither are there many CML patients. Nevertheless, due to its approach the pharmaceutical industry does provide solutions for diseases that are unattractive from an economic perspective. The last three examples demonstrate that pharmaceutical companies force their luck in finding new therapeutics. Their strategy is two-fold: bring together many different compound classes and screen these for many different biological targets. Furthermore, pharmaceutical companies bring together specialists in many different expertises, thus ensuring that the potential value of a specific compound class does not go unnoticed. See for instance the development of Gleevec. In short, what the companies do is enhance and direct the factor of chance in their finding of new pharmaceutical agents.

A new role for academic research

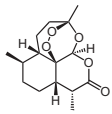
Pharmaceutical companies identify therapeutic leads by evaluating large compound collections, called compound libraries. They assemble their compound libraries using different strategies. Compounds from natural sources still serve as inspiration, and both companies and academic institutions specialise in the extraction and characterisation of such compounds. Pharmaceutical companies have extensive medicinal

chemistry departments where compound libraries are synthesised by organic chemists. Promising organic chemistry concepts from academia or developed within the pharmaceutical companies are evaluated and, if useful, incorporated. Each example comprises combinatorial chemistry as a new concept to make many compounds in one go, rather than one compound at a time, as is common practice in organic chemistry. However, pharmaceutical companies found that, although many compounds can be prepared via combinatorial chemistry, the variety in terms of diverse structures was limited. Thus, combinatorial chemistry efforts were farmed out to sub-contractors and nowadays many of these agreements have been discontinued.

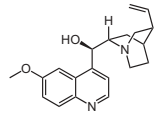
Academic institutions can emulate the pharmaceutical industry by generating compounds of a different nature than normally found in compound libraries, and assess these in many different biological screens. In the pharmaceutical industry the synthesis efforts, out of economic necessity, tend to be of a target-oriented nature and most of the compounds prepared are derivatives of structures with a proven biological effect. Academic organic chemists, by contrast, have partial or complete academic freedom in picking their target structures. They are motivated by an appealing structure of a natural product that is complicated to synthesise, or by the fundamental question of what would happen if the natural product is modified in specific ways. Individual academic researchers in organic chemistry that are driven by such motivations will not produce many compounds. However, assembling the products of numerous academic researchers will lead to compound libraries that, by the virtue of the variety of academic questions that researchers pose themselves, would be of a highly diverse nature. The nature of these libraries will differ considerably from the assemblies currently in use by pharmaceutical companies, and the chances are that they will contain new pharmaceutical leads. Academic compound collections may be assembled in screening facilities that are open to any party with an interest in drug research. Here, academic biology or medicine researchers may screen their own assays. In this way a therapeutic lead may meet with its target and at the same time the organic chemist who produced the lead will meet with the researcher who investigates a specific disease. Basically the situation as normally found in pharmaceutical industry, but with the difference that either the nature of the lead or the target (a third-world disease instead of a first-world disease), or both, is fundamentally

different. But again: chance is a factor, and efforts are made to enhance and direct this factor.

Recently, several initiatives have come to light, especially in the United States. The US-based National Institute of Health has created the Small Molecule Repository, which contains about 500,000 compounds from academic and commercial sources, and has set up a Molecular Libraries Screening Centers Network in which these compounds can be screened.[13] In Germany, related institutes have been founded and academic researchers meet in a combined programme called Chembionet.[14] Both projects aim at creating synergy between academic researchers without enforcing specific research objectives. Organic chemists can continue to pursue their favourite research objects and, along the way, find out that some of their compounds have potential as therapeutic leads. Countries that have not yet implemented or planned such initiatives are well advised to do so in the near future!

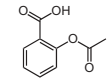


Artemisinin

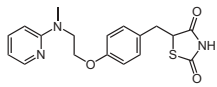


Quinine

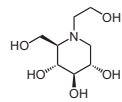
Salicylic acid



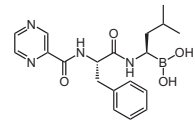
Aspirin



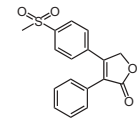
Rosiglitazone



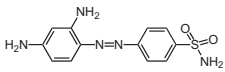
Miglitol



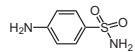
Bortezomib



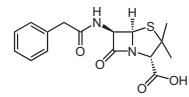
Rofecoxib



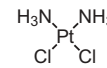
Protonsil



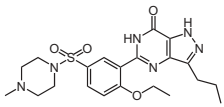
Sulfonilamide



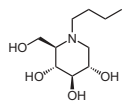
Penicillin



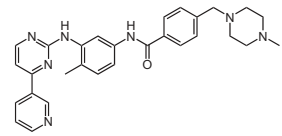
Cisplatin



Sildenafil



Zavesca



Imatinib

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3
From genomics to therapies: deductive or inductive?

Gert-Jan van Ommen

The developments in the field of genetics, notably concerning the elucidation of disease mechanisms and the development of potential therapies, have undergone major advances in the last decennia. This started in the mid-eighties with the finding of genes causally involved – when mutated – in monogenic diseases and cancer. Thus far, approximately 2000 genes have been implicated with several hundreds of diseases and cancer forms. Despite most hereditary diseases being rare, this research has yielded many unexpected, fundamental findings regarding the basic mechanisms of our genetic machinery, like triplet expansion diseases, germ line mosaicism, genetic imprinting and other epigenetic forms of modulating gene expression. While in the majority of cases the finding of the gene mutations still has not led to a full elucidation of the disease mechanism, the progress has broadly delivered reliable and specific diagnoses. The importance of this for the information and well-being of people at risk is often underestimated.

For example, in the Netherlands ca. 2000 DNA diagnoses are done yearly (data 2005). Due to the nature of the field, in which a disease occurrence in a family mostly leads to several relatives being concerned, typically only 1 of every 6 diagnoses detects a mutation. Thus, in ca. 22,000 queries yearly the *absence* of the mutation is confirmed. As every query affects around 3-4 people (proband, spouse and children), the advent of DNA diagnosis yearly provides major and final relief to 70-90,000 people, at a cost of less than 25 million euro, or only 250-300

euro per case. Given that this field started in 1985, gradually ramping up since then, it is a fair estimate that this benefit has now reached approximately 1 million Dutch individuals. This is a major benefit and a rarely heralded contribution to our national well-being which should be taken into account when considering valorisation.

In the next decennia we expect that a link will be made much more often between the *gene defect* and the *disease mechanism*. This link is often far from obvious. People rarely become ill as a result of their gene mutation, but much more often from the processes which go awry as a consequence of the mutation, or even due to secondary processes like immune reactions and inflammation. Once this link is established, rational steps can be taken towards development of causal therapies. In some cases the advances have led – or are currently leading – to focused, causal therapy. The first examples have already arrived or are in their final development, such as the blocking of a constitutively active growth signal in leukaemia and several forms of cancer by the Gleevec compound,[1] the specific therapeutic effect of Herceptin in Her2-positive breast cancer,[2] the administration of the missing proteins in metabolic disorders like Gaucher and Pompe[3] and the restoration of the genetic code for the muscle protein dystrophin by skipping one of the exons in Duchenne muscular dystrophy.[4]

In these and similar cases the discussion now will have to focus – besides on further development – on cost aspects and the establishment of a sound registration and reimbursement system, so that these advanced and costly therapies become accessible to all patients. This is a hurdle not to be underestimated, which receives far too little attention by national and international authorities. Otherwise the interest of biotech and pharma companies to develop these tremendously promising therapies will be severely hampered by the absence of a mature market.

Systems approaches

Besides the steady, albeit painstaking, advances in basic, mechanistic research and causal therapies, entirely new approaches are also in sight. These are propelled by the rapid developments in the genomics field and are based on so-called 'high-throughput' systems research platforms. Technologies are being developed to comprehensively describe the biological state of cells and body fluids in the organism and to compare

these between diseased and healthy individuals. This has started by defining the global expression status of all our genes using DNA microarrays, the so-called 'transcriptomics' approach. This is now being followed by similar approaches establishing the levels of our proteins (proteomics) and metabolites (metabolomics), and, on a yet higher level, the establishment of their connectivity. The integration of these technologies, using biostatistical and bioinformatics algorithms, and – where available – knowledge of the underlying interactions and equilibria, is often called 'Systems Biology'.

In transcriptomics approaches, typically the expression levels of 5,000-20,000 genes are determined. When comparing diseased and healthy individuals, in most cases altered levels are found for several tens to hundreds of genes, even in diseases known to be caused by a defect in only one gene. Apparently, not only is the pathway in which the defective gene operates disturbed, but in addition the aggregated effect of secondary, reactive changes plays its own part, too. This underscores the systemic nature of disease: cellular processes behave like a connected network which tries to compensate for perturbation, to adapt as well as possible to deleterious shifts in homeostasis. It is widely expected that the extensive characterisation of this connectivity, together representing sets of 'biomarkers', will yield refined and specific diagnostic tools.

These will allow the readout of the nature and severity of disease, as well as its present stage and most likely future course. This will ultimately provide the doctor with powerful diagnosticals and prognostic tools to underpin treatment decisions and, equally important, to guide prevention. While in most specialties much comparative and validation research is still required before these biomarker profiles are sufficiently robust to be used in diagnostic and prognostic applications, the first successes have been achieved in clinical applications, notably in the cancer field. Indeed, the Netherlands is playing a leading role in this development due to an early implementation of Genomics research in medical biology research.[5]

In the near future, biomarker profiling will also include proteomics and metabolomics, both of which have the advantage that these biomolecules are closer to the cellular functioning and therefore to the disease state. A further advantage of proteins is that they are often more stable than gene transcripts (notably in pathological tissue), while the advantage of metabolites is that they are identical throughout nature: citrate, glucose

and cholesterol are the same from man to mouse and from fruitfly to zebrafish. This permits us to make better connections between the study of human disease and animal models. On the other hand, a disadvantage of proteins and metabolites is their phenomenal range in quantity and diversity of physical properties. This at least necessitates very powerful and sensitive equipment, statistical approaches and a focus on molecular sub-compartments in order to be reliable and successful.

Systems approaches not only hold promise for integral patterning of health and disease to assist diagnosis, prognosis and prevention. It is also of key importance for the development of therapy to get a better picture of the early and late molecular events on the pathway towards disease. Frequently, interventions developed to counteract a deleterious process produce unexpected side effects in other pathways, due to the same connectivity described earlier. Systems approaches will highlight this in a much earlier phase, thus allowing us to better direct drug development towards the prevention or reduction of adverse effects.[6]

Pharmacogenetics and pharmacogenomics

Important developments have occurred not only on the side of medication, but also on the side of the recipient. There are large inter-individual differences in the reaction to – and effectivity of – pharmaceutical compounds. The field of *pharmacogenetics* deals with hereditary differences of drug reactions. Well-known examples are genetic polymorphisms in the enzymes involved in the conversion of ingested (pro-)drugs into their active compounds, as well as in their subsequent clearance from the body. The functional polymorphism of thiopurine S-methyltransferase has gained notoriety due to the occurrence of life-threatening haematopoeietic toxicity with standard dosage of mercaptopurine and azathioprine. But also the cytochrome P450 family, which performs a large variety of detoxifying reactions and components which play a major part in our drug conversion metabolism, has an extensive genetic variability. Thus, gene duplication of the 2D6 variant, the key uptake route of antidepressants, is a main cause of poorly responsive depression medication. And drug uptake or metabolism is by no means our only scourge: advances in the human genome project have highlighted an ever-increasing list of variations in the very molecules at which the medications are targeted.[7]

So how to deal with this Pandora's box? The pharma industry has initially hailed genomics as a panacea to identify more and better drug targets. But meanwhile it has become clear that a dearth of targets is not the actual problem. Indeed, we have rather too many than too few targets, considering the enormous cost and involvement of research capacity to subject each one of them to a drug development. This is due to the unavoidable need to establish for each drug the risk/benefit ratio in prospective clinical trials. The real bottleneck in this field is the development of faster and more powerful tests for the effectivity and toxicity, or, as a recent FDA report says: 'We do not need more targets but better and faster readouts'.[8]

In fact, one may place question marks behind the way we develop medication today. As D. Nicholson, Executive Vice President Research, Organon, has described this, somewhat caricaturally (Genomics Momentum, The Hague 2005):

'When there is an indication, from preclinical and/or animal research, that a certain compound may have the desired functionality, and is not unacceptably toxic, we first give a cohort of healthy volunteers (not particularly the target group) an increasing dose of it, to find the amount which produces a measurable deleterious effect. Then we give a second group of equally healthy volunteers a lower dose (e.g. half the toxic amount) for a longer time to see if there is long-term toxicity after all. If so, we have to reduce the dosage yet further. Only after this can we determine whether the safe dosage thus found has any useful effect with patients. This means that we have gone down a very long and costly trajectory before we have any indication whatsoever of success in the later development period. It would be much better to "fail more early and more cheaply".'

In this area, functional genomics and systems biology research, using high throughput technologies, have much to offer. The hope is justified that more systemic readout methods, using transcriptomics, proteomics and metabolomics platforms, in cellular or animal models of the diseases under study, i.e. in a much earlier stage of research, will yield more powerful indications, faster, as to the balance between effectivity and toxicity. This is why the development of cellular and animal disease models is not only important to unravel disease mechanisms, but also a

key strategy towards major cost reductions by obtaining earlier support to discontinue low-chance development trajectories.

A beneficial side product of this approach is that it will assist in gaining insights into 'favorable' prognostic patterns *per se*. This will ultimately lead to a greater diversity of functional compounds, alone, or in - otherwise not foreseeable - combinations, but potentially applicable for equally unforeseeable secondary uses.[9]

Disease and therapy as a 'black box'

There is a second component to variation in drug effectivity, and this has to do with actual disease heterogeneity. What we perceive as one disease, can increasingly be stratified into different sub-categories, which may have related etiologies but also very different ones. To define this further we can use the systems biology approaches described above as pattern recognition tools, segmenting an unknown black box into smaller and more homogeneous boxes. This will eventually lead to mechanistic insights: comparison of consistent differences using ever more refined technologies will ultimately allow this inductive 'black box' approach to be succeeded by a more deductive modeling of mechanisms. This then is commonly expected to herald an era of greatly improved prognostic, therapeutic and preventive options.

But, do we really have to wait for all these future insights, and follow a strictly deductive route, from molecular levels to patterns to mechanisms, to eventually arrive at the design of interventions? As we tend to believe and as we often argue to get funding for our costly research? In fact, mankind has dealt with diagnosis, therapy and prevention as a black box for as long as its existence – and quite successfully so. Using pattern recognition and trial-and-error-based intervention as development model, gradually replaced by more inductive reasoning, based on the outcome patterns of previous interventions. Most likely, animals have gone the same route well before us: of pattern recognition, prediction and behavioral and nutritional adaptation. Indeed, as recently as the past century it has been more the rule than the exception that therapeutic or preventive successes have led the way to mechanistic explanations, rather than the reverse. That is the essence of serendipity.

In this light, genomics, with its unprecedented strength of reading out

the systemic parameters of genes, transcripts, proteins and metabolites, will allow the precise definition of 'phenotype' at a much earlier stage than our ancestors could, and often in more accessible cellular model systems or body fluids. The integration of genotyping, phenotyping and biological modeling along these lines is ongoing in many places including our genomics centre of excellence the Center for Medical Systems Biology (http://www.cmsb.nl), and may in some cases curtail the wait for a precise understanding of the workings of our biology to design successful cures.

A recent example is a metabolomics study of Bahn and co-workers in schizophrenia,[10] which, while small and requiring confirmation, suggests the use of this approach to differentiate between drug-naïve patients and controls, to assess treatment effect and which also, quite plausibly, suggests that the system is more easily reversible by early treatment than in a later stage, after several disease episodes.

In conclusion, there is a time-tried but overlooked route towards the development of therapies and prevention, which is possibly shorter than the prior elucidation of disease mechanisms in order to found further development on this. Our greatly improved systems readout capacity should allow us to attempt an inductive, 'black box' approach in the therapeutic arena, by basing our interventions directly on the experimental determination of the systems alterations in any given disease, followed by a judicious evaluation of specific compounds with opposite system effects, alone or in combinations. The added advantage of this route, pioneered by the Lander laboratory under the term 'Connectivity map',[11] is that it offers also a very direct shortcut to secondary and tertiary use of existing drugs. This would be very attractive toxicologically. Then subsequently, the increased pattern recognition and robust modeling gleaned from initial therapeutic successes, however modest, should greatly assist our understanding of underlying mechanisms, paving the way for more specific, targeted therapies.

Notes

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4
Hempel's dilemma and the physics of computation

Carlo Beenakker

Carl Gustav Hempel (1905-1997) formulated the dilemma that bears his name in an attempt to determine the boundaries of physics.[1] Where does physics go over into metaphysics? Hempel saw himself confronted with two equally unappealing choices. Is physics what physicists have discovered so far, or is physics what physicists will eventually discover?

The horns of the dilemma

The bull of Hempel's dilemma charges – on which horn will you be caught?

- The first horn: *metaphysics is what current physics cannot explain*. With this choice the boundary is clear, but it is likely that some phenomena that you would classify as metaphysical will eventually find a sound physical explanation. According to twentieth century quantum physics an object can be at two positions simultaneously. Should a nineteenth century scientist have classified this as a case of magic bilocation?
- The second horn: *metaphysics is what future physics will not be able to explain*. With this choice the boundary is so vague that it is of little use. Who knows what discoveries some twenty-first century Einstein will make – so where should we draw the line today?

The purpose of this contribution is to indicate how a recently developed field of research, the physics of computation, might offer a new answer to this old question about the boundary between physics and metaphysics.

Matter is physical

One hundred years ago the answer was clear: the boundary between physics and metaphysics is the boundary between matter and spirit. We can estimate reasonably well how much matter the universe contains, even if we are uncertain in what form the matter is present. Matter of any kind attracts other matter through the force of gravity. By measuring the velocities of remote galaxies we can calculate how much matter the intervening space contains – even if not all that matter is visible. Some of that 'dark' matter consists of known particles (such as the neutrino), but it is likely that it also contains as yet unknown particles.

If we restrict the field of physics to the study of matter, then we are done. Hempel's dilemma does not appear, because even if we do not know what new kinds of matter future physics might discover, the amount of matter in the universe is finite and (approximately) known. But this limitation of physics to materialism implies that whatever has no mass must be a spirit, which is not a tenable proposition. The particle of light called the photon certainly has no mass, but the laws of physics are well able to describe the properties of photons.

We might try to limit physics to the study of particles, with or without mass, but this restriction cannot be maintained either: as discovered by Hendrik Casimir, the empty space, the vacuum, has a dynamics of its own that can attract or repel objects. Quite possibly it is the repulsive force of the vacuum that protects the universe from collapsing under its own weight.

Information is physical

To arrive at a new answer to the old question about the boundary between physics and metaphysics, I start from a statement of Rolf Landauer (1927-1999), a researcher at IBM who pioneered my own field of research, mesoscopic physics. *Information is physical,* was a favorite statement of

Landauer,[2] to emphasize that the processing of information by a computer is constrained by the laws of physics. Three known constraints are:

Einstein's constraint: information cannot be transferred at a speed greater than the speed of light c.

Landauer's constraint: information cannot be erased without generating heat: the minimum heat production is $kT \ln 2$ per erased bit of information, at a temperature T.[3]

Margolus & Levitin's constraint: information cannot be processed at a rate exceeding $4E/h$ operations per second, for an available energy E.[4]

Together, these three constraints represent three major frameworks of theoretical physics: respectively, the theory of relativity, statistical mechanics, and quantum mechanics. Each constraint is governed by one of the fundamental constants of nature: the speed of light $c = 2.98 \times 10^{8}$ m/s, Boltzmann's constant $k = 1.4 \times 10^{-23}$ Joule/Kelvin, and Planck's constant $h = 6.6 \times 10^{-34}$ Joule/Hertz.

The most powerful computers available today operate far below these ultimate limits, but this may change at some point in the future. The field of quantum computation aims at constructing a computer that actually reaches the fundamental limits on information transfer, erasure, and processing.[5] Once we have constructed this machine, we cannot do any better. It may take a decade, or a century, or a millennium – but once our computer reaches the fundamental limits set by the constants of nature, no amount of human ingenuity can produce a faster machine.

The fundamental limits to information processing provide an opening to the resolution of Hempel's dilemma, by restricting the capabilities of future physics based on our knowledge of current physics.

The universe is a computer

In a reversal of Landauer's dictum, Seth Lloyd from MIT has argued that *The universe is a computer.*[6] His argument proceeds as follows. First, the

universe represents information, encoded in the state of each of its particles. The number of bits of information contained in the universe equals, by definition, the logarithm (base 2) of the number of distinct states that are available to it. Lloyd estimates that the universe as a whole has the capacity to store 10^{90} bits of information. A very large number, compared to the capacity of 10^{12} bits of a typical hard disk, but a finite number.

Secondly, the universe processes information in a systematic fashion. The 'operating system', so to say, consists of the laws of physics. The particles evolve from one state to the other as a result of mutual interactions, in a way that is precisely dictated by the rules of quantum mechanics. We may not yet know these rules completely (in particular, in the case of the gravitational interaction), however the dogma of physics is that such rules exist and are knowable. The Margolus-Levitin constraint admits up to 10^{120} operations in the 10^{10} years since the birth of the universe.[7] Again, this is a large number relative to the total number of operations performed by man-made computers (some 10^{30} in less than a century) – but it is a finite number.

If the universe is a computer, then it makes sense to use the constraints on information processing as constraints for physics as a whole – not just as constraints on a sub-field of physics.

Taking the bull by the horns

Building on the insights of Landauer and Lloyd, I propose to resolve Hempel's dilemma with the definition:

The boundary between physics and metaphysics is the boundary between what can and what cannot be computed in the age of the universe.

Let us explore this boundary by inquiring on which side lie the three key ideas of metaphysics, as identified by Immanuel Kant: God, free will, and immortality of the soul.

Is God physical or metaphysical? What knowledge we have of God from the Abrahamic faiths suggests that His computational capacity is infinite. The computational capacity of the universe up to the present is certainly finite, as mentioned earlier, but what about the future? The cosmologist

Frank Tipler has constructed a physical theory of God on the premise that the amount of information processed and stored between now and the final state of the universe is infinite.[8] Indeed, if there is no limit to what the universe can compute in its lifetime, then there is no limit to the power of physics and the metaphysical is non-existent. Most cosmologists, however, would argue (based on the expansion rate of the universe) that the computational capacity of the universe between its initial and finite state is finite, leaving God in the metaphysical domain.

Is free will physical or metaphysical? Following Lloyd[9] we characterize an act of free will as the outcome of a computation (= logical reasoning) by the human brain which is intrinsically unpredictable. It is not yet known how the brain operates, but even if it were known, the unpredictability of its operation can remain because the laws of physics allow for unpredictability when referring to a single event. (Only averages over many events are predictable). No metaphysical ingredients are needed.

Is the immortal soul physical or metaphysical? In order to be physical, the immortal soul should contain and process information beyond death, which is the erasure of most information in the organism. Estimates of the amount of information lost upon death are in the order of 10^{32} bits per human.[10] Mankind as a whole has lost some 10^{43} bits of information over the course of 50,000 years. We know of no mechanism by which this amount of information could have survived by physical means, leaving the immortal soul in the metaphysical domain.

Notes

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5
Noise or signal? The dilemma of individual differences

Willem J. Heiser and Jacqueline J. Meulman

Science proceeds by the elimination of competitive theories, each of which has its unique claim to the truth. It often appears, however, that the behavioural sciences suffer from unresolved competition among theories. In clinical psychology, for example, Weinberger noted that authoritative reviews and well-controlled outcome studies seem to indicate that no form of psychotherapy is superior to any other, although all are superior to the mere passage of time.[1] One explanation for this finding is that different ways of treatment share common factors that are more important than factors that are unique to specific schools of psychotherapy.[2] In social psychology, another example, a wealth of different cognitive consistency theories dominated the field for quite some time (summarised in Abelson, Aronson, McGuire, Newcomb, Rosenberg, and Tannenbaum[3]). Cognitive consistency theory holds that behaviour that is at odds with an established belief, value or perception (or cognition, for short) demands change, either in the behaviour, or in the cognition. The dilemma between changing the behaviour and changing the cognition produces an uneasiness called dissonance, which tends to prevent action. Once the dissonance is resolved by favouring one horn of the dilemma over the other, consistency is restored, and action resumes. According to Greenwald, Banaji, Rudman, Farnham, Nosek, and Mellott, 'Competition among consistency theories in the 1960s and 1970s drew attention more to their peripheral theoretical differences than to their central areas of agreement.[4] Research on these disagreements never

produced a decisive preference among the theories. This lack of resolution could have created the impression that all of the theories had problems'. Even though the common theoretical core was never contested, the area fell from grace, and lost its attraction to a new generation of researchers.

Nevertheless, at the frontier of research we need theoretical diversity, because it is a necessary condition for the evolutionary process of knowledge acquisition.[5] Coexistence of competing theories implies that each of them has at least one advantage over each of the others, just like competing commodities in a market. Nevertheless, what if the diversity becomes so vast and complete that a field breaks down into two disciplines?, as argued by Lee Cronbach fifty years ago in his Presidential Address for the American Psychological Association.[6] The driving force in this breakdown was the dilemma of how to deal with psychological differences between individuals, or individual differences for short. Should we treat them as noise in our experiments in which we try to establish general laws of behaviour, or should we study them in their own right, and try to predict actual behaviour of specific persons? This question is one of the most persistent dilemmas in psychology since its beginning in the nineteenth century, and continues to keep research psychologists concerned.[7]

The two disciplines of psychology

In Cronbach's vision, scientific psychology was divided into two disciplines, being two streams of method and thought: experimental psychology and correlational psychology.[8] In experimental psychology, we change one aspect of the environment and observe the behavioural consequences, while keeping everything else under control. In correlational psychology, we study relationships between variations in the behaviour of individuals that may or may not be under our control. As Cronbach put it more strongly, 'While the experimenter is interested only in the variation he himself creates, … the correlator's mission is to observe and organise the data from Nature's experiments'. An alternative pair of labels that indicates the same disunion is *general* psychology versus *differential* psychology (or individual psychology). At least in the early days, the experimenter aimed at finding general laws of behaviour that are true for everyone, while the correlator aimed at establishing stable person characteristics that predict different behaviours for different people.

The focus on general laws naturally leads to a focus on the mean of the observations, while deviations from the mean point to everything that is not under control. The experimenter will therefore try hard to reduce the deviations from the mean by standardisation of the experimental tasks and conditions. Usually, all the remaining variation is regarded as noise, and the effect of the experimental treatment is evaluated statistically against the volume of the noise, called the *error variance*. If there are stable differences between the participants in the experiment that are relevant for the phenomenon that is being studied, these individual differences are a nuisance, because they increase the error variance and make it harder to detect any effect of the experimental treatment.

By contrast, prediction of individual behaviour critically relies on the possibility of coupling a specific position of an individual on some predictor variable to a specific position on the outcome (predicted, response) variable. Therefore, differences between individuals do matter; they are a signal (and a reliable signal if they are stable). The amount of difference is expressed in the standard deviation, which measures how far on average individuals deviate from each other or, which amounts to the same thing, how far they deviate from the mean. In so far as the signal leads to successful predictions, the correlator has established important determinants of individual behaviour. Although he cannot claim causality, lacking the benefits of experimental control, he does have the possibility of statistical control to correct for possible rival variables if these can be identified and measured.

The dilemma of individual differences pervades not only academic psychology, but also applied psychology, where the psychologist deals with treatments or interventions that are, for example, of an educational nature in school settings, of an ergonomic nature in work settings, or of a therapeutic nature in clinical settings. On the one hand, the aim of the applied experimental psychologist (the behavioural engineer) is to modify and optimise a treatment to obtain the highest average performance when all persons are treated alike. For instance, the behavioural engineer may be hunting for the single best strategy for teaching arithmetic. On the other hand, the aim of the applied correlational psychologist (the tester) is to increase average performance by treating persons differentially. The vocational psychologist, for instance, tries to match the right employee to the right job for maximum productivity. One of the founders of the psychological test movement, Alfred Binet, who published the first

intelligence test in 1905, was not at all interested in precise measurement across the whole range of abilities. Instead, he was interested in classification: his principal goal was to identify students who needed special help in coping with the school curriculum.

Engineers and testers have always competed forcefully with each other, because 'with every inch of success the engineer has, the tester must retreat a mile. … If tranquilisers make everybody happy, why bother to diagnose patients to determine which treatments they should have?'[9] The competition between the testers and the engineers owes much of its force to the fact that they incline to different stands in the Nature-Nurture controversy. As noted by Pastore, testers and classifiers tend to be 'conservatives', who believe that people vary by nature in their capacity for adaptation.[10] Social institutions, by demanding adaptation, serve as instruments of natural selection among individuals. The tester's task is to facilitate and anticipate this adaptation process through prediction. By contrast, engineers tend to be 'liberals', who believe that the struggle for survival is a struggle against the environment, not against competing individuals. Social institutions and traditions should not be taken for granted, but adapted to serve all men. The engineer's task is to identify the universal capacities of people and to nurture an optimal social environment that brings their potential to expression.

The existence of these two separate disciplines of psychology, as formulated fifty years ago, was never seriously contested at the time. The schism was seen as a fact. It pervaded not only all areas of the study of human behaviour, but also manifested itself in all departments of psychology, and in many professional organisations.[11] We may wonder, however, whether it still is such a centrifugal force, or whether there are signs of reconciliation.

New role emerging for individual differences in experimental settings

It is important to notice that Cronbach's distinction between the two disciplines of psychology merged two different dilemmas or oppositions into a single compound quandary. The first predicament is created by the methodological choice between an experimental approach and a correlational approach, while the second is the substantial choice between the search for uniform laws of behaviour and the search for structures of

individual differences. In the days of Cronbach, these pairs were indeed strongly associated. Nevertheless, once we logically disentangle these two oppositions, we realise that it is possible as well to pinpoint generally accepted uniform laws of effect by a non-experimental approach. In fact, correlating an environmental factor with an outcome measure has been the methodology with which the evidence that smoking causes lung cancer has accumulated to the point that it is no longer contested.[12] This evidence is primarily based on case-control studies and carefully designed cohort studies. Such quasi-experimental designs[13] today form the dominant methodology in areas such as health psychology and educational psychology. Likewise, on the individual differences side, there is no logical reason why experiments could not include a person factor that might account for part of the error variance, thus increasing the power of detecting an effect of the experimental treatment. Since the focus of this paper is on individual differences, we now elaborate on the latter option.

Without any doubt, the experimental method with its reliance on statistical hypothesis testing has gained enormous impact in many areas outside the traditional experimental discipline, such as social, clinical, and child psychology.[14] Even in organisational psychology, arguments in favour of experiments in the laboratory are now voiced regularly, for instance, to reach insights into what people judge to be just and fair, and how they react to perceived fairness and justice in organisations.[15] The following two examples illustrate the increasing role of individual differences in psychological experimentation.

Adoption of the experimental method in social psychology followed from the desire to test cause-and-effect hypotheses in the 'royal' way, and not from the wish to ignore person variables. As an example of how this type of research proceeds, let us consider a particular experiment reported in Greenwald and Farnham, in which they investigate the self-esteem-buffering hypothesis.[16] This hypothesis holds that self-esteem influences someone's cognitive reactions to success and failure, because high self-esteem creates cognitive protection against negative feedback. Persons with low self-esteem take negative feedback more to heart than do those with high self-esteem because they process their strengths and weaknesses differently after failure.[17] In the experiment, participants were asked to identify a set of target names selected from newspapers and news summaries on the web, among a larger set of background names that was created through recombination of the media names. The experimental

manipulation consisted of a success condition, which was an easy version of the task, and a failure condition, which was a difficult version of the task. First, participants' self-esteem was measured in a clever way, next they had to complete the name-identification task under either the success or the failure condition and obtained feedback on how well they did, and finally their aspiration level was determined by asking how well they thought they would do in a similar impending task. Statistical analysis demonstrated that persons with lower self-esteem indeed show a greater difference in aspiration level between success and failure conditions than persons with higher self-esteem. This experiment is a clear example of how the effect of an experimental treatment can be moderated by a person characteristic. It is not hard to find hundreds of similar studies in social psychology.

There is also a definite trend to pay more attention to individual differences in the core area of the experimental discipline, i.e., cognitive psychology. For many years, researchers in the memory domain used the distinction between short-term memory and long-term memory, where both were conceived as a memory store or filing system, and their difference was assumed to consist primarily in the durability of the records they held. In the 1960s, the theory on short-term memory developed into the more dynamic concept of a working memory that emphasises control processes and executive functions to manage information storage and retrieval. Working memory is important in normal functioning, but it also helps to understand impaired functioning, as in autism[18] and ADHD.[19] While in Cronbach's days short-term memory had a universal capacity of seven bits of information, plus or minus two,[20] the big news is that working memory has a capacity that is not the same for everybody but is strongly related to the g-factor, a major concept in correlational psychology.[21] Spearman discovered the g-factor in 1904 as the general level of a person's fluid intelligence, or 'mental energy'; it forms the major component of individual differences in a wide variety of intelligence tests. What we have here is a truly remarkable bridge between two worlds that existed in separation for a long time.

New statistical methodology to study aptitude-treatment interactions

When we include a person variable in an experimental study, the effect of the individual differences can be additive (responses of one type of person are on average higher than the responses of another type of person) or can show an interaction with the treatment (as was the case in the self-esteem example, discussed above), or both. One of the major take-home messages of Cronbach[22] was that psychologists should pay more attention to these aptitude-treatment interactions (also called moderator effects).[23] In his follow-up speech at the occasion of receiving the APA Distinguished Scientific Contribution Award, Cronbach noted that the crossbred science of aptitude-treatment interactions was flourishing, although unfortunately some complications had turned up.[24] One complication was that the interaction effect could be nonlinear, which makes it hard to detect with the standard way of modeling interaction as the product of the aptitude and the treatment variable. The other complication was that the effect of a third variable might mask the interaction effect, leading to a higher-order interaction. This would be the case, for instance, if the self-esteem-buffering hypothesis were to work in reverse for introverts compared to extraverts. Fortunately, there is now well-developed statistical machinery available that allows automatic detection of interactions, taking care of both complications.

This methodology has become known under the name CART, the acronym for Classification and Regression Trees.[25] A regression tree is the appropriate tool if the outcome (response) variable is numerical, while the classification tree is appropriate if the outcome variable is categorical. Tree-based methods predict the response variable by partitioning the individuals optimally into a hierarchy of groups based on the predictor variables (be it aptitude, treatment, or any other relevant variables). In this way they form a set of rules, a diagnostic key, and therefore the result is also called a decision tree. A classification or regression tree fits a simple model (like a constant) in each of the groups. The diagnostic key and the simple fitted model allow prediction of the response and assessment of interaction effects at the same time.

Dusseldorp and Meulman adapted the general idea of regression tree methods to the by then almost forgotten problem of detecting aptitude-treatment interactions, including nonlinear and higher-order effects,

without having to specify the kind of effects a priori.[26] To give the flavor of this kind of method, consider an application in which the differential effectiveness of cognitive psychotherapy and antidepressant treatment was investigated in a sample of panic disorder patients. We show some of the results as discussed by Dusseldorp and Meulman, referring the interested reader for more detail on the psychological content to Dusseldorp, Spinhoven, Bakker, van Dyck and van Balkom.[27]

The data analysed in the panic disorder study, consisted of a group of $N = 102$ patients with panic disorder, divided into a group that received treatment with antidepressants (AD: $n = 55$) and a group that received cognitive psychotherapy (CT: $n = 47$), both during twelve weeks. The outcome measure analysed was the Post-test Anxiety, recorded with the Hamilton Rating Scale for Anxiety. Variables acting in the analysis as possible determinants of Post-test Anxiety apart from the Treatment variable included Age, Gender, Duration of Panic Attack, Severity of Panic Attack, Pre-test Anxiety, Agoraphobia, Depression, and four scales for Locus of Control. The concept of Locus of Control (LC) refers to an individual's generalised expectations of who or what will play a role in controlling subsequent events. 'Internal LC' in this context is the extent to which one believes that factors within oneself are responsible for health or illness, labeled Internal HLC, short for Internal Health Locus of Control. The other three scales were 'Chance HLC', 'Medication HLC', and 'Therapist HLC', referring to the major role assigned either to chance, medication, or the therapist.

Dusseldorp and Meulman fitted several competing models on these data, from which the one called the 'Regression Trunk Approach' (RTA) turned out to give the best (future) prediction, as determined by cross-validation. RTA is a procedure that separates model search and model selection, and was the main topic of Dusseldorp.[28] The solution in Dusseldorp and Meulman explains 46.3% of the variance in the outcome variable Post-test Anxiety, while a standard stepwise regression analysis including the standard way to test for interactions explains only 38.1%. The RTA solution showed that only the predictor variables Treatment (cognitive therapy versus antidepressants), Duration of Panic Attack, Agoraphobia, and Medication HLC were statistically significant determinants of Post-test Anxiety as main effects. In addition, there was an interaction between Internal HLC and Treatment, which is shown in detail in Figure 1.

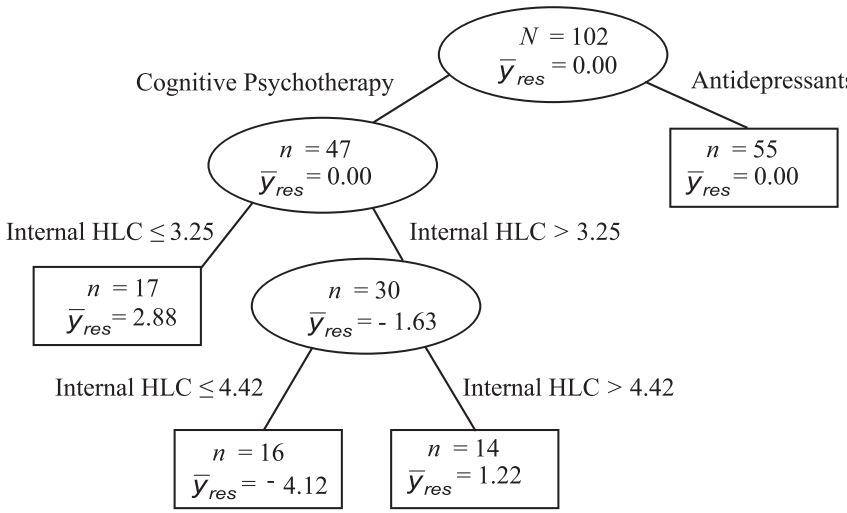

Figure 1. Cross-validated regression tree for the Panic Disorder study*

The rectangular boxes in Figure 1 represent the solution found for the four groups of patients involved in the interaction. These groups were obtained by repeatedly dividing the total sample by means of the three subsequent binary criteria that are indicated next to the splits in the tree. The ellipses represent intermediate groups that are formed during the process. The first split distinguishes the CT and AD group, so it represents the Treatment variable.

The value $\bar{y}_{res}$, reported in a box or an ellipse together with the group size, gives the mean value of each group according to the Post-test Anxiety variable, corrected for the main effects (technically, because there is a main effect of Treatment, the CT and the AD group both have an $\bar{y}_{res}$ of zero at the top of the figure). Prediction of Post-test Anxiety can not be improved by further splitting of the AD group. A second split within the CT group, however, forms a low Internal HLC group (with mean 2.88 on the outcome variable) and a higher Internal HLC group (with mean -1.63 on the response variable) with a threshold value of 3.25 on the Internal HLC scale separating the two groups. Finally, a third split is based on Internal HLC, but now for a threshold value of 4.42, so it splits the patients with the higher Internal HLC values into patients with an

intermediate value and those with the highest values, two groups that have very different residual Post-test Anxiety (-4.12 and 1.22, respectively).

Figure 2 displays the interactions found by the regression trunk approach in the conventional way, that is, by using a regression model of Post-test Anxiety on Internal HLC. Figure 2 gives a straight regression line for the patients treated with antidepressants (the group that was not further divided in the regression tree), which means that none of the predictor variables had a interaction (differential) effect in the antidepressant condition. However, the splits on the left of the regression tree translate into a nonlinear effect of Internal HLC for patients who were treated with cognitive therapy. When these patients acquire an extreme score on the person variable Internal HLC, their residual Post-test Anxiety is much higher than when they have a moderate expectation, in which case the mean of Post-test Anxiety is even lower than in the antidepressant group. This example shows that antidepressants work fine for everybody, but that the combination of cognitive psychotherapy and a moderately strong belief in self-control of panic disorder is at least as effective. More generally, it shows that the Regression Trunk Approach

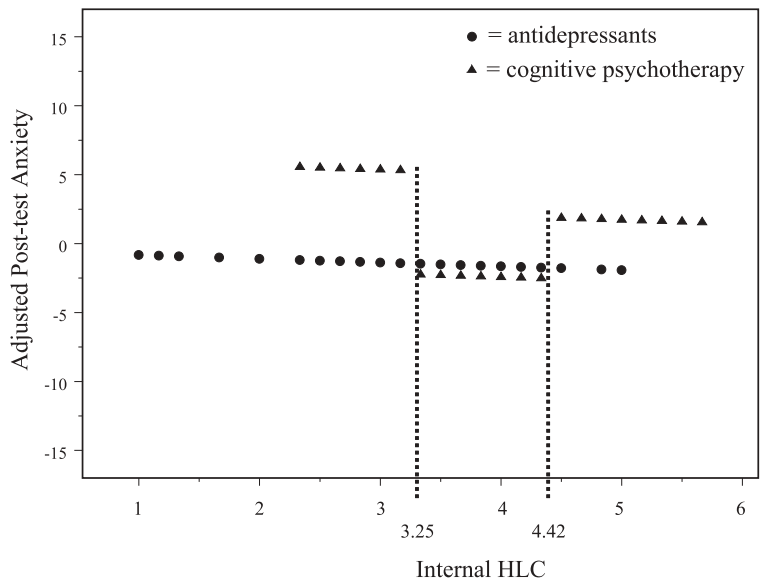

Figure 2. Regression of Post-test Anxiety on Internal Health Locus of Control for the Antidepressants condition and for the Cognitive Therapy condition*

can spot nonlinear effects of person characteristics on the outcome of treatment effectiveness studies, and all other experiments of a similar nature.

Conclusion

The dilemma of individual differences remains a major concern in psychology and in the other behavioural sciences. However, it is no longer such a centrifugal force as it was fifty years ago when differences in methodology added a lot of weight to the controversy. It is evident that experimental methodology has gained momentum in all parts of psychology, while the old correlational methodology has been under pressure. No doubt, the strong emergence in clinical and health psychology of forms of empirically supported psychotherapy[30] and evidence-based practice[31] has contributed greatly to this development. The fact that cognitive psychologists for their part finally recognise individual differences as a serious factor and start modeling them, is a further significant development. It seems fair to say that the two disciplines of psychology are merging into one, which creates new opportunities for both of them.[32]

Yet, even when we no longer regard individual differences as noise, it does take sophisticated quantitative methods to pick up the signal. Availability of advanced statistical methodology becomes even more critical now that the field is eager to use response measures that are of a physiological nature, and devices for functional magnetic resonance imaging (fMRI). Solving the problem of unresolved rivalry between treatments or theories calls for the use of quantitative methods that have more power to detect differences than the ones that are currently used in the mainstream psychological literature. Therefore, there is more reason than ever for behavioural scientists to pay full attention to new developments in psychometrics, statistics, and data theory.

Notes

The authors wish to thank Hans Knegtmans and Bernhard Hommel for inspiring conversations during the preparation of this article.

* Figures 1 and 2 are redrawn and revised versions of figures 7 and 8 respectively, in Dusseldorp and Meulman (2004), see n. 26.

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6
The struggle for the guiding principle in health care ethics and health law

Dick P. Engberts

Principles

In medical ethics and health law two (and sometimes three) principles struggle for supremacy. Both principles represent an established tradition. They are in contention for primacy. Not so much by aiming at confrontation as by the self-evident way in which they give expression to their own approach. Whereas one principle emphasizes respect for autonomy, the other focuses on beneficence. Accentuating these two very principles is not meant as a lack of appreciation for the work of Beauchamp and Childress, who, since the first edition of their book *Principles of biomedical ethics* in 1979, have distinguished four principles: respect for autonomy, non-maleficence, beneficence, justice. As the sphere of application of the principle of justice is mainly relevant in the field of issues of distribution, this principle can be disregarded in this contribution. The principles of non-maleficence and beneficence are related to each other and the one principle can be seen as the radicalisation of the other – and vice versa! Since in this contribution the principle of respect for autonomy will mainly be confronted by the perspective of the health care professional, beneficence has been chosen as the predominant corresponding moral precept. However, the principle of non-maleficence will not be completely ignored here.

Respect for autonomy

The principle of respect for autonomy is the principle of the vocal citizen. It has its roots in a classical image of man in which the subject has freedom and responsibility in making his own choices based on his own convictions and the criteria which he considers true. As early as Roman law we see the contours of this subject: the well-to-do (male) citizen who, as a contracting party, creates his place in society.

In this day and age, this image of man has manifested itself in a variety of forms: when the reformational dissenter claims that responsibility for his own religious beliefs while rejecting the exclusive intermediary role of priest and church. The characteristic personality structure matching this religious conviction and perception was perceptively depicted in 1904-1905 by Max Weber in his essays on *Die protestantische Ethik und der Geist des Kapitalismus*. In this context we do not see a single trace of complacency or triumphalism. On the contrary, the dialectic approach is the only way in which we can understand how the religious individual who has an insight into his own insignificance and imperfections knows he has been accepted as a servant in honour of something that is much greater than he himself, and that this is the very reason why he is not susceptible, or is at any rate less susceptible, to the intimidating exercise of power by an authority based on tradition or prosperity.

In Weber's vision the relationship between protestantism and capitalism is not monocausal. Mainly under the influence of reformed protestantism the establishment of a rational market-oriented entrepreneurship correlated with the development of an individual self-awareness which was characterised by responsibility, straightforwardness and austerity. In the first half of the seventeenth century, Grotius presents us with an intriguing glimpse of this image of man. In his dissertation on the validity of the law of nature he does mention God as the self-evident source and legitimacy of this law, but then, more or less in passing, he draws our attention to another criterion which he rejects straight away: in theory the validity of the law of nature is not dependent on the existence of God (… *etiamsi daremus, quod sine summo scelere dari nequit, non esse Deum* …, is how he formulates his vision in *De iure belli ac pacis*).

Although it is clear that Grotius relinquishes the indispensability of God as a hypothesis in principle, the materialisation of the autonomous and secularised cosmopolitan is primarily linked to the Age of Reason. More

or less independently, the postulate of the autonomous subject as a contracting party had managed to hold its ground in Roman law for centuries. The philosophies of secularised enlightenment and traditional contract law meet in the late nineteenth and the twentieth century in the concept of *informed consent*, a notion which is to become the concise summary for a kind of health care in which an adequately informed vocal citizen makes a well-considered decision whether or not to undergo the medical treatment he has been offered. This concept is primarily associated with the Anglo-Saxon legal and moral tradition, but it is plausible that a comparable development took place in nineteenth century Germany.

Beneficence

Beneficence is the principle in which the competitive point of view is expressed. Applied to health care it is the dominance of the perspective of the medical professional, who, based on a professional standard, decides which interventions can be deemed beneficial to the patient. The principle is visible as early as the formulation of the Hippocratic Oath (c. 400 BCE), that states:

'… I will prescribe regimen for the good of my patients according to my ability and my judgement and never do harm to anyone … In every house where I come, I will enter only for the good of my patients, keeping myself far from all intentional ill-doing and all seduction, …'.

For many centuries the principle of beneficence has been accepted as a matter of course by both the medical profession and the societies in which doctors who were educated in this spirit operated. The keen observer will see changes in the interpretation of the legitimacy of medical activities, and the role of information and consent in the process of decisions on treatment from the second half of the nineteenth century, but these changes have not led to an essentially different practice as yet.

Confrontation

Not until after the Second World War, especially from the sixties of the twentieth century onwards, do we see the image of man characterised by

(respect for) autonomy and the traditional moral principles of beneficence of the medical professionals enter each other's fields of vision. This created an area of tension which was there to stay, and which now has an enduring influence on the practice of medical ethics and health law, not being the least of stimuli to give the practice of health care a different status.

In the Netherlands the debate about this subject has become of current interest with the publication of a book by the physician J.H. van den Berg in 1969: *Medical power and medical ethics*. It was written from the perspective of the dichotomy between power and powerlessness, but can easily be understood within the terms of the theme of this contribution. Van den Berg argued that as long as medicine was not capable of achieving a great deal, the physician could focus on preserving life without any restrictions. Not until doctors had powerful medicines and interventions at their disposal, did it become clear that a new type of victim had appeared on the scene: the patient who could hold on to life as a result of medical intervention, but for whom a liveable life could no longer be realised. He reproaches modern medicine and health care for the lack of awareness of the damage they cause as a result of their dated professional ethics motivated by beneficence. Van den Berg's point of view is first and foremost that of the physician, but he does not lose sight of the patient's autonomy, which is still insufficiently respected. Thus his publication is a plea and a manifesto as well as a record of conflicting principles in health care.

Paternalism

The tension between the principles of respect for autonomy and beneficence fuels a continuous debate on the justification of paternalism: when are we allowed to ignore the actual will of the subject by invoking the subject's interest or benefit? The theme of paternalism also leaves an indelible mark on the issue of the incompetent patients (patients who are in need of medical care/treatment, but are not – not yet or not anymore – capable of making well-considered decisions). Is the patient's will – previously recorded orally or in writing; possibly reconstructed afterwards – or the medical professional's judgment routinely taken as the decisive factor in such situations?

The struggle for principles has a conspicuous place in legislation,

because within the continental-European tradition it is the legislator's responsibility to transform moral consensus into rules that bind all the citizens. It is true that the interpretation of those rules by the courts is also a normative process, but their contribution to jurisprudence is less prominent than in the Anglo-Saxon tradition.

The way in which this tension has been expressed in recent health care legislation is sketched below. The focus is on legislation concerning compulsory admissions and the treatment of psychiatric patients as well as on legislation concerning medical treatment contracts and biomedical research.

Medical Contract Act

Until well into the twentieth century, the legal component of the physician-patient relationship was negligible. That is to say: in the perception of those involved. The fact that a patient had to pay for a doctor's services did not mean that his work belonged to the realm of commercial services. Liability under civil law for medical activities was also rare. As far as legislation was involved in medical activities, it was mainly concerned with disciplinary law, in which the medical professionals themselves defined the meaning of good professional practice. For the rest there was incidental intervention from penal legislation. Initially, this mainly concerned cases of liability regarding medically-induced abortions: later it concerned other medical activities inducing the termination of life: euthanasia, physician-assisted suicide and activities resulting in the termination of life without the patient's request. The physician-patient relationship was such a matter of course that an explicit legal formulation was not needed. Not until notions like the ones formulated by Van den Berg sparked the insight that medical activities did not by definition contribute to the patient's well-being, but could also cause the patient to reach a permanently deplorable state, did a situation evolve in which the concept of patients' rights saw the light. The fact that medical activities could cause damage made it necessary to consider beforehand if the risk of an undesired outcome was acceptable under the given circumstances. This was the backdrop against which, after a long period of preparation, the *Medical Contract Act* entered into force in the Netherlands in 1995 as part of the contract law section of the Civil Code. As a matter of fact, this was unavoidable after the decision had been taken

that the physician-patient relationship would be legally defined as a contract within the meaning of the Civil Code.

The *Medical Contract Act* is an elaboration of the article of the Constitution which lays down the provisions for statutory protection of the integrity of the human body. Medical activities in all their manifestations almost always involve a violation of the body, and consequently need justification in order to be legitimate. The legal framework for this justification consists of the simultaneous presence of two legitimising factors: indication and consent. Indication means that the medical activities can be defended in accordance with the standards formulated by medical science. Consent is the procedure of *informed consent*, the process of information and consultation between physician and patient which results in the patient's acceptance of the physician's treatment proposal. Or, as the case may be, the patient's rejection, of course, but then the relationship has either ended or entered a new phase of consultation.

It is clear that the pattern behind this act is the principle of respect for autonomy. From this principle the patient derives the right to resist medical activities that he rejects. The physician's conviction that this is detrimental to the patient may lead to renewed consultation, but at the end of the day this cannot prevent the physician from having to refrain from medical activities if a patient refuses to give his consent. The right to refuse a treatment proposal does not imply that a patient is entitled to claim a particular treatment. After all, establishing indication for treatment on medical grounds is the professional's competence and not the patient's.

This balanced model is complicated when incompetent patients are concerned: (young) children, the mentally handicapped, unconscious patients, (some) psychiatric patients. Since the statutory protection of the integrity of their bodies is not an issue here, the violation of that integrity by medical activities requires legitimisation provided by indication and consent. As far as indication is concerned, the situation is not fundamentally different, but for the other requirement, consent, the situation is problematic. Since the patient himself cannot give *informed consent*, someone else will have to do that for him: the proxy. But by what is the proxy motivated when he operates in this capacity? By the patient's power of attorney granted when he was still competent? This would be the simplest solution and the easiest way of acting in accordance with the

principle of respect for autonomy. Then the proxy is the intermediary between the physician and the patient, provided the patient has previously made clear what his views are, or he is the authorised proxy if the patient has decided who has power of decision. These situations rarely occur, because it is generally impossible to adequately predict and define complex situations in which it is uncertain what treatment would be required. If the patient has previously appointed a particular person as his proxy, he may have given this person a powerful legitimacy, but it remains unclear to what extent this person is authorised to act on his behalf. The most important objection, however, is quantitative in nature: most incompetent patients have not committed any instructions to paper or have not appointed a proxy. Many of them would not have been able to do so, anyhow.

Respect for the patient's autonomy requires an effort to reconstruct what his wishes would have been if he had not been incompetent (substituted judgement). If that is not possible – which will often be the case – the physician and the proxy are obliged to weigh the patient's best interests. This approach is bound to lead to an emphasis on the principle of beneficence. The crucial question to be answered then is whose views are decisive if the physician and the proxy cannot agree on what is in the incompetent patient's best interests. Legislation on this point is balanced, but ultimately the physician's position is usually stronger than the proxy's, which in these circumstances makes the principle of autonomy subordinate to beneficence. The above mainly applies to adult incompetent patients and their proxies, but in principle the situation is not different for minor patients. There are two major differences. In the first place the minor patient's parents are the child's obvious proxies. Secondly, when children are over twelve years old the assessment of the child's competence will play a crucial role. If the child is considered competent, his judgement as well as that of his parents must play a part. If the child is not (yet) competent, the parents are the physician's contacts. By law the physician is ultimately responsible and decides on the child's competence.

In conclusion we can say that the *Medical Contract Act* emphasises the principle of respect for autonomy as long as we are dealing with competent patients. If we are dealing with incompetent patients, the balance shifts in favour of the principle of beneficence.

Medical Research involving Human Subjects Act: the Medical Research Act

This act was also long in the making before it entered into force in 1999. The cultural context within whose framework the need for this act originated was determined to a large extent by events that drew the attention of the international community. Medicine has a long tradition as regards experimentation on human beings. The attitude towards this history has often been ambivalent. On the one hand, without any doubt this approach made it possible to acquire important scientific knowledge which benefited patient care. On the other hand it cannot be denied that from time to time excesses took place in several countries which were extremely detrimental to patients and healthy test subjects and which sometimes caused irreparable damage. For the time being the absolute nadir is formed by the medical experiments carried out on prisoners by German and Japanese researchers in the Second World War. Twenty-three German scientists and other responsible persons, most of whom were physicians, had to account for their deeds at the Nuremberg tribunal in 1946-1947; seven were sentenced to death. What was most significant was that the tribunal published a number of criteria, which, provided they were adhered to, are characteristic for legitimate medical and scientific research involving human beings. These criteria are known as the Nuremberg Code.

It would take a considerable amount of time before the effects of the Nuremberg Code became tangible in legislation. In 1964 the World Medical Association drafted the Declaration of Helsinki, in which the Nuremberg norms were converted into a professional code for biomedical researchers. It would be fascinating to discuss the differences in accentuation between Nuremberg and Helsinki, but within the framework of this contribution it suffices to say that continuity prevailed. A startling article by the anaesthesiologist Beecher, *Ethics and Clinical Research* (1966), proved, however, that with the proclamation of the Declaration of Helsinki the problems were certainly not behind us. With examples from research results which had been published in regular scientific magazines, Beecher showed that (also) in the United States a research practice existed which sometimes caused serious and irreparable damage to patients. These patients were generally unaware of the fact they were participating in scientific research. Beecher emphasises that the

researcher's integrity is the best guarantee for the test subjects' well-being, but he also realises that the requirements related to *informed consent* will have to be adhered to and tightened for the protection of the test subjects.

In a completely different way the theme of medical and scientific research involving human test subjects became of topical interest in the sixties. It became clear that a disaster had taken place as a result of the use of a drug called thalidomide. Pregnant women in Europe were prescribed this drug on a grand scale to fight nausea. The result was that thousands of handicapped children were born whose most striking feature was abnormally short and malformed limbs. Closer examination showed that the safety and efficacy of the drug concerned had not been tested properly before it was prescribed for patients. The conclusion was that more research involving human test subjects was required when developing new drugs, while avoiding the abuses which Beecher had pointed out in the same period. Since the Declaration of Helsinki was the initiative of a non-governmental organisation, this state of affairs underlined the need for new legislation. As the theme of patients' rights had sparked the initiative for the development of new legislation anyhow, it was possible to incorporate the regulations concerning biomedical research involving humans smoothly.

However, the tension between the different principles led to a slightly different type of legislation. In the *Medical Contract Act* the decision-making process concerning medical treatment is left to the parties directly involved: physicians, patients. The legal framework specifies how decisive the principles of respect for autonomy and beneficence are in a variety of situations. In the event of a conflict between the parties involved, it is left to them to arrive at conclusions: end the physician-patient relationship or possibly, afterwards, commence civil proceedings to make good the damage suffered by the patient. However, when individuals play the role of research subjects the third principle, non-maleficence, also plays a dominant role. Because the third principle as it were forms the boundary that screens the framework within which both other principles may operate, it is in the public interest that these boundaries are not crossed. This accounts for the fact that the application of the *Medical Research Act* is not entirely left to the parties involved, but that the government supervises compliance with this act. Biomedical research involving human beings takes place in the public domain under public supervision.

The influence of the principle of respect for autonomy is apparent in the *Medical Research Act* in the strict requirements the procedure of *informed consent* must meet. The researcher's obligation to give complete and understandable information to the prospective research subject has been tightened. The act specifies the aspects on which the prospective research subject must be informed: aim, nature and duration of the research programme; risks, including the risks of premature termination; burden on the research subject; the possibility to withdraw from the programme after the test subject's prior permission without giving reasons and without having to compensate the researcher. Both the information given and the research subject's permission must be in writing. Furthermore, the written information must contain the name of a physician who is not involved in the research programme and to whom the research subject can apply for further details. If the research programme may potentially entail damage, an insurance policy, which compensates the damage in case of death or injury, must be taken out on behalf of the research subject, with the exclusion of inevitable damage that could be foreseen before the programme started. The research subject must also be informed about this beforehand. Finally, the clause that in principle forbids the participation of research subjects who are in fact or legally dependent on the researcher or the organisation carrying out research, such as inmates of prisons, subordinates and students, can be interpreted as protection of the potentially threatened autonomy of the research subject.

It is problematic to involve incompetent patients in scientific research. A logical argument is that these patients' proxies are obliged to take care of the patient's interests and that, consequently, these proxies cannot allow the incompetent patient they represent to participate in a research programme that is not in the patient's interest. For incompetent patients this could lead to the conclusion that the only scientific research involving incompetent patients that would be admissible would be the type of research programme that not only aims at knowledge acquisition, but is also for the therapeutic benefit of the test subjects involved. This rigid conclusion is not drawn so simply in the *Medical Research Act*. The underlying insight is that an improvement of possible methods of treatment for incompetent patients is sometimes dependent on the type of scientific research these patients are involved in. Therefore proxies of incompetent patients can sometimes allow their participation in a

research programme, albeit under strictly formulated conditions. The conditions which must be met are: the research programme can provide scientists with important knowledge which can only be acquired by these patients' participation, the risks for these patients are negligible, the extent to which they are burdened is restricted to a minimum and, if a patient resists, his participation in the programme is terminated. A similar exception provision exists for de facto and legally dependent persons: They can participate in research programmes from which they can possibly derive benefit themselves and in programmes that can only be carried out with persons belonging to the category they are in.

In the distinction made between therapeutic and non-therapeutic research, a distinction we also find in the Declaration of Helsinki, it becomes clear that, as far as the position of incompetent persons is concerned, there is some room for the principle of beneficence. Therapeutic research gives the proxies of incompetent patients an opportunity to give permission for participation as a research subject. However, it is often not simple to make a distinction between therapeutic and non-therapeutic research. The extent to which the test subject is likely to benefit is not just a matter of science, but also of semantics and psychology.

In the *Medical Research Act* the mutual relationship between respect for autonomy and beneficence is essentially different from the relationship we find in the *Medical Contract Act*. In the event the physician/researcher and the incompetent patient's proxies do not agree on what is in the patient's interest, the patient's proxies cannot be overruled by the physician/researcher who invokes the patient's interest. Here the principle of beneficence is at the service of respect for (indirect) autonomy.

The principle of maleficence is much more important than its mirror image of beneficence in the *Medical Research Act*. This is illustrated in the requirements the *informed consent* procedure has to meet, in the regulation applicable to non-therapeutic research involving incompetent patients, but also in a more comprehensive way. The *Medical Research Act* requires the research programme planned to be defined in a research protocol. This protocol must contain information on the scientific soundness, the scientific importance, the necessity to carry out research on human beings, the extent to which the programme is therapeutic, the way in

which the prospective test subjects will be recruited and informed, the way in which and the extent to which provision has been made for possible damage by taking out insurance, the researchers' qualifications and the suitability of the location in which research is carried out. The protocol, including any documentation on a drug or medical device to be researched, must then be submitted to an approved medical-ethical review commission, together with the written information for the test subjects, the informed consent form and the insurance papers. Violation of these and other provisions found in the *Medical Research Act* are liable to punishment. Finally, extra focus on the importance of the principle of maleficence can be found in the provision which refers the assessment of certain forms of research involving human beings, which are delicate issues from a moral point of view (for example stem cell research, gene therapy, xenotransplantation), to a central (national) review commission which is hierarchically superior to the local review commission (Central Committee on Research involving Human Subjects).

Psychiatric Hospitals Compulsory Admission Act

Medical practice, medical ethics and health law have always struggled with the question how to deal with psychiatric patients who are not willing or able to ask for medical treatment. An act based on the 'bestwil' principle (the 'for your own good' principle) entered into force in the Netherlands in 1884. This act enabled physicians who considered treatment to be in the patient's interest to admit the patient to a psychiatric hospital, if necessary by compulsory admission. This arrangement was subject to severe criticism in the sixties of the twentieth century. It was not in compliance with the emerging concept of patients' rights and it was an extremely explicit manifestation of what was called the medical establishment. It was the symbol of an authoritarian and merciless treatment of vulnerable and maladjusted people, who as a result of incarceration and compulsory treatment mainly suffered from iatrogenic damage. In many countries the view was heard that psychiatry was mainly a repressive instrument of power from which patients had to be protected. It must be admitted that this view was not wholly unfounded since there were examples of civil authorities which were sometimes inclined to remove troublesome and maladjusted people from society by sticking a psychiatric label on them.

Such views affected the level of legislation and ultimately led to the introduction of the *Psychiatric Hospitals Compulsory Admission Act* in 1994. This Act had an entirely different premise. The patient's presumed interest is no longer the central concept, for such a starting point would leave too much room for the unchecked exercise of power by physicians. The new law's main criterion is: danger. Danger to the patient himself, to certain other people and to the safety of persons and goods in general. Of course, the Act only applies to persons with a mental disorder, there has to be a causal relationship between disorder and danger and it must be impossible to avert the danger in any other way. Not until all these requirements have been met is compulsory admission to a psychiatric hospital an option. However, compulsory admission does not imply compulsory treatment. The basic assumption of the Act is that there is no more danger as soon as the patient has been admitted to a psychiatric hospital. Compulsory treatment after forcible admission may be an option, but only if there is danger to the patient or others which cannot be averted in any other way. Psychiatrists have often objected to this legal system. Their main objection was that because of this law patients who, as a result of their disorder, were not capable of accepting medical treatment, suffered unnecessarily, and sometimes even were denied the opportunity to recover. They considered the Act an obstacle which prevented them from successfully treating seriously ill patients. This criticism has subsided to a certain extent in the course of the years, because a broad interpretation of the danger criterion by the courts gradually increased the possibilities of compulsory treatment. The Act itself was also modified to a certain degree.

Recently, two new amendments have been passed, both of which express the existing principles. As a manifestation of the principle of beneficence, it became possible to admit patients for a short period of time if they tried to avoid any contact with medical professionals as a result of their disorder. During a few weeks, compulsory admission medical professionals can try to further diagnose the patient and convince him to accept help.

The legal recognition of the voluntary commitment contract (Ulysses contract) can be regarded as a manifestation of respect for autonomy. The name of this arrangement refers to the Odyssey (Book 12, 146-200) where Ulysses manages to escape from the Sirens who with their seductive songs lure ships to them which are then wrecked on the rocks.

He seals his crew's ears with wax, has himself tied to the mast and orders the crew to bind the ropes with which he is tied even tighter should he order them to untie him. A Ulysses contract means that a patient can be admitted against his will if certain previously described circumstances occur which the patient himself has defined as a good reason for being admitted to a psychiatric hospital, even if he were to resist at the moment of admission. This possibility may be especially important for patients with a bipolar disorder, who are alternately competent and incompetent. The idea behind this arrangement is that a compulsory admission respects the authentic will of the patient, because the wish to be admitted was formulated when his will was not influenced by the psychiatric disorder.

It must also be said here that the issues concerning compulsion and freedom in connection with psychiatric patients are so complex that preparations for a completely new legislation are now in progress.

Reflection

Each in their own way the regulations discussed attempt to find a balance between the respect for autonomy and beneficence or non-maleficence. Because the respect for autonomy presupposes an image of man which is to a large extent characterised by the rationality of the active subject, the principle finds its most obvious application within the context of competent adults who themselves determine how the values that direct the course of their lives result in a concrete situation when they make a certain decision. The validity of the principle extends as far as their mental competence. The possibility that the competent adult might act in a way that could be seen as opposed to what the physician and (most) other citizens would consider in his interest, is a consequence accepted wholeheartedly. This may lead to continued consultation, lack of understanding and disappointment, but the principle is beyond discussion. The grown-up Jehova's Witness, who refuses a life-saving blood transfusion, will not be compelled to undergo the life-saving treatment at the end of the day. The boundaries of the validity of the principle come into view where other people's interests are at stake. By definition autonomy is bound to 'autos', the 'self' of the active person. As soon as a decision to be taken is about somebody else, the active person's 'self' is not at stake, but the manifestations of that 'self' which are open to objectification, for instance the integrity of his body, his privacy or any

other interest. Then the guiding principle becomes beneficence or non-maleficence.

There is no doubt at all that these are the guiding principles when treatment decisions are taken. Another issue, however, is who is entrusted with the application of these principles: the physician or the incompetent patient's proxy. Sometimes there is disagreement on who represents the incompetent patient. A poignant example of this was the conflict round the death of coma patient Terri Schiavo in Florida in 2005, in which the patient's parents and her partner were each other's diametrical opponents.

The systematic structure of the *Medical Contract Act* would justify the allocation of the decisive vote to the patient's proxy, but the statutory regulations are not in agreement with this without any restrictions and – we assume – neither is the actual practice. The *Medical Contract Act* hesitates to accept the consequences of its own systematic structure and allocates ultimate responsibility for the treatment decision to the incompetent patient's physician. This is not only inconsistent, it is also wrong.

Admittedly, every health care professional has his own responsibility as regards the well-being and treatment of incompetent patients. This responsibility is based on the values-oriented nature of health care and medicine. That orientation is not the health care professional's private responsibility, but a characteristic of professional health care and consequently part of the public domain. Therefore a health care professional confronted with the standpoint of an incompetent patient's proxy should not be entitled to overrule this proxy. Instead, it should be reviewed in the public domain – for example in court – if the incompetent patient's proxy has been able to define his position in reasonableness. In reasonableness, because it is not second opinion that the medical professional cannot always resort to, but an objective review which takes the proxy's primary responsibility as its starting point. We do not see the above problem in the *Medical Research Act*. There the refusal of the incompetent patient's proxy means that participation in medical research is out of the question. Which is right, but at the same time we are confronted with another problem which is at least as acute: the protection of the incompetent patient from the proxy who does give his consent to participation in medical research. The system of medical-ethical review and its supervision seem a reasonable safeguard. Of course,

it is important to evaluate regularly what the practice is, and it is at least as important to be explicit about what can and may be asked within the framework of clinical research of incompetent patients.

A provisional conclusion may be that Dutch legislation has managed to achieve a balanced concept in the *Medical Contract Act* and the *Medical Research Act* as far as the validity of the principles of respect for autonomy and beneficence/maleficence are concerned. The relationship between the physician and the incompetent patient's proxy is unbalanced, though, since the representation of the incompetent patient's interests should be the proxy's domain, whereas the test of reasonableness concerning the quality of the representation should take place in the public domain. The question remains of how to judge the way in which the *Psychiatric Hospitals Compulsory Admission Act* deals with these principles. To begin with, it is very well possible that psychiatric health care takes place first and foremost within the framework of the *Medical Contract Act*. Most psychiatric patients are quite capable of being their physician's contact and insofar as this is not the case they, just like any other patient, have to rely on representation by proxy.

Another dimension becomes relevant as soon as the normal patterns of the physician/patient relationship no longer suffice, because a completely new theme presents itself: deprivation of liberty. In a state under the rule of law such a theme by definition belongs to the public domain. Two circumstances may necessitate this step: danger caused by the patient and the necessity to treat a patient who, as a result of the disorder to be treated, is no longer capable of giving his consent to the treatment. In the latter case we have to do with paternalism aimed at realising the principle of beneficence, but it is weak paternalism. It is not the explicitly worded will of a competent adult which is ignored, but a will which has been distorted by the disorder and which is not the expression of the free subject but of his powerlessness with respect to the disorder. Because paternalism is only admitted if it is weak paternalism, justice is done to the principle of respect for autonomy. The principle of beneficence overrules the distortion of the autonomy caused by the disorder. The extent to which criteria of danger should create the legitimacy required is a moot point. Also, statutory regulation ought to be possible without becoming embedded in the criterion of danger. The preference for either the one or the other approach will probably be determined by which danger is feared most: denying patients a chance of high-quality treatment

or depriving defenceless people of their freedom with the help of the authorities because physicians are of the opinion that they need treatment.

Two more problems

There is one area in which the tension between the principles of respect for autonomy and beneficence stands out with unprecedented sharpness: the position of the unborn child. In this context we are not concerned with the fact that in most countries the woman has the chance to break off pregnancy provided certain conditions are met. No, what we are concerned with here is the pregnant woman who damages the unborn child by her lifestyle (drugs, alcohol, smoking). Respect for the woman's autonomy prevents others from intervening in the unborn child's interest without her permission. Orientation on the beneficence principle, on the other hand, would argue for intervention for the sake of the unborn child. In exceptional cases we may conclude that the woman is suffering from a serious mental disorder (addiction) as a legitimate reason for intervention for her own benefit. More often than not, however, the only recourse for those who would want to dedicate themselves to the well-being of the unborn child is persuasion. Would it not be advisable if, in appropriate cases, a statutory regulation disregarded the respect for the woman's autonomy? This is not desirable in such simplistic term. In such conflict situations the difference between ethics and the law manifests itself: morally speaking, the pregnant woman is obliged to devote herself to the well-being of her unborn child, but legally speaking, she cannot be compelled to do so. There is a difference between what is morally required and what can be enforced by law. This is not a coincidence, but a logical consequence of the difference.

Quite a different problem, which has hardly been elaborated on in the foregoing, is what exactly is meant by beneficence with regard to incompetent patients. The only people who will not consider this a problem are those who adhere to the fundamentalist view that keeping patients alive is always and in any circumstances the option to be preferred. Those who accept the more balanced view that life is not always and in any circumstances preferable to death, however, are confronted with a complex problem. A statutory regulation can quite easily establish who represents an incompetent patient. It is also possible in such cases to decide almost unanimously that incompetent patient's

interests should be the guiding principle. This provides us with an important starting point, but also with a lot of uncertainty, because it is often not immediately clear what is in the patient's best interest.

Conclusion

The 'solution' to the problem of conflicting principles in Dutch legislation is partly procedural (here the autonomy-oriented approach can be discerned), partly (lastly) the domain of the medical professional and partly 'exported' to the domain of public order with some room for the perspective of the medical professional (compulsory admission of psychiatric patients on the grounds of danger). In other countries with a comparable liberal/democratic form of government similar problems play a part. It is true that there are differences between the regulations in force in different countries, but the conflict between the principles is visible everywhere. The development of the principles was related to an image of man just as the struggle between the principles is related to an image of man and preferences in the field of social order. Only if we expose the conflict do we have an opportunity to formulate a comprehensive point of view in which the antithesis is removed, or at least sensibly integrated in a comprehensive concept. A concept of man and society in which, while preserving individual freedom, responsibility is accepted for the sake of the well-being of people who are bound by conditions determined by lack of freedom.

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7
Law *v.* Psychiatry
The case of Bert Boxwell

Willem A. Wagenaar

In the morning of 17 April 2005 Bert Boxwell, a 35 year-old cab driver, left his job unexpectedly. He drove his cab to the company's garage, reported himself ill, and went home. He grabbed a stiletto and went to the town's busy main street. It was a quarter past eleven. Bert saw his father of 71 walking on the other side of the street. He crossed the street, assaulted his father with the stiletto, and killed him with 38 stabs. Bystanders heard him say: 'He threatened me and my children, it was him or us'. No-one stopped him, but someone called the emergency number. Bert did not try to escape and was arrested. A few hours later, when the police interrogated him, Bert said that he was completely amnesic for what had happened. When Bert was tried for murder about one year later, the amnesia had almost disappeared.

At the trial the defence counsel argued that Bert could not be held responsible for his action, because at the time of the assault he suffered from a psychiatric disturbance which made it impossible for him to control his behaviour. Two experts reported on Bert's psychological condition during the assault. The psychiatrist Dr. King concluded that Bert had suffered from a severe dissociative disturbance, possibly a dissociative fugue. The other expert, the clinical psychologist Prof. Bentham, suggested that Bert had suffered from a *disconnection*, not a *dissociation*, but without a reference to any literature about this novel diagnosis, and without explaining the difference between the two notions. The DSM IV classifications of Bert's disorder were, according to Prof.

Bentham: 309.4, Mixed Disturbance of Emotions and Conduct and 301.9, Disturbance Not Otherwise Specified. However, neither of these classifications mentions dissociation, disconnection, or amnesia as characteristic symbols. Surprisingly the experts, who disagreed about the diagnosis, produced as their joint opinion that Bert was not responsible for this action.

Bert's own account of the event, as far as he remembered it, was that he did not feel well, and therefore went home. On his way home, he passed through the main shopping street, where unexpectedly he bumped into his father, and everything became red before his eyes. From that point he still cannot remember exactly what happened, although he admitted that he had killed his father. He explained that his father was an ex-convict, a dangerous arms dealer who always carried a gun, and who had committed many violent crimes. Bert and his children had repeatedly been threatened by him; he had said that he would kill the children. Bert's own opinion was that this might explain his sudden loss of control. Dr. King and Prof. Bentham referred to Bert's account of the facts as one of the foundations of their judgment. Prof. Bentham wrote the following:

'Of course a psychologist cannot be the judge of the extent to which the father was really a violent or threatening person, nor whether he really carried a gun, dealt in fire arms, or had been convicted. But if there is any truth in the elements brought forward by the accused, it is easy to understand that not only he and his family felt threatened by the father, but also that they felt unprotected by the police'.

The crucial term is of course the little word 'if'. Should an expert not verify whether there is any truth in the statements of a defendant, and should the diagnosis not be based on such statements if and only if they are corroborated by the facts?

The police investigation revealed that the father did not carry a gun, was not known to the police as an arms dealer, and had never been convicted for any violent crime. Moreover, no evidence emerged of any threats by the father against his son or grandchildren. Also Bert's account of his behaviour on that day was incorrect. He did not pass through the shopping street on his way home; the way from the garage to his home does not even come close to the shopping street. He went home first, got the stiletto, and decided to go to the shopping street. He knew that his

father had the habit of leaving the house around eleven o'clock, in order to buy a newspaper and a cinnamon bun. Witnesses testified that Bert had been waiting in front of the bakery store. When his father appeared he hid himself behind a car, and followed his father for 20-30 metres. Then he crossed the street and killed him. He never 'bumped into him unexpectedly'.

These details are not unimportant for the diagnosis of what befell Bert. His actual behaviour demonstrates planning and even criminal intent. He made a detour to pick up the knife, he went to the place where he knew his father would be, he waited a while, and then assaulted him, quite deliberately. He clearly realised what he had done, and justified it by the threats against him and his children. How does this match with the dissociative fugue that made Bert lose control of his behaviour? Do we know what dissociation is, how it is diagnosed, how it works, how long it lasts, and whether Bert's seemingly purposeful behaviour is in accordance with the hypothesis that he did not control it?

DSM IV and the Dissociation Disorders

The presence of the Dissociative Identity Disorder (DID) in DSM IV, the Diagnostic Statistical Manual that contains all possible psychiatric diagnoses and their symptoms, has a long history. In DSM III, DID was still called Multiple Personality Disorder (MPD). The notion of more than one personality residing in the same body came into disrepute when it appeared that the diagnosis had become prominent only in a few other countries than the US, and that the diagnosis depended to a large extent on the particular orientation of the therapist. The situation became even more questionable when patients began to claim the unlikely number of hundreds or even over a thousand different personalities. Yet, many psychiatrists believed in the reality of MPD and did not want it removed from DSM. Finally the compromise was reached that in DSM IV the name of MPD was replaced by DID, although diagnostic symptoms remained the same. In DSM IV, DID is one of four dissociative disorders. Bert Boxwell was diagnosed by the expert witnesses as suffering from one of these dissociative disorders. A brief overview of the possibilities may help to comprehend how problematic this diagnosis is.

Dissociative amnesia (300.12). DSM IV says that these patients experience

impaired recall of important personal information, usually involving emotional trauma. Since Bert's amnesia started only after the assault, he cannot have suffered from this disorder when he killed his father. Moreover, patients of this type are not known for being aggressive.

Depersonalization disorder (300.6). Patients in this category feel detached from themselves. They may also feel a lack of control, but this is only a feeling; they do not really lack control. Bert's description of his symptoms is different.

Dissociative Fugue (300.13). These patients suddenly leave their home towns or countries without knowing where they came from or who they are. A well-known recent example is the 'Piano man' discovered on the beach of Sheerness in Kent; who, however, turned out to be an impostor. Although Dr. King diagnosed this affliction in Bert's case, he showed none of these symptoms.

Dissociative Identity Disorder (300.14). Patients of this type suffer from alternation of two or more distinct personality states with impaired recall among the different personalities, i.e. they are said not to know of each others existence and actions. Before being diagnosed with this disorder, these patients usually have had a long therapeutic history. Bert has never undergone or sought any psychotherapy, and has never mentioned the presence of different personality states.

Dissociative Disorder Not Otherwise Specified (300.15). A catch-all 'dustbin' diagnosis without any diagnostic symptoms.

The value of a diagnosis is usually characterised by the statistical notions *validity* and *reliability*. Validity means that the diagnosis is correct; that the psychiatric diagnosis and the disorder from which the patient actually suffers are in agreement. The problem is, of course, that there is no golden standard for the 'true disorder', so that we cannot know the validity of psychiatric diagnoses. Reliability means that there is agreement between two independent diagnoses, for instance by two independent psychiatrists. It is obvious that if two psychiatrists disagree, they cannot both be right. Therefore a lack of reliability implies a lack of validity. Theoretically the coefficient of reliability constitutes the upper border of the validity. Validity can be considerably lower than reliability because, even when two experts agree, they might both be wrong. Reliability is easier to measure than validity. Typically, the reliability of psychiatric diagnoses never exceeds 70%, which means that the validity is probably lower.

An estimated validity of less than 70% is further inflated when psychiatrists do not stick to standard methods and definitions. The diagnostic criteria of DSM IV are notoriously vague. For instance, the definition of dissociative amnesia as 'one or more episodes of inability to recall important personal information, usually of a traumatic or stressful nature, that is too extensive to be explained by ordinary forgetfulness' gives considerable room to interpretation. What is important information, what is stressful, what is ordinary forgetfulness? And most important what is inability? How do we know that a patient is really unable to recall complete episodes? Should the diagnosis not involve a test for malingering? The validity is even more compromised when psychiatrists introduce their own definitions, such as substituting *dissociation* with *disconnection*, or *borderline tendencies* instead of the *Borderline Personality Disorder* in DSM IV (301.83).

The validity of psychiatric diagnoses was hardly ever studied systematically, and there are not even rough indications of the validity with which the separate disorders can be diagnosed. But given their low reliability, it cannot be high. One reason why such important evaluations are lacking might be that a psychiatric diagnosis is a clinical art rather than a strict scientific method. That could be acceptable even within a scientific community, if the psychiatrists calibrate themselves, by which is meant that they keep track of their performance, either through peer evaluation, or by evaluating their diagnoses at the end of therapy. Such a practice is virtually non-existent, however, and it is unnecessary to say that Dr. King and Prof. Bentham did not report how good their diagnoses have been in the past. The probably low validity of their opinions is demonstrated by the fact that they disagreed; they cannot both have been right, although the court accepted both expert testimonies.

Alternative diagnosis: delusion

One basic rule for making diagnoses, which is essentially a probabilistic process of revision of opinion on the basis of incoming information, is that alternative diagnoses are tested against one another, the result of which is called a 'differential diagnosis'. In his posthumous 1763 essay, the reverend Thomas Bayes already described in mathematical terms how this is to be done. Diagnoses have a likelihood only in relation to other diagnoses, and it is essential that the option favoured in the end be

rigorously tested against the most probable alternatives. Which alternatives were considered when Dr. King and Prof. Bentham diagnosed a dissociative disorder? And why were these alternatives rejected?

The most important alternative is, of course, that Bert Boxwell was not suffering from any psychiatric disorder at all. His actions appear to have been well-planned and controlled. As long as we are not told about the possible rational motives that he might have had for killing his father, we cannot decide whether or not he committed first-degree murder. Another alternative that springs to mind is that he suffered from a delusion. What if Bert seriously believed in the delusion that his father would kill him and his children? That is what Bert told the bystanders immediately after the event. Did the experts consider any evidence about this delusion? Had Bert mentioned this to anyone before? Did he tell the psychiatrist and the psychologist afterwards about this fear, or did it also get lost because of his amnesia?

DSM IV lists at least three different types of delusional disturbances: *Delusional Disorder (297.1)*, *Paranoid Schizophrenia (295.30)*, and *Paranoid Personality Disorder (301.0)*. The first of these is defined by five criteria: a non-bizarre delusion, no symptoms like hallucinations and other related phenomena, no other markedly impaired behaviour, no extended mood disturbances, no influences of substances. Although we cannot diagnose Bert's problem from a distance, we can establish that Bert's behaviour does fit these criteria. The case file contains nothing that contradicts such a diagnosis, and the experts present no arguments as to why it should be rejected.

A critical issue is the fact that Bert stabbed his father 38 times. This points to a highly emotional state, a type of blind rage very unlike a planned or even calculated murder. Is such an emotional discharge more compatible with a dissociative disturbance than with a delusion? DSM IV provides almost no guidance here. Among the diagnostic criteria of dissociative disorders, aggressive behaviour or any other form of emotional discharge is not mentioned. The dissociative fugue, diagnosed by Dr. King, rather suggests passiveness or sullenness. And one of the possible symptoms of the Paranoid Personality Disorder is: 'perceives attacks on his or her character or reputation that are not apparent to others and is quick to react angrily or to counterattack'. Why then, did the experts not consider a delusional disturbance? One reason might be that they let themselves be guided by Bert's claim of amnesia. Delusions

do not come with amnesia, while dissociative disturbances do. But was Bert's amnesia real?

There are at least two tests for malingered amnesia that have some validity: Structured Inventory of Malingered Symptomatology (SIMS), and Symptom Validity Testing (SVT). In SIMS the defendant is interviewed about 75 symptoms of memory disturbance. Most of these are very rare and therefore unlikely. If a defendant claims to experience a large number of them, he is probably malingering. In SVT the defendant receives a number of two-alternative forced-choice questions about the event that he claims not to remember. Pretending that he does not know the answers, a malingerer is likely to produce more than 50% errors, while a true amnesiac would score close to chance level. Bert stated that his amnesia had almost disappeared when the experts saw him for the first time. As a consequence, SVT could not be administered because Bert knew too much about the details of the crime. SIMS was applied, but produced a negative result, which is not surprising, because Bert did not claim amnesia at that time. But, if it is not possible to test the reality of Bert's amnesia claims, can we still accept the diagnosis of a dissociative disturbance?

Prof. Bentham tested Bert on a number of personality scales, the most important of which was the Dissociative Experiences Scale (DES). The result on this test was negative: Bert had never experienced any dissociative symptoms. Does this not imply that other diagnoses should be seriously considered?

The problem seems to be that in psychiatry it is not customary to test for alternative hypotheses. In a therapeutic session, if a dissociative disorder is more probable than a delusion, Bert would have been treated accordingly. The fact that in a criminal context a greater certainty is required than is customary in a therapeutic setting, meaning the criterion of 'beyond reasonable doubt', may go unnoticed by the psychiatric expert.

Going back in time

The present case is even further complicated by the fact that both experts were not asked to diagnose Bert Boxwell's present disorder, but the disorder from which he suffered prior to the assault on his father. The symptoms he displayed at the time of the assessment may also have been

caused by the traumatic experience of killing his father, of being in prison, and of facing a possibly very long sentence. Must not effects of these experiences somehow be subtracted, so that only symptoms caused by a pre-existing disorder remain? However, there is no known technique for doing this, and the two experts did not even attempt it. Instead they diagnosed Bert's disorder on the basis of his behaviour and his own explanations of this behaviour. Doing this, they made two serious mistakes. The first is that they accepted Bert's declared amnesia as a symptom of dissociation, thus ignoring the fact that the amnesia only started after the assault, and that it is rather easy to fake amnesia. The second mistake was that the psychiatrists ignored everything that became known about Bert's actual behaviour in the last hour before the murder. For reasons unknown, they simply assumed that Bert had not planned the encounter with his father, and that it caused a sudden and uncontrollable red-out. In its verdict, the court rejected this vision.

In summary, the conclusion that a diagnosis of a sudden dissociative disorder many months in the past, made on the basis of a suspect's own but refuted story, and using a technique that was never standardised or calibrated, cannot have a very high validity. Should the experts not have pointed this out to the court? Or even better: should they not have refused their commission, on the grounds that science has no reliable methods for answering the court's questions?

Therapy *v.* expert opinion

Do my objections against the lack of validity in psychiatric diagnoses constitute an attack on psychiatry as such? I do not think so. Practising psychiatrists face a difficult choice: treating patients on the basis of the best possible diagnoses, even if they are not very good, or not treating them at all. Under such circumstances diagnoses with a low or unknown validity may be considered acceptable, if only because there is always the option of adjusting the diagnosis during the therapy. For similar reasons, it may be acceptable to use the patient's statements about himself and about his past behaviour as a starting point, keeping in mind that all those statements need not be true.

In a court of law, the position of a psychiatric expert is entirely different. Bert Boxwell is not a patient but a suspect accused of murder. The diagnosis is not made in order to initiate therapy, but to assist the

trier of fact in its dual task: to establish the truth, and to decide on an appropriate sentence. The assumption that any help is better than no help, does not hold here. The expert's opinion may influence the court's decisions to such an extent that errors would constitute a significant interference with justice. Experts must express their opinions only if there is a sound scientific basis for them, and if the likelihood that they are wrong is extremely low. But who controls this? In my country, the Netherlands, there is no established legal criterion for the acceptance of expert testimony. In everyday practice a court has no means to verify the validity of statements made by a psychiatrist. Hence the control is placed in the hands of the psychiatrists themselves. They are the judges of whether their diagnoses are safe enough, not as a basis for therapy, but as a basis for a criminal verdict. I fear that Dr. King and Prof. Bentham were unaware of this grave responsibility.

The lack of explicit criteria is caused by the fact that the Netherlands has an *inquisitorial* system of criminal law, as have many other countries in continental Europe. The courts are supposed to investigate each case, guided by reason. According to the rationalist's insight, reason will automatically lead to the truth. In inquisitorial systems rules of evidence are usually scarce, and judges are assumed to weigh directly the value and validity of evidence, using their inbred sense of logic.

In *adversarial* systems of criminal law the assumption is quite different. Here the belief is that the truth will emerge from a clash of opinions. For the system to work, these opinions must not be misleading. This requires an elaborate system of rules of evidence, including criteria for the admissibility and presentation of expert opinions. Adversarial systems are more often found in the Anglo-Saxon world. In the United States expert opinion is subjected to one of two criteria: the Frye Criterion, or the Daubert Criterion. The first of these requires that opinions expressed must be 'generally accepted' in the expert's scientific community. The second criterion requires that the expert's opinion be based on knowledge acquired by way of methods generally accepted within the scientific community. It is obvious that the opinions of Dr. King and Prof. Bentham do not satisfy either of the two criteria. There is no 'general acceptance' among psychiatrists and psychologists about the reality of dissociative disorders. There is no 'general acceptance' about the validity of such diagnoses. There is no 'general acceptance' of the possibility of diagnosing a temporary disorder that occurred many months ago. There is no 'general

acceptance' of a method for testing the possibility that suspects may fake amnesia, or how that should be tested. There is no 'general acceptance' of the implicit assumption that dissociation may lead to murder. It is a court's duty to establish criminal responsibility, before any form of punishment can be imposed. Judges are required to determine criminal responsibility according to legal criteria. Why should these be replaced by arbitrary psychiatric criteria?

Clark *v.* Arizona

On June 29 2006, an important decision was taken by the Federal Supreme Court of the United States in the case against Eric Michael Clark. Clark had killed a uniformed police officer, which is a specific crime in Arizona, for which a death penalty can be imposed. Clark admitted that he had shot the officer, resulting in the victim's death. But he argued that he had acted under the influence of a delusion: he thought that he was threatened by aliens, and that his victim was an alien dressed as a police officer. He had had no intention to kill a police officer. So, in absence of *mens rea*, he could not be convicted of first degree murder of a police officer.

This is in fact an insanity defence. Traditionally, insanity is assessed on the basis of the M'Naghten Rule, which specifies that a criminal conviction is precluded if the defendant suffered from a mental disease or defect that made it impossible for him to know that what he was doing was wrong. The M'Naghten case was decided in 1843 in Great Britain, but the rule is still invoked by many Anglo-Saxon courts. In Clark *v.* Arizona it was claimed that the rule should be applied on two levels. On the first level it is argued that Clark did not know that shooting a police officer was wrong. On the second level it was argued that his insanity prevented him from realising that the victim was a police officer. Psychiatric testimony was presented to support the argument on both levels. The State of Arizona in its turn argued that psychiatric expertise on the issue of knowing right from wrong can be accepted. But expertise about the intentions of defendants was unacceptable, because psychiatric knowledge on this issue does not pass the Frye test or the Daubert test. In the end, the matter was submitted to the Federal Supreme Court, which confirmed the State of Arizona's right to exclude psychiatric testimony about the intentions of defendants. The Court quoted a previous ruling, in

which it was said: 'No matter how the test for insanity is phrased, a psychiatrist or psychologist is no more qualified than any other person to give an opinion about whether a particular defendant's mental condition satisfies the legal test for insanity'. The Court concluded: 'Even when an expert is confident that his understanding of the mind is reliable, judgment addressing the basic categories of capacity requires a leap from the concepts of psychology, which are devised for thinking about treatment, to the concepts of legal sanity, which are devised for thinking about criminal responsibility'.

Although legislation and jurisprudence are limited to national boundaries, there is no reason for other jurisdictions, such as our own, to reject this well-considered rationale. If psychiatrists and psychologists cannot reliably make the leap in the United States, how could they do so in other countries, employing identical scientific principles? The law may be restricted by geography, but science is not.

Recidivism

Dr. King and Prof. Bentham were also asked about the likelihood that Bert Boxwell would repeat his criminal behaviour. Prof. Bentham said:

> 'The defendant did never commit a similar aggressive crime. This is a unique situation, especially directed against the father. But defendant is a person who accumulates tensions and aggression, which implies that there is always a risk of maladjustment and an aggressive discharge'.

Dr. King's opinion was:

> 'The likelihood of a repetition is minimal, and fully dependant on the reoccurrence of such extreme conditions. That is highly unlikely, because the charged murder was related to the extreme emotional nature of the relationship with his father. But it must be said that the defendant has a tendency to repression of aggression, which was the cause of his present behaviour. Now he experiences a post traumatic stress disorder, which can again cause him to avoid or repress emotions. In extreme conditions he may then repeat his aggression'.

Both opinions are somewhat ambiguous. First it is stressed that the likelihood of the repetition of such a crime is minimal. For this the experts could have referred to the statistics of recidivism among persons convicted for patricide, which show that less than one per cent is ever again involved in violent crimes. But then they became more circumspect: obviously Bert will not kill his father again, but his aggression might very well be directed against other people in a similar manner. This logic was not supported by any consideration of their diagnosis. Should their prognosis of future behaviour not reflect that the diagnosis of Bert's affliction was not clear? Or alternatively, reflect their conviction that his behaviour was caused by a sudden dissociation? Should they not have considered the usual course of such a disturbance, possibly with and without therapy? Should they not explain, on the basis of their expertise in psychotherapy, why Bert's case is exceptional and why the overall one per cent recidivism for patricide does not apply to him? And also why his dissociation (or delusion) cannot be treated, such as to reduce the likelihood of recidivism?

The more general question is whether psychiatrists can predict recidivism any better than what is already known from the base rate reflected in rather rough statistics. In the case of Bert Boxwell, this question became urgent, because he was not only sentenced to a long time in prison, but also to a forced treatment in a mental institution, to be ended only when the psychiatrists were convinced that he was cured. The reason for this virtual life sentence was the two experts' prediction that he would become aggressive again. The assumption apparently was that psychiatrists and psychologists possess a scientifically validated method for predicting violent behaviour, not only in the short term, but even after a sentence of ten years or more. No extensive search of literature is needed to conclude that such a method does not exist. Perhaps the two experts chose to be on the safe side with their prediction. Their opinions seem to be inspired by a fear of being held responsible if Bert, after his release from prison, were to murder again. But this is hardly an acceptable motive for misleading the court about the validity of such expert opinions, and even less for a court to decide about the freedom of a person after he served his sentence.

Two sciences clash

Questions about criminal responsibility and the likelihood of recidivism are typically legal questions, and are highly relevant in the context of an individual case. All parties involved, prosecution, defence, and the court, must address such questions in a professional manner. Experts who may be able to help answering these questions on the basis of their scientific knowledge, may be asked for their help. But the relevance of a question does not imply that it can be answered. The needs of criminal justice cannot always be met by empirical science.

Anyone familiar with the literature about the validity of psychiatric diagnoses will come to the conclusion that a psychiatric diagnosis does not constitute a sound base for an expert opinion on either criminal responsibility or recidivism; with the possible exception of a few extreme cases. This conclusion is supported by the cautionary statement included in DSM IV: 'The clinical and scientific considerations involved in categorization of these mental disorders may not be wholly relevant to legal judgments, for example, that take into account such issues as individual responsibility, disability determination, and competency'.

Psychiatry and the law clash over this issue; the legal community wants more than psychiatry can deliver, and psychiatrists accepting the role often assigned to them by the law may well exceed the limits of their science. If relevant questions go beyond the undisputed knowledge of a science, it should be left to the courts to answer them as best as they can. This may be a problem, but it is not the scientific expert's problem. Experts must not take on a responsibility which they cannot deliver, irrespective of whether they are solicited by the prosecution or by the defence. Both sides may hope that science will support their case, and an adroit selection of experts or expert reports will fulfil such expectations. But even if one believes that truth will emerge from a clash of opinions, it would be a miracle if truth would emerge from a clash of opinions that lack any scientific base, or even disregard the knowledge that is available. This occurs when scientists express opinions that are based on speculation or worse, intentional neglect of available knowledge.

The two experts serving in the case of Bert Boxwell can be accused of ignoring a vast body of literature on the validity of diagnoses, even more so when such diagnoses are not obtained by means of strict and

standardised methods, or when the classifications used spring more from fantasy than from empirical research. Since experts should always be aware of the validity of their conclusions, it is the experts' task to inform the courts about this, or if necessary, to refuse to act as experts.

The danger can be reduced through the adoption of legally defined limits for expert testimony. When scientists go beyond these limits, as was the case in Clark *v.* Arizona, they should be corrected and their testimony rejected. The courts have the primary obligation to control this process. But control will be difficult if such limits do not exist, as is the case in the Netherlands. Then the courts can hide behind the argument that the testimony is not illegal, no matter how absurd it is. But legality is not the same as validity. The two notions come from different scientific domains, and the clash is obvious. Legality may be required, but cannot replace validity. In fact, it is highly undesirable that invalid testimony can still be legal.

A cynical example is the court's acceptance of the testimony about Bert Boxwell, both with respect to criminal responsibility and recidivism. Although the members of the court are no experts in psychiatry, they should have understood that the expertise about both issues was based upon the assumption that Bert's account of the events was correct. This account was explicitly rejected by the court. Dr. King's and Prof. Bentham's assessment of the facts went clearly far beyond their expertise, and should have been corrected. How, then, could the court reject Bert's version of the story, and simultaneously accept the expert testimony based on it?

Courts are responsible for who is accepted as an expert, and which questions are asked. If there are no rules or criteria for such decisions, the necessity for the courts to become acquainted with the field of expertise is even greater. Inquisitorial courts cannot hide behind the scarcity of rules of evidence, but must acquire at least some expertise before they accept expert testimony. The issues of criminal responsibility and recidivism are of such an overwhelming significance in criminal law, that professional judges can be expected to have received a thorough training in this, in order to contain the usurpation of the law by psychiatrists. In the case of Bert Boxwell the needs of the court were wholly incompatible with the available knowledge, and the naivety of the experts took them far beyond the limits of a fair trial.

The case of Bert Boxwell is by no means unique. On the contrary, psychiatrists all over the world testify about responsibility and recidivism, thousands of times each year. Many feel that justice cannot be done without the contribution of psychiatry. The dilemma is that justice is also not served when psychiatric expertise about these issues is accepted.

8
'Hard cases' in the law

Janneke Gerards

In 2001, Mrs. D became pregnant with twins. Following some tests in a hospital in Ireland, it transpired that one of the twins had stopped developing at eight weeks old and that the second foetus had a severe chromosomal abnormality. The hospital doctor explained to Mrs. D that this abnormality was a lethal genetic condition and that the median survival age of children affected with it was approximately six days. Mrs. D was devastated by the loss of her twins and dismayed by the prospect of carrying the pregnancy to term. She felt unable to bear the burden of a further five months of pregnancy with one foetus dead and the other dying. She considered an abortion. When she explained her wish to the hospital doctors, they were, as she said, 'very guarded' in their responses and they 'appreciated that she was not eligible for an abortion in Ireland'.[1] Indeed, abortion is prohibited by the Irish Constitution and even though a few exceptions to the prohibition have been accepted, the circumstances in which Mrs. D found herself are not covered by any of these.

Mrs. D did not consider it useful to start legal proceedings in Ireland in order to be allowed an abortion, as she did not think it likely that the Irish courts would be willing to interpret the Constitution and the accepted exceptions in such a way as to permit an abortion in her situation. She was also afraid that such proceedings would be time-consuming and she found it intolerable to have to wait for an abortion until the proceedings were concluded. Instead, she made arrangements to travel to the United Kingdom, where she underwent an abortion by medical induction.

Having travelled back to Ireland directly after the abortion, she experienced various physical difficulties which she did not dare to explain to her family doctor, nor to the hospital she attended for the complications; there, she contended that she had had a miscarriage. She also suffered from psychological difficulties which caused her and her partner to separate.

As she felt that the strict Irish abortion regulations had caused her strong personal suffering, Mrs. D decided to bring proceedings before the European Court of Human Rights in Strasbourg (ECtHR or Court). In her opinion, the Irish legislation violated her fundamental rights as protected by the European Convention on Human Rights.

The case of Mrs. D provides an excellent example of a so-called 'hard case': a case brimming with difficult moral and legal controversies, which cannot simply be decided by the application of clear legal norms.[2] The moral dilemma in the case is clear: should an abortion be permitted in a case such as Mrs. D's, where the foetus has a very low life expectancy, or should religious and ethical views regarding the right to life prevail? The moral aspect of this case brings about a number of other dilemmas, such as the question of whether it is up to a court to decide about such moral issues or whether it is preferable for such issues to be decided by the legislature after long and extensive public debate. Though this dilemma is present in many court cases, it is even more distinct in cases brought before a supranational court such as the ECtHR. After all, if the ECtHR were to decide that an abortion should be allowed in such circumstances as present in Mrs. D's case, this might upset a highly delicate and sensitive national balance. On the other hand, if the ECtHR were to find that the national authorities should decide on these issues, it would fail in its duties as a court of last resort for the protection of fundamental rights. The case of Mrs. D thus constituted a dilemma for the ECtHR. More properly said, it constituted at least three distinct legal dilemmas: the question how moral issues can be solved by judicial means; the relation of the courts to the democratically elected bodies; and the position of supranational courts vis-à-vis national legislation, traditions and cultural and religious choices.

Each of these dilemmas has been extensively dealt with by the most famous of legal scholars. Many valuable solutions to the dilemmas have also been supplied by the courts themselves, which is the result of judicial practice simply requiring 'hard cases' to be decided. However, most

solutions have evoked as many new questions as they have answered in the first place, and many of them show important shortcomings. This is problematic, as the dilemma of deciding 'hard cases' has become even more relevant in recent decades – the growing importance of fundamental rights and the increasing complexity and 'multi-levelness' of modern legal orders has resulted in ever greater numbers of 'hard cases' to be brought before the courts.[3] For this reason, it seems valuable to provide an overview of the legal debate about the three dilemmas indicated and of the gaps in legal theory and practice which are still widely open. The sad case of Mrs. D will thereby serve as a guideline.

The dilemma of deciding moral issues

In her statements before the European Court of Human Rights, Mrs. D invoked two fundamental individual rights: the right not to be subjected to inhuman and degrading treatment, and the right to respect for her private life. Both of these rights are contained in the European Convention of Human Rights, but their content and meaning have not been defined with any precision or clarity. To a large extent this means that it is up to the courts, in particular the ECtHR, to decide what kind of behaviour constitutes 'inhuman and degrading treatment', or when there is an unacceptable interference with the right to respect for private life. Such decisions often require value judgments to be made. This is true in particular for judgments regarding the right to protection of private life which Mrs. D invoked. The Court will there have to answer the question of whether the choice for abortion, or, more specifically, the interest in having an abortion performed in one's own country without the need for being secretive and anxious about it, forms part of an individual's 'private life'.

In deciding whether Ireland has violated Mrs. D's privacy rights, the Court will also need to take into account that the right to protection of one's private life is not absolute; this means that the state does not have to respect this right at all costs, but that it may restrict the right if there are convincing arguments to do so.

However, if the government has interfered with privacy rights because it wants to protect other fundamental rights or interests, such as the right to life of unborn children, the courts face a quandary. They will then have to decide whether the government has struck a fair balance between such

incommensurable values as the desire to protect the right to life and the need to respect an individual's autonomous choices. Such a choice can hardly be made on the basis of purely rational, legal and neutral criteria. In deciding 'hard cases', even the most honest judges will be inspired to some extent by personal, political or religious convictions and stereotypes. This is clearly true if a case relates to the right to live or die, but it is also relevant with respect to many other issues arising in modern, multicultural states, whether it concerns the case of an employer who has dismissed an employee who refuses to shake hands with men, or the case of a newspaper publishing a cartoon which offends the religious feelings of Muslims or Jews, or the case of a political party that excludes women from the possibility of exercising public functions.

The fact that moral considerations of judges will unavoidably influence judicial decision-making in 'hard cases' is widely recognised by legal scholars. Simultaneously, the idea of a judge taking personally-tinted decisions about highly delicate issues is one that is hardly reconcilable with the ideal of rational legal decision-making by an impartial and independent judiciary. It is exactly this ideal which has led legal scholars and legal professionals not to accept the unavoidability of value judgments as a final conclusion. If no theoretical solutions can be found, there might be practical solutions that can help to reduce the influence of the personal element in judicial decision-making. Such solutions have been found, for example, in such relatively simple rules as the one that 'hard cases' may never be decided by a single judge. Indeed, the ECtHR decides controversial cases always in Chambers of seven judges and the hardest cases may even be referred to a Grand Chamber constituted of seventeen judges, as happened in the case of Mrs. D. The obvious value of this rule is that the opinions and personal convictions of one single judge can never be decisive. Moreover, the requirement to decide unanimously or at least by a majority creates the need for debate, which in itself constitutes a test for the pertinence and value of all arguments exchanged.

A more substantive instrument to limit the influence of subjective opinion on judicial decision-making can be found in the requirement to hand down well-reasoned judgments which can stand scrutiny by higher courts, legal professionals and the general public. This requirement induces a court to select only those arguments which, taken together, form a legally sound, logical, convincing and objective basis for their conclusions. Even if a court were to arrive at a certain outcome on

irrational and intuitive grounds in the first place, it will need to reconsider the outcome if no such rational arguments can be found. The process of deciding a 'hard case' is thereby to some extent objectified.[4] Case-law analyses reveal, however, that the ideal of transparent and rational judicial argumentation is still far from being satisfied. Many judgments are poorly or obscurely reasoned, containing overly rhetorical arguments, empty formulas or improper references to earlier case-law, or even defying the demands of logic.[5] Unfortunately this is not only true for lower court judgments, but also for cases decided by national supreme courts or even the European Court of Human Rights.[6]

An important solution to the dilemma of deciding 'hard cases' may thus be found in an improvement of the quality of judicial argumentation. Some efforts have already been made to draft decision models which may provide structure to the courts' argumentation.[7] Legal scholars have identified various aspects of cases which can be decided by the courts on purely factual grounds and which may be conclusive for the whole case.[8] For example, if a legal measure affects fundamental rights with the purpose of protecting important social interests, but appears to be ineffective and unsuitable, such a measure is clearly unacceptable. Such a judgment as to the effectiveness of a certain measure can be given on purely factual grounds. If a case is decided in this way, there is no need for the court to address the actual balance of interests that underlies a certain measure or decision, which means that the need for giving value judgments is strongly reduced. Much is still to be gained here – many roads to rational decision-making lie relatively unexplored. To further investigate the possibilities for improvement of judicial argumentation is thus an important assignment for both legal scholars and legal professionals.

The dilemma of the 'counter-majoritarian difficulty'

The second dilemma faced by a court having to decide a 'hard case' concerns the position of the judiciary vis-à-vis democratically elected bodies. In the case of Mrs. D, for example, the ECtHR mentioned that, at the relevant time, an amendment to the Irish Constitution had been proposed to permit an abortion in the situation where it was necessary to prevent a real risk of loss of the woman's life.[9] The proposal was defeated in a referendum held in 2002, albeit only just (50.42% against and

49.58% in favour) and in a very low turnout (42.89% of the electorate). This meant that a slight majority of the Irish population was still in favour of the very strict abortion rules contained in the Constitution. Of course, it is not entirely predictable how the Irish would vote on an amendment permitting an abortion in the situation where the foetus would not survive to term or live outside the womb, as would be relevant in Mrs. D's case. Nevertheless, the strong division of votes in the Irish abortion referendum makes clear that the fundamental issue of abortion is highly controversial and subject to intense debate. May a court, such as the Irish Supreme Court or the European Court of Human Rights, then simply cut the Gordian knot by deciding to prohibit or permit a certain type of abortion? Or should this type of decision be left to the elected representatives of the people?

The dilemma here at issue is known in legal theory as the 'counter-majoritarian difficulty' and relates to the question of whether the courts, notwithstanding their lack of a democratic mandate, are permitted to give an interpretation of fundamental rights which conflicts with the wishes of the majority of the population. The counter-majoritarian difficulty is often invoked in legal theory to oppose the possibility of 'judicial review', which is the competence to test the compatibility of legislation with the Constitution, international treaties and the like. One of the most eloquent antagonists of judicial review has voiced his criticism as follows:

'… When the Supreme Court declares unconstitutional a legislative act …, it thwarts the will of the representatives of the actual people of the here and now; it exercises control, not on behalf of the prevailing majority, but against it. That, without mystic overtones, is what actually happens …. [I]t is the reason the charge can be made that judicial review is undemocratic'.[10]

Regardless of this criticism, many legal systems have accepted the possibility of judicial review. Interestingly, the ECtHR's decision in the case of Mrs. D discloses that it clearly accepts judicial review, regardless of the counter-majoritarian difficulties attached to it. An important conclusion reached by the ECtHR was that the case should have been decided by the Irish Supreme Court in the first place. After all, Mrs. D had not attempted via the Irish courts to obtain permission for an abortion, as she did not expect them to decide in her favour. This meant,

according to the ECtHR, that she had in fact deprived the Irish Supreme Court of the opportunity to give a fresh interpretation of the Constitution. The ECtHR considered that there would certainly have been a chance of success, as the Irish Supreme Court in two earlier cases had decided in favour of a woman requesting an abortion. In both cases it had done so without there being a clear consensus about the desirability of the accepted exceptions, which meant that there was no reason why it should not do so in Mrs. D's case. From these considerations it can be derived that the ECtHR regards judicial review as a feasible and well-accepted way of arriving at constitutional reform, and that it even accepts that a court may hand down a judgment which seems contrary to the majority's opinions. In itself this is a revolutionary statement, as it means that an important and influential court such as the ECtHR does not take the counter-majoritarian difficulty really seriously.

The acceptance of judicial review by the ECtHR does not mean, however, that the dilemma is non-existent. The objections of the opponents of judicial review are real and cannot be swept aside simply by stating that the courts in practice accept the possibility of judicial review. More relevant to solving the dilemma may be the many theories which have been advanced to reconcile the notion of judicial review with the notion of representative government.[11] One of the most convincing of these is the argument that representative government and majoritarian decision-making are not the only important elements of a state governed by the rule of law (or *Rechtsstaat*). Equally important is the protection of the interests of minority groups and fundamental rights, in particular if it seems likely that the democratically elected bodies will neglect or even thwart these rights and interests. Indeed, the value of guaranteeing such rights and interests may sometimes be considered greater than that of satisfying the desire of the majority.[12] It is this argument that seems to lie at the heart of the US Supreme Court's famous decision to declare unconstitutional the American policy of segregation of schools, even though it was clear that the policy was still supported by a majority of the American population (at least in the South).[13] The same argument was used by the ECtHR to uphold a Turkish judgment dissolving the Welfare Party – in spite of strong support for the party in Turkey (polls indicated that the Welfare Party would obtain an absolute majority of the votes at the next elections), it held that the party's ideas and opinions were contrary to the notion of the rule of law and constituted a danger to the

individual rights and freedoms guaranteed by the Convention.[14]

Although this justification for counter-majoritarian judgments generally seems to be rather convincing, it does not entirely solve the problem. In the cases of racial segregation and the Welfare Party, the reasonableness of the counter-majoritarian judgments may seem obvious – indeed, racial segregation evidently frustrates the rights of persons of a certain skin colour, while it is relatively clear that the Turkish Welfare Party would have used the power of government to impose forceful religious legislation to the disadvantage of the rights and interests of religious minorities. In a case such as that of Mrs. D, the situation is less clear-cut. Although Mrs. D evidently holds a highly important and valuable interest in self-determination and human dignity, the interest of the protection of the life of unborn children (even if they have a low life expectancy) and the respect for religious convictions can be considered at least as weighty and legitimate. Whatever decision were taken by the Court, some fundamental and social interest would have to be sacrificed. The argument of the protection of the rule of law thus provides hardly any direction as to how to decide such a case.

How should the ECtHR then deal with such a case? It cannot, after all, simply leave the case undecided as this would amount to denial of justice. What it can do, however, is to respect as much as possible the choices made by the legislature, as long as it is clear that the contested legislation is the result of open and extensive social and political debate, the outcome of which seems to be well-balanced and representative, even if it is unacceptable and harsh in the opinion of some. On the other hand, a court may reasonably be expected to intervene in cases where there has been no such careful process of decision-making, where the debate seems to have been one-sided or where the majority seems to have neglected important interests of minority groups. In such cases, according to procedural democratic theory, it may be justified for the courts to restore the balance in the democratic system by favouring the interests of the minority over those of the majority, or at least by ascertaining that the minority has a proper voice in the decision-making process.[15]

The ECtHR has shown itself to be very careful indeed in its relations towards the other powers of government, its counter-majoritarian judgment in the case of the Welfare Party being a rare exception. In many cases, it will respect the choices made by the national legislature, especially if they are well-considered.[16] It seems not far-fetched to

suppose that the ECtHR is, to some extent, influenced by the procedural theory of democracy, taking an activist stance in cases where the democratic system wreaks havoc, while showing restraint where it produces acceptable (albeit controversial) measures.

The dilemma of deciding hard cases in a multi-level legal system

This brings us to the third dilemma which is connected with the judicial decision of 'hard cases'. The ECtHR is a supranational court, established by the member states of the Council of Europe (CoE). The CoE itself was erected by a number of member states directly after the Second World War with the primary aim to defend fundamental rights, democracy and the rule of law. It presently groups together 46 European countries, including such non-EU states as Russia, Ukraine, Turkey and Azerbaijan. All of these states are signatories to the European Convention of Human Rights and have accepted the individual right to complain before the ECtHR about violations by the state of their fundamental rights, or about a state's failure to protect these rights in an effective way. By considering and deciding these complaints, the ECtHR has been able to elucidate the meaning of the various rights protected by the Convention, each judgment providing more clarity about the exact obligations and prohibitions arising from the Convention provisions.[17] This has had important consequences for almost all the states falling under the Court's jurisdiction; the Netherlands, for example, has had to change its entire system of administrative procedure because the system showed, according to the ECtHR, important deficits from the perspective of procedural rights.[18]

It is important to realise that such fundamental changes have been brought about by a judgment of an independent international body, constituted by judges from 46 different states and, importantly, lacking a police force to help it enforce its judgments. Indeed, the ECtHR can be considered to be highly successful: by far the majority of its judgments have been followed up by the states against which they were directed.[19] It is difficult to explain the reasons why states have been willing to comply with so many of the ECtHR's judgments, but to some extent this seems to be due to the authoritative and convincing character of the Court's judgments. Some researchers have even stated that the quality of the Court's legal reasoning and its moral integrity form its main legitimating

assets.[20] It is highly probable that, if the ECtHR's arguments were to lack power of persuasion, the member states would be much less willing to implement them in their legal systems.[21] The ECtHR therefore constantly has to safeguard the quality of its judgments, which is a task that is growing more and more difficult given the rapid increase in its workload – whereas the ECtHR had to deal with only 404 applications in 1981, some 41,000 applications were lodged in 2005.[22] Even though the number of judges also rose during this period, their number did not keep pace with the increasing number of cases. This means that it is becoming ever more difficult for the judges to elaborately discuss each individual case and to deliver consistent, carefully drafted and well-reasoned decisions. Indeed, there have been signs that case-law is showing problematic inconsistencies and lack of clarity.[23] A variety of measures have been introduced to solve this, including the introduction of a 'Case-law Conflict Prevention Group' and a 'Conflict Prevention Board', but it is questionable whether this is sufficient.[24]

The preservation of consistency and quality of argumentation of supranational decisions is a highly important topic, but it constitutes only a small part of the conundrum of deciding 'hard cases' in a 'multi-level' legal order. National legal systems have increasingly become part of larger regional or global jurisdictions with their own powers of decision and judgment. The interaction between the various 'levels' of jurisdiction (national, European, global) and the co-operation between the relevant authorities is highly complicated.[25] For example, as regards fundamental rights protection, important work is not only done by the ECtHR and the national legal orders, but also by the Court of Justice of the European Communities, which is competent to decide on fundamental rights issues arising from European Union law.[26] In addition, there are many international human rights treaties containing specific requirements that have to be met by the states.[27] It is obviously very difficult for both the national and European courts to navigate between the specific and sometimes conflicting requirements set by each of these levels. Stronger co-operation between the various levels is essential to create concordance between the various requirements and to enable both the national and the European courts to operate in an effective way, but there are still many political and practical obstructions that hamper the development of successful solutions.

For the ECtHR, one of the most pressing multi-level problems relates

to the diversity that the CoE members show in their approaches towards fundamental rights. Even if the ECtHR wished to provide a uniform level of human rights protection, this would seem hard to attain in the CoE as it presently stands. After all, fundamental rights issues are close to the individual, but also to national and cultural traditions: the way these rights are protected often reflects deep-seated social choices.[28] The example of abortion and the right to life of unborn children, which is central to the case of Mrs. D, may help to illustrate this. One of the questions that is relevant to deciding abortion cases is concerned with the point in time as from which someone is protected by the Convention's 'right to life' – does such protection start at the moment of conception, at birth, or somewhere in between? It is clearly highly difficult for the ECtHR to decide this for all 46 member states of the CoE, especially because certain states have not even reached internal agreement on the question. Similarly, it is hardly possible for a European court to decide whether the right to protection of private life contains a right to self-determination which is sufficiently strong as to allow a pregnant woman to choose whether to carry her child to full-term. Such decisions not only involve highly difficult moral choices, but they may also thwart national sensitivities or upset a delicate political balance. This may not be acceptable to some states and might even result in a refusal to execute the ECtHR's decision.[29] As the protection offered by the ECtHR is very much dependent on voluntary compliance, this constitutes a dilemma indeed.

Hence, 'multi-levelness' means that the ECtHR will have to respect national differences in interpreting the Convention, while simultaneously providing a high and uniform level of protection of fundamental rights that is not out of pace with international and European Community requirements. In its case-law the ECtHR has been trying various approaches and methods to meet these differing demands. A well-established method is the use of the doctrine of the 'margin of appreciation'. According to this doctrine, each state has a certain amount of discretion which it may use to take decisions it considers important and necessary, even if such decisions interfere with the exercise of fundamental rights. In principle, the states thus remain free to strike a balance between competing interests, as long as the minimum level of protection of a fundamental right is guaranteed.[30] The scope of the margin of appreciation differs, however, according to the circumstances of the case. If a case concerns a sensitive or highly technical policy area, such as

planning policy or national security, the states will be given much room indeed to determine the necessity and suitability of certain restrictive measures.[31] The ECtHR will only superficially scrutinise the national measures, merely controlling whether the balance struck by the state is not manifestly unreasonable or arbitrary. By contrast, the ECtHR will leave a far more narrow margin of appreciation in those cases where the 'core' of a fundamental right seems to be affected (*e.g.* the freedom of the press), or where the complaint concerns a serious interference (*e.g.* censure) – it is then that the uniform protection of the Convention really matters. In those cases, the Court will apply rigorous scrutiny and it will accept only very important general interests as a justification. Interestingly, the ECtHR uses such a strict test primarily in cases where there is a high degree of consensus within the CoE as to the importance of the right concerned; indeed, it is then made easier for a supranational court to accept a certain application of fundamental rights.[32]

Normally, the ECtHR uses the 'margin of appreciation' doctrine only to decide whether an interference with a certain Convention right can be *justified*, *i.e.* if good reasons can be given to restrict a certain individual right. A new development is that it also uses the doctrine in *interpreting* the rights contained in the Convention. Until recently, it was considered to be one of the Court's main tasks to provide an authoritative and uniform explanation of the meaning and scope of the various rights contained in the Convention. Such difficult interpretive questions as whether assisted suicide or legal recognition of a change of gender are covered by the right to respect for one's private life, were willingly answered by the Court.[33] The result of this has been that, throughout the Council of Europe, an identical definition has been given to the Convention rights, even though the margin of appreciation doctrine implies that there will be differences as regards the extent to which restrictions of these rights can be considered acceptable. In the case of *Vo v. France*, however, the Court adopted a controversial new approach.[34] The case concerned in essence the question of when life begins, or, more specifically, as from what moment one's life is protected by the 'right to life' contained in the Convention. If life is considered to start at conception, even unborn life would be protected by the Convention. As the right to life is considered to be absolute – which means that interferences cannot in principle be justified – this would be a highly important decision.[35] An interpretation of 'life' as encompassing 'unborn

life' would imply, after all, that national acceptance of abortions would be in violation of the right to life as protected by the Convention, which would probably have been unacceptable for those member states that permit abortions on principled grounds. If the Court had ruled, on the other hand, that 'life' starts at birth, this would have run counter to the views of other member states. The case of *Vo* thus left the ECtHR with a quandary, but it found a way out by considering that opinions are so much divided that the question of when life begins should be left to the relevant state's margin of appreciation.[36] This decision has been widely criticised, as it is not in conformity with the Court's perceived role as final interpreter of the Convention.[37] It is clear, though, that the ECtHR considered this a lesser evil when compared to the problems that might be caused by accepting a uniform definition.

One last solution to the dilemma of deciding 'hard cases' in a multi-level legal system can be found in the case of Mrs. D. It may be recalled that Mrs. D had passed over the Irish courts because of her lack of confidence in their ability to reach a favourable decision. In fact, she thereby asked the ECtHR to decide on a controversial and sensitive issue which had not yet been discussed by the national courts. For the ECtHR this implied that there was no possibility of tracing relevant arguments, views and sensitivities in national judicial decisions which could help it to take a reasonable and acceptable decision. In addition, it meant that the national authorities themselves had not been given the opportunity to 'remedy' any possible fundamental rights violations. For these reasons, the Court decided that it would not pronounce a substantive judgment in the case, declaring the case inadmissible because of a failure to exhaust all available national remedies.

The ECtHR's decision not to pronounce judgment in this case may be considered as a disregard of Mrs. D's interests, as it means that the Court did not reach the substantive question of whether the Irish anti-abortion legislation violated her fundamental rights. A substantive decision might have been considered the more reasonable because of Mrs. D's understandable lack of confidence in the willingness of the national courts to accept a change of the widely-supported Irish anti-abortion policy. Nonetheless, the Court's decision seems to be an acceptable way out of a difficult dilemma, as it places the primary responsibility to decide on moral issues and 'hard cases' with the national authorities. Indeed, it may well be contended that the national courts are more suited to deal with

such issues than a distant Court constituted of judges who will hardly know and understand local difficulties and opinions.

Conclusion

Drawing the lines together, it is clear that judicial decision-making in 'hard cases' discloses important dilemmas: such cases require difficult value judgments and moral choices, which are difficult for the courts to make. The dilemma is even more distinct when account is being taken of the need for the courts to respect the decisions of representative bodies and, where relevant, the differences between states. During recent decades, these dilemmas have become even more important because of the increasing complexity of multi-level and multi-cultural societies, where conflicts of fundamental rights and interests seem to occur with ever greater frequency.

The dilemmas inherent to deciding hard cases will probably never be completely resolved. Nonetheless, it is essential to endeavour to make visible and, wherever possible, to reduce the impact of subjective opinions on judgments; to find an acceptable balance between courts and elected bodies; to create closer co-operation between the various 'levels' of jurisdiction; and to find a middle ground between the need for uniform European protection of fundamental rights and the need to respect diversity and national traditions. To do so will be one of the great challenges for both legal scientists and legal professionals, preferably in close co-operation.

Notes

1 See the description of the facts of the case in: ECtHR, admissibility decision of 28 June 2006, *D v. Ireland*, not yet published, § 4. All unpublished ECtHR cases can be found via http://www.echr.coe.int/ECHR/.

2 Cf. A. Bhagwat, 'Hard Cases and the (D)evolution of Constitutional Doctrine', in: *Connecticut Law Review* 921 (1998), p. 966 and J. Bomhoff and L. Zucca, 'The Tragedy of Ms Evans: Conflicts and Incommensurability of Rights', in: *European Constitutional Law Review* 424 (2006), at p. 429.

3 See e.g. T. Koopmans, *Courts and Political Institutions. A Comparative View* (Cambridge: Cambridge University Press 2002), ch. 10.

4 See L. Epstein and J.F. Kobylka, *Supreme Court & Legal Change. Abortion and the Death Penalty* (Chapel Hill/London 1992) p. 13ff.

5 See e.g. J.H. Gerards, *Judicial Review in Equal Treatment Cases* (Leiden/Boston 2005), in particular with respect to equal treatment cases (which often qualify as 'hard cases').

6 Cf. J.H. Gerards, 'Rechtsvinding door het Europees Hof voor de Rechten van de Mens', in: T. Barkhuysen, R.A. Lawson, *et al*. (eds.), *55 Jaar EVRM*, *NJCM-Bulletin (special)* 2006, p. 106/107.
7 E.g. Gerards 2005 (*supra*, note 5).
8 See e.g. F.T. Groenewegen, 'In hoeverre schrijft het evenredigheidsbeginsel iets voor?', in: A.J. Nieuwenhuis, B.J. Schueler and C.M. Zoethout, *Proportionaliteit in het publiekrecht* (Deventer 2005) p. 18/19.
9 See in particular §§ 43-45 of the ECtHR's admissibility decision.
10 A.M. Bickel, *The least dangerous branch. The Supreme Court at the Bar of Politics*, 2d Ed. (New Haven/Londen 1962) p. 16/17.
11 See in particular Federalist Paper No. 78, *The Judiciary Department* (1788), H. Kelsen, *Reine Rechtslehre*, Vienna: Franz Deuticke 1960, p. 271ff, J.H. Ely, *Democracy and Distrust: A Theory of Judicial Review* (Cambridge (Mass.) 1980) and R. Dworkin, *Freedom's Law. The Moral Reading of the American Constitution* (Cambridge (Mass.) 1996).
12 See in particular Ely (*supra*, note 12) and cf. J.H.H. Weiler, *The Constitution of Europe* (Cambridge1999) p. 103.
13 *Brown v. Board of Education*, 347 U.S. 483 (1954). For the history of the case, see e.g. H.J. Abraham and B.A. Perry, *Freedom and the Court. Civil Rights and Liberties in the United States* (New York/Oxford 1998) p. 342ff.
14 ECtHR, Grand Chamber judgment of 13 February 2003, *Refah Partisi and Others v. Turkey*, Reports 2003-II, § 11.
15 See in particular Ely (*supra*, note 11).
16 See *e.g. Maurice v. France*, Grand Chamber judgment of 6 October 2005, not yet published, and ECtHR, Chamber judgment of 7 March 2006, *Evans v. UK*, not yet published, § 42.
17 See further e.g. J. Polakiewicz, 'The Execution of Judgments of the European Court of Human Rights', in: R. Blackburn and J. Polakiewicz (eds.), *Fundamental Rights in Europe* (Oxford 2001) p. 72/73.
18 See ECtHR, judgment of 23 October 1985, *Benthem v. The Netherlands*, Series A, Vol. 97; further T. Barkhuysen and M.L. van Emmerik, 'A Comparative View on the Execution of Judgments of the European Court of Human Rights', in: Th. Christou and J.P. Raymond (eds.), *European Court of Human Rights. Remedies and Execution of Judgments* (London 2005) at p. 11ff.
19 See Polakiewicz (*supra*, note 17), p. 74; see, however, E. Bates, 'Supervising the Execution of Judgments Delivered by the European Court of Human Rights: the Challenges Facing the Committee of Ministers', in: Th. Christou and J.P. Raymond (*supra*, note 18) p. 55 ff.
20 E.g. Bates (*supra*, note 19), at p. 53; see also Epstein & Kobylka (*supra*, note 4) p. 21/22.
21 See in particular L.R. Helfer and A.-M. Slaughter, 'Toward a Theory of Effective Supranational Adjudication', 107 *Yale Law Journal* 273 (1997) p. 318ff.
22 See the statistical information at www.echr.coe.in; see also *Reform of the European human rights system. Proceedings of the high-level seminar, Oslo, 18 October 2004* (Strasbourg 2004).
23 See Gerards 2006 (*supra*, note 6), p. 93-122 and Bates (*supra*, note 27) p. 70.
24 See E. Myjer, 'Straatsburgse Myj/meringen: CLCP en CPB', 31 *NJCM-bulletin* 1067 (2006).
25 M. Mayer, 'The European Constitution and the Courts. Adjudicating European constitutional law in a multilevel system'. *Jean Monnet Working Paper* 9/03 (2003) p. 36ff.
26 See D. Spielmann, 'Human Rights Case Law in the Strasbourg and Luxembourg Courts: Conflicts, Inconsistencies, and Complementarities', in: Ph. Alston (ed.), *The EU and Human Rights* (Oxford 1999) p. 757ff.
27 See e.g. R. Holtmaat and Ch. Tobler, 'CEDAW and the European Union's Policy in the Field of Combating Gender Discrimination', in: *Maastricht Journal of European and Comparative Law* 399 (2005).

28 See J.H.H. Weiler, *The Constitution of Europe* (Cambridge 1999) p. 102ff.
29 See e.g. M. Claes, *The National Courts' Mandate in the European Constitution* (Oxford 2006).
30 See Weiler (*supra*, note 28) p. 106/107.
31 E.g. ECtHR, Chamber judgment of 2 November 2006, *Giacomelli v. Italy*, not yet published, § 80 and ECtHR, admissibility decision of 29 June 2006, *Weber and Saravia v. Germany*, not yet published, § 106. For other factors, see Gerards 2005 (*supra*, note 5) p. 165ff.
32 See Gerards 2005 (*supra*, note 5) p. 171 ff. and Weiler (*supra*, note 28) p. 114.
33 E.g. ECtHR, Grand Chamber judgment of 29 April 2002, *Pretty v. UK*, Reports 2002-III, §§ 61-63 and ECtHR, Grand Chamber judgment of 11 July 2006, *Christine Goodwin v. UK*, Reports 2002-VI, §§ 89-93.
34 ECtHR, Grand Chamber judgment of 8 July 2004, *Vo v. France*, Reports 2004-VIII.
35 Cf. T. Goldman, '*Vo v. France* and Fetal Rights: The Decision Not To Decide', 18 *Harvard Human Rights Law Journal* 277 (2005) at p. 281.
36 § 85.
37 E.g. J.A. Bomhoff in his case-note in *European Human Rights Cases* 2004/86; see also the separate opinion of Judge Ress, § 3ff.

9
The compatibility of *sharia* with the rule of law. Fundamental conflict: between civilisations? Within civilisations? Or between scholars?

Jan Michiel Otto

On 13 September 2006 the Dutch minister of Justice, Piet Hein Donner, was urgently summoned to parliament in The Hague. In a heated debate, politicians from all parties, including his own Christian-Democrats, strongly condemned him for comments he had made in an interview. The minister had remarked that introducing the *sharia*[1] should also be an option in the Netherlands if a large enough majority demanded this. His opponents, however, stressed that the *sharia* was incompatible with the very essence of the Dutch state, the rule of law. This was the thousand-and-first episode of a public debate about Islam and rule of law, or *rechtsstaat*, which has been raging in the Netherlands and elsewhere in Western Europe since the 1990s.

In fact, the issue has been discussed for centuries in many Muslim countries.[2] While rulers of early Muslim states usually governed under the banner of Islam and undertook to 'implement the *sharia*', in reality a growing body of state law and institutions had emerged. In the Ottoman Empire, which was never colonised by European powers, ruling elites in the nineteenth century decided to modernise and codify law. Those Muslim countries which had been colonised by Britain, France, and the Netherlands had to make crucial decisions on this issue at the time of their independence. Colonial jurisdictions had accepted Islamic law and customary law as legitimate parts of their legal systems. Changing these pluralist systems into unified national systems would prove to be a daunting task. But such historical considerations hardly played a role in the first years of the twenty-first century.

Other frames of reference had taken their place. Italy's Prime Minister Berlusconi had caught the headlines in September 2001 (!) with his pronouncements. He said: 'We should be conscious of the superiority of our civilisation, which consists of a value system that has given people widespread prosperity in those countries that embrace it, and guarantees respect for human rights and religion'. He added: 'This respect certainly does not exist in the Islamic countries'.[3] It was not only populist politicians who made such statements. A respectable institution like the European Court of Human Rights declared in a ruling of 13 February 2003 on the case of the *Refah* Party versus the Turkish state that *sharia* is incompatible with democracy as laid out in the European Convention.

The claims of Berlusconi and the Court very much reflected the thrust of an influential American publication called *The Clash of Civilizations,* by Samuel Huntington, a professor of political science at Harvard University. Huntington claims that in the new world order a clash between *civilisations*, in particular between 'the West' and 'the Muslim world', is the greatest threat to world peace. He states that s*haria* conflicts with the rule of law, a cornerstone of Western civilisation. Moreover, he suggests that extreme *sharia* is spreading rapidly, replacing 'western' law. Therefore Huntington urges the West to defend its civilisation against the rise of the 'other' civilisation.

Many have called Huntington's *clash* a self-fulfilling prophecy. The beginning of the twenty-first century was indeed marked by the 9/11 attacks by Al-Qaeda in 2001, the immediate US reprisal against the Taliban regime in Afghanistan, the war in Iraq, deadly bombings in Madrid and London, and the ongoing conflict between Israel and Palestine. Many westerners are frightened by the idea that this violence, this destructive human behaviour, is essentially caused by the dictates of fixed religious Islamic rules, believed to come from Allah and therefore beyond rational human criticism.

In fact, Muslim peoples and states throughout history have always exercised human judgment in establishing the scope of *sharia* in their communities. Slavery, which was permitted in classical *sharia*, has been abolished, even in Saudi Arabia. Tariq Ramadan, a leading Islamic scholar in Europe, proposed a worldwide moratorium of the infamous *hadd* punishments. The term 'hadd' refers to certain specified crimes and punishments which are prescribed by divine sources such as the Qur'an and the Sunna, as laid down in the so-called Traditions. Therefore their

suspension is no minor step. Moreover, over time the status of women under national marriage legislation has significantly improved in most Islamic countries, and recently in Egypt (2000) and Morocco (2003). Clearly, modernist interpretations of *sharia* have brought such national laws closer to the rule of law in several countries. Modernisation was also the trend in Iran under the authoritarian Shah until it backfired and the conservative clergy launched an Islamic revolution. They received much popular support. This episode demonstrates that forcefully separating a society from its religious roots may have a high price. As Wael Hallaq, a well-known expert of Islamic law, warns, referring to recent destructive violence by Muslim radicals: 'And let there be no doubt that, at the end of the day, the culprit is the rupture of history. The abrupt disconnection from the past, from its legacies, institutions, and traditions, lies at the heart of these problems'. [4]

For most Muslims, Islam indeed represents a major source of public morale, virtue, rightness and self-respect. For the poor and oppressed it also is a source of consolation, acceptance and trust. Therefore, enacting provisions of national laws, which are perceived as an attack on Islam or *sharia*, is a politically hazardous adventure.

The incompatibility of Islam with the rule of law has become a multilevel issue. On a global level Osama bin Laden resorted to discourses on Islamic Holy War (*jihad*) against the United States. The answer of Bush and Blair was a war justified in terms of 'freedom and democracy'. However, it would be a serious mistake to think that the incompatibility issue is merely or even mainly a problem *between* two civilisations. For most Muslim states it contains fundamental *domestic* dilemmas. Should, for example, governments with liberal interpretations of *sharia* admit powerful Islamist groups to form political parties and participate in elections, even if these groups deny the legitimacy of their government and promote orthodox interpretations which would, if enacted, lead to barbaric punishments and serious discrimination of women and religious minorities?

In Egypt this is called 'the Algerian dilemma'. Zakaria refers to this problem in *The Future of Freedom* as 'the Islamic exception'. Meanwhile in Western Europe, increasing problems and accusations surrounding ethnic communities of Muslim migrants have led to feelings of enmity on both sides. A few radical Islamist groups in Europe also proclaimed that they

would rather take guidance from *sharia* than from state laws, and resorted to violence. One proponent was the murderer of Dutch film director Theo van Gogh. Against this background it is no wonder that Minister Donner's remarks caused a row in Dutch politics.

Emotions run high in the public debates about the compatibility between *sharia* and democratic rule of law, both in the Muslim world as well as in some European countries. Rational discussions are often complicated by feelings of pride – 'our system is superior to theirs' – or fear and suspicion – 'they will attack, conquer and rule us and destroy our value system'. Clearly, in order to have a fruitful and rational debate about compatibility, a solid knowledge base is needed. How then has this problem been addressed by academic research? How have scientists coped with its sheer magnitude? Which academic domains, approaches and methods have been employed? Are there any main findings which could serve as a point of departure? Could such findings assist policy-makers and law-makers in their efforts to manage or even solve this nagging problem of compatibility?

Some academics have already been working hard on this issue. For example, Ann Mayer, in an important effort to disentangle this problem in *Islam and human rights* (1995), demonstrated that there was compatibility in most areas, but incompatibility in four specific areas (see below). Halliday, in *Islam and the Myth of Confrontation: Religion and Politics in the Middle East* (2003), dissected the argumentation of Islamic groups who believed in compatibility – by 'assimilation' or 'appropriation' – or incompatibility – by 'rejection' or 'particularism'. The proposals of An-Na'im, a leading proponent of compatibility, range from a radical reinterpretation of Meccan verses of the Qur'an, in *Towards an Islamic Reformation, civil liberties, human rights and international law* (1990) to, in his more recent research project *The future of Sharia*, a rejection of *sharia*-based legislation for the sake of preserving *sharia* as a valuable, religious source of morale and justice. However, the extensive knowledge of such scholars has hardly resonated in the polarized public debates.

In 2003 a high-ranking independent think-tank of the Dutch government, the Wetenschappelijke Raad voor het Regeringsbeleid (WRR; Scientific Council for State Policy) commissioned several projects for a critical investigation into the changes in Islamic thought, political activism and national laws in the Muslim world, in an effort to contribute

to the scientific knowledge base and a more balanced debate. As director of the '*Sharia* and national law' project I undertook and coordinated cooperative research on the position and role of *sharia* in twelve Muslim countries and its compatibility with the rule of law. The selected countries are Egypt, Morocco, Sudan, Turkey, Saudi Arabia, Afghanistan, Iran, Pakistan, Malaysia, Indonesia, Mali and Nigeria. In 2006 the findings were published in three books.[5] This chapter, which presents some of the data, is a reflection on that project, in particular on the different perspectives I encountered in various academic domains.

In the course of our comparative study a five-step method was developed: (1) the concepts of *sharia* and rule of law were first defined and operationalised, (2) the national law of the twelve countries was investigated focusing on certain areas and topics of the law where *sharia* might have a serious impact, (3) the results of the research were put into comparative and historical perspective, (4) explanations were sought from the political and social contexts of *sharia* and law, (5) and finally to draw a general conclusion about the compatibility question data on different countries, areas and topics were aggregated and analysed.

Essentialism and multiplism, two competing perspectives

Two different perspectives prevail in the writings about compatibility of *sharia* with the rule of law. The first perspective considers *sharia* to be a fixed set of binding norms found in authoritative sacred texts representing the essence of an Islamic civilisation, an Islamic culture, an Islamic legal system. This, in short, is the essentialist perspective.

A second perspective focuses attention on the variety of social contexts in which such norms have been formulated, interpreted and applied in practice. It recognises that social change may bring new textual interpretations as well as different practices. Rather than seeing Islamic civilisation, culture and law as having one clear, identifiable core, the second perspective is prepared to see a highly differentiated picture. It accepts that people have multiple identities and live in polynormative societies. Scholars applying this perspective do not hesitate to speak of Islams rather than of Islam. They assume different concepts of *sharia* in different contexts. This is the multiplist perspective.

Huntington's clash theory reveals an essentialist perspective. His theory conceptualises 'the Islam', 'the Muslim people', 'the Muslims' and 'Islamic

law' as fixed and delineated entities. Such reduction sometimes leads to rather absurd theoretical statements that contradict social realities that everybody can observe. Bernard Lewis,[6] a major mainstay of this school of thought, for example states that '(...) in the Muslim perception, the state itself is a manifestation and an instrument (of religion)'. Ibn Warraq, another outspoken critic of Islam, maintains that women 'do not work under Islam'.[7] In the same vein the American activist for 'religious freedom' Marshall states that 'in the past 25 years the number of countries and regions being governed by a radical version of shari'a has increased';[8] his boss, Nina Shea, an adviser of George Bush, sees a common 'definable state ideology of extreme *sharia*'[9] in a variety of different Muslim countries including Indonesia.

In spite of such mistaken statements, we should not conclude that an essentialist approach has nothing of value. Its key hypothesis, namely that there *is* a core of Islamic values and norms which *can* be mobilised to make *any* Muslim behave accordingly, deserves at least investigation. Such research should address two dimensions, i.e. preaching and practising. Studying written and oral pronouncements which suggest that all Muslims ought to comply with *sharia* is one thing, what we need just as much is empirical evidence of how this is actually practised.

Sociologists and anthropologists have produced a vast body of research findings which demonstrate differences within the Muslim world, its societies, politics and legal systems. The available ethnographic, socio-historical and legal research refers to a much more complex and differentiated idea of 'Islamic tradition' than essentialist approaches suggest. Eickelman, a leading American scholar in anthropology of the Middle East states that the heterogeneity of local normative patterns in the Muslim world is so great that 'as a consequence, the notion of "an Islamic essence" has been difficult to sustain'.[10] Peters, a leading scholar of Islamic law, doubts whether it is appropriate to refer to *sharia* as 'a legal system' anyway. For the discursive and open nature of Islamic, jurisprudence has produced a multitude of legal opinions which often contradict one another. Indeed, today the existence of a variety of liberal, conservative and orthodox interpretations of *sharia* is an undeniable fact. Understandably, in contemporary socio-legal studies multiplist perspectives are fairly dominant. The question about compatibility of *sharia* with the rule of law is thus approached with the hypothesis that certain versions of *sharia* are more compatible with the rule of law than others.

Regarding the concept of *sharia*, we found in the research that three different meanings have been attached to it, which are seldom explicitly distinguished from one another. The first meaning refers to *sharia*'s divine origins: *sharia* is God's plan and contains his guidelines for the community of believers. This definition is general and creates space for different manifestations of *sharia*. The second meaning of *sharia* is: the corpus of rules, principles and cases that were drawn up by *fuqaha* in the first centuries after Muhammad before 'the gate of free interpretation' (*ijtihad*) was closed. This definition is more concrete and refers to the classical writings. The third meaning comes from the endlessly varied reality of the contemporary Muslim world: *sharia* then represents any one of the many possible interpretations of God's will by a certain person, group, institution, or state which states that it is based on 'the' *sharia*.

Rule of law, we found, is regarded as an umbrella concept comprising three types of principles: procedural, substantive as well as control mechanisms.[11] Examples of each type are the principle that laws should be written and consistent (procedural), the fundamental human rights (substantive), and independent courts (control mechanisms).

Research questions and academic domains

Aware of essentialist and multiplist presumptions, we now turn to an analysis of the problem of incompatibility of *sharia* with the rule of law by asking some research questions. A first question to be asked is: what are the relevant prescriptions in the *sharia*? This leads us to the study of Islamic jurisprudence, i.e. the investigation of sacred Islamic texts which are the sources of *sharia*. These texts include the Qur'an, the Traditions, the fiqh books of authoritative scholars filled with casuistry and reasoning by analogy. The research methodology has been developed by the experts (*fuqaha*) of jurisprudence (*fiqh*).

A second question is: how have national legal systems of Muslim countries dealt with the *sharia*. Sub-questions are: which position and role has been attributed to *sharia* within national law? What are the contents of national *sharia*-based law? How do we assess the present *sharia*-based laws in terms of rule of law standards? This calls for investigation of constitutions, national and sub-national laws as well as administrative decisions and case law of higher and lower courts, as well as treaties such as human rights conventions.

A third question relating to the compatibility issue is how do political forces influence the processes of shaping, interpreting, fixing, mixing, invoking, adopting and enforcing the rules of *sharia* as well as of national law? An empirical investigation of such forces coupled with a study of political documents may explain whether (and when, where, to what extent and how) *sharia*-based law has been made compatible with the rule of law. It also helps to understand fundamental dilemmas of governance in the Muslim world.

A fourth question is about how practices and ideas of social groups and individuals are shaped by rules of *sharia*, national law or international law, and vice versa. Empirical socio-legal research will shed light on people's practices, motives, needs and attitudes toward different normative systems. This requires qualitative methods of anthropological research coupled, if possible, with quantitative surveys.

A fifth question concerns the historical dimension: how has the relation between *sharia* and the rule of law evolved over time? Have legal systems of Muslim countries indeed been incorporated by extreme *sharia* over the last 25 years, as Marshall claimed, and moved away from the rule of law?

Each of these five questions leads us to a different academic domain. While I suggest approaching the compatibility issue in this fivefold interdisciplinary way, within the limitation of this article, I will pay special attention to the second question, concerning law.

Islamic jurisprudence

The study of religious texts has been treated above in the context of essentialism. It takes us, in the footsteps of the religious scholars, back to classical texts and traditional doctrines. After all, the most impressive development of *sharia* took place during the eighth and ninth centuries when scholars diligently expanded the body of rules until it was found sufficiently comprehensive and clear. Only then was the 'gate of free interpretation' (*ijtihad*) closed. Henceforth the scholars, many of whom specialised as *fiqh* experts (*fuqaha)* acting as legal scholar-adviser (*mufti*) or judge (*qadi*), were bound to the opinions of authoritative scholars of the first centuries, notably of the founders of the main *fiqh* schools. *Sharia* became rather stagnant and static. However, as a complement, rulers of early Muslim states, according to Islamic doctrine, could also issue laws based on their own discretionary power (*siyasa*). According to a well-

established *sharia* rule, they were allowed to enact laws (*qanun*) in the public interest *as long as these laws did not violate the sharia*. This rule established a wide space for state law besides religious law. So rulers could publicly adhere to *sharia* as the supreme law while making and enforcing their own state law at the same time.

Moreover, since the late nineteenth century, scholars increasingly began to question the closure of the gate of *ijtihad*. A growing number of *fuqaha* argued for new interpretations in a rapidly modernising world. In the twentieth century both intellectuals and political elites have appropriated the right to exercise free interpretation, 'neo-*ijtihad*' in the words of Coulson, professor of Islamic law at SOAS. This practice became of great importance to modernisation of national law. Reinterpretations of family law could thus be approved by moderate and progressive *ulama*, and then enacted by the state. This happened for example with the liberal family laws of Pakistan (1961), Indonesia (1974), Egypt (2000), and Morocco (2004). Yet modernist interpretations are often contested by conservative and orthodox scholars. The Saudi-based Hanbali school, for example, has promoted its conservative views throughout the Sunni world, causing regressive reforms, for example in Malaysia (1994). Thus, a brief excursion into this domain teaches us that, even though the emphasis is on texts and learned interpretations, *sharia* is more than static and stagnant. While an essentialist approach is useful in sensitising us to rules and practices that do *not* change, the potential for innovation warrants a multiplist approach to the compatibility question.

Law

Which position and role have been attributed to sharia within national law? The first place to look for an answer is the constitution. From the twelve constitutions we investigated, six had provisions which pronounced the *sharia* to be an eminent or major source of national law, and the other six had no such provisions. Five constitutions proclaim an 'Islamic state' and seven do not. Surprisingly, the research revealed that such constitutional provisions tell us very little about the actual position of *sharia*. The constitutional provisions of Egypt and Morocco suggest a bigger role for *sharia* than the constitutions of Nigeria and Sudan do, but an investigation into family law and criminal law of these countries shows the opposite. What we did learn from the comparison of constitutions is

that the normative foundation of most constitutions is twofold; both the constitution itself and the tenets of Islam or Islamic law are mentioned as supreme or basic. Such a dualist basic norm keeps the door open to a constant review of national law by *sharia* standards as well as a continuous review of *sharia* rules by constitutional standards. The latter usually refer to rule of law principles, including a host of human rights, enacted in the constitution. So, implicitly the dualist constitutions of most Muslim countries contain a foundational norm which says: the basic idea of this state is the compatibility of *sharia* and rule of law, but working out the details requires continuous review and negotiation. To be sure, in political and legal practice, state institutions usually have the final say in these reviews and negotiations.

Apart from the constitution, family law, inheritance law, criminal law, banking law and some scattered provisions, most areas of law have fairly little to do with *sharia*. So, state review bodies, as in Egypt for example, have declared this large part of legislation not to be in conflict with Islam or *sharia*.

What are the contents of national *sharia*-based laws? *Sharia* owes its negative reputation in the first place to its inhumane punishments, such as stoning to death for adultery and amputation of hands for theft, which are incompatible with international rule of law standards laid down in the 1984 UN Convention Against Torture. Our research demonstrates that these two punishments are legally in force in six of the twelve countries we examined. However, in these six countries they are never or very rarely[12] applied in practice, with the exception of Saudi-Arabia. The non-application rests on judicial policies of the supreme courts. Classical *sharia* also prescribes heavy punishments for apostasy. Seven out of twelve states have not made apostasy a crime under criminal law; three did, and in the laws of two countries, it is contested. Executions for apostasy do not take place at all in eight out of twelve countries; in two countries no executions have taken place in recent years, and in two other countries executions rarely occur.[13]

The significance of the aforementioned *sharia*-based criminal provisions is largely political and symbolic. They signal religiosity of the incumbent regime, an anti-western stance, and a physical threat to the opposition.

The second aspect of *sharia*'s negative connotation can be found in marriage law, especially the status of women. We see in most countries,

generally speaking, a gradual trend of liberalisation. The change, though, has been quite slow and cautious, and with some instances of regression as in Iran (1979) and Malaysia (1994). In five countries, unilateral repudiation of a wife is still permitted without any interference by the state, while in seven countries the state's role as a guardian of women's rights has been enhanced by law. Concerning rights of women to initiate divorce from their husbands, in six countries such rights were considerably extended by national law over the last decades. In Pakistan, for example, a remarkable emancipation was facilitated by liberal judge-made law.[14] In all country studies legislators, administrators and judges play a vital role in shaping national law by mediating between rule of law, *sharia* and custom.

How do we assess the present sharia-based laws by rule of law standards? We have seen that in most areas of law incompatibility is not an issue. As for the procedural elements of the rule of law that we mostly find in constitutions, compatibility prevails. Such procedural elements include the principle that laws are written in a clear and consistent way, that government action must be based on law, and that laws must be based on the democratic consent of an elected parliament. *Sharia*, in most common interpretations, has no problems with this. The rule of law also comprises control mechanisms such as independent judiciaries, ombudsmen and human rights commissions. Most Muslim countries have laid down such principles in their constitution. Again, we see no major incompatibility with *sharia*. However, in 'delicate areas' of the law, where *sharia* and the rule of law might actually conflict, the principle of clarity and consistency of laws is often sacrificed to the perceived need of vagueness and the ambiguity of politico-legal compromise.

Substantive elements of the rule of law are fundamental principles of justice, and human rights, including civil and political rights, social and economic rights and group rights. According to in-depth research by Mayer there are four key areas of conflict between conservative interpretations of *sharia* and human rights: (1) severe corporal punishment which is in conflict with the right of humane treatment; (2) hierarchical distinction between men and women; (3) heavy penalties for apostasy and forms of blasphemy which is in conflict with the right to freedom of religion; (4) a hierarchical distinction between Muslims and non-Muslims. As for the first three subjects, our research demonstrated

that long term trends suggest convergence between recent *sharia* interpretations and the rule of law. The fourth topic, the position of non-Muslims, could unfortunately not be included in our project. Further research is recommended.

The twelve country reports confirm Mayer's observation that since the 1990s the governments of Muslim countries have abandoned their previously dismissive attitude towards the human rights standards of these treaties. Eight out of the twelve countries acceded to the 1966 International Convention on Civil and Political Rights. Nine out of twelve acceded to the 1984 Convention against Torture, and other Cruel, Inhuman and Degrading Treatment or Punishment. Ten out of twelve acceded to the 1979 Convention on the Elimination of All Forms of Discrimination against Women. The periodical reporting by Muslim countries under these treaties, the human rights declarations of international Muslim organisations themselves, the inclusion of human rights in national constitutions and the creation of national human rights commissions have all contributed to the legitimacy of human rights in the Muslim world. However, national governments also take into account political and social feasibility. Implementation of human rights in national law has often been contested by conservative and orthodox groups. So, for the time being some more or less serious contradictions between *sharia*-based law and human rights continue to exist especially with respect to the four above-mentioned points identified by Mayer.

As is evident from the above, our research has shown that the degree of compatibility of *sharia* with the rule of law definitely differs per country. In all areas of law Saudi Arabia, and to a lesser extent Iran, show the highest degree of incompatibility. Turkey's national law is secular, so compatibility with *sharia* is no issue. The other nine countries belong to a large middle group. Within this group Indonesia and Mali are more oriented to secularism, Sudan, Pakistan and Afghanistan tend more to classical *sharia*, and Egypt, Morocco, Malaysia and Nigeria are in the middle of the middle group.

Political science

Present relations between *sharia* and rule of law result from power configurations of state institutions, religious leadership and strategic social groups. The latter include feminists, human rights lawyers, intellectuals

and radical Islamists. The status of *sharia* under national law has been a dominant political issue throughout the history of Islam. Political forces in Muslim states can be arranged along an ideological-religious spectrum of secularists, modernists, moderates, conservatives, orthodox and radical Islamists. At present, conservative and orthodox forces rule in Saudi Arabia and Iran. But governments of Muslim countries are rather moderate and modernist. Also, in these countries the 'historical compromise' between state and religion, between national law and *sharia,* that we found, usually contains two major elements. First is the interpretation of the *sharia* which allows the state its own regulatory space (*siyasa*) as long as it does not contravene the *sharia*. Second is the acceptance by the clergy of the fact that state institutions exert the power to appoint officials who will advise and make binding decisions about what is and is not allowed in this space.

When Islamists have the potential to rise to power, governments of Muslim states face serious dilemmas of governance. Should they allow Islamist movements to form religious political parties and participate in elections for the sake of democracy or pursue an authoritarian course for the sake of stability and rights of women and minorities? Should they co-opt the conservatives and increase the role of *sharia* in law to take the wind out of their opponents' sails, or pursue a course of secular unification and modernisation? Beside their development policies, dealing with problems of insecurity, poverty, unemployment, and illiteracy, they must walk the tightrope to maintain stability on the *sharia* front.

In the Muslim countries of the large middle group ministries of religious affairs promote the spread of moderate interpretations of *sharia* and try to block or restrain the rise of radical, puritanical versions. At the same time they often promote a substantive role for the *sharia*. The position of *sharia* within national law can be regarded as the outcome of political and bureaucratic competition, clashes and compromise, an outcome that differs in each country.

Sociology and anthropology

The fourth domain addresses the ideas and behaviour of commoners. How do ordinary people view norms of *sharia* as well as of the rule of law, and how do they use them in their livelihood strategies and when they are involved in conflicts and disputes?

Most societies in the developing world live under normative pluralism and lack of consensus. In settling their disputes Muslims can invoke norms from various strands of Islam, Sufism, tribal custom of extended families, national law or international human rights. Often none of these normative systems offers full predictability and certainty. In the words of the American anthropologist Bowen, who did extensive field research in Aceh, 'the outcomes of … cases … are not predictable on the basis of norms …'.[15] Moreover, corruption can be rampant in state institutions but also in non-state institutions.

In the end, the main concern of ordinary people is with their livelihood, their basic needs. National politicians declare incompatibility of *sharia* and the rule of law, often they couldn't care less. To fulfil their needs, people, especially in rural areas, want pragmatic solutions that work in given socio-political contexts. When, as in Afghanistan, the rules of *sharia*, in whatever interpretation, are more advantageous to women than the rules of tribal custom, they prefer *sharia*. When in the West Bank the religious courts are the only ones which work, Palestinian people value these courts above state courts.

History

An important finding of our study is that the Islamisation which occurred between 1972 and 1985 and which introduced versions of *sharia* which were less compatible with the rule of law, did not have a domino effect on the laws of most Muslim countries. This seemed counter-intuitive to us and to most people who developed their opinion through press reports. The findings are clear, however, the most plausible explanation of a positive trend enhancing the compatibility between *sharia* and the rule of law is a historical one.

Over the last 150 years the Muslim world has witnessed the formation of national states led by new political elites, socio-economic development, and modernisation. In this context national legal systems have gradually been constructed. Since the mid-nineteenth century professional, national state law has been on the rise. Sharia rules were for the first time codified and thus appropriated by the state. Henceforth their meanings were interpreted by state officials and judges instead of by purely religious scholars. This led to a gradual displacement of religious authority.

This trend has been most pronounced after World War II when all

former colonies subscribed to an ideology of 'development' and their legal systems underwent modernisation, unification and secularisation, often in the name of socialism.

In the 1970s, several regimes of Muslim countries opted for a return to *sharia*. This was most dramatically manifest in the 1979 Islamic revolution in Iran, but there were similar changes in Libya (1972), Pakistan (1979), Sudan (1983), Afghanistan under the Taliban (1994), and Northern Nigeria (2000). Western world public opinion was shocked by press reports about several incidents which reinforced the image of *sharia* as a cruel and 'barbaric' system. Popular enthusiasm for the reforms in the Muslim world, however, did not last long. The country reports show that the core countries of Islamic revolution like Iran and Pakistan – as well as Libya – have retreated in several respects to a more moderate position. The ebb and flow of religious activism and nationalist unification have caused a permanent search and struggle of social and political groups trying to determine a collective identity and gain power. In the long run state formation and socio-economic progress exert a decisively modernising influence on law and *sharia*. The pursuit of modernisation, with its universal 'development goals', including the emancipation of women, is common and most likely permanent across the Muslim world.

Concluding observations

Is 'the' *sharia* compatible with 'the' rule of law which includes democracy and human rights? We have identified the different perspectives and positions in the Muslim world as well as in the West. Essentialist authors like Huntington or Marshall and conservative Muslims will answer this question with a plain 'no'. For them, the *sharia* is part of a dangerous and destructive overall program of Islamisation, which conflicts with 'western values such as the rule of law'. But they miss a vital part of the real debate by overlooking that in practice the concept of *sharia* has different versions and meanings. Many rules of the classical, medieval versions are indeed incompatible with most elements of today's rule-of-law concepts. But if we look at the numerous modernist interpretations in the contemporary national laws of Muslim countries, we see many versions of *sharia* which are quite compatible with common rule of law concepts.

Some argue that the works of authoritative Islamic scholars (ulama) reveal their predominantly conservative doctrines, and that liberal

versions are supported only by a few modernist intellectuals. But such an argument overlooks the influence of three other groups of actors in the Muslim world: first, the state with its parliaments, administration and courts, secondly the pragmatic silent majority in most Muslim countries, and thirdly the professional jurists, many of whom adhere to the rule of law. Admittedly, in the legal systems of most Muslim countries a certain degree of incompatibility with the rule of law remains, which is different for each country and topic, depending on the seriousness, the most problematic areas being non-discrimination and freedom of religion. However, the trends of legal development in the twelve countries researched, show that the degrees of incompatibility vary and have been decreasing, although with stops and starts, in a long-term, incremental process. Marshall may be right about Islamisation of law in the last 25 years, but this period includes the 1979-1983 events in Iran, Pakistan, and Sudan. If we take the 1985-2005 period, we see that the first wave of this Islamisation did not really persist.

A purely essentialist analysis of sacred texts assuming that *sharia* is a fixed body of rules determining the behaviour of all Muslims and in unbridgeable conflict with the rule of law is unrealistic and insufficient. In order to disentangle the compatibility problem and to determine where precisely we find areas of conflict, what is the nature of such conflict and to which extent and under what conditions the conflict can be solved, we need solid knowledge from different academic domains. The study of Islamic jurisprudence offers an important point of departure which needs to be coupled with the study of law as well as empirical research in the social sciences.

Essentialists have often tried to stereotype their multiplist colleagues as 'cultural relativists' and themselves as 'universalist' defenders of human rights. This dichotomy is outdated and not very helpful when it comes to studying legal development in Muslim countries. Most multiplists show a clear preference for the rule of law and human rights, but because of their broader knowledge, their judgments of the best way to get there differ from their essentialist colleagues' opinions. Multiplists strongly disapprove of cultural clichés leading to blunt, confrontational approaches which they deem to be counter-productive. They see the enthusiastic atheist vanguard in the fight against Islam as bulls in a china shop. Most multiplists are convinced that a more sophisticated approach should be

possible based on sound knowledge of legal, political and social practices.

Since the 1970s international human rights policies have strengthened human rights and their defenders in most countries, also in the Muslim world. In the early 1990s even the governments of Saudi Arabia and Iran joined in this global trend. Brems has theorised about how to further pursue this trend and proposed a policy of 'inclusive universality' which aims at making human rights legitimate for people everywhere, including the Muslim world.[16] Such policy should encompass an open approach of religious diversity. It should not too soon invoke 'the incompatibility of the *sharia* with the rule of law' since that would be a *de facto* acceptance of conservative definitions of *sharia*. It should also accommodate criticisms of double standards and hypocrisy in Western foreign policy.[17] Such constructive policy must not be unconditional, however, and can succeed only if Muslim countries actually show that they are actually striving towards implementation of human rights, notably on the four difficult points discussed above. Yet, Western policy-makers should also learn to understand the governance dilemmas of Muslim governments. This means they must realise that progressive laws can also strike back at society like a boomerang; they should acknowledge the need in Muslim countries for symbols of Islamic belief, culture and national consciousness in the search for a collective identity; they should recognise the permanent danger of security threats by radical Islamists to Muslim governments as well as to the West and respond with caution and determination to both. Especially with the present unstable political climate due to the wars in Afghanistan and Iraq they should understand that Western models of liberalisation have, at least temporarily, lost much of their appeal and that quick, unambiguous solutions to the remaining conflicts between *sharia* and the rule of law cannot be expected.[18]

In summary, the best human rights policy toward the Muslim world is persistent, inclusive, incremental, pragmatic, and based on a flexible universalism that takes context realistically into account. To be effective, such rule of law promotion should be informed by and responsive to all major groups who take part in the ever-changing debate about *sharia* and the rule of law within the Muslim world.

Notes

The author would like to thank Theo Veenkamp, Benjamin van Rooij, Anne Saab and Miriam Otto for their useful comments and editorial suggestions about earlier versions of this article.

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10
Should we 'teach the controversy'? Intelligent Design, science and religion

Willem B. Drees

All theories are equal, but some are more equal than others.[1]
(after George Orwell, *Animal Farm*)

Dilemma: Openness and Selectivity

All ideas merit equal consideration. Criticism of dominant theories should be unhampered by considerations of authority or seniority. The competition of ideas drives scientific progress. The openness of science for criticism serves also as an example of sound conduct. Political and academic freedom strengthen each other.

The flat Earth is gone for ever, and so are phlogiston and stable continents. Science is selective. Of the many theories floated, only a few have survived criticism so far. 'Standing on the shoulders of giants', as Isaac Newton is supposed to have said, allows us to see farther than our predecessors, not because we are smarter but because we have come to accept their insights. Science is a cumulative enterprise, resulting in progress because of all the mistakes that have been weeded out. Not all ideas have equal standing.

The philosopher Gilbert Ryle introduced in 1966 in his book *Dilemmas* dilemmas as two lines of thought which seem irreconcilable while for each one there are good reasons. In the competition for scientific grants researchers often face the dilemma between aiming at an innovative proposal or one that extends approaches already well established – a

dilemma between originality and openness on the one hand and credibility and selectivity on the other.

The dilemma between openness and selectivity plays a role in controversies over Intelligent Design (ID) as an alternative to evolutionary biology. Advocates of allowing ID a place in the science curriculum argue that this is an issue of fairness and openmindedness. They suggest that the case for ID might be similar to examples of scientific proposals that were dismissed or marginal at first but became successful in the end. Evolutionists are criticised as falling short of the scientific ideal of open, critical discussion. Thus, advocates of ID now have websites such as www.standupforscience.com and www.teachdarwinhonestly.com; they portray themselves as the true advocates of the scientific attitude of open, critical discussion.

The controversy over ID is not just about science. It is also about room for religious beliefs in relation to the practice of science. Some advocates of ID claim that their scientific work does not get a fair hearing *because of* their religious beliefs – and thus, that the issue is also a matter of freedom of religion. That their religious convictions play a role in the reception of their ideas should not have surprised them, as the connection between the scientific programme and the religious agenda has been made by some of their major advocates. In 1999, in an internal strategic document of the Discovery Institute, the argument against neo-Darwinism has been presented as 'the thin edge of a wedge' to change materialist science and materialist culture at large.[2]

In the later part of this paper we will return to some of the general issues. How about *openness* as an attitude which allows for religiously inspired approaches, and the acceptance of *selective* pressures resulting in the abandonment of some approaches? However, we'll begin with a closer look at controversies over ID, especially in relation to pleas for 'balanced treatment', 'teaching the controversy', and 'encouraging critical analysis'.

'Teach the Controversy'

In the fall of 2004, a school board in Dover, Pennsylvania, adopted a statement to be read to children in secondary school at the beginning of biology courses on evolution.[3]

The Pennsylvania Academic Standards require students to learn

> about Darwin's Theory of Evolution and eventually to take a standardized test of which evolution is a part.
>
> Because Darwin's Theory is a theory, it continues to be tested as new evidence is discovered. The Theory is not a fact. Gaps in the Theory exist for which there is no evidence. A theory is defined as a well-tested explanation that unifies a broad range of observations.
>
> Intelligent Design is an explanation of the origin of life that differs from Darwin's view. The reference book, *Of Pandas and People*, is available for students who might be interested in gaining an understanding of what Intelligent Design actually involves.

With respect to any theory, students are encouraged to keep an open mind. The school leaves discussion of the Origins of Life to individual students and their families. As a Standards-driven district, class instruction focuses upon preparing students to achieve proficiency on Standards-based assessments.

This statement seems fairly innocent. There is an occasional unhappy formulation ('Gaps … for which there is no evidence'), but what arguments could there be against continuous testing of theories, considering an alternative point of view, and keeping an open mind? The American president George W. Bush, too, has taken the position that 'both sides ought to be properly taught … so people can understand what the debate is about'.[4]

'Teach the Controversy' is a phrase used in the USA in arguing for the inclusion of Intelligent Design in the biology curriculum. It is an expression that may confuse liberals who have no sympathy for the anti-evolution movement but who do believe strongly in freedom of expression and related rights. Is academic freedom as currently practised genuine freedom if the dominant understanding of such freedom excludes religious people?

Openness is not sought for its own sake, but in the expectation that a particular result would follow. If only the dogmatism of the evolutionists could be replaced by genuine openness, the truth (as the faithful see it) would come out victoriously. As a leader of the ID movement, Philip Johnson, writes: 'If we get an unbiased scientific process started, we can

have confidence that it will bring us closer to the truth'. Evolutionary naturalism is like a great battleship, 'armored with philosophical and legal barriers to criticism, and its decks are stacked with 16-inch rhetorical guns to intimidate would-be attackers'.[5] The evolutionists, according to the criticism, resort to legal defences rather than exhibit genuine openness.

A Brief Legal History

The vocabulary of fairness, occasionally treated quantitatively in terms of equal time in curricula, has been stimulated by the legal history of American controversies over evolution. Let us briefly trace the trajectory from the prohibition of evolution to the most recent incarnation, the plea for 'critical analysis of evolution'.

In 1925 the State of Tennessee legislated that it would be unlawful for state-supported schools 'to teach any theory that denies the story of Divine Creation of man as taught in the Bible, and to teach instead that man has descended from a lower order of animals'.[6] On the initiative of the American Civil Liberties Union the law was tested by having a teacher, Scopes, transgress the law. He was convicted and fined. The appeal court upheld the conviction (and the validity of the law), but revoked the fine on the basis of a technical error. This law and similar ones in some other states led to avoidance of 'evolution' in textbooks. Of interest for us may be the observation that in the Scopes Trial the argument that both views ought to be taught came from an attorney defending the right to teach evolution.[7]

By the 1960s the situation had changed considerably. A new biology curriculum had given evolution a prominent place. In 1968 the Supreme Court struck down the Arkansas law prohibiting the teaching of evolution. Concerned parents found refuge in scientific creationism or creation-science – a version of creationism that presented itself as being based on scientific, empirical arguments. This change of label suited the legal situation as this allowed the creationist view to be presented more easily as an alternative scientific theory rather than as a religious point of view.

In 1981 Arkansas passed a law that self-described its purpose as 'An act to require *balanced treatment* of creation-science and evolution-science in public schools; to protect academic freedom by providing student choice;

… to bar discrimination on the basis of creationist or evolutionist belief'.[8] This law was challenged in court by parents and teachers, as well as by bishops of the United Methodist, Episcopal, Roman Catholic and African Methodist Episcopal Churches, the principal representative of the Presbyterian Churches in America, United Methodist, Southern Baptist and Presbyterian clergy, three types of Jewish organisations, and various mainstream religious organisations. The judge, William R. Overton, declared the law unconstitutional. 'Creation-science' was deemed a religious rather than a scientific position. Thus, the law favoured a particular religious position, which was against the separation of Church and State.[9]

The main shift, broadly speaking, between this - failed - Arkansas law and the decision of the School Board in Dover on the disclaimer for the biology lessons is in the formulation of the alternative for evolution. 'Creation science' has been replaced by 'Intelligent Design'. In the ruling on 'Dover', Judge Jones argued at length that this would not do, as a well-informed observer would recognise that the presentation of Intelligent Design as an alternative and the reference to gaps in evolutionary theory are particular creationist strategies.[10]

The phrase 'teach the controversy' may already have passed its peak, as the Dover ruling described 'teach the controversy' as part of the same religious tactics. Thus, a more indirect strategy now seems to be rising, namely '*critical analysis of evolution*', advocating that arguments against evolutionary explanations are taught (without invoking 'intelligent design' as an alternative view). The Discovery Institute has a one-page document on its website, titled 'Is Critical Analysis of Evolution the Same as Teaching Intelligent Design', a question they answer negatively. The educational approach is logically distinct, school districts and states that sanctioned 'critical analysis of evolution' have explicit disclaimers that this policy does not call for teaching ID, scientific critique is legally distinct from teaching alternative theories, and some critics of Darwinism do not support ID. Last but not least, the Darwinists so far have not filed a lawsuit, which they would have done if they had believed 'critical analysis of evolution' was the same as teaching ID.

Not all its advocates, however, keep 'critical analysis' and ID fully apart. Joel Borofsky, who serves as assistant to William Dembski, a major author in the Intelligent Design movement, said when 'critical analysis' was an issue in Kansas in response to the concern of a person religiously

interested in ID that critical analysis had nothing to do with ID: 'It is really ID in disguise'. And Karen Floyd, the Republican candidate in South Carolina for the position of State Superintendent for Education, connected Intelligent Design and critical analysis of evolution as well, and reached even further back, to Young Earth Creationism: 'Students are smart, she said, and they connect the dots: Some will wonder: "How many dinosaurs boarded Noah's Ark?"'[11]

Why evolution? The hermeneutics of intent

If the dominance of a particular theory were the point, not only would the theory of evolution deserve to be challenged, but so, too, would the Periodic Table of Elements in chemistry and Maxwell's electromagnetism in physics. Pleas for balanced treatment, teaching the controversy, and critical analysis, time and again single out evolution. This continuous focus on biology is intelligible in light of the symbolic function which both evolutionary theory and the argument from design have acquired in the course of the last few centuries. Evolution has become a symbol that stands not just for a particular scientific theory, but for modernity in general, including historical approaches to religious scriptures and liberal family values. It seems that this continuous focus on biology has brought Judge Jones to the conclusion that a reasonably informed observer would recognise behind the wording of the Dover disclaimer a long standing religious argument.[12]

The argument about the Dover disclaimer is hermeneutical in kind. The conclusion does not show up by looking up in a dictionary the meaning of the words used, but requires consideration of the meanings of these phrases when used in this particular context of teachers, parents and students. When the opening sentence of the Dover disclaimer tells students that we have to teach evolution because the state's academic standards require it, this is taken to signal that evolution is not taught because it is taken to be the most adequate theory. Such a hermeneutical reading of the interaction is, of course, context-dependent. Thus, if the issue were to arise whether to teach ID in an atmosphere where evolution had not been singled out as the centrepiece of a religious argument, the judicial argument based on legislative intent would not have held. That is why more secular liberals in a European context may miss why this disclaimer is so controversial. In this perspective, it is strangely odd that

Steve Fuller – a philosopher of science who places great emphasis on social conditions – denied in his testimony in favour of the Dover disclaimer that the religious motivations surrounding ID are of any significance. He did so by distinguishing between the context of discovery and the context of justification, where biographical considerations are irrelevant. That may be adequate when assessing the truth or plausibility of the theory, but in an educational setting, meanings that particular educational choices have for persons involved, are relevant.

Marginal science that became successful?

Let us consider a different issue. Should ID not be compared to other theories that at one point were marginal? This is not a sufficient argument in a plea for the curriculum at the level of secondary schools; there was no reason to teach Wegener's drifting continents before a modified form of this idea was accepted by the scientific community. But if we concentrate for the time being on the scientific standing of ID and pleas for more opportunities in the scientific sphere, what about the analogy between ID and scientific ideas that once were marginal? To answer this question, Matthew J. Brauer, Barbara Forrest and Steven G. Gey have taken a look at some controversies.

They initially consider 'endosymbiosis', the idea advocated first by Lynn Margulis that mitochondria and some other organelles in eukaryotic cells have not evolved within the cell, but rather go back to independent cells that have been incorporated into the eukaryotic cell. This theory does not fit the model of shifting frequencies in the gene pool. The idea was rejected for many years. 'However, initial scepticism was overcome by several strongly suggestive observations. … In the light of such evidence, all of her former critics have been won over. Her hypothesis was not a vague statement of the inadequacy of current evolutionary science to explain certain patterns (though this would in some sense have been true). Rather, it was a robust statement, entailing a definite outcome that could be and was tested'.[13]

The idea that AIDS is caused by a virus, HIV, has also been controversial. However, this debate had a quite different development. Peter Duesberg published in 1987 a paper stating that HIV was a benign 'passenger virus'. At that point this was still an option, as not much was known about AIDS at the time. However, soon thereafter the majority of

the scientific community found that the evidence for a causal link between HIV and AIDS had become convincing. By 1988, the controversy had escaped the boundaries of the scientific community, and Duesberg was using the popular press to attack his research colleagues as part of the 'AIDS establishment'. Duesberg and others who challenged the understanding of HIV were not successful among the scientific colleagues, but had a substantial public podium. 'Duesberg's HIV scepticism has not been a significant *scientific* controversy' and his ideas 'are clearly not significant enough to be part of a biology course'.[14]

The authors compare the impact of these two controversies to the impact of the ID literature. Research on citations shows remarkable differences: Margulis' work was referred to quite often, also before it was accepted; Duesberg's far less. Behe's work was referred to even less than Duesberg's. Besides, Behe is cited mainly 'in the context of the philosophical and cultural controversy'.[15] Searching for major ID-specific terms such as 'irreducible complexity' unearthed only a few publications. These differences in the reception by the scientific community do not show that the ID position is wrong. What it does indicate, however, is that there is no genuine scientific controversy over ID, but rather a cultural and political one, which undermines significantly the legal demand for attention in science curricula.

Rather than aligning themselves with Duesberg (though there are some direct links) pleas for 'teaching the controversy' have included references to global warming, stem cell research and cloning.[16] However, controversies over cloning and stem cell research are of a moral rather than a scientific nature. 'Global warming' seems closer to the mark, but there, too, dissident views over global warming are largely motivated by dissident policy views. There is less of a scientific controversy than a debate about the relative role of uncertainties in modelling and measuring changes, which alas allows policy-makers to play down the message.[17]

'Teach the controversy in science class', is the demand, but what if the controversy is not a scientific one but rather a cultural, political or religious one? And if one wants to be generous and one considers ID to be an immature science, why then give it already a podium in secondary schools? Academic freedom, freedom of speech and freedom of religion are important, but they do not require a podium of this kind to be provided.

Which philosophy of science? Popper and Kuhn

In the Arkansas trial of 1981-1982, the philosopher of science Michael Ruse testified that science must 'be, at least, explanatory, testable, and tentative'.[18] He admits that scientists have come to accept certain theories as reliable, but in principle these theories too can be challenged. If theories are falsified, scientists will try to fix the problems and thus stick to their theoretical framework as much as possible. If the fixes are too *ad hoc*, and if a successful alternative theory surfaces, the time has come to accept the alternative theory. Ruse refers to Karl R. Popper, but to some extent these additions make Ruse's description of science resemble Imre Lakatos's 'methodology of scientific research programmes', which is a version that is Popperian at heart but gives much more room to the tenacity with which researchers stick to a theory or rather, a research programme.

Ruse argues that evolutionary science is falsifiable, and would have been falsified if the fossils had been distributed more or less randomly over the geological column or if there had been no genetic similarities as one would expect in a theory of common descent. By the same standards, he considers creation science not as scientific, since there is no explanatory theory that makes specific predictions, creation science claims certainty rather than tentativeness, and is not testable. After the trial, a fellow philosopher, Larry Laudan, challenged Ruse on this point. Laudan argued that creationism was testable, had been tested, and had been found false. Ruse replied that the legal situation did not allow this strategy, as the First Amendment to the American Constitution bars the teaching of religion in public schools, but it does not bar the teaching of outdated science.[19]

In the Dover trial, expert witness Steve Fuller also drew upon Popper, but in this case to argue *for* the teaching of ID.[20] This may be explained to some extent by differences between ID and creation science. Partly it is also the wording of the Dover disclaimer, which stresses the provisional character of Darwin's theory and the explanatory intent of ID, and ends with the encouragement to keep an open mind. But the main difference between Ruse and Fuller may be not in these redefinitions of the anti-evolutionary position, but in the aspects taken from Popper. Fuller focuses on the social conditions that make for scientific theories. He stresses the *Open Society*, as the world in which there is an open competition of ideas. In focusing on this side of Popper, rather than on falsification and the

abandonment of theories, he puts ID in the role of the underdog.

Thomas S. Kuhn, however, is the favoured philosopher of science for most ID theorists. There are two sides to this appeal to Kuhn. One is the language of *a theory in crisis*, as the subtitle of a major book in this mood has been, which suggests that the time has come for a revolution. The other approach is rather stressing the role of paradigmatic assumptions that are themselves beyond science. If the assumptions of evolutionary theorists are different from those of ID theorists, the two might be equally legitimate, the choice being beyond empirical science. In this second way of appealing to Kuhn, the equal opportunity claim comes out best. Another author in this vein wrote: 'Those who reject the "teach the controversy" model on the grounds that ID violates the current rules of scientific practice only beg the question. The present regime of methodological rules cannot prevent controversy for the simple reason that those rules may themselves be one of the subjects of scientific controversy'.[21]

In an extensive book against creationism and ID, Pennock points out that Kuhn is not as much the relativist as he has been made out to be, since for him theory judgement was not so much a matter of taste but rather a matter of judgement, weighing the options according to certain general values that themselves were shared by advocates of various paradigms.[22] Kuhn has been read widely as treating sciences as if they are humanities, a domain of co-existing incompatible paradigms or schools. This comes through in the concluding paragraph of the introduction to a book pleading for 'teaching the controversy', when three options are presented, namely teaching evolution 'dogmatically and intelligently', avoiding the topic, and 'teach it in the spirit of the humanities as the current reigning, though contestable, theory and thereby honour in science education the integrity of informed dissenting opinion that grounds our American tradition of unity in diversity within politics, religion, and culture'.[23] Pluralism in the social sphere is transferred to the scientific sphere.

The competition of ideas

In his book *Abusing Science*: *The Case Against Creationism*, Philip Kitcher comes to discuss the claim to openness in a chapter titled 'exploiting tolerance'. Openness to new ideas is a good thing. However, 'ideas have to

earn the right to our respect. Not every wild speculation deserves our detailed scrutiny. Not every crackbrained proposal merits a place in our curriculum'.[24] What criteria should we use? Does creationism satisfy those criteria?

Kitcher suggests that for the purpose of analysis, we distinguish three categories of scientific claims. Some claims, though perhaps not free of all problems, are more strongly confirmed than any alternative. Some may be less successful, but deserve our attention because they work well for a particular problem. These one would not call true, but they are of interest as they have some promise in guiding research. Thirdly, there is the remainder, claims that currently offer no solution to interesting problems. If the scientific community has to divide its labour, the larger amount of its resources would be expected to be invested in the context of the most promising theory, but some resources would be appropriately directed towards ideas in the second category, seeking to actualise their potential promise. Furthermore, there is reason to reconsider from time to time the categorisation of theories.

Alfred Wegener's theory in geology (1915), that suggested that our current continents came from a single landmass that broke apart, would have been in the second category: problematic but interesting. A few continued to develop the theory. After half a century, a revised version of Wegener's continental drift emerged, now in terms of plate tectonics, and with it the evidence that moved the idea up to wide acceptance.

If couched in these terms, 'scientific creationism' never made it to the middle category, worthy of serious consideration. It did not solve any problems, and it offered no predictions or other indications of advances in knowledge. At first sight, ID might have been different. For instance, Michael Behe's explorations on 'irreducible complexity' could have done a useful job by signalling significant puzzles though not by offering an explanation. However, no genuine cases so far seem to have survived closer scrutiny,[25] thus, making it more an example of the third than of the second category. Furthermore, the ID theory is almost exclusively a theory of the gaps, rather than offering original scientific explanations.

Kitcher also comes to consider the suggestion, advocated by Fuller with respect to ID, that it is too early. 'Finally, there is no excusing on the grounds that its resources are, as yet, untapped. Ample opportunity has been provided. Numerous talented scientists of the eighteenth and early nineteenth centuries tried Creationism. Nothing has come of their

efforts, or the efforts of their modern successors. Where the appeal to evidence fails completely, the appeal to tolerance cannot succeed'.[26]

Freedom of religion and the neutrality of the state

Whereas democratic states under a rule of law almost always have explicit stipulations regarding the freedom to practise one's religion (up to the point were this obstructs the freedom of others, of course), the scientific community has not only a certain indebtedness to the general principle of freedom *for* religion, but also a basic sentiment of freedom *from* religion. This created a fruitful framework for scientific research and assessment.

Let us begin with the idea that the state and the scientific curriculum should be neutral with respect to religious positions. This seems fine, but what if a particular scientific theory is deemed to violate religious sensibilities (as evolution apparently does)? The government takes positions in many different ways. And positions considered religiously relevant by their adherents are all over the map. Thus, all-encompassing neutrality is impossible. Think of religious groups that reject vaccines and blood transfusions or that preach and practise pacifism, polygamy, a flat earth, self-mutilation, the possibility of flying without mechanical assistance, circumcision of young girls and boys, slavery, violence against blacks and Jews, or a theocratic government. Actions by the government may well violate sensibilities of adherents of some such groups. The government cannot be neutral, but neither is there a good reason why it ought to be. The government may impose a general law on all citizens, such as rules against racial violence, polygamy, or female circumcision. The government may also seek to convince people to make a certain behavioural change, e.g. by encouraging the use of vaccines.

If general neutrality is impossible and undesirable given certain widely shared moral sensibilities, the issue becomes more limited. Basically it seems that the line to be followed is that new policies should not be aimed at giving benefits to a particular religious group, nor should they create unreasonable disadvantages for a particular group. However, this, too, can be overdone as any reasonable course of action does, of course, have differential effects. It nonetheless is relevant to note that allowing ID a place in the curriculum would favour a particular form of monotheism over other beliefs.[27]

In the context of controversies over the teaching of evolution, parents

may argue that the evolutionary ideas that children learn at school do interfere with the message these parents want to give at home. However, the solution to introduce the religious views of those parents opens a Pandora's Box of all that would have to be added in order to satisfy all personal beliefs of the parents. The trick, which science has learned since the early period, is to aspire to a particular type of neutrality, one which opts out of debates on morals and metaphysics, at least, for science teachers in their professional role though not necessarily as citizens. Of course, to be completely at ease with this line, believers would be best advised to take the complementary approach, and opt out of statements that purport to be in the realm of science. From the variety of religious organisations represented among the plaintiffs in the Arkansas trial, it may be clear that many mainstream religious leaders and organisations have found ways to do so while remaining, at least to their own satisfaction, faithful to the tradition of their choice. In that sense, the controversy over ID and evolution is a major controversy *within* the religious communities.

Concluding remarks

The dilemma regarding openness and selectivity is a genuine one in science policy, in that there is a matter of judgement rather than rules where innovative prospects outweigh the risks of less tested approaches. As discussed above, the investment of resources in science is not a matter of equal treatment, but of judgement regarding the merits and prospects for various theories.

The dilemma applied to the controversies over evolution and Intelligent Design in various strategies (demands for balanced treatment, for teaching the controversy, or for critical analysis of evolution) is not serious. If ID is treated as alternative science in its infancy, there is no particular reason to give it much room in the curriculum in schools. However, as becomes clear when one pays attention to the meanings attached to the opposition to evolution, the controversy is a cultural and political one. Most importantly, it is an intra-religious controversy as to the nature of religious belief relative to scientific claims.

Let me return to the concept of a dilemma, as analysed by Gilbert Ryle. 'There often arise quarrels between theories, or more generally between lines of thought, which are not rival solutions of the same problem, but rather solutions or would-be solutions of different

problems, and which, none the less, seem to be irreconcilable with one another'. ID and evolution are not rival solutions to the same problem; ID is an attempt to root a metaphysical or religious view in scientific practice. It is in direct competition with a separation model such as alluded to above, but could be less antagonistic to evolutionary theory, were it not for the symbolic significance evolution has acquired in the course of our religious history of the last two centuries.

Notes

1 'All animals are equal but some animals are more equal than others' is the commandment that remained towards the end of George Orwell's *Animal Farm* (orig. 1945).
2 *The "Wedge" Document: "So What?"*, at http://www.discovery.org/scripts/viewDB/filesDB-download.php?id=349, accessed November 25, 2006.
3 *Kitzmiller et al. versus Dover Area School District, et al.,* Memorandum Opinion by Justice John E. Jones III, p. 2, at http://www.pamd.uscourts.gov/kitzmiller/kitzmiller_342.pdf , consulted on November 25, 2006; book title italicized by WBD.
4 August 1, 2005; quoted from *Time*, see http://www.time.com/time/nation/article/0,8599,1089733,00.html, accessed Nov. 25, 2006; see also: http://en.wikipedia.org/wiki/Teach_the_Controversy.
5 Johnson, Philip, 'Afterword: How to Sink a Battleship. A Call to Separate Materialist Philosophy from Empirical Science', in: William A. Dembski, ed., *Mere Creation: Science, Faith and Intelligent Design* (Downers Grove, IL. 1998) p. 453.
6 The law as quoted in: Numbers, Ronald L., *Darwinism Comes to America* (Cambridge, Mass. 1998) p. 77.
7 Numbers, *Darwinism*, p. 91.
8 Act 590 of 1981, General Assembly, State of Arkansas, in: Michael Ruse (ed.), *But Is It Science? The Philosophical Question in the Creation/Evolution Controversy* (Buffalo 1988) p. 283-286; emphasis added.
9 Judge William R. Overton, *McLean v. Arkansas, United States District Court Opinion* (Jan. 5, 1982), in: Ruse (ed.), *But Is It Science?*, p. 307-331.
10 *Kitzmiller v. Dover Area School Board District*, e.g., p. 18-35.
11 Borofsky: see http://en.wikipedia.org/wiki/Critical_Analysis-of_Evolution, accessed Dec. 5, 2006; Floyd, see http://www.charleston.net/assets/webPages/departmental/news/default_pf.aspx?NEWSID=116506, published November 2, 2006, accessed Dec. 5, 2006. Mrs Floyd was defeated by a margin of a few hundred votes in a state-wide election on November 7th, 2006.
12 Similarly, Wexler, Jay D., 'Intelligent Design and the First Amendment: A Response'. In: *Washington University Law Review* 84 (1, 2006) p. 63-98, e.g. p. 75; see also Numbers, Ronald L., *The Creationists: From Scientific Creationism to Intelligent Design* (Cambridge, Mass. 2006).
13 Brauer, Matthew J., Barbara Forrest, Steven G. Gey, 'Is It Science Yet?: Intelligent Design, Creationism and the Constitution', in: *Washington University Law Quarterly* 83 (1, 2005) p. 1-149, p. 78.
14 Ibidem, p. 79 and p. 80.
15 Ibidem, p. 81.

16 Ibidem, p. 79; for instance, Philip E. Johnson and Jonathan C. Wells of the Discovery Institute have supported the AIDS reappraisal movement (see www.virusmyth.net, s.v. 'The Group', accessed Dec. 5, 2006), and the publisher of Well's work on ID also published Duesberg's book. As for the preferred examples, see Wikipedia, s.v. 'Teach the Controversy', section 'Shift in strategy' (p. 3-5), and references therein.

17 Petersen, Arthur C., *Simulating Nature: A Philosophical Study of Computer Simulation Uncertainties and Their Role in Climate Science and Policy Advice* (Apeldoorn 2006).

18 Ruse, Michael, 'Witness Testimony Sheet', in: Ruse (ed.), *But Is It Science?*, p. 287-306, p. 301.

19 Laudan, Larry, 'Science at the Bar – Causes for Concern', in: Ruse (ed.), *But Is It Science?*, p. 351-355; Michael Ruse, 'Pro Judice', in: Ruse, *But Is It Science?*, p. 356-362; Laudan, Larry, 'More on Creationism', in: Ruse, *But Is It Science?*, p. 363-366.

20 Steve Fuller, professor of philosophy of science at Warwick University, testified on October 24, 2005 in the procedure against the disclaimer policy of the School Board in Dover, Pennsylvania; for transcripts of Fuller's testimony, see http: www.talkorigins.org/faqs/dover/day15am.html and, at the same website, day15am2.html, day15pm.html & day15pm2.html (consulted most recently on November 25, 2006).

21 Campbell, John Angus, 'Why Are We Still Debating Darwinism? Why Not Teach the Controversy?' In: John A. Campbell, Stephen C. Meyer (eds.), *Darwinism, Design, and Public Education* (Michigan State University Press, 2003) p. xxv.

22 Pennock, Robert T., *Tower of Babel: The Evidence Against the New Creationism* (Cambridge, Mass.: MIT Press, 1999) p. 206-214, esp. p. 208; similarly on rationality in Kuhn's theory of theory change, McMullin, Ernan, 'The Shaping of Scientific Rationality: Construction and Constraint', in: E. McMullin (ed.), *Construction and Constraint: The Shaping of Scientific Rationality* (Notre Dame, Ind. 1988) p. 1-47.

23 Campbell, 'Why Are We Still Debating Darwinism?', p. xxx.

24 Cambridge, Mass. 1982, p. 166; see also Kitcher, Philip, *The Advancement of Science: Science without Legend, Objectivity without Illusions* (Oxford 1993).

25 Pallen, Mark J., Nicholas J. Matzke, 'From *The Origin of Species* to the Origin of Bacterial Flagella'. In: *Nature Reviews Microbiology* 4 (10, October 2006) p. 784-790.

26 Kitcher, *Abusing Science,* p. 172.

27 Wexler, Jay D., 'Intelligent Design and the First Amendment: A Response'. In: *Washington University Law Review* 84 (2006), p. 63-98; Wexler's article, esp. p. 86ff. inspired the two preceding paragraphs.

11
Cracks in the cradle? On the quest for the origins of humankind

Wil Roebroeks

East Africa, two and a half million years ago: the distinctive sound of one stone being hit against another has been around for ages. Debris of this production of crude but very efficient stone tools is littered over the vast African plains, mostly around the edges of bodies of water, such as the shores of lakes and the banks of rivers. Water is a key resource in the African landscape, attracting a colourful variety of animals, both carnivores and herbivores, both hunters and prey. The tool-makers use razor-sharp flakes struck from cobbles of stone to butcher the large mammals that they managed to get access to, either by scavenging the remains of the kills of carnivores or by hunting small and/or weakened individuals. Their tools give them a cutting edge advantage over other meat eaters: when large carnivores have finished eating their prey, hominins can come in and use their heavy, sharply edged stone tools, from the cores of which they have struck the cutting flakes, to crack open the bones and feast on the nutritious marrow.

The earliest stone artifacts recovered thus far date to around 2.6 million years ago, when tool-making became an archaeologically visible part of the hominin behavioral repertoire. Tools did not make us 'human' though: when the first stone knapping debris was dropping to the ground, the last common ancestor of chimpanzees and humans had already disappeared from the globe about four million years earlier. As far as the evidence goes, the first stone artifacts – sharp-edged flakes and the cores from

which they came – were produced in Africa, but we have little idea which hominin species was (or even: were) responsible for the manufacture of these simple but efficient cutting implements. At the present state of our knowledge, at least two genera of hominins were present in East Africa at 2.6 Ma, encompassing at least six species.[1]

These stone artifacts are associated with the broken up and cut-marked remains of large mammals. The tools provided their makers with easier access to meat, fat and bone marrow, the food that was locked up in the bodies of large mammals roaming the grasslands of East Africa. These grasslands had emerged in Africa as a result of the gradual global cooling down of the earth, which began millions of years before the split between humans and chimpanzees.

At about 1.8 million years ago, the variety of African hominin species can be organised into two groups, which differ from each other in a number of aspects. It is in the second group that the ancestor of modern humankind is situated: this group displays a large body mass, a more modern human-like physique adapted to open types of environments, walks fully upright on two feet and has teeth whose shape are indicative of a modern human-like diet. The first group, that of the Australopithecines, is more 'ape'-like, the second is more 'like us' and is commonly referred to as *Homo ergaster*. This hominin was a proficient tool-maker and meat and animal fat formed an important part of its diet.

The first good evidence of this species comes from exposures near Lake Turkana, in northern Kenya, and dates to about 1.8 million years ago (but see below). The best example of this new body form is somewhat younger, and is provided by a 1.6 million years old skeleton of an adolescent known as Turkana boy. If this individual had reached maturity, he would have stood six feet tall, his brain would have been twice that of a chimpanzee and something over half of the modern human average.

Because of its large energy-demanding body and its higher position in the food chain, *Homo ergaster* needed significantly larger home ranges than its predecessors with more 'ape'-like, vegetarian diets. And as individuals needed larger home ranges, extension of the range of the species was to be expected.[2] Individuals of this species, large-bodied, big-brained and all tooled up, were ready to hit the road and leave Africa by about 1.8 million years ago, starting a tour that led to the colonization of major parts of the

Old World, of Eurasia. While early *Homo,* endowed with a typically insatiable human *Wanderlust,*[3] was conquering the world, *Australopithecus* stayed at home, in Africa, where its last representatives petered out around one and a half million years ago. Humanity's origin lies in Africa, the cradle of humankind, and from there our earliest ancestors dispersed all over the world.

Origins attract

At least, this is one of the main narratives of the field of palaeoanthropology and archaeology, commonly known as *Out of Africa 1* (as opposed to *Out of Africa 2*, the model according to which our own species, *H. sapiens*, evolved in Africa by about 150,000 to 200,000 years ago and then migrated outwards into Asia and Europe, replacing all the indigenous populations there and eventually colonizing the whole world). In western society, we have become so accustomed to these major stories in the domain of human evolution that they have become part of our cultural heritage. *Out of Africa 1*, or in more general terms: humanity's African roots, is certainly amongst these. In a world that is wrestling with many of its traditional grand stories, including biblical and other creation myths, these scientific narratives have a great appeal, and constitute one of the reasons why each year so many young students are ready to delve into the early humankind business. It is through these students that our discipline reproduces itself, and having attractive stories certainly increases the discipline's fitness. However, we do not create such narratives (or as we prefer to call them: hypotheses) to attract students, but in order to give meaning to the patterns we discern in our data, and this is not an easy enterprise, for a variety of reasons.

Palaeoanthropology and archaeology are not experimental disciplines. The fossil record is a very fragmentary one, biased by the complex interaction of haphazard geological preservation and accessibility, and by historical developments enabling research in some areas and prohibiting it in others. Spectacular finds in one fieldwork area can draw in large numbers of teams into that same region at the expense of other areas. Our sample size of past populations is extremely small in both time and space, as illustrated by the increase in the number of palaeospecies in the last two decades in Africa, as well as by the surprising archaeological discoveries

that have recently changed our view of the earliest occupation history of the Old World, including Europe.[4] Despite significant progress in dating techniques, our chronological resolution is usually pretty low, making it difficult to sort out what is cause and what is effect. Nor do these combined factors make it easy to identify core areas in which species originated and, by implication, the peripheries to which they subsequently spread. Hence, many of our theories are not easily refutable, and our literature is abounding in 'might have been' style models, narratives or stories, that try to give meaning to the stones and bones we unearth.

The *Out of Africa 1* narrative very much operates on one important assumption: that the fossil record is sufficiently robust to enable us to distinguish core from marginal areas, and to demonstrate adequately both the origin and subsequent dispersal of species. But is this a reasonable assumption? How strong is this narrative, how strong is its empirical backing? Could we have become so used to it through the mere power of repetition, that we forget that we are dealing with a *hypothesis* that needs to be continually tested? After all, for the first half of last century, eastern Asia was considered to be the cradle of humankind, and the *Homo erectus* fossils from Java (including Eugène Dubois' famous *Pithecanthropus*, see box) were seen as the first representatives of the genus *Homo*. The origin of humankind was relocated to Africa in the 1960s; around that time fieldwork in East Africa started yielding significant amounts of early hominin material, which was triggered by the pioneering activities of Louis and Mary Leakey in Olduvai Gorge, Tanzania. This relocation of the centre of human origins made the French prehistorian Abbé Henri Breuil comment that the cradle of humankind has above all to be seen as a cradle on wheels ('Le berceau de l'humanité est un berceau à roulettes').

In a recent review of the *Out of Africa 1* model, my colleague Robin Dennell and I have highlighted the main weaknesses of this particular hypothesis.[5] Since that review paper was published, two important studies have appeared which further undermine major pillars of *Out of Africa 1*. In this paper, I will review the weak points of this pet story of human evolution, and address whether there is a good alternative scenario available at this moment. While focusing on *Out of Africa 1, w*hat follows has to some degree implications for other fields of archaeology and

palaeoanthropology that deal with 'origin' studies, be it of early humans, of art, agriculture or whatever other phenomenon: what are the problems one encounters when trying to identify centres and peripheries in the deep prehistoric past?

Absence of evidence and evidence of absence

In order to designate one area as the centre of origin of a species (or for that matter: of a specific type of behaviour, or of an artifact) and another as peripheral, we need to be reasonably sure that both areas have seen a comparable intensity of research, that both are adequately sampled and the regional records are balanced enough to make meaningful comparisons. Absence of evidence is not enough: if we postulate that species A migrated into area B, we need comparable datasets in order to legitimately infer that the species was indeed *absent* before its first *appearance* in the fossil record from Area B. In other words: we not only need the *first appearance date* of a taxon in a new area, but also information on its *last probable absence* (see Figure 1). This is more easily said than done, because absence can never be 'demonstrated' in the fossil record. We can, however, at least put some constraints on its probability by comparing the quality of the record from the inferred core and its peripheries. Only then can we at least be reasonably sure that the absence of evidence can be read as evidence of absence. Again, that is a tricky matter when one is working with the fossil record.

A nice example of the problematic nature of the fossil record in this domain comes from a recent study[6] of the Latitudinal Diversity Gradient - a term for the fact that most organisms show a distinct decrease in biodiversity from the tropics to the poles. The authors did a comprehensive global analysis of taxa of marine bivalves over the past 11 million years in an attempt to document their patterns of origination, extinction and migration. Their results support an 'out of the tropics' model, in which taxa preferentially originate in the tropics and expand to the poles. However, the authors could not use the fossil record in a straightforward way for these centres and origins questions: the bivalve record in the tropics is relatively poor, because of lack of outcrops, deep tropical weathering of sediments and the limited amount of research. As an estimate, at the very least 25 times as many bivalve fossils have been

recovered from areas outside the tropics as from within the tropics. Using the biased fossil record in a straightforward way would mean that even if a genus originated in the tropics and then expanded into temperate latitudes, the fossil record would very probably indicate the opposite signal, i.e. an extra-tropical origin followed by a range expansion towards the Equator (Jablonski and co-authors were able to work their way around this bias, by using genera from only the best-preserved families, see their paper for details).

Now this is a problem that is familiar to palaeoanthropologists and archaeologists, as we too work with a strongly biased record. On top of that, we study the record of a species which, compared to bivalves, was very thin on the ground and whose traces, fossils and artifacts, need specific types of environments to be preserved and specific geological circumstances for them to be accessible for research. As the species we are interested in were not very abundant in their world and hence in the fossil record, knowledge of biasing factors is a prerequisite for the study of their past distributions. The higher the trophic level of a species, the smaller its abundance in the real world and hence in the fossil record (for the whole of the Asian Late Pliocene [the period between 3.6 to 1.8 million years ago], we have, for example, only two records of a puma, from two locations separated by more than 3,000 air miles).[7] Open, mesic-to-arid environments tend to preserve fossils better than do forested and wetter environments, which is probably why the first Pleistocene ancestor of extant chimps was only reported very recently.[8] Should faunal remains become buried by sediments, those sediments obviously have to survive and be accessible for researchers – a condition that is rarely met for sediments from the time range at stake here. The African Rift Valley constitutes a unique exception by its sheer size and the exposure of fine-grained sediments of the relevant age. It includes the majority of the African hominin flagship sites, as well as some of the earliest sites outside of Africa, in the Jordan Valley in Israel, which is a continuation of the Rift Valley.

Apart from accessibility of sediments from the right time range, current large-scale imbalances between regional records are often the result of differences in research history and intensity. To some degree the Javan and Levantine records result from research initiated during colonial times,

and the East African record likewise owes a great deal of its incipient (and prolific) research to the consequences of its colonial history. In contrast, most parts of Asia have experienced only a very limited survey of relevant exposures, compared to the heavy palaeoanthropological investments made in East Africa during the last four decades. These regional imbalances are crucial. After all, in the early twentieth century East Asia was thought to have been the centre of human origins because it had the oldest fossils, and one of the marginal areas, Africa, had not seen any significant fieldwork yet. For Africa and Asia comparability is still many generations of research grants away. Nevertheless, we could do much more to reduce the level of uncertainty over when hominins were last absent in Asia by increasing the number and quality of fossil assemblages immediately prior to their first alleged appearance. Current palaeontological fieldwork in Georgia tries to do exactly this, by focusing on faunal assemblages that date a bit earlier than the approximately 1.75 million years old assemblage from Dmanisi, which have yielded five hominin crania thus far (see below).

To an important degree our distribution maps of human fossils and sites often reflect colonial history, and in our interpretation of distribution maps we need to be more aware of this bias. The recent suggestion that Gibraltar was the last stronghold of Neanderthals in a Pleistocene Europe conquered by anatomically modern humans[9] provides a good example of this: the intensity of (British) Palaeolithic research in that tiny part of the Iberian peninsula is very high, and the resulting pattern of 'the Neanderthal stronghold' is probably more related to the fact that the 'Rock' is one of the last strongholds of the British empire, than to the range of the latest Neanderthals.

Many factors are at stake in the formation of a fossil, its survival over hundreds of thousands of years of geological histories of sedimentation and erosion, political turmoil and scientific fieldwork campaigns. A good way to appreciate the complexity of fossilization and the recovery of a fossil is to start from one's own body: in your opinion, what would you have to do to enhance your chances of being preserved for future palaeontologists in about 500,000 years from now? Where would you like your body to be covered by sediments, what type of sediment and in which country? Remember, countless Turkana boy-like adolescents must

have died at the margins of former lakes, but only a tiny fraction of those will have ended up fossilized; and only a fraction of those will feature in museums and text books.

Asian signals

Despite the imbalance in research intensity (and hence the number of sites), Asia has produced a number of major surprises in recent years. Until the last decade of the previous century, the earliest traces of human presence outside of Africa were located in 'Ubeidiya (Israel), in the continuation of the African Rift Valley, and dated to about 1.3 million years ago, roughly 300,000 years older than those from the eastern parts of Asia, China and Java. That was the situation only one and a half decade ago. Now, the first appearance dates of hominins in Asia are significantly older, a testimony to its palaeoanthropological potential. As an example, the recent political developments in China and the concomitant palaeoanthropological research have resulted in an extension of its earliest hominin presence up to 1.66 million years ago.[10] These traces consist of simple stone tools associated with broken up faunal remains, recovered from lake sediments in the Nihewan basin in northern China. The archaeological excavations there have not reached the lower fossiliferous layers yet. The evidence for the presence of Early Pleistocene hominins in China and Java stretches the limits of current thinking on hominin evolution, as do the approximately 2 million year old stone artifacts from Riwat, Pakistan. When Robin Dennell reported these in the late 1980s he caused quite a stir as they were clearly in the wrong place and from the wrong time; now they start to fit into a sparse pattern of early traces of hominin activities in Asia. In Ian Tattersall's words: 'Their great age is now looking decreasingly anomalous'.[11]

The most spectacular developments in Asia, however, result from excavations in the tiny medieval town of Dmanisi, Georgia, where a real treasure trove of early hominin fossils, associated faunal remains and stone artifacts has been excavated. The finds date to about 1.75 million years ago, with the hominins being referred to a primitive form of *Homo ergaster/erectus* (see below). These Asian discoveries underscore our poor ability to discern, let alone, predict, the design on the picture we try to piece together from the few pieces of the jig saw that we have.

Biogeography and our classical heritage

Since the Greeks and Romans, we have become used to referring to 'Africa' and 'Asia' as separate continents, each somehow homogenous and distinct from the other (Pliny the Elder's comment, *Ex Africa semper aliquid novi* (From Africa there is always something new), did help to keep Africa a special entity). Plants and animals are less respectful of our Graeco-Roman heritage: the landmasses we now call Africa and Asia are very diverse, but they also have many plants, animals and environments in common. In recent decades, palaeoanthropologists have emphasized the importance of open environments and especially savannah grasslands in hominin evolution, both as a place where many hominin species (including *Homo ergaster*) lived in the Late Pliocene and Early Pleistocene, but also as having played an important role in influencing hominin brain size, post-cranial anatomy, and diet (the slender physique of Turkana boy, the adolescent *Homo ergaster* mentioned above, has for instance been interpreted as an adaptation to the hot and arid African open savanna).

In the Pliocene (roughly between 5.5 and 1.8 million years ago), grassland-like environments extended all the way from West Africa to North China. Robin Dennell and I have somewhat provokingly suggested that the concept 'Savannahstan' might prove a more useful spatial unit for modelling early hominin adaptations and dispersals within and between these continents than simply an undifferentiated 'Africa' or 'Asia'. For example, the African hominins at Koobi Fora (Kenya) and their slightly later (but: see below!) counterparts in Asia at 'Ubeidiya (Israel), and in the Nihewan basin (northern China) were all living in broadly comparable grassland environments, and it makes sense to place them within the same frame of reference. This might also highlight the significant variation that is sometimes buried under a blanket term such as 'Asia'. For example, the hominins in the Nihewan basin, northern China, and those in Java are both clearly in East Asia, but those in Java inhabited a region that was more densely wooded. It is not the continent that matters in studying human origins so much as the type(s) of environment with which early hominins were associated, and hence we should stop using continents as proxies for biogeographic units.

Furthermore, Asia might not simply have been the passive recipient of whatever *aliquid novi* migrated out of Africa, but could have been a donor to speciation events, as well as dispersals back into Africa. Such two-way traffic is well documented for other mammals in the Pliocene and Early Pleistocene, such as for horses and bovids, with more taxa migrating into than out of Africa.[12] There is no reason why hominin migrations were always from Africa into Asia; movements in the opposite direction may also have occurred, as has been suggested for some of the African fossil material as well as, independently of this, on the basis of genetic evidence (see below).

Homo ergaster: a hominin without an ancestor?

The view that *Homo ergaster* originated in East Africa has not gone without question marks. *Homo ergaster* marks a radical departure from previous hominins in Africa, including *Homo habilis*. *Homo ergaster* is taller, differences between males and females are smaller (reduced sexual dimorphism – mainly because females have increased in size), it has long limbs and modern body proportions. At present it is hard to identify its immediate ancestry in East Africa. It is not without reason that *Homo ergaster* has been described as a hominin '… without an ancestor, without a clear past'.[13] One should even allow for the possibility that *Homo ergaster* originated in Asia and perhaps explain its lack of an obvious East African ancestry as the result of immigration. Indeed, Tim White considers it: 'more likely that *Homo erectus* is an immigrant from Asia to the eastern African area than an anagenetic, *in situ* derivative from *Homo habilis*'.[14] (Granted, he published this somewhat heretic vision in an endnote of a paper on Pliocene pigs …).

We should bear in mind that these interpretations of the record were made before the Georgian site of Dmanisi yielded the hominin remains that are now in the centre of the debate on the origins of *Homo ergaster/erectus*.[15] Five skulls, and a wide range of postcranial remains have made Dmanisi into one of the richest hominin-bearing localities worldwide. The taxonomic status of the hominin remains has been heavily debated. The bottom line is that we are dealing with an assemblage that is variable, but which unambiguously shows that these early Asians were small-brained and short in stature, probably more like *Australopithecus* than *Homo ergaster* in these regards (140 cm vs 180 cm for Nariokotome boy).

The reason why *Homo ergaster* was assumed to be uniquely capable of migrating out of Africa into the Asian grasslands was because of its long limbs, human-like body proportions and a sufficiently large brain to deal with the challenges of a more carnivorous niche. Dmanisi almost single-handedly showed that this argument is flawed. A comprehensive study of the Dmanisi cranial remains by Rightmire and colleagues[16] now concludes that the Dmanisi material is very close to the stem of *Homo erectus* (*sensu lato*, see box), and that '… dating does not presently rule out the possibility that *H. erectus* originated in Eurasia and that some groups then returned to Africa, where they evolved towards *H. ergaster*'.[17]

Although Darwin's (1871) suggestion that '… it is somewhat more probable that our early progenitors lived on the African continent than elsewhere' is widely quoted, it is worth noting his following sentence: 'But it is useless to speculate on this subject … since so remote a period the earth has certainly undergone many great revolutions, and there has been ample time for migration on the largest scale'.[18]

Order, order!

Much of the fossil evidence for hominin evolution between seven and one million years ago has been unearthed in the African Rift Valley. One of the richest fossiliferous areas in the Rift is the Turkana Basin, situated in north-western Kenya and southern Ethiopia. Here, around Lake Turkana, fossil-rich lake and river sediments extend far inland and are accessible in badland-types of environment. East of the lake is the Koobi Fora area, well-known for its wealth of Plio-Pleistocene fossils (including hominins) and early archaeological sites. These have been recovered from the upper half of a sedimentary sequence of about 560 metres thick, and outcropping east of Lake Turkana. The almost ubiquitous presence of volcanic ash layers in this sequence has provided an excellent basis for stratigraphic subdivision as well as absolute dating of the hominin-bearing deposits. Individual crystals from volcanic deposits can be dated from the radioactive decay of Argon isotopes, and the dated horizons have a unique chemical composition which allows geologists to follow such layers over large distances. This has been instrumental in correlating find-bearing deposits and building a precise and accurate time scale for human evolution, and for the hypothesis that the earliest *Homo* emerged in East Africa.

However, in a recent series of papers, Frank Brown and colleagues have come up with a new chronology for early hominins from this very area. In their view, virtually all key fossils attributed to *Homo* in Koobi Fora, previously thought to be around 1.8 million years old, are in fact approximately a quarter of a million years younger than Brown and others previously thought:

'Indeed, all specimens from Koobi Fora assigned to *H.* aff. *erectus* by Wood (1991), many of which are now referred to *H. ergaster* (Wood and Richmond, 2000), are now estimated to be 1.45 to 1.65 myr old with the exception of KNM-ER 2598'.[19]

We have an interesting problem here, as the earliest *Homo* in this core area of human evolution is now younger than its inferred descendant in the periphery (Dmanisi)! The new chronology for this key area in human evolution studies will be heavily scrutinized by other earth scientists. Should the interpretation by the team of Frank Brown survive rigorous testing (and should the current 1.75 Ma date for Dmanisi prevail) we would have the situation visualised in Figure 1-4, where the inferred ancestor of a species that colonized a new area turns out to be younger than its descendant. Obviously, there is something wrong here. These new data from the inferred core area bring very strong grist to the mill of those who doubt the classic version of *Out of Africa*: there is virtually no *erectus*-like hominin left in the African record prior to 1.65 million years before present, i.e. around the time when the skeleton of the boy from Lake Turkana became enveloped in lake sediments.

As far as the dating evidence goes, the primitive *erectus/ergaster*-like hominins known from Dmanisi, Georgia, are now very probably older than their inferred African ancestors.

Discussion

To conclude, the classic version of *Out of Africa 1* suffers from some major weaknesses, some of which were already pointed out in the *Nature* review mentioned above.[20] Robin Dennell's and my earlier review of Out of Africa 1 could not benefit from the studies by Rightmire *et al.* and from the results recently obtained by the Frank Brown team. These heavy blows

to *Out of Africa 1* were only just submitted for publication when we came up with a few alternatives to the *Out of Africa 1*-model. One alternative is that hominins of the genus *Australopithecus,* the first group mentioned in the introduction to this paper, might have left Africa before about 1.8 million years ago. The surprising presence of *Australopithecus* in Chad, 2500 km west of the Rift Valley, shows that hominins were distributed over very large parts of Africa at 3 to 3.5 million years ago, i.e. in a much larger area of Africa than adequately sampled.[21] This raises the interesting question whether it is possible that some hominins had adapted to other environments than the ones we know from work in the East African Rift Valley? Where else could hominins have been living when *Australopithecus bahrelghazali* became fossilized in Chad? Some of these 'unknown' ecologies, such as coastal environments with their often abundant resources,[22] would make an early (and fast) dispersal very plausible.

As environments broadly comparable to the Chad and East African grasslands were present in Asia around that time, hominins could have expanded their range in a north-eastern direction too (in fact grasslands, the inferred preferred original environment for *Homo*, were well established over major parts of Asia from around 5 million years ago, i.e. a few million years before they became dominant in Africa). Once in Asia, these (or later variants of) hominins could have developed into an Asian ancestor for both the Dmanisi, Java and east African *Homo erectus/ergaster* populations.

This hypothesis is, however, very difficult to test, for the reasons discussed above. We simply do not have *Australopithecus*-fossils outside Africa, and given the current locations and pace of fieldwork it will take some time before such a theoretical find might be unearthed, if *Australopithecus* was ever there at all. Fossil and archaeological discoveries do not constitute the only data in this debate though. The geneticist Alan Templeton has suggested that the prospects for testing our hypothesis with molecular genetic data will be excellent in the near future.[23] Templeton claims genetic evidence for an expansion out of Africa at around 1.9 million years ago and infers that a two-directional gene flow between Asian and African populations was established by 1.5 million years ago.[24] His findings do suggest that migration began earlier than commonly thought and that hominins were moving back and forth between the two continents, as did other mammals.

Given the imperfections of the fossil record, the chances of being able to identify core areas seem very remote. The point has already been made with respect to African Australopithecines, following the discovery of the above mentioned *Australopithecus bahrelghazali* in Chad: '… if the origins of hominids occurred rapidly, followed by rapid range extension, as seems likely, it may be as futile to seek a specific and localised place of origin for hominids as it is for any other group'.[25] If the origin of *Homo ergaster* was also followed by rapid range extension, it likewise follows that it might be futile to postulate its place of origin, Asian or African.

A recent paper on this issue[26] suggests that 'Made in Savannahstan' would make a good alternative to 'Out of Africa 1'. It is certainly a catchy phrase. But rather than replacing one sound bite with another, I prefer to stick to what is observable, as building our hypotheses on what is possible and plausible would open up a virtually endless array of untestable scenarios. What is clearly observable is that the *Out of Africa 1* narrative does not work any more as a good explanation for the patterns in the fossil and archaeological record. Given the difficulties of testing alternatives, we just may have to live with ambiguity in this domain, with a full awareness of our current state of ignorance, focusing on the discrepancies and conflicting data, as these are the domains where we can make significant progress in our studies of our early past.

Box

Homo erectus and Homo ergaster

H. erectus - or more properly, *Pithecanthropus erectus* - was first discovered at Trinil in Java by Eugène Dubois in 1891 (these key fossils are now stored – and on display – in Museum Naturalis at Leiden). In the 1930s, further discoveries of hominin remains elsewhere in Indonesia and at Chou-kou-tien (now Zhoukoudian) in China were seen as broadly similar, even if initially given their own generic names (such as *Meganthropus* and *Sinanthropus*). In 1950, Ernst Mayr re-classified all this material as *Homo erectus*, with the Trinil specimens as the type fossils. Subsequent African specimens were also called *Homo erectus*, as were much later specimens from Europe. *Homo erectus* thus became for a while the earliest hominin that was thought to have lived in Asia, Africa and Europe.
In the last few years, some experts have doubted that the East African specimens should be placed within the same palaeospecies as the Asian ones. It has been suggested that the earliest African examples should be called *Homo ergaster*, after the specimens found at Koobi Fora, including WT 15000, the magnificent 1.6 million years old skeleton of a young boy from Nariokotome that was initially published as *Homo erectus*. Consequently, it is the African *Homo ergaster* that is now seen by many as the hominin that expanded Out of Africa, colonized Asia and formed the founding population of what later became *Homo erectus* in China and Southeast Asia.

European specimens once regarded as late examples of *Homo erectus* or 'archaic *Homo sapiens*' are now increasingly classed under the taxon of *Homo heidelbergensis*, a term first used to classify the mandible from Mauer, Germany, found in 1907, and now estimated to be half a million years old. To avoid ambiguity, the term *Homo erectus* is used here *sensu stricto* to denote only the specimens from eastern Asia.

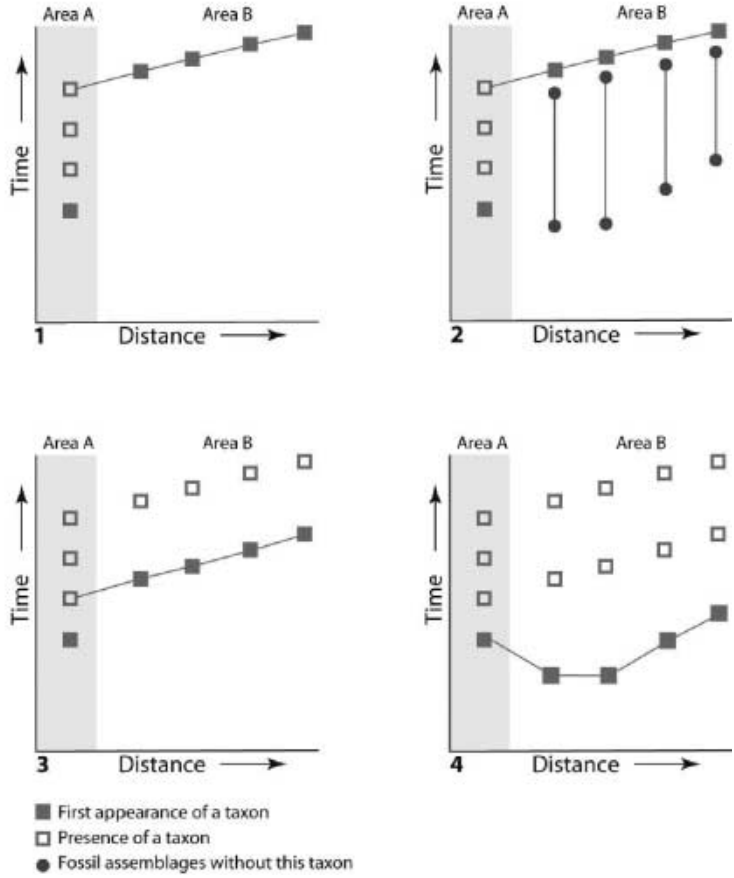

Figure 1 (From: Dennell and Roebroeks 2005): Dispersals, cores and peripheries. The dangers of over-reliance on First Appearance Dates of when a taxon migrated from its core area (A) into new territory (B): (1) shows a hypothetical situation in which a taxon originated in area A, and then migrated into a new territory (B). In (2), the reliability of these First Appearance Dates is considerably strengthened by the numerous well-dated instances when its Last Probable Absence can be documented. Without these, future discoveries might indicate (3) that previous estimates of when a taxon first appeared were too recent, as happened when the earliest Javan hominins were redated from ca. 1 million years old to ≥1.6 million years before present. Even more alarmingly (4), future discoveries might show that the taxon probably originated in the area that it was supposed to have colonized – as happened when the cradle of humankind was relocated from Asia to Africa in the 1960s.

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12
Classical challenges: Black Athena, Thucydides in Iraq, Plato in the courtroom

Ineke Sluiter

Black Athena

In 1987, Martin Bernal published the first part of a projected four-volume project: *Black Athena. The Afroasiatic Roots of Classical Civilization*.[1] The book created a veritable shock-wave in the field of Classics and beyond. The controversy produced separate sessions in conventions of professional associations, special issues of professional journals, numerous articles, and whole books dedicated to criticism and reply, largely through the indefatigable exertions of Bernal himself, who kept answering his growing number of critics in detail.[2] Bernal argued that our picture of classical Greece as the cradle of Western civilization is seriously flawed in that it not only simply fails, but outright refuses to recognise its roots in Egyptian ('African') and Phoenician ('Asian', 'Semitic') culture. Our views are fatally influenced by the perverse work of especially German philologists and 'Altertumswissenschaftler' in the nineteenth and twentieth centuries. In Antiquity, historians like Herodotus freely acknowledged the indebtedness of Greek culture to the much admired Egyptians. This does not just represent what the Greeks liked to think about themselves, but, according to Bernal, it is objectively true. This so-called 'Ancient Model' of ancient culture, one where the 'Afroasiatic roots' are still clearly visible and admitted, was replaced, Bernal claims, in a wave of post-Romantic racism and nationalism by what he styles 'the Aryan model'. Scholars who defended the Aryan model (obviously

without calling it that) rejected the mythological beliefs of the Greeks themselves as historically untrue, and replaced them with a theory of invasions from the North by 'Pelasgian' speakers of an Indo-European language in an attempt to deny or suppress all suggestions of 'Semitic' influence on the Greek miracle. Bernal's own theory combines these insights: Greek is indeed an Indo-European language, although Bernal alleges that a very substantial part of its lexicon should be explained as Semitic. But this does not invalidate the relevance of the Ancient Model. Bernal therefore proposes a so-called Revised Ancient Model, which gives full credit to the Greeks' Egyptian and Phoenician roots, while integrating ideas on their Indo-European background.

The reception of the first volume was not unfavourable. Scholars were rightly impressed with the achievement of one single scholar, not a classicist, but a Sinologist by training, who claims amateur status, but is rather, as one critic puts it, 'a one-man University'.[3] Bernal adduces linguistic, archaeological and historical arguments ranging over many of the cultures and languages in the Aegean from the second millennium onwards. He discusses the history of scholarship at great length. His language skills, the vast number of sources and the huge amount of secondary literature that he uses are stunning. However, it was also clear that the work had serious problems. One was the ideological uses to which the work was put – not completely the fault of the author, but definitely facilitated by Bernal's own none too subtle rhetoric, particularly his title and the choice of the term 'Aryan'. The title *Black Athena*, though inspired by the commercial interests of the publisher, turned the book into an important piece of ammunition for Afrocentrist politics: Greek culture could ultimately be regarded as African or black, and the prevalent picture of the history of Western civilization rested on a 'stolen legacy', a phrase that had been used decades earlier to defend a similar view on a much more frivolous basis than that of Bernal's hefty volumes.[4] Bernal would only consider himself 'Afrocentrist' in the restricted sense that there are in fact many important 'African' contributions to 'Western' culture,[5] but academic reception of his work was influenced by the political uses to which it was put: they did not seem warranted by 'academic standards'. The term 'Aryan model' also created a highly charged polemical atmosphere: many critics had to admit that there is indeed a strong nationalist and also anti-Semitic streak in the culture which developed and fostered the new discipline of *Altertumswissenschaft*,

but the term 'Aryan' seemed to evoke a direct link between old-fashioned Classics and Nazi ideology. Much of the debate over *Black Athena* took place in this electric, politicised context, and it explains the heat and sense of urgency in the debate.

Bernal offers a sweeping theory based on a host of details. Specialists in the several disciplines invoked by Bernal replied to him and demonstrated, mostly convincingly and correctly, that Bernal made many, many mistakes. But when does the accumulation of individual mistakes add up to the destruction of a theory? The most scathing criticism concerned Bernal's sketchy command of linguistic theory (while acknowledging his mastery of many languages): any kind of sound resemblance between words in Greek and words in the languages whose influence was under investigation would serve as corroboration of linguistic affinity. Thus he claimed that the name Athena went back to the Egyptian locution for 'House of (the goddess) Neith'.[6] But criticism also comes from archaeologists, who note that the important influence from the alleged repeated Egyptian invasions and colonisation in Greece should have left material traces, which are simply absent. Similarly, students of cultural memory and the role of myth in it claim that Bernal is altogether too naive in his evaluation of ancient Greeks' assessment of their relation with the Near East: they admired Egypt and were happy for it to form part of their own (mythological) history, but that does not prove that it was.[7] Students of the history of scholarship have demonstrated in several individual cases that Bernal seriously misjudges or misrepresents academic history in nineteenth-century Germany.[8]

Corrections of details cannot immediately topple a big-picture theory like Bernal's, but a lot of evidence against his views has accumulated. More fundamental objections to his thesis are that it threatens to replace one idea of essentialised 'beginnings of civilisation' with another.[9] He also simplifies *the* Ancient Model: the Greeks had many versions of their past.[10] And an alternative model to that of Bernal is available – and had been before Bernal:[11] it distinguishes carefully between roots and influences and subsequent internal developments within a culture. Thus, while acknowledging the embeddedness of Greece in the Aegean and its Near-Eastern cultural context, it is able to accommodate, for example, the unique features of the *polis* system and certain characteristics of Greek philosophy, religion and literature that distinguish it from its Near-Eastern predecessors. *Black Athena* made the articulation of those alternative views

sharper and advanced and nuanced our thought on the relations between cultures from many different perspectives as well as adding to our knowledge of all the specialised fields involved. The immense sense of urgency with which all those topics were studied and elucidated is entirely to the credit of Martin Bernal's project, even if one of his own stated intentions was not this, but rather 'to lessen European cultural arrogance', clearly a political statement.[12]

Classics in the Culture Wars

The *Black Athena* debate put Classics in the eye of a storm that had been gathering force ever since the so-called 'culture wars' from the 1960s, and especially in the 1980s.[13] What is the justification of the discipline, what are or should be its core activities? Is it necessary to argue for the value of classical literature and civilisation, or does that go without saying? Are Classics in fact irrevocably symptomatic of the cultural hegemony of the dominant classes? How important or legitimate is it to emphasise the contemporary relevance of phenomena or ideas from Antiquity or to interpret ancient material from a modern point of view? Can't classicists simply stay aloof from all that and just preserve the inheritance from the past? Is knowledge of the original texts and contexts indispensable? Is it enough?

The political uses to which *Black Athena* was being put in Afrocentrist circles provoked outrage among numerous academics, but the discussion often seemed to be conducted at cross-purposes. Classicists like Mary Lefkowitz would invoke academic criteria (good ones!) to invalidate Afrocentrist interpretations, but mostly without addressing (or deeming irrelevant) the question of the political value of being able, as she clearly is, to speak for the Classics and with their authority. On the other side, instead of rejecting out of hand a white-dominated discourse based on the Classics, Afrocentrists tried to appropriate them through the notion of the 'stolen legacy'; this only confirmed the authority of the Classics. Interestingly, progressive and conservative classical scholars often discovered, quite possibly to their consternation, that they were on the same side of this debate. Both 'hard' scholars on the search for factual truths and 'softer' ones trying to discover ideological contexts or, for instance, studying the history of mentalities, where the object of research does not have the same epistemological status, agreed that they shared a

discipline with strong academic standards. A good interpretation should at least live up to a standard of intersubjectivity, where objectivity is out of reach, the methodical steps of a research project should be clear and repeatable, one should be able to check the results, scholars must give a full account of how they reach their results, and theories should be economical and falsifiable. The unfortunate aspect of this consensus was that it looked as if ranks were being closed to an academic outsider.

However that may be, their unexpected alliance never silenced the internal debate in Classics between 'hard' scholars, who think it their task to preserve the Classics for posterity through text editions and the study of grammar, and 'soft' scholars who feel that the value of Classics also lies in the demonstration of its continued relevance to contemporary issues.[14] In fact, these positions are not that difficult to reconcile: recent theories of memory suggest that the workings of cultural (collective) memory are not so different from those of individual memory. Rather than being a mechanism for 'storage-and-retrieval' and thus oriented towards the past, both individual and collective memory are dynamic systems geared towards enabling their agents to function in the present with an eye to optimal future performance. We simply forget what is of no use to us. In that sense, the place of Classics in our present-day world is itself evidence for its continued usefulness. Further, it is becoming increasingly clear that in establishing what the reading of a text is (an activity traditionally deemed 'hard'), one is in fact interpreting. Except in trivial matters, it is not possible to establish a detail if not on the basis of a view of the whole, while at the same time the meaning of the whole derives from that of its parts. So the distinction between textual criticism and interpretation does not stand up to scrutiny: it is fluid at best. On the other hand, as long as Classics retains its contemporary interest, it will always be necessary to have specialists who devote themselves to the technical sub-disciplines in order to have a firm academic basis for wider-ranging studies. The technicians provide the backbone of the discipline, but the applications prove its continued relevance, and instantiate its proper use.

Thucydides in Iraq

Seeing connections between contemporary problems and the Classics is not just a ploy invented by some classicists to ensure that they will continue to be funded. Society at large values traditions, wonders about

its relation to its past, and looks for models to understand its own processes. Since classical literature has functioned as a mirror to our society for such a long time, reflecting or distorting as the case may be, the reflex to refer to it is virtually ineradicable, particularly in times of crisis. A war constitutes such a crisis. The heated debate of the court-room constitutes another one. In both cases, appeals to the Classics are common and they are not restricted to specialists. But specialists can be helpful in contextualising both the new use made of such texts (mostly they are texts) and their original function.

From the beginning of the Iraq War, the fourth-century Greek historian Thucydides was a constant point of reference in countless newspaper articles in the United States. Thucydides' *Histories* is a brilliant analysis of the precarious situation of the Athenians, whose ever-growing empire became a trap from which they could no longer extricate themselves. Their position of power meant that they could not afford not to control their allies. Like all ancient historians, Thucydides works in all registers: he analyses, he describes, but he also often shows the point of view of the actors in his history by putting long speeches into their mouths in direct speech. These speeches purport to offer the general gist of what was actually said, couched in words that Thucydides deems appropriate to the specific circumstances and characters.[15] Defenders of the war and opponents thus all take the floor. As a result, if one wishes to appropriate the authority of Thucydides for one's own point of view in a matter like 'Operation Iraqi Freedom', it will not be hard to find a suitable quote from the *Histories*. But since in many cases those quotes will only offer the partial and one-sided perspective of one of the characters, it would be a bad mistake by academic, though not necessarily by rhetorical standards to attribute the quote to 'Thucydides' and to pretend that one has the authority of the historian's view behind one. Arjen van Veelen has analysed the use made of Thucydides in American public debate in newspapers on the Iraq issue and the results are depressing.[16] It is rare to find a case where a genuine understanding of Thucydides and the context of his work informs its use. Mostly, it is just Thucydides' authority that is sought after, and at times this leads to shocking manipulations of his words by people who should have known better – from both ends of the American political spectrum.

From its beginning, the war in Iraq apparently invited comparison with the war between Athens and Sparta, the Peloponnesian war, which would

end so disastrously for the Athenians. Thucydides had a special appeal to neoconservative adherents of the Bush government because of his analysis of power relationships that could underpin a *Realpolitik*.[17] One important advisor to the US government was Victor Davis Hanson, classicist and military historian, decidedly a conservative both in his political and academic convictions. In the latter capacity he wrote *Who Killed Homer?*, in which he claims to defend Classics against the onslaughts of postmodernism.[18] In spite of his professional qualifications, he published an 'interview' with Thucydides on modern issues,[19] in which he quotes selectively from Thucydides to legitimate hard military action. He chooses both character text and texts where we hear the narrator 'Thucydides', without an indication of the original context (apart from the bare reference to book and chapter). He takes texts from defenders and opponents of the same view as if they were one person. Texts that in Thucydides apply to the position of Athens can be made to refer to terrorists and vice versa. All the complexity of the original work is lost. One example (about the motives for the attacks on September 11, 2001) must suffice:

'Question: The hijackers were relatively educated and affluent, as was Bin Laden himself. So their desire for power doesn't seem to arise out of economic exploitation, colonialism, or real need, does it?

Thucydides: Fear was the principal motive, though honor and self-interest afterwards came in. (*Histories* 1.75)'.

In its original context, the 'answer' by Thucydides formed part of a speech by the Athenians. The speech attempted to prevent a major conflict by explaining to the Spartans why the Athenians had built their Empire: they pointed out how they had defended Greece against the Persians, how their allies (some of whom were now turning against them) had joined them voluntarily, and how they had only later expanded their power, 'mostly, because they had been afraid [of the Persians], although ambition and self-interest had also played a role'. Athens, of course, is usually seen in these contexts as being in the same embattled position in which the US found itself. Yet, when it suits his purpose, Hanson can suggest that it is Thucydides' personal view that these sentiments motivated the terrorist attack inspired by Bin Laden. At the same time, the leading question that precedes this 'answer' excludes social, economic and political factors as viable explanations of the attack. Hanson is apparently quite aware of

possible objections to his modus operandi, for he adds a remark in which 'Thucydides' himself seems to offer a justification for the timeless and decontextualised use of his text: the *Histories* are 'a possession for all time'.[20] Is the whole text meant humorously? It is the best one can hope for.

Thucydides was not just used by the neocons: opponents of the Iraq war offer similarly sad examples of the abuse of his text to support a contemporary political position without respect for its original context. Fortunately, Van Veelen has also unearthed at least one instance of the use of Thucydides which shows a genuine understanding of that author.[21] When the Abu Ghraib scandal broke, Anne Applebaum commented: 'This is hardly news. Thucydides wrote of war as a time when the "conventions of human life are thrown into confusion" [*Histories* 3.84], and so it remains'. In war time humans will manifest the very worst aspects of their nature. That is Thucydides' pessimistic vision in his analysis of Civil War (*stasis*) in Cercyra, and Anne Applebaum recognises an instantiation of this view in the events in Abu Ghraib. She does not simply use Thucydides to reinforce her approval or disapproval of the war, but 'to temper any naive expectations about human beings'.[22] Americans are not morally better than anyone else: put them in the wrong circumstances and things will go wrong: 'Indeed, it is not difficult to create a situation in which ordinary soldiers of any nationality feel entitled to mistreat prisoners of war. All that is needed is a sense that the ordinary rules don't apply'.[23] Reference to general theories of human behaviour in ancient authors is usually a more sensible and careful way to use the Classics than the suggestion of one-on-one applicability, such as created by the Hanson approach. Traditionally, ancient texts have formed a common frame of reference. They can anchor public discourse both under the guise of a distancing device (their use puts contemporary issues in perspective) and as a way of establishing common ground and points of recognition amidst controversy. But from a scholar's point of view there are decidedly better and worse ways of using them, and there are criteria to distinguish between the two.

Classics in the courtroom: Caligula

If Victor Davis Hanson represented a conservative classicist's abuse of sources, American court-rooms have been known to offer both liberal and

conservative academics a podium for their expertise that is hard, though maybe not impossible, to reconcile with academic standards. What the court-room setting can make explicit, while other contexts leave it more ambiguous, is a certain lack of that impartiality that under normal conditions would be on anyone's list of academic norms. When asked to serve as an expert witness, one usually does so in favour of one of the contesting parties. This makes it harder to deal with conflicting evidence in a completely even-handed way, an ideal that may never be quite attainable anyway, but that is not often under such obvious pressure.

Classicists have been asked to serve as expert witnesses in very different kinds of cases. Glen Bowersock, eminent ancient historian and permanent member of the Institute for Advanced Study, reports his experience in two cases.[24] One took place in 1993 in New York City before a jury for the State Supreme Court, and 'involved possession of a superb silver treasure' from late Antiquity, the 'Sevso Treasure', worth about 70 million dollars. 'It had been impounded at Sotheby's after claims were lodged by the governments of Lebanon, Hungary, and Croatia'. The problem was the Danube frontiers in late antiquity, and Bowersock lectured on this issue for three hours. Clearly, in this case historical circumstances were highly relevant to the outcome and the case did not involve major ethical dilemmas. The other case played in the summer of 1980 in Boston and involves more sensitive issues of public morality. Bowersock reports on it as follows:

'The District Attorney had closed down the film *Caligula* as pornographic (which it certainly was – I gather that a less explicit version was distributed later). I was invited to testify both by the prosecution and by the defense, and I agreed to appear for the defense partly in sheer indignation over the action of the DA in a city where pornographic cinemas flourished undisturbed and partly because I thought that the undoubtedly historical elements in the picture, awful as it was, deserved to be underscored before a sanctimonious prosecutor. This decision of mine did, of course, align me with *Penthouse Magazine* and its head, Mr. Guccione. But I always met and worked closely with Roy Grutman, who was Guccione's principal litigator at the time ....

Much of my testimony was devoted to a recitation of the material about Caligula in Suetonius. At one point the judge asked me if there was anything at all unhistorical in the film. Yes, I told him – the sets (they were

truly absurd): pronouncing the word "sets" very slowly, he remarked wryly, "In this case you have to pronounce that word very carefully". I then was able to assure him that much of the sex was also unhistorical, notably anal penetration by the fist. We won the case, and as a result Roy Grutman tried to persuade me to appear in a similar trial in Atlanta. I refused since the situation in Atlanta was very different, and I think that one must not do this sort of thing too often. But the monetary blandishments were monumental'.

In this letter Bowersock is very straightforward: he felt moral indignation at the prosecution, i.e. he had a personal view about the ethics of the issue; he took a professional interest in the historical aspects; and there was a monetary reward. He is clearly well within his professional competence in explaining the Roman historian Suetonius, and well outside it in his verdict about the absence of a particular type of sexual behaviour in antiquity. That statement seems completely unwarranted from an academic point of view. It also looks fairly gratuitous (if maybe not unfunny) in the context in which it was delivered.

Classics in the Courtroom: Plato on Homosexuality

A much more famous case, and one that had a direct impact on the lives of real people, was *Evans vs Romer*, better known as the *Colorado amendment 2* case. In 1992, the State of Colorado had passed a State constitutional amendment by referendum ('Amendment 2'). It provided that no official body in Colorado may 'adopt or enforce any statute, regulation, ordinance or policy whereby homosexual, lesbian or bisexual orientation, conduct, practices or relationship shall constitute or otherwise be the basis of or entitle any person or class of persons to have or claim any minority status, quota preferences, protected status or claim of discrimination'.[25]

Several cities (Aspen, Boulder, and Denver) had in fact passed such regulations and now proceeded to sue the State. Plaintiffs included tennis star Martina Navratilova and the City of Aspen. Amendment 2 was found unconstitutional. The Supreme Court required the State to show that there was a compelling State interest in the prohibition of special protection for this group. The State had argued that their 'compelling interest' lay among other things in the protection of personal, familial and religious privacy and promoting the well-being of children, but especially emphasised the State's interest in 'public morality'. Both sides called upon

a wide range of expert witnesses. The plaintiffs claimed among other things that the primary purpose of Amendment 2 was to advance a particular religious belief. If true, this would give them an important constitutional argument. If 'moral condemnation of the conduct in question cannot be justified, and in history has not been argued for, except on religious ("theological") grounds … any law predicated on such a condemnation must be unconstitutional'. For 'the Constitution forbids any "establishment of religion"', and its spirit 'forbids restrictions of autonomy that are theologically based'.[26]

This is where philosophers, such as John Finnis from Oxford University and Robert George from Princeton, came in. Clearly, in view of the constitutional argument about religion that the plaintiffs were likely to make, the philosophers called in for the defendant State had an interest in discussing serious moral thought outside the influence of Christianity. And that is how they ended up giving testimony on the moral thought of Plato and Aristotle, apart from propounding moral arguments in their own right, which cannot be discussed here.

Finnis concentrated on demonstrating ancient philosophers' intrinsic negative moral evaluation of the orgasmic gratification of homosexual desire. And here lay the problem for the plaintiffs: theoretically, it was enough for Finnis c.s. to uphold just one single case where a big name in ancient philosophy vented outright moral condemnation of homosexual conduct on rational grounds. The plaintiffs, on the other hand, could not afford to leave even one instance uncontested in order to maintain their point. That was a highly unfortunate rhetorical starting position, creating a lot of pressure on the argumentative abilities of their expert witness: it is not at all obvious that no ancient philosopher ever rejects homosexual practices on moral grounds (the strong thesis), or that every text which seems to do just that is in fact ambiguous enough to make it useless for Finnis' case (a weaker version). Most academics would probably feel some discomfort in this situation. However, the plaintiffs had at their disposal the formidable competence and engagement of none less than Martha Nussbaum, of whom Glen Bowersock wrote to me: 'Bear in mind that Martha, unlike me, worked *pro bono*'.[27] In the interest of full disclosure I should say here that I am a great admirer of Martha Nussbaum's philosophical acumen and the uses to which she usually puts it. I also share many of her moral convictions, including a strong disapproval of the Colorado Amendment 2. Moreover, I think Nussbaum's position is well

argued for and tenable in virtually every text that she discusses, but in the couple of texts that seem exceptional and so form a direct threat to her position, I feel that the advancement of academic insight would have been better served by a different line of argumentation.

The debate between the specialists produced a whole series of interesting publications, although on a smaller scale than the *Black Athena* controversy.[28] Nussbaum went over her position again in a long and powerful essay published in the *Virginia Law Review,* an unusual place for a classicist and philosopher to publish. The position she sets out is basically the following:[29] the type of homosexual relationship best known from the classical period in Athens is the idealised picture of the upper-class bond between a younger man, the *erômenos*, and an older (but still relatively young) man, the *erastês*. Although the *erômenos* is often called '*pais*', or '*paidika*', 'kid', this should not lead us to think that we are dealing with paedophilia as we understand it (compare our own terminology of 'boy-friend' or 'girl-friend').[30] The ideal picture, the way people speak about the normative form of this relationship, is asymmetrical, because of the age difference, but also with respect to what parties are supposed to 'get out of it'. The *erastês* is the one who is consumed with passion. He is definitely interested in sexual gratification, but his love is supposed to lead him to a genuine interest in the spiritual well-being of his *erômenos*. Thus, he is supposed to provide guidance and training and to promote the educational and political advancement of his boy-friend. The *erômenos* is not supposed to be as interested in the physical side of the relationship, nor to enjoy it, but he 'will do his *erastês* a favour', as allowing intercourse is euphemistically called (the Greek is *kharizesthai*). The relationship is a temporary arrangement. The *erastês* is supposed to get married to a girl at some point, and the *erômenos* will become an *erastês* in turn. Women are not thought to provoke the same kind of intense attachment as the 'paederastic' relationship. What was considered problematic, in the sense of shameful and dangerous, was to form a habit of passivity (i.e. never to outgrow the role of the *erômenos*). This conflicted with the ideal of active citizenship. Another taboo was to sell one's body. If a citizen did that, the punishment was disenfranchisement, since it was deemed impossible for such a person ever to be a responsible citizen again. The association between selling the free and inviolate body of an Athenian citizen and treason was too strong.

The above is a simplified version of popular morality about homosexual

relationships in classical Athens. Martha Nussbaum devotes particular attention to Plato, in whose dialogues homoerotic attraction plays such a large part, especially as an incentive to philosophy. There is a significant difference between the views expounded in the earlier and some mature dialogues of Plato and those in his later works. Nussbaum makes a strong case for a non-condemnatory reading of many difficult passages, with careful philological examination of the precise connotation of potentially problematic terms and attention to the context. The dialogue that is hardest to deal with if one insists on defending Nussbaum's strong claim that homosexual sexual gratification never incurs moral reproof, is the *Laws*, Plato's last work. In this text Plato takes his usual suspicion of the appetitive elements in the soul to new extremes. Human beings fall victim to their baser needs all too easily, and their desires should be regulated. Plato's *Laws* was the subject of extensive discussion during the Colorado trial. In the dialogue, 'the Athenian Stranger' discusses homosexual conduct with his Spartan and Cretan interlocutors in two contexts. In the first,[31] the three men are talking about public gymnasia (fitness centres) and common meals for men. The Athenian Stranger thinks that these meeting-places for men are dangerous, because they make it easy to form factions and they corrupt sexual pleasures that are 'in accordance with *phusis*' (usually translated as 'nature'). He continues that even when joking one should think that sexual pleasure 'in accordance with *phusis*' is when men and women have intercourse for procreation, while sexual pleasure that is 'contrary to *phusis*' involves male-male or female-female sexual contact. Finally, he refers to the 'daring of those that first did this'.

Discussion in the trial focused on the words 'contrary to nature (*phusis*)' and 'the daring (*tolmêma*) of those who first did this'. Did Plato hold homosexual conduct to be 'unnatural', and thus morally objectionable? Is the Greek word for 'daring' negatively charged ('enormity', 'shameless act'), or is it a neutral or positive term? Nussbaum tried to prove that *tolmêma* by itself does not have any negative connotation and that 'contrary to nature' in fact referred to the sterility of the coupling. There could be no *phusis*, in the sense of production of offspring. She denies any intention of moral condemnation. Finnis does read the text as a moral rejection of homosexual behaviour as 'unnatural' and a 'shameless act' (*tolmêma*) and he has *a priori* likelihood on his side, but Nussbaum's case deserves serious consideration. She points out that all that is indicated here is that homosexual attraction is regarded as so

intense that it makes the pleasures of procreative sex pale by comparison, which is entirely in line with Plato's low appreciation of the spiritual and physical aspects of marriage. The earlier dialogues invariably consider producing physical offspring infinitely inferior to spiritual offspring. But I think the main point is that the focus of the *Laws* passage is on the political dangers of gymnasia and communal meals, as we will see.

A later passage in the *Laws*[32] forbids men to have sex with any free-born person but their wife (note the exception for slaves!); no reproductive intercourse with concubines; no 'sowing sterile seed in males contrary to nature'. And they must refrain from male-male sex and must be banned when having extramarital sex with a woman and being found out. Finnis[33] just stresses the aspect of 'unnaturalness' and claims that all other Platonic texts can easily be brought in line with this passage – which is a sleight of hand, given the exceptional nature of this text. But Nussbaum also has trouble with this text and the way it fits the bigger picture of Plato's views on homosexuality. She ends up by just creating doubt that only one interpretation is possible, which is enough to eliminate the text as useful court-room evidence. Creating doubt is a legitimate scholarly activity. However, a different line of argument altogether would have seemed preferable to me. Plato's views in the *Laws* (and already to a certain extent in the *Republic*) should not form the basis for moral judgment in an American constitutional case. For in these works Plato is so pessimistic about normal people's capacity for functioning in a just state, that he literally proposes to abolish some of America's most cherished constitutional rights, like freedom of speech, and freedom of education. People must do the jobs assigned to them (if necessary with the help of a Noble Lie), there is a programme of eugenics to guarantee permanent intellectual and physical excellence, the family must be abolished, etc. In the *Laws*, we also find a proposal for a Ministry that deals with population regulation. Measures seem to include contraception, abortion and abandoning children. Plato has gone totalitarian! The ideology of homosexual relationships, promoting intellectual and spiritual goals and thereby a sense of freedom and independence, becomes a threat. When men assemble in fitness centres and at communal meals they could create 'factions', says the Athenian Stranger; apparently that is a bad thing. But in fact the popular Athenian view of Athens' history knows of a positive form of such strong male-male bonding, namely that between Aristogeiton and Harmodius, the tyrannicides, who delivered Athens from tyranny and

supposedly ushered in democracy. The relationship between *erastês* and *erômenos* belongs on the side of freedom and democracy and that also explains why *Laws* proscribes it. But such totalitarian and anti-democratic implications of this condemnation of homosexuality should make it unsuitable as a model of moral argumentation in the context of the US. However, this must have seemed like a 'weak line of defence' in the context of a court case. In any event, it was not pursued.

There are more passages in ancient philosophers that may not be entirely clear-cut, but certainly look problematic to the 'strong thesis' that Martha Nussbaum chose to defend. There is no question that objections were raised against homosexuality in Antiquity. The Plato of the *Laws* is not saying something neutral about it when he proposes to curb it by law. But the objections raised are either not confined to homosexual love, or they do not condemn homosexuality as such, but the forming of habits which were seen as a threat to one's ability to function as an active, manly and reliable citizen. They should be read against passages that leave no doubt that the ancient Greeks realised that the good of friendship can be actualised in a same-sex relationship.

So how did this end? Well, the plaintiffs won and Amendment 2 was ruled unconstitutional, but the arguments about ancient homosexuality were not taken into account by the judge – they were utterly confusing. For our purpose, however, the main point is that a court-room setting is a difficult one if one wishes to do justice to the Classics and their role in our society. Their best academic use always acknowledges the original historical context, however useful creative anachronism may be to elicit additional meaning from these texts.[34] It is tricky to be made to discover theories about homosexuality, or indeed, sexuality, in ancient sources, when these very concepts relating to personal sexual identity were not developed until the nineteenth century.[35] Obviously, sexuality is based in biology, but that does not mean the concept is available, produced or performed in the same way in any culture. But of course the problem in a court-room will always be formulated in contemporary terms, with real-life consequences, and anachronism will be hard to avoid. Intellectual pursuits thus lose whatever innocence they might have had.

Several scholars who had worked on issues of ancient homosexuality all of a sudden found themselves cited in a politically charged case. Both Anthony Price and Sir Kenneth Dover, authorities on ancient concepts of love and homosexuality,[36] found the thought that they were being used by

the 'wrong side' hard to digest. They both felt compelled to clarify (and to some extent modify) their positions in writing.[37] In his book *Greek Homosexuality* Dover had made the claim that Socrates 'condemn[ed] homosexual copulation'.[38] Martha Nussbaum testified that the scope of that statement was limited: Socrates had just been referring to sexual contact between teacher and pupil and to sexual conduct involving bribery or prostitution. Finnis wrote to Dover, who confirmed that he had not intended any such limitations.[39] But once he realised what was going on, Dover co-authored an appendix to Nussbaum's long article in the *Virginia Law Review*, in which they drew a careful distinction between 'condemning' as 'rejecting because there are better alternatives', and 'condemning as morally depraved'.[40] Dover also mitigated his views on the *Laws* passage, as did Anthony Price. Is this justified? Neither denied the results of their scholarship. It may be argued in their defence that they clearly had to adapt their terminology in view of the new audience and the legal context in which their work was now used. Thus, translating *tolmêma* as 'crime' (as Dover had done) would clearly have a different impact in a court-room than in colloquial usage – so he nuanced it. Court-room rhetoric also has intimidating aspects of its own: Martha Nussbaum was not just attacked for having got her facts or interpretations wrong: intimations of 'lying' and even of 'perjury' were made![41] The ethical and professional implications of these new contexts for classical scholarship are enormous.

Conclusion

In this paper, I have looked at three contexts creating dilemmas for classical scholarship. Yet, while the problems and difficulties are clear, none of these cases simply amounts to a 'classical catastrophe'. All three involved academic problems with wider cultural, political and legal implications. The *Black Athena* issue also highlighted the problem of the relationship between the technical sub-disciplines of Classics and the construction of big pictures with contemporary relevance. The fascinating tensions between these two at the same time demonstrate their reciprocal relevance. We are forced to be more precise than ever: 'blackness', for instance, would not have meant the same to an ancient Egyptian as it does to a present-day African American. 'Race', 'nationalism', 'homosexuality' and other such notions must be understood in their historical contexts

and cannot simply be transplanted into different settings without serious consequences and distortions. The Classics belong to everyone. If amateurs make these equations and 'think with the Classics' that is one thing: but as academics we have a special responsibility in this respect. There apparently still is a constant need in society to refer to the common classical tradition; that makes it all the more important for academics to draw clear distinctions between better- and worse-founded uses of that tradition, without becoming territorial. The example of Thucydides in Iraq demonstrates the embeddedness of the Classics in public discourse, but also the very limited sense of academically appropriate use. The performance of classicist expert witnesses in cases involving public morality shows just how difficult it is to reconcile that setting with normal academic standards – on the other hand, it also provokes and promotes deeply intelligent research, sometimes prompted by exciting new questions. There is no inherent reason why we should not be able to reconcile the highest academic standards with societal engagement and involvement. The challenge to the discipline is to preserve both our undeniable relevance to contemporary society and the high academic and professional standards that are rightly associated with our discipline.

Notes

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2 E.g.: Binsbergen, W.M.J. van (ed.), *Black Athena: Ten Years After* (Hoofddorp 1996-1997) = special issue of *Talanta*: Proceedings of the Dutch Archaeological and Historical Society, vol. 28-29 (1996-1997); Lefkowitz, M.R., *Not Out of Africa: How Afrocentrism became an excuse to teach myth as history* (New York 1996); Lefkowitz, M.R. and G. MacLean Rogers, (eds.), *Black Athena revisited* (Chapel Hill & London 1996); Bernal, M., *Black Athena Writes Back. Martin Bernal Responds to His Critics* (Durham & London 2001); Berlinerblau, Jacques, *Heresy in the University. The* Black Athena *Controversy and the Responsibilities of American Intellectuals* (New Brunswick 1999). An extensive bibliography up to 1997 http://www.shikanda.net/afrocentrism/strict.htm.

3 Ray, J.D., 'An Egyptian Perspective', in: *Journal of Mediterranean Archaeology* 3 (1990) p. 77-81, here at p. 77.

4 James, George G.M., *Stolen Legacy: the Greek were not the Authors of Greek Philosophy, but the People of North Africa, Commonly Called the Egyptians* (New York 1954, repr. 1989).

5 See Bernal, Martin, review of Mary Lefkowitz' *Not Out of Africa*, in: *Bryn Mawr Classical Review* http://ccat.sas.upenn.edu.bmcr/1996/96.04.05.html
6 Jasonoff, Jay H. and Alan Nussbaum, 'Word Games: The Linguistic Evidence in *Black Athena*', in: Lefkowitz, M.R. and G. MacLean Rogers, (eds.), *Black Athena revisited* p. 177-205.
7 Hall, Edith, 'When is a Myth not a Myth? Bernal's Ancient Model', in: Lefkowitz, M.R. and G. MacLean Rogers, (eds.), *Black Athena revisited* p. 333-348.
8 E.g. Blok, Josine H., 'Proof and Persuasion in Black Athena: the Case of K.O. Müller', in: *Journal for the History of Ideas* 57 (1996) p. 705-724.
9 Binsbergen, Wim van, 'Black Athena Ten Years After', p. 28.
10 Burstein, S.M., 'The Debate over Black Athena', p. 14.
11 E.g., Burstein, S.M., 'The Debate over Black Athena', p. 7ff.; Burkert, W., *The Orientalizing Revolution. Near Eastern Influence on Greek Culture in the Early Archaic Age* (Cambridge (Mass.) 1992).
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14 Culham, Phyllis, and Lowell Edmunds (eds.), *Classics. A Discipline and Profession in Crisis?* (London 1989).
15 Thucydides *Histories* 1.22.
16 Veelen, Arjen van, Thucydides in Irak. Citaten uit de Historiën in krantenartikelen over operatie *Iraqi Freedom* (unpublished MA thesis, Classics Department, Leiden University 2006). I gratefully acknowledge my use of his material in this section by permission of the author.
17 Veelen, Arjen van, Thucydides in Irak, p. 58f.
18 Hanson, Victor Davis, *Who Killed Homer? The Demise of Classical Education and the Recovery of Greek Wisdom* (New York 1998).
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20 *Histories* 1.22; Veelen, Arjen van, Thucydides in Irak, p. 80
21 Veelen, Arjen van, Thucydides in Irak, p. 82.
22 Veelen, Arjen van, Thucydides in Irak, p. 67.
23 Applebaum, Anne, 'Why America can't cope with these images', *Sunday Telegraph,* 16 May 2004, quoted from Veelen, Arjen van, Thucydides in Irak, p. 67f.
24 All quotations are from a personal letter from Glen Bowersock to Ineke Sluiter, Jan. 15, 1995 (on file with the author).
25 Colorado Const. art. II, §30b (= Colorado Amendment 2).
26 Finnis, John, "Shameless Acts" in Colorado: Abuse of Scholarship in Constitutional Cases', in: *Academic Questions*, 7 (New Brunswick 1994) p. 20.
27 Personal letter from Glen Bowersock to Ineke Sluiter, Jan. 15, 1995 (on file with the author).
28 The most important ones are: Nussbaum, Martha C., 'Platonic Love and Colorado Law: The Relevance of Ancient Greek Norms to Modern Sexual Controversies', in: *Virginia Law Review*, 80 (1994) p. 1515-1651; Finnis, John, '"Shameless Acts"', p. 10-41; Finnis, John, and Martha Nussbaum, 'Is Homosexual Conduct Wrong? A Philosophical Exchange', in: *The New Republic*, 15 November 1993, p. 12-13; George, Robert P., '"Shameless Acts" Revisited: Some Questions for Martha Nussbaum', in: *Academic Questions* 8, (New Brunswick, 1995-1996) p. 24-42.
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30 Dover, Kenneth J., *Greek Homosexuality* p. 85-86; Nussbaum, Martha C., 'Platonic Love and Colorado Law', p. 1551.

31 Plato, *Laws* I 636b1-d4; Nussbaum, Martha C., 'Platonic Love and Colorado Law', p. 1579ff. and 1623ff.; Finnis, John, "Shameless Acts", p. 22ff.; George, Robert P., '"Shameless Acts" Revisited', p. 32ff.

32 Plato *Laws* VIII 835d-842a.

33 Finnis, John, "Shameless Acts", p. 29.

34 McGillivray, Murray, 'Creative Anachronism: Marx's Problem with Homer, Gadamer's Discussion of "The Classical", and Our Understanding of Older Literature', in: *New Literary History* 25 (1994) p. 399-413. For an interesting discussion of the relevance of ancient Greece to modern issues, see Nussbaum, Martha C., 'Platonic Love and Colorado Law', p. 1597ff.

35 Boswell, John, *Christianity, Social Tolerance and Homosexuality*; Dynes, Wayne R., & Stephen Donaldson (eds.), *Homosexuality in the Ancient World*; Halperin, David M., *One Hundred Years of Homosexuality*; Winkler, John J., *The Constraints of Desire.* (See note 29).

36 See note 29.

37 A hostile version of these incidents in George, Robert P., '"Shameless Acts" Revisited', p. 28 ff.

38 Dover, Kenneth J., *Greek Homosexuality*, p. 160.

39 Finnis, John, "Shameless Acts", p. 30.

40 Nussbaum, Martha C., 'Platonic Love and Colorado Law', p. 1641-1651 (Appendix 4).

41 Finnis, John, "Shameless Acts", p. 27 'Professor Nussbaum was aware of facts about the semantics of the word that she did not disclose to the court'; George, Robert P., '"Shameless Acts" Revisited', p. 26 'Since Professor Nussbaum testified under oath, her misrepresentations, if she knew them to be such, probably constituted perjury'.

13
The autonomy of syntax

Lisa Lai-Shen Cheng

One of the most often discussed and disputed hypotheses in linguistics since the time of American structural linguistics concerns the independence of grammar (or formal processes), more precisely, the 'autonomy of syntax hypothesis', which, according to Croft is:

> '… the hypothesis that the syntactic component of the grammar is independent, (largely) systematic and (largely) self-contained with respect to the semantic, pragmatic and/or discourse-functional components'. [1]

Broadly speaking, the basic claim is that formal syntactic processes of grammar are independent of other parts of grammar, such as semantics (governing interpretation), phonology (sound), and the rest of the linguistic system. The claim is usually disputed since to most of us it is obvious that when we use language, we do not separate sentence structure and interpretation, and then we are not even mentioning the fact that pragmatic and social factors play a role in determining the use of the language.

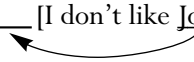
In this article, I provide an overview of the debate centring around the autonomy of syntax hypothesis. I first discuss a particular syntactic phenomenon to demonstrate what the proponents of the hypothesis have in mind. I then discuss objections, which are based on the fact that (a) syntax clearly interacts with other systems, and (b) languages change

(including their syntax). I then proceed to review results from child language acquisition studies, as well as psycholinguistic and neurolinguistic studies. In other words, I introduce interdisciplinary material for (re)-evaluation of the hypothesis. Though not all results in these studies should be taken at face-value, it is clear that the field of linguistics has advanced to the extent that theoretical research and interdisciplinary studies can better inform each other, which may help settle some old disputes.

Background

To appreciate the issues involved in the autonomy of syntax debate, it is necessary to understand what syntacticians mean when they talk about syntactic processes. A good example of a syntactic process is verb movement. It is generally assumed that in the derivation of a sentence, words or constituents may be generated in one place in the structure and surface in another; we say that the element in question has moved from one place to the other. For example, in (1a), *John* occupies the first position in the sentence, but it is also interpreted as the object of *like*; since in English the canonical object position is the position immediately behind the verb as shown in (1b) (in other words, English is 'VO'), we assume that that is the original position of *John* in (1a) and that it reached sentence initial position by way of movement, as shown in (1c).

(1) a. John, I don't like.
b. I don't like John.
c. ____ [I don't like John]



Verbs also move, as can be shown by looking at the Verb Second phenomenon (henceforth 'V2') in Germanic languages such as Frisian, Dutch and German. Unlike English, which is VO as we just saw, these languages are OV, which means that the canonical position of the object is immediately before the verb.[2] The following examples from Frisian and Dutch make this clear.

(2) a.	Jan seit dat er	in âlde freon	seach.	[Frisian]
	Jan zegt dat hij	een oude vriend	zag.	[Dutch]
	Jan says that he	an old friend	saw	

'Jan says that he saw an old friend'.

b. Jan wol in âlde freon sjen. [Frisian]
Jan wil een oude vriend zien. [Dutch]
Jan want an old friend see
'Jan wants to see an old friend'.

c. Jan hat in âlde freon sjoen. [Frisian]
Jan heeft een oude vriend gezien. [Dutch]
Jan has an old friend seen
'Jan has seen an old friend'.

In these sentences, the constituent *in âlde freon/een oude vriend* 'an old friend' is the object of the different forms of the verb *sjen/zien* 'to see': past tense *seach/zag*, infinitive *sjen/zien* and past participle *sjoen/gezien*. In all these cases, *in âlde freon/een oude vriend* appears in front of the verb of which it is the object; thus we conclude that Frisian and Dutch are OV. In Frisian, Dutch and German, the object appears to the right of the verb it is the object of in one context only, *viz.*, in main clauses with just one verb:

(3) Jan seach in âlde freon. [Frisian]
Jan zag een oude vriend. [Dutch]
Jan saw an old friend.

It is generally acknowledged that in such cases the verb has moved from its position at the end of the sentence to the second position. This can be stated in more general terms. Dutch and Frisian (and German) require that the finite (or 'inflected') verb (that is, the verb that agrees with the subject) of a sentence appears in the second position of the sentence, leaving the non-finite verbs, such as past participles and infinitives, in final position, as we saw in (2a-c) and see again, illustrated in Dutch, in (4a,b) and (5a).

(4) a. Piet heeft veel boeken gelezen.
Piet has many books read
'Piet has read many books'.

b. Dit boek heeft Piet een paar keer gelezen.
this book has Piet a few time read
'This book, Piet has read it a few times'.

(5) a. Morgen moet Jan een praatje geven.
tomorrow must Jan a talk give
'Jan must give a talk tomorrow'.

b. In Leiden hebben wij feest op 3 oktober.
in Leiden have we celebration on 3 October
'We have a celebration on October 3rd in Leiden'.

Aside from showing that the finite verb is always the second constituent in the sentence, these examples also show that it is in second position regardless of what constituent appears in the first position: it can be an agentive subject (4a), an object (4b), a temporal adverb (5a) and a prepositional phrase (5b).

Since den Besten's book,[3] it has been generally assumed that the inflected verb in V2 clauses has moved to a position immediately above the core sentence. In (5a) for instance, the modal verb *moet* 'must' has moved past the subject, which can be considered to define the sentence boundary, as shown in the derivation of (5a) in (6).

(6) Derivation: [morgen ____ [$_s$ Jan een praatje moet geven]]

In other words, the derivation of every V2 clause involves a syntactic process, namely verb movement. What is crucial to the argument is that verb movement in V2 sentences is a process that is on the one hand obligatory and on the other entirely independent of any phonological or interpretational considerations. With respect to the phonology, any verb with any phonological make up can move to the V2 position (as long as it is inflected), which means that its own phonological characteristics play no role. It is equally independent of what type of phonological material appears in the first position. As for interpretational considerations, no interpretational effect is associated with the movement of the verb to the V2 position and what appears in the first position can be interpreted as a topic as in (1a), (4b) and (5a,b) or an informational focus as in information questions such as (7a,b). In other words, the verb must be moving to the V2 position for purely syntactic reasons. Its movement must be triggered by considerations of a purely syntactic nature.

(7) a. Wat wil Jan kopen in de winkel?

what will Jan buy in the shop
b. Wanneer komen ze hier?
when come they here
'When are they coming here?'

Since its application is in all respects independent of interpretation and phonology, the V2 verb movement operation is the type of formal process which has given rise to the claim that syntax is an autonomous system (see also Ref 3, section 3.5 in Chapter 3 for more discussion on evidence for the autonomy of syntax).[4]

Verb movement is one of the formal processes that syntacticians consider to be independent (to be autonomous). This does not mean to say that there are no syntactic operations that also have semantic ramifications, or that there are no syntactic operations that are triggered by semantic considerations (the movement of the topic to sentence initial position in (1a), (4b) and (5a,b) is an example). The main question is: if only a limited set of operations exist that are independent of other (interpretational, phonological) systems, is that enough to claim that syntax is autonomous and forms its own computational system?

The dispute: Against syntactic autonomy

As Croft points out,[1] the arguments against the autonomy of syntax claim can be divided into two types. The first type comes down to providing counter-examples to argue that the constraints and restrictions on certain syntactic operations are due to semantic, pragmatic or discourse-functional principles. Leaving aside whether the counter-examples in question are really counter-examples or not, this type of objection, as Croft also notes, does not constitute a principled threat to the autonomy claim, because it at best reduces the number of operations that can be viewed as autonomous. I will not discuss this type here.

The second type of argument forms a more serious challenge not only to syntactic autonomy, but also to the autonomy of the grammar as a self-contained system, according to Croft. That is, what is at stake is larger: is the linguistic knowledge of an individual a self-contained system or not? The more challenging arguments can be divided into two categories, which we deal with in the following two sections.

Interactions

First, many analyses can be called 'mixed' analyses in the sense that it can be argued that the syntactic operations involved in such analyses pay attention to semantic considerations. Consider for example the phenomenon of agreement. Subject-verb agreement is cross-linguistically very common, and syntacticians deal with it syntactically. However, in some languages, there are restrictions on agreement, which cannot be argued to be syntactic in nature. For instance, agreement may be restricted to certain persons/genders only. I illustrate this with Bemba, a Bantu language. Bemba, like all Bantu languages, has noun-classes (which are in some ways similar to a classifier system). The verbal sequence agrees with the subject in terms of noun classes. For instance, if the subject noun is class 1, the verb will show class 1 agreement, and if the subject noun is class 5, the verb will show class 5 agreement (in example (8a,b), the number in the gloss indicates the noun class, and SM indicates subject marking; data from Givón).[5]

(8) a. abaana ba-aliile
2child 2SM-left
'The children left'.
b. ili-ishilu li-aliile
this-5lunatic 5SM-left
'This lunatic left'.

There is one situation in which subject-verb agreement in Bemba shows 'leakage' to semantics. The regular subject-verb agreement for class 1 nouns in Bemba is the morpheme *a*, as illustrated in (9b), which contains a 'long' subject relative. In (9a) we see that in 'short' subject relatives, this morpheme is replaced (data from Cheng).[6]

(9) a. umulumendo ú-u-ka-belenga ibuku [Bemba]
1boy 1REL-1SM-FUT-read 5book
'the boy who will read the book'
b. n-ali-íshiba umwaana uo Peter a-léé-tóntonkanya (ati)
á-ilé mailo
I-TNS-know 1child 1DEM Peter 1SM-TNS-think
that 1SM-left yesterday
'I know the child who Peter thinks left yesterday'.

What is important is that this kind of 'replacement' agreement only happens with class 1 nouns, typically singular nouns that are not only animate but also human. In other words, the system of agreement has to pay attention to the noun class, or the 'human' factor, in the case of 'replacement' agreement. This can be interpreted as an argument that shows that syntax is not self-contained because non-syntactic information interacts with syntax.

Variation

There are two types of variation that are problematic to syntactic autonomy. The first type is variation in terms of language change (through time), and the other is variation in individual speakers in terms of language use (a speaker may use a certain phrasing in one (social) context and a different phrasing in another). This fact can be used to argue that the adult's grammar is inherently variable and dynamic, and is easily affected by external forces, such as social function.

Leaving aside the problems that Croft points out (e.g., how the dynamic processes should be represented cognitively), the fact that variation or change exists appears problematic for the autonomy of syntax claim because in such a model of variable dynamics, the syntactic system never achieves a fixed and final (adult-level) stage as it is constantly susceptible to social interaction.

The variation or change does not just relate to individual word changes. It also includes changes that are syntactic. Take English as a concrete example. Unlike Frisian, Dutch and German, Modern English is not a V2 language (though it has residual V2 in interrogative sentences such as *what will John buy*[7]); this is easily seen in sentences in (10).

(10) a. *This book has {read} Piet a few times {read}.
(Intended sentence: This book, Piet has read a few times).
b. *Tomorrow will {give} Jan a talk {give}.
(Intended sentence: Tomorrow, Jan will give a talk).
c. *In Leiden have we a celebration on October 3rd.
(Intended sentence: In Leiden, we have a celebration on October 3rd).

Old English, however, is like Frisian, German and Dutch. It exhibits full V2 properties:

(11) a. Se Hæland wear ð þa gelomlice ætiwed his leornung-cnihtum.[8]
the Lord was then frequently shown his disciples
'The Lord then frequently appeared to his disciples'.
b. On twam þingum hæfde God þaes mannes sawle gegodod.[9]
in two things had God this man's soul endowed
'With two things had God endowed the man's soul'.

Middle English still preserves consistent V2. English has apparently changed from V2 to residual V2, with only a limited amount of V2 properties remaining. The fact that English syntax/grammar has changed suggests that children may not have acquired English to a final/steady stage; a conclusion which is problematic for a formalist view of grammar. Further, language change of this kind appears incompatible with syntactic autonomy since some changes appear to be triggered by external forces.

The dispute: For syntactic autonomy

Interactions

The objection to the autonomy of syntax which hinges on the fact that the system interacts with other systems is based on the assumption that if a system is self-contained, it should not be susceptible to 'external' influences. This is a curious assumption, I think. In biology, there are many systems which are generally recognised as autonomous systems but clearly interact with other systems. Take the visual system, which is clearly a self-contained system. However, vision certainly interacts with other systems (such as recognition, emotion). We can even be tricked into seeing things we do not see when we are fed information through different means. Vision, nonetheless, remains a separate system.

The most important task for linguists, especially syntacticians, is to figure out how the system interacts with other systems, and to develop a theory of interaction, or 'mapping' as current syntactic theories call it. In particular, if syntax interacts with interpretation, how does it interact? How are syntactic representations mapped into semantic representations? Since each system has its own internal rules and constraints, it cannot be the case that mapping is unconstrained.

Note that if the so-called 'leakages' in syntax from other domains are due to interactions between systems, we make a clear-cut prediction. If something goes wrong in the domain of system interaction (see below on interdisciplinary perspectives), the systems themselves remain intact. In the case of Bemba subject-verb agreement, for example, such a model predicts that in cases of interaction failure or inaccessible interaction, the replacement phenomenon will manifest itself differently (e.g., not restricting to only class 1 nouns). However, it is only in rare occasions that we detect 'interaction failures' (see below).

Variation

The problems posed by language change and individual variation constitute interesting and serious problems for the autonomy of syntax or grammar hypothesis. Autonomist linguists who do research on language change have developed the following model to deal with this problem. They assume that each individual has acquired a grammar, which is by and large the same as that of the other members of their speech community; the 'by and large' is crucial: individual grammars are never entirely identical since children who are acquiring a language never have entirely identical linguistic input. In other words, this model claims that language change occurs during language acquisition.

However, this cannot be the whole story. Consider the loss of V2 that took place between Old English and Modern English. It is conceivable that at a particular stage before Modern English, some speakers had V2, while other did not, and a third group had both. Synchronically speaking, at that stage, the rule of V2 was optional. The question is how come some speakers 'feel free' not to use this rule, while others use it optionally and others do not use it at all? The questions that arise are (a) whether a self-contained system can be subject to 'internal' change, that is, without an external trigger; (b) if change is triggered externally, does that necessarily lead to the conclusion that the system is not self-contained?

A self-contained system which changes through 'internal' pressure is not problematic for the notion of the autonomy of grammar/syntax. It is more problematic for the notion that when children achieve an adult-like level of language, they have attained a steady state. But this is probably the same question that one can ask about any biological system which evolves. How come a certain system changes through time? To answer this question, we must also answer the question of how language evolves through time.

As for external triggers of change, it seems to be rather problematic for a self-contained system. It is possible to appeal to interactions with the external systems, in the sense that since the system is not closed off to the outside (because it interacts), it is conceivable that it is through the interactions, for example the mapping between syntax and pragmatics, that change is brought about. Kroch has argued for co-existing/competing grammars.[10] The idea is that individuals can possess two grammars at the same time. This solves the problem of optionality in the sense that it is not the rule that is optional, rather, there are in fact two grammars. However, the question still arises as to why there are two grammars. Is it the case that children somehow develop two grammars at the same time or is it the case that adults develop an additional grammar at a later stage?

Recent work in computational models of language change and language evolution shows that a dynamic formal system is compatible with internal and external change.[11] Following this line of work, the disagreement between the autonomists and the non-autonomists may be resolved.

Interdisciplinary perspectives

There is a large body of work done since the early 1980s on child language acquisition, psycholinguistics and neurolinguistics. These studies provide an interesting and important perspective to the debate discussed here. In studies on child language acquisition and aphasic speech, it is possible to detect patterns that we do not see in normal adult language. Children acquiring a language do not have the whole system in place, or the interactions between systems may not be fully functional. For aphasics, due to brain damage, certain parts of the system do not function well.

Language acquisition

To fully appreciate evidence from child language acquisition research, we must first understand that children at a very early age already have a sophisticated system in place, even if they still produce syntactic errors (errors, that is, from an adult point of view). I discuss here first a phenomenon called 'Optional Infinitives' that shows this. Then I briefly discuss two studies which suggest that syntax develops separately from interpretation, and in fact runs ahead of interpretation.

OPTIONAL INFINITIVES

We discussed above that some languages, for example, Frisian, Dutch and German, have V2 properties, with the verb appearing in second position in main clauses. Consider now data from child language acquisition. In early linguistic development (until about 3 years old), children acquiring Dutch and German go through an Optional Infinitive (OI) stage. At this stage, children use both finite and infinitival verbal forms in main clauses, as shown in (12) and (13) (examples from Wexler).[12]

(12) a. pappa schoenen wassen [infinitival verb]
daddy shoes wash
b. pappa kranten weg doen
daddy newspaper away do

(13) a. ik pak 't op [finite verb]
I pick it up
b. baby slaapt
baby sleeps

Note, however, that children are extremely consistent (99%) when it comes to correlating V2 and finite verbs. That is, if they use a finite verb form, the verb is fronted to the V2 position while the infinitival verb remains in final position (98%) (as the examples in (12) and (13) also show). The OI stage shows that despite the young age, they already have a system in place: they do not put infinitival verbs in the second position and they do not put finite verbs in final position. It may not be the whole adult system, but it is a system nonetheless.

LONG DISTANCE QUESTIONS

In languages like German, interrogative phrases can either undergo long distance movement, as illustrated in (14a), or move 'partially' to an embedded location, allowing the main clause to be marked by a dummy question word, as in (14b) (examples from Herburger).[13]

(14) a. Wen glaubt der Georg [dass die Rosa geküsst hat]?
[long distance movem't]
who believe George that Rosa kissed has

'Who does George believe that Rosa kissed?'

b. Was glaubt der Georg [wen [die Rosa geküsst hat]]?
[partial movement]
what believe George who Rosa kissed has
'Rosa kissed someone, who does George think it was?'

In (14a), the interrogative word *wen* moves from the embedded clause (marked by the square brackets in (14a) to the front of the main clause. In (14b), the interrogative word *wen* moves to a position in front of the subject of the embedded clause; and *was*, which is a dummy question word here, appears in the front of the main clause. Herburger shows that these two sentences differ in their interpretation, in particular in the presupposition. The partially moved question (14b) is not felicitous in a context where Rosa did not kiss anyone, while there is no such presupposition requirement for long distance questions such as (14a).

Demirdache and Oiry studied long distance questions and questions with partial movement in child language (with children ranging from 3 to 6 years of age) in French.[14] Put simply, they found that children produce both long distance questions and questions with partial movement. However, they also found that the children's production of these different types of questions is independent of the presupposition requirement. This means that children have both strategies of question formation in place, but they are as yet unassociated with interpretation (in this case a presuppositional requirement).

RELATIVE CLAUSES

The fact that children have trouble comprehending object relatives such as (15b) in contrast with subject relatives as in (15a) has long been documented.[15]

(15) a. the girl that is kissing the granny (is my sister) [subject relative]
b. the granny that the girl is kissing (is my granny) [object relative]

In (15a), *the girl* is the subject of the clause *is kissing the granny* (a relative clause, which modifies *the girl*), and in (15b), *the granny* is the object of the clause *the girl is kissing*. In a recent study on Hebrew child language (around 4 to 5 years old), Arnon replicates the results of comprehension asymmetry between subject and object relatives.[16] Children comprehend

subject relatives 95% of the time while object relatives yield a chance level result (51%). Interestingly, however, children produce correct subject and object relatives without any significant difference (88% subject relatives vs. 93% object relatives).

INTERPRETATION

Both child language experiments just discussed show that production runs ahead of comprehension: the children form both types of long distance question sentences and have no problem making both subject and object relatives. Forming these sentences correctly is a syntactic matter. At the same time, it is clear that the children do not have a full grasp of the meaning of the sentences they build correctly. This may be due to the fact that the comprehension part requires connection with another system and that either this system or the connection between syntax and this other system has not fully developed yet. In any case, there is an asymmetry between comprehension and production, which may lead to the conclusion that syntax is indeed a separate system.

Aphasics

There is an abundance of studies on aphasic patients, who have suffered brain damage in various regions of the brain. I review here very briefly two different case studies, which both point in the direction of there being a syntactic system (despite the brain-damage), though a semantic or a non-syntactic system is no longer accessible/usable.

TRANSCORTICAL SENSORY APHASIA

The first case is a case of transcortical sensory aphasia which provides support for the autonomy of syntax claim. Patients with transcortical sensory aphasia hardly produce any spontaneous speech, but they can repeat the questions and statements that their interlocutors say to them. In a case study by Dogil et al.[17], a German-speaking patient with transcortical sensory aphasia is investigated. The authors test the patient using sentences with syntactic errors (e.g., agreement errors) and sentences with semantic deviance (e.g., having the wrong type of verb), as shown in (16a) and (16b) respectively.

(16) a.	Der Junge	spiel-*en/-t	mit	dem Hund
	the boy	play-PL/3SG	with	the dog

b. #Der Schwan	schwimmt	auf dem Tee
the swan	is.swimming	on the tea

Dogil et al. show that in the case of agreement errors (for subject-verb agreement), the patient did not just repeat the sentence; the patient corrected the error as well (80% of the time); he hardly ever repeated the ungrammatical sentences verbatim. In contrast, sentences with semantic deviance are repeated verbatim (72% of the time, and in two sessions 87.5%); he hardly ever corrected the semantic deviance.

Studies of this kind show that the agreement system triggers a different response from a non-syntactic system. This can be interpreted as showing that the syntactic system operates separately from the non-syntactic system. In normal adults, the constant interactions between these systems make it difficult to tear apart the individual systems. In the case of brain-damaged patients, we get a glimpse of the individual systems, operating separately.

There are two possible reservations concerning the results in this study. First, one may argue that sentences such as (16b) are difficult to correct since it may be unclear as to what the interlocutor really means. However, it should be noted that there is a significant difference in terms of the responses – the patient did not even randomly attempt to correct semantic deviance. Second, the semantic deviance is basically a selectional restriction violation; while the sentences may be semantically deviant, it is not the case that they do not mean anything.

BROCA'S APHASIA

Broca's aphasia is a more common type of aphasia, with disfluent and effortful speech. Broca's aphasics are typically characterised as having a deficit in both production and comprehension. In a case study on a Broca's aphasic patient, Saddy shows that although the patient showed a severe comprehension deficit in 'act-out' tasks, he can provide correct grammaticality judgments on the same type of sentences.[18]

Concretely, the patient was asked to create a cartoon representation of the sentence that was read to him, with cards depicting individuals as well as events. The sentences ranged from simple active sentences, to passive sentences (17b) and sentences with subject/object relatives (17a). The patient performed poorly in these act-out tasks, as indicated by the percentages given.

(17) a. The friend who sprayed Rose is sad. [relative clauses: 17% correct]
b. Bill was sprayed by Ken. [passive sentences: 8% correct]

In contrast, the patient could provide rather good grammaticality judgments (something that has been noted before in aphasics research).[19] The patient was asked to distinguish between sentences that he could say (e.g., *The boy kissed the girl*) and sentences that he could not say (e.g., **girl kissed the boy the*). It was clear from the results that the patient correctly judged sentences such as (17a,b) to be sentences that he could say, even though he had trouble with these sentences in a comprehension task. Furthermore, the patient was also tested on complex sentences with long distance dependencies, such as the one in (18), in which *who* originates as the object of the preposition *of* and is fronted to the beginning of the sentence to form an interrogative.

(18) Who_i do you think Bill likes pictures of $\_\_\_i$?

Note that long distance dependencies do not always yield grammatical sentences. There are constraints and restrictions as to (a) the 'length' of the movement, and (b) the original position of the moved element. Interestingly, the patient under investigation provided correct grammaticality judgments for sentences with long distance dependencies. For example, he, correctly, judged sentences such as (18) to be grammatical, and the ones such as those in (19a,b) as ungrammatical.

(19) a. *Who_i do you think pictures of $\_\_\_i$ are on sale?
b. *Who_i do you like stories that criticize $\_\_\_i$?

In other words, though the patient had trouble with comprehending sentences such as (17a,b) (perhaps having trouble distinguishing which is the agent of the action, and which is the patient of the action), he had no trouble judging whether a sentence is a good sentence or a bad sentence. This suggests that the patient has access to grammatical structure and grammatical rules, allowing him to provide correct grammaticality judgments. The poor performance with comprehension tasks suggests that interactions with other systems are disturbed or damaged. An

interpretation for this can be that the syntactic system is autonomous.

Neurolinguistic research

Within the last ten years, due to technological advances, it is possible to study brain activity, including linguistic activity, more directly. Here I discuss two methods in neurolinguistic research which may have some bearing on the issue of the autonomy of syntax.

'EVENT-RELATED' fMRI FINDINGS

We can make recordings of brain activity with electrodes placed on the scalp, and the recordings can be 'time-locked' to specific events (for example, stimuli). These recordings offer very fine-grained temporal resolution, and they can help us identify different brain potentials associated with different temporal stages of processing.

In neurolinguistic research, there are currently two indexes which appear relevant to syntactic and semantic processing. The first is the so-called 'N400', which is a negative-going event-related potential or ERP, which peaks around 400 ms following the onset of an anomaly. The second is the 'P600', which is a positive-going ERP, which peaks around 600 ms after an anomaly. The N400 is typically linked to semantic incongruities (e.g., using the wrong word as in **I take my coffee with milk and concrete*), while the P600 is linked to syntactic violations such as word order and grammatical category violations (e.g., **The scientist criticised Max's of proof of the theorem*),[20] indicating syntactic processing costs.[21] Although it must be noted that the nature of the P600 is not entirely clear since it seems to be also related to reanalysis of structure in general (including musical structure), we can still conclude that semantic anomalies are registered differently from syntactic ones.

While ERP registers temporal resolution, the fMRI technique (functional magnetic resonance imaging) offers spatial resolution, which allows us to identify regions in the brain involved in linguistic processing. For example, the Broca's area (the left inferior frontal gyrus) has been repeatedly shown to be activated with linguistic activities such as reading, as well as judging grammatical and ungrammatical sentences.

In a study using a combined ERP-fMRI technique[19], researchers found that syntactic violations lead to activation of brain areas that are different from areas that are activated due to semantic violations, confirming the results of the ERP studies distinguishing syntactic and semantic anomalies.

Taken as a whole, the results support the autonomy of syntax in the sense that syntactic processing depends on different neurolinguistic processes from semantic processing, with activation of different brain regions.

I would, however, add a word of caution with respect to this interpretation. The syntactic violations used in typical ERP and fMRI studies are often apparently word order violations, e.g., sentences such as **Yesterday I cut Max's with apple caution* (instead of the well-formed *Yesterday I cut Max's apple with caution*). Such word order violations are different from violations of syntactic rules. That is, sentences with reversed word order of this kind are ungrammatical because of a compositional error – e.g., after *Max's*, we expect to have a noun phrase (e.g., *apple*) and not a preposition. Though it is related to syntax, it is possible to interpret the violations as more 'selectional' than purely structural (recall the purely structural properties of the operation moving the finite verb into the V2 position). In other words, experiments on syntactic processes are called for (and not just simple word order variations).

Conclusion

In conclusion, the dispute on the autonomy of syntax can perhaps be settled by introducing results from interdisciplinary research into the discussion, such as neurolinguistic and psycholinguistic studies on normal speakers, aphasics and child language. The studies I reviewed above seem to point into one direction (syntax is autonomous), but it must be acknowledged that only a small number of studies have been done that yield results which seem to bear on the issue. More research is necessary. As was pointed out above, another area of research that may produce results relevant to the debate is the area of language change and language evolution. In all, this presents a rather exciting prospect for the years to come.

Notes

I would like to thank Rint Sybesma, Hamida Demirdache, Riny Huybregts, Doug Saddy and Ian Roberts for their help, suggestions and discussions.

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14
The dilemma of national history

Henk te Velde

To contemporary historians the name of the chair of Dutch history at Leiden University is a dilemma in itself. The chair was founded in 1860 as the first chair of history in the Netherlands and its Dutch name was and still is 'Vaderlandse Geschiedenis', or 'History of the Fatherland'. The word fatherland obviously does not necessarily refer to the Netherlands, but this is not the most important potential misunderstanding that is avoided by the neutral translation Dutch history. The emotional content of the word seems to suggest that this is not only the history of a certain geographical unit, but a patriotic duty. This was of course how the term was conceived in the first place. According to a well-known literary critic, the first holder of the chair, Robert Fruin, occupied 'a pulpit in a cathedral with the nation as an audience'. Fruin himself was a scrupulous historian who was famous for his critical use of sources. He debunked a lot of national historical myths that had become the accepted wisdom of the general public. He considered this to be his duty and it had become second nature for him, but sometimes he wondered whether he really performed his duty as a historian of the fatherland. Should he not rather try to stimulate national feeling? He need not have worried; while he busied himself demolishing all kinds of petty national myths, as a conservative liberal he was also helping to fortify the national consensus and the great myth of national history as the basis for the existing constitutional monarchy cum parliamentary system. It seemed also self-evident that national history was first and foremost political history. Like

Fruin, his first two successors, P.J. Blok and H.Th. Colenbrander, in fact also equated national and political history – Blok tried to write social history, but in practice mostly wrote plain political history. They were modern historians who checked their sources, but the national political history they wrote had patriotic overtones. For them there was no contradiction between objectivity and patriotism, and they saw it as their duty to be patriotic.[1]

This is no longer the normal conception of national history, neither in the Netherlands nor elsewhere. The writing of national political history is still common practice in almost every country, but its legitimisation is no longer based on a patriotic duty, not in Western Europe at least. More often than not, however, its importance is still being taken for granted. This seems natural, because nation states still exist and still are the main objects of political loyalty – even though many political decisions are already being taken at for instance a European level. Within the world of political historians a national orientation is still dominant: British political historians mainly write about Britain, French political historians mainly about France, etc. This is partly due to the demands of the public, but to a large extent also to the traditions of the discipline. As I will argue, historians used to defend this orientation – if they publicly defended it at all – on the basis of the uniqueness of their particular national history. Since the 1980s this defence of the writing of national history has increasingly come under attack and the dilemma of national political history has become clearer than ever: it is still not only a legitimate but also an important object of research, but how should we take into account the limits of this approach, now that the claims to exemplary uniqueness sound increasingly shallow? As long as there are nation states, the dilemma of national political history will not go away, if only because the nation state, the historical discipline and the writing of national political history have developed together and seem to have become inextricably intertwined. Of course there are areas of history such as parts of economic, social or cultural history etc. where this dilemma does not exist, and even many political historians ignore it, but it could be argued that it has been a dilemma for the historical discipline since at least the early nineteenth century. It is one of the oldest fundamental dilemmas of the academic historical discipline.

The historical discipline and national uniqueness

When the relatively new academic discipline of history developed in the nineteenth century, it went almost without saying that national political history was at its core. It is true, at the beginning of the nineteenth century the German historian Leopold von Ranke, often considered as the father of modern historiography, was still mainly interested in the international system of great powers. But to him it also seemed self-evident that national states were the basis of the international system, and he devoted a lot of time to his great ambition of writing a comprehensive history of Germany – which, by the way, he did not nearly finish. This side of his work presupposes the other side as it were. In order to be able to determine the uniqueness of German history he had to be aware of the histories of other nations and of the international system. At least this seems to be a logical interconnection. In the course of the nineteenth century, however, the old eighteenth-century conception of world history disappeared and international history became to a large extent the domain of specialist historians of diplomacy and international relations, whereas many historians tended to concentrate exclusively on their own national history.[2] Many of the most famous historians of the nineteenth century won their fame by writing national history, from Thomas Babington Macaulay in Britain to Jules Michelet in France, Heinrich von Treitschke in Germany, and Robert Fruin in the Netherlands. Even if they also wrote on other subjects, the emphasis was clear. Increasingly the uniqueness of each separate national history was taken as axiomatic. At the same time national states solidified; they were the centre of attention of the growing political participation and their actions increasingly penetrated individual lives. The marriage between (national political) history and the nation state seemed complete.

At the beginning of the twentieth century the proliferation of new disciplines of history such as social and economic history accelerated. The new disciplines did not necessarily concentrate on national states, and in order to get a more modern, perhaps also a more 'scientific' historiography, they started experimenting with comparative history. In the period between the wars one of the protagonists of the renewal of the historical discipline Marc Bloch, the French social historian, historian of mentalities and founder of the Annales school, was one of the first historians to theorise about the possibilities of comparison. In his

pioneering study he nevertheless assumed that the obvious units of comparison would be nations and states, and that in particular political differences were calling for comparison.[3] Recently the writer of an overview of comparative history still concluded that the legacy of transnational comparison was so strong 'that we often forget that nations do not have to be our units of comparison'.[4] But in most cases they have been, and this alone demonstrates the continuing self-evidence of the importance of national states.

On the other hand, explicit comparison could make historians aware that they were in fact always comparing, but without clear criteria. Every claim that a certain nation is distinguished by certain features, presupposes a comparison, albeit an implicit one. This is even more true when a nation is claimed to have 'exceptional' features. Even if most historians were more interested in the particular than in the general aspects of the past, which most of them indeed are, they could have used the 'individualising' instead of the 'generalising' kind of comparison, the comparison that aims at highlighting the specific characteristics of one case, instead of looking for similarities between all cases.[5] Partly as a consequence of the availability of source material, they concentrated exclusively on their own nation, and the uniqueness of the history of their particular nation was often just assumed or argued on the basis of the continuity of a few remarkable features. This type of historiography was far from neutral. It entailed implicit and more often than not explicit moral exhortations to keep the national identity pristine or to return to the values of a lost Golden Age.

In his famous *The Whig Interpretation of History* (1931) the British historian Herbert Butterfield turned against this sort of historiography in his own country. He attacked the tendency of historians in general to see the past as only the history of the present, and the tendency of British historians in particular to assume that the British people had in fact always been a people of freedom-loving and constitution-abiding liberals, and to identify with a certain moderate liberalism themselves. 'It is astonishing to what an extent the [British] historian has been Protestant, progressive, and Whig [= liberal], and the very model of the nineteenth-century gentleman'.[6] Butterfield did not so much charge the nationalism of British historians as such, but their teleological fallacies and their moral instead of true historical judgments. This tradition proved to be tenacious. At least until the 1960s, elements of the Whig interpretation of British history as a

moderate and liberal progress still lingered on, albeit often in a more sophisticated way.[7]

Even the Second World War, which demonstrated the dangers of extreme nationalism, had not ended the teleological view of national history and the assumption of continuity in national political history. On the contrary, the outcome of the war in a sense confirmed some historians in their national self-congratulation: they were happy that they were not German. The interpretation of the origins of the Second World War by the British historian A. J. P. Taylor is probably the most extreme example of this tendency. 'The history of the Germans is a history of extremes. It contains everything except moderation, and in the course of a thousand years the Germans have experienced everything except normality'. The problem with Hitler was that he was German.[8] In its extreme simplicity this view contained some elements of what was to be called the German *Sonderweg*, the idea that German history had known, at least since Bismarck, according to some since Luther, a course that differed from the normal western pattern. This explained the derailment of the 1930s and 1940s.

Until the Second World War the idea of a particular German history was valued positively by most German historians. After 1945 it was used to explain what had gone wrong. Because this was such an extreme case and it was so obvious that not only general but also particular German causes had to be found for the great tragedy, it demonstrated clearly the peculiarities of the writing of national history, in particular its stress on continuity and moral judgments. If there was a German *Sonderweg*, there ought to be a normal way, but this was hardly made explicit by historians. Historians mostly concentrated on just one nation, and were not so much interested in generalisations, let alone 'laws' of history. They assumed, however, that the nation they studied deviated from an allegedly general pattern, and were especially interested in these deviations. The attention of historians and the historical consciousness of most nations focused much more on the extraordinary aspects of their respective nations than on what they had in common with others.[9]

To concentrate on unique instead of common features seems the logical thing to do, but if you do not study other nations, the uniqueness of one particular nation is just an assumption. The idea of the *Sonderweg* implies a comparison with other nations. In this case the allegedly normal way was clearly the liberal British example. This was a strange situation. The British

had often prided themselves on their unique national history and liked to think of themselves as different from 'the Continent'. Who could be sure that the British did not have their own *Sonderweg*? In fact, the Whig interpretation of history claimed just that, although in this case, of course, not in a negative but in a very positive sense. It claimed that British exceptionalism had given them a head start and in fact thus showed the road to the future to other peoples. Macaulay, probably the most influential nineteenth-century defender of the thesis, even stated that the British historical fight for liberty and parliamentarianism had a universal meaning: 'That great battle was fought for no single generation, for no single land. The destinies of the human race were staked on the same cast with the freedom of the English people'.[10] The case of the *Sonderweg* demonstrates the pitfalls an assumption of 'deviation' from a general pattern will entail. It will lead to an exaggerated attention for the peculiarities of one nation, to the detriment of things they have in common with other nations, and to an overstressing of the elements of continuity in the history of that nation. As shown by the literature on the *Sonderweg,* this even remained the case after the tone of self-congratulation of national historiographies had come under attack.[11]

It may be logical to look for the specific elements of your object of research, but the almost universal claim to uniqueness seems to be a special feature of national history during most of the nineteenth and twentieth centuries. The navel-gazing of national history led to claims of 'exceptionalism': one's own nation was not only unique, but it was more unique than other nations. Take the example of French 'exceptionalism'. The French were supposed to have a unique history in view of the legacy of the Revolution and the special role of the state in French society. Even recently, historians who thought that French exceptionalism was coming to a close or who thought that exceptionalist claims were a bit extravagant, argued that France was unique as 'the universal nation' that pretended to have fought her Revolution not for her own sake but for the sake of humanity (and whose struggles therefore implicitly deserved more attention than those of others). Even if there was something such as a German exceptionalism, too, this universalism made French exceptionalism more exceptional than others.[12] The view of Macaulay, quoted above, makes this claim a bit doubtful, though: the British had also fought their revolution for the sake of freedom in general, and also had their 'welthistorische Zivilisationsmission'.[13] In the nineteenth century

this British claim seemed to be justified by the admiration of many foreign politicians and historians who took the British experience as a guideline for their own history. This admiration was connected to a political preference for British gradualism (radicals preferred France).

The persuasiveness of British or French exceptionalism depended to a large extent on the power of Britain and France. As long as they were great powers, dominating the world, their claims that their struggles for freedom and democracy were an example for everybody, seemed convincing. Their loss of power in the twentieth century, especially after the war, diminished their power as role model for other countries.[14] This affected, for instance, American ideas about the nature of the history of the United States. When criticising American exceptionalism, the president of the Organization of American Historians said that 'exceptionalism is America's peculiar form of Eurocentrism'.[15] She meant in particular romantic ideas about national destiny, but she could have focused on the content of American exceptionalism. This contained elements such as the importance of the 'frontier' in the West, and the absence of a feudal past and a socialist future. All these elements of American history were exceptional compared to European powers, but not, for instance, compared to Australia or South Africa. The achievements and history of the United States were thus measured against those of European countries. The inverse happened in the 1960s when political scientists Almond and Verba tried to define the 'civic culture' that a democratic system needed in order to avoid the dangers of fascism, and in fact took the United States (and Britain) as the yardstick to measure democratic achievements elsewhere.[16]

It could be argued that the ideas of exceptionalism and *Sonderweg* in different countries not only had some things in common but to a certain extent even depended on, and presupposed each other. This was also partly true for smaller countries. When in the late 1980s a few Dutch historians tried to define the exceptionality of the history of the Netherlands (in a decidedly non-nationalist way), they selected among other things Dutch 'pillarisation', the system of closed religious and political communities (Catholic, Orthodox Protestant, social-democrat) that had catered for their adherents from the cradle to the grave, and had dominated Dutch political and socio-cultural life between the late nineteenth century and the 1960s.[17] When in the 1960s political scientists analysed pillarisation or consociational democracy, they did so in reaction

to Almond and Verba, who had supposed that a divided society – which the Netherlands undoubtedly was – could not really provide a stable basis for democracy. Arend Lijphart tried to solve the paradox of the Netherlands with its divided society yet very stable democratic system, and contributed to the long discussion about the pillarised peculiarities of Dutch society.[18]

Claims of exceptionalism often rested on the juxtaposition of two extreme cases. In this vein the *Sonderweg* presupposed an implicit notion of British virtuousness. Once the Whig interpretation had lost its power, the *Sonderweg* was criticised, too.[19] British historians who specialised in German history, and were irritated about the tacit assumption that British history would be the 'normal' way, attacked the teleology of the *Sonderweg* in a way that would have been difficult for German historians; as outsiders it was easier for them to drop the moral judgments implied in the *Sonderweg*.[20] A few years earlier, a leading British socialist historian had still defended the 'peculiarities' of the British, that did not conform to Marxist laws, as against a dogmatism inspired by the French example of the French Revolution.[21] The new, also left-wing, criticism, on the contrary, was inspired by the conviction that British liberalism should not be presented as the pinnacle of political achievement.

Uniqueness and Comparison

In order to write the history of a certain object, most historians will be primarily interested in what distinguishes it from other objects. It could be argued that a certain measure of exceptionalism is thus the normal strategy in national historiography. According to François Furet: 'Chaque nation a une histoire exceptionnelle dans la mesure précisément où elle est nationale, donc particulière'.[22] A historian of American exceptionalism argues 'that every national history, American or non-American, is based on a more or less dominating and comprehensive notion of exceptionalism'. He even concludes that 'without a certain amount of exceptionalism national history is basically inconceivable'.[23] To top it, this tendency has been reinforced by the traditions of the historical trade. Most historians concentrate on the history of their own country and many debates in the historical world are about national history.

Most people would agree that national history is still an interesting and legitimate discipline and that the national framework is especially

important in political history – as parliaments, political parties, newspapers, and public opinion in general have all developed within a national context. How then to avoid concentrating too much on unique national characteristics? This has proved to be so difficult, that even successful attempts at renewal of national history have tended to consolidate the concentration on national uniqueness. The French history of the 'lieux de mémoire' could have been used to describe the memory of all kinds of communities, but was almost exclusively used to write a new national history. This resulted in fruitful new research and the idea was copied in other countries. Pierre Nora, the historian who introduced the idea, doubted however whether the idea was really fit to be exported. According to him the 'lieux de mémoire' were connected with the typically French mix of continuity and discontinuity in the national history.[24]

This modernisation (or 'postmodernisation') of national history was arguably not meant as a criticism but as a renewal. Even modernisation that was explicitly meant to criticise national history, however, tended paradoxically to reinforce it. The Marxist British historian Eric Hobsbawm introduced the notion of 'invention of tradition'. This expression was used to show that allegedly age-old traditions were in fact often recent inventions. The most telling example was provided by the British monarchy. David Cannadine's article about the invention of traditions surrounding British monarchy is critical of nationalist romanticism, but it does not leave the national framework; in fact, Cannadine was even pleading for a revitalisation of national history.[25] Hobsbawm himself, who wrote a lot about European (and partly even world) history, hardly uses his notion to look for international connections. Even the invention of the socialist holiday Mayday, which was obviously an international event, did not inspire Hobsbawm to really do comparative research or look for the mechanisms of international inspiration and imitation.[26]

The explanation for this paradox is perhaps that Hobsbawm was not really interested in particular nations as such. He wrote a book about nationalism that belongs to the wave of new literature on nationalism of the 1980s, which also includes books by Ernest Gellner and Benedict Anderson.[27] This literature argued that it was not nationalist ideas that mattered, but the social practices of the new nations. Nations were instruments of modernisation and homogenisation, and were partly their result, too. Their peculiarities did not matter, but the fact that nations

performed the same 'task' in many parts of the modern world did. This ironic reduction of the uniqueness of nations was a useful criticism of the exceptionalist stories of national history. The explicit or implicit type of comparison that was connected to this criticism did not look for the specific characteristics of one particular nation, but was looking for the things most or many nations had in common. This was a way out of the dilemma of writing national history, without denying the importance of nations and nationalism in the modern world.

It seems plausible that a generation of historians that had learned to be sceptical about nationalist claims concentrated on the function of nations rather than their identity. Connected to this is also a new interest in 'transnational' history which no longer takes nation states as its point of departure. Still, even for progressive historians this approach had its limits. It led to generalising rather than particularising comparisons and to arguments about nations in general rather than about one nation in particular, whereas most historians still were experts on the history of one nation. The debate on the *Sonderweg*, for instance, prompted a lot of comparative research, but after all mainly in order to determine the extent to which German history was particular. In the 1980s Jürgen Kocka directed a comprehensive comparative study of the European bourgeoisie, but its subtitle was 'Deutschland im europäischen Vergleich', and probably its main impetus was the question whether German history showed a *Sonderweg* in terms of the absence of a strong bourgeois culture or a 'Defizit an Bürgerlichkeit'.[28] The starting point was still the particular course of German history, but the comparative method was used to put national history into perspective. In the wake of the debate about the *Sonderweg,* a strong German tradition of comparative research developed which at least put all claims to exceptionality into perspective.

Transfer and National History Today

The comparative approach has been an important renewal, but it seemed to suggest that contemporary historians should leave their classic methods in order to avoid the pitfalls of nationalist teleology. Doubtless, modern comparative history has opened new vistas of research. Nevertheless, even many historians who want to avoid national navel-gazing prefer to stick to certain traditional methods. Historians of politics have traditionally used the method of the chronological narrative in order not only to tell how

things happened but also why. They have often tried to explain historical developments by looking for the origins of things. In classical national political history the history of a nation was more or less tantamount to its identity. Even if few contemporary historians would now make such a claim without qualifications, they still use the search for origins as one of their preferred methods of explanation. International comparison could be used to put this search into perspective, but would not necessarily change it: implicitly national political phenomena seem to call for national explanations. This seems plausible because the main institutions of political life and also the main political loyalties have developed within national contexts. Yet, as has been argued, the fact that both political life itself and the study of the history of politics have a strong national focus has led to exaggerated attention for the uniqueness of national history. Beside comparative history, there is another alternative that could put this uniqueness into perspective, in this case by making use of more traditional methods.

As an addition to – not a substitute for – national history, attention could be focused on what has been called the history of 'transfer', the use of foreign inspiration and foreign examples within a national context.[29] This concept has mainly been used to study intellectual history, but it is also fruitful to study political transfer or the migration of political practices across national borders.[30] This will obviously not solve the dilemma of national history, but it, too, will help to put it into perspective, and it will yield important new insights. Comparison had already shown a long time ago that nationalism itself was a very international phenomenon. In many countries, a form of nationalism developed around 1800, and this simultaneity was no coincidence. According to historians of nationalism such as Gellner, the same needs produced the same results, and the development of nationalism was the result of the emergence of nations (not the other way around) called for by the modern world. Undoubtedly, this was an important part of the story, but partly it was even simpler than that. The famous case of German nationalism developing as a reaction to French expansionism shows that nationalism did not develop in a vacuum, but that nationalists were aware of what happened in other nations. Even a phenomenon as nationalism was at least partly the result of a foreign impetus and the imitation of what was happening elsewhere.

For a long time historians considered it to be their patriotic duty to

concentrate on national history, and neglected foreign inspiration, or thought it an improper aping of foreign habits. For instance the introduction of a modern parliament in the Netherlands after 1795 was inspired by French examples which have not been studied adequately until today. This was clearly and undeniably borrowing from abroad, but often the national bias of political history has tended to completely obscure the foreign origins of political developments. The French parliamentarianism which was introduced in the Netherlands was itself at least partly an imitation of British parliamentary practices. When the French had to set up a new parliament after the Revolution of 1789, they used the example of Westminster where a modern parliament had already been in the making for over a century.[31] This imitation has long gone unnoticed and the same holds true for parliamentary history in general – it seems obvious that parliaments are national institutions, and international influences that determined their nature have long been neglected or even denied.

Also the history of political parties has been written almost exclusively from a national perspective. Especially their origin and foundation at the end of the nineteenth century were often inspired by foreign examples. Even if this is not unknown, the concentration on the national context has tended to reinforce the underestimation of the international dimension of politics. We are only beginning to see how many interconnections there were between social movements in different countries in the nineteenth and twentieth centuries. The student movement of the 1960s took a different form in different national contexts, but its international inspiration is clear. The same holds true for older social movements, such as the women's movement or anti-slavery.

Attention for international transfer would mean a useful supplement to national history, without denying the importance of the national level in nineteenth and twentieth century political history. It would also enable a better understanding of the development of political life. By looking for foreign origins of national political institutions and traditions, it will demonstrate the international framework of political development. To put it in a paradoxical way: national politics have been an international phenomenon.

This addition would not mean the end of national history, but its adaptation to a new situation. On the one hand the current European trend of putting national history in an international perspective has

something to do with the emergence of the European Union and the loss of power of the national states. In 1992 Pocock, the well-known historian, asked 'what becomes of such a [nationally orientated] historiography, and the identity it offers the individual, when sovereignty is to be modified, fragmented or abandoned?'[32] In this sense there is nothing new under the sun: historiography is following the shifts in power. Perhaps one day the premature appeal of David Cannadine that the 'first task' of British historians was 'to persuade the public' that their national history was 'a subject worthy of study in 1987', will be reiterated.[33] On the other hand dilemmas are a stimulus for the historian once they have been recognised as such. Implicit dilemmas are blind spots which should be 'enlightened', but once they are recognised they stimulate debate and research. So let us hope that the dilemma of national history will still stay with us for some time.

Notes

1 See e.g. Tollebeek, Jo, *De toga van Fruin. Denken over geschiedenis in Nederland sinds 1860* (Amsterdam 1990) p. 34: quotation from Conrad Busken Huet ('een kansel in eene kathedraal met eene natie tot gehoor'); cf. P.B.M. Blaas, 'The touchiness of a small nation with a great past: the approach of Fruin and Blok in the writing of the history of the Netherlands', in: A.C. Duke and C.A. Tamse (eds.), *Clio's mirror. Historiography in Britain and the Netherlands* (Zutphen 1985) p. 133-161, and N.C.F. van Sas, 'De mythe Nederland', in: id., *De metamorfose van Nederland. Van oude orde naar moderniteit, 1750-1900* (Amsterdam 2004) p. 523-534.

2 Schulin, Ernst, 'Universalgeschichte und Nationalgeschichte bei Leopold von Ranke', in: Wolfgang J. Mommsen (ed.), *Leopold von Ranke und die moderne Geschichtswissenschaft* (Stuttgart 1988) p. 37-71; cf. p. 57: 'Die eigentlichen Vertreter der neuen quellenkritischen Geschichtswissenschaft konzentrierten sich auf die eigene Nation'.

3 Bloch, Marc, 'Pour une histoire comparée des sociétés européennes' (1928), in: id., *Mélanges historiques* 2 vols. (Paris 1963) I, p. 16-40; in particular p. 17.

4 Berger, Stefan, 'Comparative history', in: id., Heiko Feldner and Kevin Passmore (eds.), *Writing History. Theory and Practice* (London 2003) p. 162.

5 Cf. ibidem.

6 Butterfield, Herbert, *The Whig Interpretation of History* (1931; Harmondsworth etc. 1973) p. 13.

7 Stuchtey, Benedikt, 'Literature, Liberty and Life of the Nation. British Historiography from Macaulay to Trevelyan', in: Stefan Berger a.o. (eds.), *Writing National Histories*; Arnd Bauerkämper, 'Geschichtsschreibung als Projektion. Die Revision der "Whig Interpretation of History" und die Kritik am Paradigma vom "deutschen Sonderweg" seit den 1970er Jahren', in: Stefan Berger, Peter Lambert and Peter Schumann (eds.), *Historikerdialoge. Geschichte, Mythos und Gedächtnis im deutsch-britischen kulturellen Austausch* (Göttingen 2003) p. 396.

8 Berger, Stefan and Peter Lambert, 'Intellectual Transfers and Mental Blockades: Anglo-German Dialogues in Historiography', in: ids. and Schumann (eds.), *Historikerdialoge,* p. 49; Kavin Passmore a.o., 'Historians and the Nation-State. Some Conclusions', in: id. a.o. (eds.), *Writing national histories,* p. 295.

9 Cf. Conrad, Christoph, and Sebastian Conrad (eds.), *Die Nation schreiben. Geschichtswissenschaft im internationalen Vergleich* (Göttingen 2002).
10 Macaulay (1824), quoted by Stuchtey, 'Literature, Liberty', p. 32.
11 Apart from the other articles quoted in the footnotes see e.g. Helga Grebing, *Der 'deutsche Sonderweg' in Europa 1806-1945* (Stuttgart 1986).
12 Furet, François in: id., Jacques Julliard and Pierre Rosanvallon, *La république du centre. La fin de l'exception française* (Paris 1988) p. 54; Laurent Wirth, *L'exception française 19e – 20e siècles* (Paris 2000) p. 18.
13 Weisbrod, Bernd ,'Der englische "Sonderweg" in der neueren Geschichte', *Geschichte und Gesellschaft* 16 (1990) p. 249.
14 Cf. David Cannadine, 'British History, Past, Present and Future'. In: *Past and Present* nr. 116 (1987) p. 169-191.
15 Appleby, Joyce (1992), quoted by Peter Bergmann, 'American Exceptionalism and German *Sonderweg* in tandem'. In: *The International History Review* 23 (2001) p. 505.
16 Almond, Gabriel A. and Sidney Verba, *The Civic Culture. Political Attitudes and Democracy in Five Nations* (Princeton 1963). The five nations were the U.S., Mexico, Great Britain, Germany and Italy.
17 Davids, Karel, Jan Lucassen and Jan Luiten van Zanden, *De Nederlandse geschiedenis als afwijking van het algemeen menselijk patroon* (Amsterdam 1988).
18 Lijphart, Arend, *The Politics of Accommodation. Pluralism and Democracy in the Netherlands* (Berkeley 1968).
19 Bauerkämper, 'Geschichtsschreibung als Projektion'.
20 Blackbourn, David and Geoff Eley, *The Peculiarities of German History. Bourgeois Society and Politics in Nineteenth-Century Germany* (Oxford 1984). All literature on the *Sonderweg* stresses the importance of the book, that had appeared in German a few years earlier.
21 Thompson, E.P., 'The Peculiarities of the English' (1965), in: id., *The Poverty of Theory & Other Essays* (London 1978) esp. p. 36-37, 56, 78.
22 Furet in id. a.o., *République du centre*, p. 54.
23 Guggisberg, Hans R., 'Amerixan Exceptionalism as National History?', in: Elisabeth Glaser and Hermann Wellenreuther (eds.), *Bridging the Atlantic. The Question of American Exceptionalism in Perspective* (Cambridge 2002) p. 265 and 276.
24 E.g. Pierre Nora, 'La notion de "lieu de mémoire" est-elle exportable?', in: Pim den Boer and Willem Frijhoff (eds.), *Lieux de mémoire et identités nationales* (Amsterdam 1993) p. 3-10.
25 Cannadine, 'British History'.
26 Hobsbawm, Eric and Terence Ranger (eds.), *The Invention of Tradition* (Cambridge 1983); this collection of essays contains Cannadine's article about the British monarchy.
27 Anderson, Benedict , *Imagined Communities. Reflections on the Origin and Spread of Nationalism* (Norfolk 1983); Ernest Gellner, *Nations and Nationalism* (Oxford 1983); Eric Hobsbawm, *Nations and Nationalism since 1780. Programme, Myth, Reality* (Cambridge 1990).
28 Kocka, Jürgen (ed.), *Bürgertum im 19. Jahrhundert. Deutschland im europäischen Vergleich* 3 vols. (München 1988). In his preface Kocka (I, p. 7-8) refers to the impetus provided by the book by Blackbourn and Eley. 'Defizit' on p. 1 (without page number).
29 Espagne, Michel and Michael Werner (eds.), *Transferts. Les relations interculturelles dans l'espace franco-allemand (XVIIIe et XIXe siècle)* (Paris 1988) was the starting point for the debate about cultural transfer. An interesting recent development is the combination of transfer and comparison in a history of international connections, most recently in Michael Werner and Bénédicte Zimmermann, 'Beyond Comparison. Histoire Croisée and the Challenge of Reflexivity', in: *History and Theory* 45 (2006) p. 30-50.

30 Velde, Henk te, 'Political transfer. An introduction'. In: *European Review of History* 12 (2005) 205-221.

31 See e.g. Edna Hindie Lemay, 'Les modèles anglais et américain à l'Assemblée constituante', in: *Transactions of the Fifth International Congress on the Enlightenment* (Oxford 1980) p. 872-884; Nicolas Roussellier, 'The Political Transfer of English Parliamentary Rules in the French Assemblies (1789–1848)', in: *European Review of History* 12 (2005) p. 239-248.

32. Pocock, J.G.A , 'History and Sovereignty. The Historiographical Response to Europeanization in two British Cultures', in: *Journal of British Studies* 31 (1992) p. 358-389 (p. 364).

33 Cannadine, 'British History', p. 188.

15
Region or discipline? The debate about Area Studies

Erik-Jan Zürcher

Soul searching in Leiden

In 2005-6 the Arts Faculty of Leiden University witnessed heated debates about proposals, put forward by the Faculty Board, for the introduction of a 'core curriculum'. The idea was that all students, irrespective of their chosen field, would be obliged to take courses in a number of subjects, the relevance of which to all students in the humanities was incontestable (at least in the eyes of the Board): linguistics, literary theory, cognitive philosophy. The proposals were suspect in the eyes of many because they were partly motivated by the desire to have departments share courses in order to alleviate the dire financial circumstances of the Faculty, but they were in part also motivated by a sincere conviction that broadening the educational profile of the BA degrees on offer made sense. After all, with the introduction of the two-tier BA-MA structure in the Netherlands in 2002, it could be argued that the function of the BA was to provide a college-type education, which would provide graduates with the basic skills, knowledge and 'habitus' of an academic, while specialist training in a particular field was best left to the graduate phase, which led to MA, M.Phil. and Ph.D. degrees.

Opposition to the idea of the core curriculum came from different quarters. There were those, of course, who saw in this a further erosion (after the introduction of sizeable minors in the BA programmes) of their ability to reach acceptable academic standards in their fields of

specialisation. How could one be expected to produce 'real' Arabists or Romanists with little over two years of specific teaching? The most determined opposition, however, came from a group of professors of East Asian studies. They addressed a letter to the Faculty Board in which their argument ran as follows:

> 'We feel that the plans communicate the idea that this Faculty persists in identifying itself narrowly, in terms of the "classical (Western) humanities". However … the Faculty of Arts in Leiden is home to a wide range of multidisciplinary research and teaching programmes, many of which do not fall under the classical humanities. We refer specifically to the various departments of Area Studies, which provide education in various disciplines of the social sciences in addition to the humanities'.

The authors of the letter then went on to argue that the very minimum that would be required, would be:

> 'Adding an extra "core" course that addresses the needs of Area Studies (such as a course on the theoretical and methodological requirements of combining the study of region with the study of conventional disciplines)'.

This, however, was not their preferred solution. That would be:

> 'a system of disciplinary minors throughout the university and [utilising] the in-house core curriculum to support the core educational needs of all Arts students'.[1]

So, in a roundabout way, a discussion triggered by Faculty plans for improved efficiency and a broadening of the educational profile of the BA through a renewed emphasis on core courses in humanities drew attention to the complicated relationship between Area Studies and the disciplines.

It was not the first time this issue was tabled within the Faculty of Arts in Leiden. Just a few years earlier, in February 1999, a study had been commissioned by an earlier Faculty Board to

> 'map all the existing facilities in the field of contemporary oriental studies and to advise on ways to achieve a higher degree of integration between these facilities, both in teaching and in research'.

In its report the study group charged with producing a report on this matter (and which I had the privilege of chairing) noted that Leiden had an old and very strong tradition in oriental studies, but that relatively little attention was being devoted to contemporary developments within these studies and that:

> 'Oriental studies were seen as an entirely separate sector, in which discipline-based departments did not have any role to play'.[2]

Special attention was drawn to the case of history. Each of the Area Studies departments within the faculty (East Asian studies, Southeast Asian studies, South- and Central Asian studies, Middle East studies, African studies and Latin American studies) taught courses and conducted research on historical subjects, but the central role allocated to source materials in the languages of the region meant that within these departments there was very little awareness of the useful role that could be played by historians of other areas. The reverse was also true: the large and successful history department devoted some attention to the history of non-European areas, but it did so entirely from a European perspective and without having recourse to the existing expertise in the Area Studies departments.

In its advice on ways to remedy the situation, the study group made the classic case for Area Studies, saying that situating the study of the contemporary non-Western world within the study of its historical and cultural heritage, makes it possible to gain a deeper understanding, adding that, for this not to become a hollow phrase, in 'classical' courses, the ties to contemporary developments needed to be made explicit. In a course on Confucianism, contemporary Singaporese neo-Confucianism needed to be treated as much as debates about veiling in a course on the sources of Islamic law. Likewise, the importance of historical and cultural source materials for a better understanding of contemporary developments needed to be made explicit in courses on Chinese government or Islamic political movements.

Interestingly, the argument thus employed was an exact copy of that made by the advocates of the introduction of Area Studies fifty years earlier, but, like the group of concerned professors from oriental studies departments cited above, the study group seemed to disregard the more recent debates on the pros and cons of Area Studies that had raged in the United States in recent years. To understand why this should be so, I will now discuss the development of oriental and Area Studies in the United States and in Europe, more particularly in the Netherlands. In doing so, I will mostly use examples from that regional specialisation with which I am most familiar, the Middle East. Area Studies have traditions and subcultures of their own, which often differ widely from those in other parts of the field, but I am confident that some of the more basic problems faced by Middle East studies are valid in other areas as well.

Area Studies in the United States

Area Studies were first developed as a concept in the United States after World War II. The concept entailed the attempt to 'know, analyse and interpret culture through a multidisciplinary lens'.[3]

The study of non-western societies and cultures had been late to develop in the United States. The first oriental institute had been founded in 1919 in Chicago and in its first decades it occupied itself exclusively with the philological and to a certain extent historical study of the ancient Near East. The first programme in Arabic and Islamic studies started at Princeton in 1927. In the decades that followed oriental studies developed, but only slowly and there was still so little home-grown talent that foreign orientalists had to be imported to man the chairs. When we look at Middle East studies, we see that the programme in Princeton was created by a Lebanese historian, Philip Hitti, while Sir Hamilton Gibb, the dean of British orientalists founded the Near East institute in Harvard and the German Gustav von Grunebaum the programme at UCLA. The departments, institutes and programmes founded by these foreign specialists largely conformed to the well-established European pattern of oriental studies, that is: they were centres for text-based study of monuments of high culture.

The underdevelopment of oriental studies in the U.S. to an extent reflects the political realities, more precisely American isolationism, which, except for the brief period of 1917-19, was the dominant political

current for more than three decades. In the isolationist climate of the interbellum there was no political support for investment in non-Western studies, but there were those in the private sector, who realised their importance. Decades before the federal government, the Rockefeller Foundation and even more so the Ford Foundation, which had grown out of multinational corporations with worldwide activities, sponsored programmes on contemporary Asia, Africa and Latin America.

World War II, which was a 'world war' to a far greater extent than World War I had been, created an awareness of the lack of expertise available in the U.S. on the non-Western world. Many of the experts in oriental departments in the U.S. and the U.K. were drafted into the intelligence establishment, but clearly this was not enough. This problem became all the more acute when the Cold War started to take shape in the later 1940's. The places where the confrontation with communism erupted into a 'hot war' or threatened to do so, were often in non-Western regions (the short-lived Soviet republic of Azerbaijan in Iran, the Korean War and, of course, that 'loss of China' that had such a traumatic impact on the United States). The emergence of left-wing and nationalist military regimes in the Arab world in the 1950's as well as the creation of the 'non-aligned' movement (with strong anti-Western leanings) by Nasser, Nehru and Sukarno seemed to threaten the position of the United States worldwide.

There was a growing realisation that it was not enough to apply economic and political theory developed in and for the West to non-Western areas and that a deeper knowledge of cultural factors was needed to understand developments. At the same time the humanistic, text-based scholarship of the classical orientalist scholars and departments did not seem to be able to provide knowledge that was relevant to contemporary developments. This realisation provided the impetus for the creation of multidisciplinary area studies. Middle East Studies was first off the mark. The Middle East Institute was founded in Washington DC in 1946 and in 1947 its journal, the Middle East Journal, was the first to be devoted to contemporary developments in the Middle East. The Fairbank Center at Harvard University, then still called the Center for East Asian Research, was founded in 1955 with a brief to create a multidisciplinary environment for Chinese studies from the start.

In 1957 the National Defence Education Act, a product of Cold War anxieties, made large scale federal investment in academic study of the

non-Western world a possibility. This led to a spectacular growth in the number of specialists in disciplinary departments (for instance: history, sociology or political science) but also to the creation of a number of Area Studies departments, where different disciplines were combined, both in research and in teaching.

In the fifties and sixties, the overarching theoretical framework in which most of the research on Asia, Africa and Latin America was set, was that of modernisation. This assumed that all societies, in order to 'develop' had to make the transition from traditional to modern through a process of social engineering and economic change that first would allow them to achieve 'take-off' and would eventually bring them onto the same level of development as the industrialised countries of the West, more specifically the United States, whose social organisation was implicitly or explicitly considered the final destination of the process of modernisation. In this context United States involvement with the development of non-Western states was obviously seen as a force for progress, spreading modernisation and development.

By the late sixties and early seventies, however, more and more of the area specialists that had achieved academic positions thanks to the investments of the 1950s became vocal critics of U.S. policies. The Vietnam War of course played the dominant role, but for Middle East specialists (many of whom had their roots in the Middle East themselves) the 1967 Israel-Arab War, in which the U.S. sided unequivocally with the Israelis, was the defining moment. Theoretically, the specialists were most often inspired by the ideas of dependency theory that had originated in Latin America and that saw underdevelopment as the result of a specific form of integration of peripheral countries in a world economic system dominated by the industrialised nations ('the development of underdevelopment').

From the 1970s onwards Area Studies have become the object of intense criticism, partly for its ideological and political tendencies and partly for its scientific contribution (or lack thereof).[4]

The first wave of criticism came from the left, which denounced Area Studies as a Cold-War product, designed to buttress American control over the Third World. This critique was given a far greater degree of sophistication by Edward Said, who in 1977 published his seminal *Orientalism*.[5] Said, a literature professor, combined the post-modernist deconstructivist approach borrowed from Jacques Derrida, with its

sensitivity to subtexts and hidden messages, with an anti-imperialist critique inspired both by Franz Fanon's ideas on the psychology of colonialism and Antonio Gramsci and Michel Foucault's ideas on hegemony and cultural capital. He argued that the 'orient' and geographical units like 'the Middle East' were ideological constructs used to ascribe identities to foreign populations in such a way as to support claims for Western domination. He accused both classical orientalists and area studies specialists of serving colonial and imperialist interests by constructing an 'exotic, inscrutable and essentially weak' other whose subjugation to the rational and strong West was inevitable and essentially good. Said's ideas had an enormous impact,[6] but from the eighties, with the rise to power of the new right under the presidency of Ronald Reagan, they were mirrored by a rising tide of criticism from the American right, that denounced the Area Studies specialists as being politically motivated and intellectually corrupt. Like Said's critique, this one was mainly concentrated on the field of Middle East studies and it was influenced by the 'loss of Iran' (the fall of the Shah's regime in 1979), which was almost as traumatic an experience for the U.S. as the 'loss of China' had been thirty years earlier. Scholars sympathetic to Israel called attention to what they saw as the ideological prejudices of many people active in the field of Middle East studies and the crest of this wave of criticism was the establishment by neo-con Middle East specialist Daniel Pipes after the attacks of 9/11 of Campus Watch, an organisation that aimed at identifying anti-Israeli and anti-American indoctrination in American universities.[7]

The American victory in the Cold War in 1989-91 of course fundamentally weakened the position of Area Studies as a political project, except perhaps for the study of the Middle East, which received a renewed impetus from the fact that in the post-Cold War world radical Islam, which had been on the rise throughout the 'Islamic world' (a term baulked at by Said), was increasingly perceived as the next big threat to Western hegemony. This made Area Studies vulnerable to criticisms of their intellectual content. Within the disciplines, Area Studies were and are often seen as descriptive, analytically weak and without strong theoretical foundations. The contribution of the Area Studies to the advancement of theory was said to be negligible. Robert Bates, an Africanist himself, in 1996 went so far as to say that:

> 'within the academy the consensus has formed that Area Studies have failed to generate scientific knowledge'.[8]

In addition, there were those who argued that in an era of accelerated globalisation taking an area as the unit of analysis was increasingly meaningless.

The Area Studies did not take all this criticism lying down. The case for the defence was mainly constructed around two points: First, that the borderlines between the different disciplines were increasingly blurred and that, in fact, the most interesting research was being done on the interface of, say, psychology and anthropology; second, that data from Area Studies were helping to make theories that had by and large been devised in the West, truly universal through the application of a comparative method, or, to put it rather more aggressively: that the mission of Area Studies was to de-provincialise the disciplines. Spokesmen for Area Studies pointed to the theoretical contributions made by people like the anthropologist Clifford Geertz with his method of 'thick description' and his new interpretation of the concept of culture. The weakness of this argument lay in the fact that the undoubted influence of Geertz lay within the discipline of anthropology and thus could hardly be used as evidence of the unique contribution of the interdisciplinary Area Studies approach. Likewise, the group of Indian historians who launched the concept of 'subaltern history' have undoubtedly had a global impact, but *within the field of history*. Perhaps the theoretical contributions of Benedict Anderson, who, as head of the Southeast Asia programme at Cornell certainly qualified as an Area Studies person, provides a better case, as his concept of the 'imagined community'[9] has been taken up by historians, sociologists and anthropologists alike. James Scott's analysis of everyday peasant resistance[10] is another example of a study that has been influential across the boundaries of a single discipline, but these are relatively rare examples and in the early twenty-first century the intellectual credentials of the Area Studies as a separate field remain deeply contested in the United States.[11]

The Netherlands: Area Studies Light

The debates about the Area Studies, their relationship with the disciplines, their validity in the face of globalisation and their political implication

hardly found an echo in the Netherlands. The debate was followed with keen interests by social scientists with an interest in Latin America or Africa, but it had very little impact where it mattered most: in departments of Chinese Studies, Middle East Studies or South-Asian Studies.

The reason for this can be found in the very different antecedents of these studies in the Netherlands. Where in the United States before World War II oriental studies were so underdeveloped that European orientalists like Gibb and Von Grunebaum had to be imported, in the Netherlands and in Europe in general some oriental studies had centuries of development to look back on, development that had given them a strong position in a number of centres of learning. This is true in particular for the study of the Semitic languages and related literatures. The importance of bible studies brought with it an interest not only in Hebrew, but also in Arabic and in its wake the other main languages of the world of Islam, Persian and Turkish. The great humanist Scaliger, the first academic with a European reputation to teach in the Northern Netherlands (in Leiden from 1593) studied Arabic manuscripts, and his successors Golius and Erpenius made Leiden into a centre of world repute for oriental studies in the seventeenth century. In the nineteenth century, partly under the impetus of colonial expansion, the number of chairs in oriental languages and literatures grew considerably. Chinese, Japanese and Sanskrit were added to the curriculum and Arabic studies experienced a new flowering with names like Dozy, De Goeje and Snouck Hurgronje. Up to the middle of the twentieth century the people who held these chairs were scholars whose authority ultimately rested on their philological skills in the handling of monuments in the classical languages (Mandarin Chinese, Classical Arabic, Javanese, Sanskrit, Ancient Egyptian) of their region, often – though not always – coupled to first-hand experience of living in the area. Their research interests generally ranged over a broad selection of the humanities: history, philosophy, religion, linguistics and literature and both the academic world and society at large recognised their authority in all matters relating to their field from ancient times to the present day.

The nearest thing to something that somewhat anachronistically could be called Area Studies developed, not in the faculties of Dutch universities, but in an independent institution established in Delft in 1851, the 'Royal Institute for the Linguistics, Geography and Ethnology of

the Indies' (KITLV). It was a direct by-product of the colonial government's hunger for experts and administrators.

When chairs developed into departments in the second half of the twentieth century, the situation changed in the sense that, as Sinologist Wilt Idema noted in his 1990 stocktaking of Area Studies in the Netherlands,

> 'Today such wide-ranging interests … are spread over several scholars within a department. In the Netherlands, although traditional names of chairs have been retained, the departments are usually named "Languages and Cultures of …", in order to distinguish them from departments dealing with the modern Western languages, which genuinely restrict themselves to language and literature … . Through their choice of name non-Western studies indicate clearly that in their view, not only language and literature, but also history, art, religion, and philosophy, in short the whole culture of the area in question, are included in their domain'.[12]

According to Idema, this qualifies these units as Area Studies departments, because the region rather than the discipline constituted the common denominator, but the range of research areas he enumerates shows that what we have here is a differentiation within the humanities, with separate specialists taking upon themselves one aspect of the activities of the original chair. That is something completely different from the American concept of an Area Studies department, as that would entail the combination of humanities and social science disciplines. Idema recognises that 'valuable multidisciplinary groupings' had been set up but he also notes that 'multidisciplinary groupings have become fashionable, but are not sufficient' because of the need for 'constant linking back to the core disciplines to guarantee the methodological quality of the research'.

The deep roots of Dutch oriental studies in the humanities have meant that there has been a lot of institutional resistance to the inclusion of social scientists in the core departments, even though in 1990 a leading orientalist like Idema already recognised that 'the borderline between humanities and social sciences becomes meaningless'.[13] Some anthropologists and political scientists were appointed in 'departments of languages and cultures of …', but they remained on the fringes. At the same time in the social sciences there were a few, but only a few, attempts

to create genuine Area Studies centres, such as the Africa Studies Centre in Leiden and the Centre for Research and Documentation on Latin America (CEDLA) in Amsterdam. Most social science experts on non-Western regions were appointed in departments of anthropology, 'non-Western sociology' or political science. Their status within their own faculties was not equal to that of their colleagues in the humanities, however, and until the establishment of supra-university institutes like the International Institute of Asian Studies (IIAS) and the Institute for the Study of Islam in the Modern World (ISIM) in the 1990s they rarely reached full professorial level. Especially the founding of ISIM was a venture designed to break the monopoly of the classical oriental departments in top-level academic positions.

As far as teaching is concerned, the 1980s witnessed the creation of a small number of truly multidisciplinary Area Studies programmes, not all of them concerned with the non-Western world: European Studies in Amsterdam and Middle East Studies and Mediterranean Studies in Nijmegen. Although these, perhaps predictably, suffered from problems with coherence, they proved popular with students and have survived to this day. These teaching programmes produced very little in the way of multidisciplinary or comparative research, however.

Conclusion

What can we conclude about the current situation and the future prospects of the Area Studies and their sisters in the Netherlands? The question cannot be usefully answered in a general sense. One has to look at specific aspects of the field and may well reach different conclusions for each of them.

The original premise for the Area Studies approach was that it would be possible, indeed advantageous, to combine representatives of different disciplines in a multidisciplinary, geographically defined, framework. The idea was that the object of study would define the questions. As we have seen, this approach has been criticised and attacked from all sides, but has it yielded significant results? If this question pertains to the advancement of science and scholarship, I think the answer has to be mostly negative. The best work produced in the past generation has tended not to be multidisciplinary at all, but has consisted of the application of advanced methodologies and theoretical concepts from a single discipline to a given

field. The disciplines involved have ranged widely, from philology to history and from economics to literary theory and cultural studies. Data gleaned from non-Western areas and presented by Area Studies specialists have had an impact within the disciplines and exposed their Eurocentrism. Sometimes theoretical or methodological innovations by scholars in Area Studies have proved important, like the subaltern history school or Geertz's thick description, but the point is that these contributions were made within a single discipline: the Indian subaltern history people influenced historiography worldwide and Geertz became a leading influence within anthropology. So the claim that the multidisciplinary character of Area Studies in itself has enriched scholarship seems ill-founded.

That is not to say that Area Studies have proved a failure overall. Teaching programmes centred on regions or even individual countries have proved popular and stimulating, and Area Studies centres or institutes have often doubled as centres of expert knowledge with an important outreach function through their links with the worlds of education, policy-making and the media.

The oriental studies departments in Europe and in the Netherlands, where they are now almost completely concentrated in Leiden University, are still very much based in the humanities, but some political scientists and anthropologists now work there as well. The departments display some of the same characteristics as the Area Studies departments in the United States. Research, which is often of a high standard, is generally not multidisciplinary. How could it be if it is the work of individuals rather than research groups? Nor is it comparative. Even where different oriental or Area Studies departments live in close vicinity to each other (as is the case in Leiden) comparative research that transcends the boundaries of the area is surprisingly rare. Teaching, on the other hand, is multidisciplinary. Students in the programmes of the oriental departments receive a grounding in the language, literature, religion, history, anthropology, social and physical geography, and sometimes also law and economics of a given region, but because of the different roots of these departments, when compared to their American sisters, the emphasis is always very much on language training and the study of texts, with the other disciplines being add-ons. With the possible exception of Chinese studies, this means that the composition of the faculty involved and the course programmes is of necessity lopsided. The acquisition of a non-

European language takes up so much time and effort that the other aspects together typically have to make do with one-third of the credits of a BA programme. Quite apart from the philosophical problems with the concept of Area Studies this leaves the departments with the impossible task of offering adequate language training as well as a grounding in a range of disciplines within the space of three to four years.

This of course helps us understand why representatives of these departments squeal (as we have seen at the start of this essay) when they are faced with demands for minors and core curricula. The preferred solution offered by the representatives of the Area Studies departments, the institution of university-wide disciplinary minors, may well yield better results and equip students better for graduate studies in which an awareness of a discipline is a must, but at the same time it seems to undermine the fundamental idea of the Area Studies as well. After all: if the solution is to offer students of, say, Japanese, the option of doing a minor in economics *or* political science *or* history, the end result will be disciplinary specialists focused on Japan. There is only a gradual difference between a Japanologist so educated and a political scientist who has had the opportunity to acquire adequate Japanese.

Whatever the merits of the argument, what does seem to be clear is that the reasons behind the decline of the number of Area Studies *departments* in the U.S. with the simultaneous increase in the number of Area Studies *programmes and centres* (that is noted by Szanton in his article) are universal. Researchers may have a valuable contribution to make to teaching or outreach in a field of Area Studies, but they learn to ask interesting research questions and to develop their skills through interaction with colleagues *within a discipline*. Idema was undoubtedly right sixteen years ago when he argued that 'constant linking back to the core disciplines' was needed. Having myself worked for twenty-nine years in oriental studies departments and (partly simultaneously) for ten years at the International Institute of Social History, I can personally testify to this point. For a historian in an Oriental Studies or Area Studies department it is far more difficult to keep up with developments in a discipline than if he is located in an environment where his ideas are challenged by historians working in different fields and periods. His ability to keep 'in touch' is entirely dependent on the efforts he is prepared to make himself.[14]

When taken to its logical conclusion this would mean that the most

promising organisational structure for a university (in the sense of offering the best chances of good research and good teaching) is one in which the faculty are based solidly within departments of history, linguistics, literary and cultural studies, sociology or political science, that is to say: within the disciplines, and that from there they contribute to programmes in which teaching on specific regions is interwoven with disciplinary training and which cross the traditional boundaries between Faculties of Arts (or: Humanities) and Social and Behavioural Sciences.

Notes

1 Letter signed by Maghiel van Crevel, Chris Goto-Jones, Arlo Griffiths, Jos Schaeken, Axel Schneider, Patricio Silva, and Ivo Smits and sent to the board on 20 November 2006.

2 *De Leidse paradox. Rapport van de interfacultaire werkgroep contemporaine niet-westerse studies*. Report of the commission consisting of Peter Nas, Jan Michiel Otto, and Erik-Jan Zürcher.

3 Szanton, David, 'Introduction: The Origin, Nature, and Challenges of Area Studies in the United States', in: David Szanton (ed.), *The Politics of Knowledge. Area Studies and the Disciplines* (Berkeley and Los Angeles 2004) p. 1-33.

4 Ibidem, p. 19-26.

5 Edward Said, *Orientalism* (New York, 1978).

6 But Timothy Mitchell shows in his 'The Middle East in the past and Future of Social Science', in: Szanton (ed.), *The Politics of Knowledge*, p. 74-118, that some of the central ideas of Said had already been formulated by the Egyptian Samir Amin in a 1957 Paris doctoral thesis.

7 Campus Watch is organised by a pro-Israeli grouping called Middle East Forum and based in Philadelphia.

8 *American Political Science Association. Comparative Politics* 1 (1996) p. 1-2.

9 Anderson, Benedict, *Imagined Communities: Reflections on the Origin and Spread of Nationalism* (London 1983).

10 Scott, James C, *Weapons of the Weak. Everyday Forms of Peasant Resistance* (New Haven 1985).

11 For an overview of the debates, see Szanton (ed.), *The Politics of Knowledge*; John Harbeson's rejoinder to Robert Bates, in: *Issue: A Journal of Opinion*, vol. 25, no. 1 (1997) p. 29-31; and Nachtwey, Jodi and Mark Tessler (eds.), *Area Studies and Social Science. Strategies for Understanding Middle East Politics* (Bloomington 1999).

12 Idema, Wilt, 'Area studies in the nineties: prima donna or member of the chorus?' , in: E. Zürcher and T. Langendorff (eds.), *The Humanities in the Nineties* (Amsterdam 1990) p. 337-354.

13 Idema, 'Area studies in the nineties: prima donna or member of the chorus?', p. 340.

14 I was reminded of this point when recently a colleague handed me an off-print of a small article he had written on a late seventeenth-century Ottoman treatise on buying and selling. It was an exquisite little piece of impeccable philological scholarship, but on reading it, it was immediately apparent that the analysis could have benefited enormously from reference to the rich historical literature on the 'moral economy' in Europe and China, of which the author, a historian himself but working solely in an oriental department, seemed to be unaware.

16
Consilience: reductionism and holism

Science, scholarship and the creative and performing arts

Frans de Ruiter and Adriaan in 't Groen

Near the end of his life, Immanuel Kant collected and edited his many notes and lecture preparations. This work also generated three essays published in 1798, entitled *Der Streit der Fakultäten*.[1] The reason Kant published these was a conflict he had with King Friedrich Wilhelm II of Prussia about the relational patterns between the various fields of science and scholarship within the university and the dominance of certain fields relative to others. Looking back to that moment of the so-called 'Berliner Aufklärung', we are inspired to consider the nature of the relations between the various scientific disciplines or faculties today, at this present juncture in history, and the dilemmas which arise in teaching and research. This article contains a number of contributions to this argument. Since Douwe Breimer became Rector of Leiden University, the scope of the University has been extended to include the creative and performing arts. We will, therefore, focus our consideration on the relationship between the creative and performing arts on the one hand, and science and scholarship on the other.

Der Streit der Fakultäten is not the most homogeneous work Kant has ever written. It deals with the relationships between the then existing faculties from various perspectives, in various time frames and using various styles. This heterogeneity fits perfectly with the notion of diversity that is central in this article. At the transition from the eighteenth to the nineteenth century, with the rapid development of academic knowledge in relation to

the changing society, new opinions emerged that formed the framework for new approaches. This initiated an irreversible development towards a continuous process of classification, branching and specification of the various fields of academic knowledge. The university of the Middle Ages had been built upon the faculties of theology, law and medicine, with only a secondary role for the faculty of philosophy. This structure was now challenged. Together with the increase in knowledge, the need emerged for specialisation and for organisational structures to support this, in the form of separate institutes or faculties for the new fields of science and scholarship that were now coming into being. New forms of research also appeared, which in turn produced new approaches, representations and formulations of the reality around us. This trend has been apparent from the Middle Ages right through to the present day. The increasing diversity in academic knowledge caused the scientists and scholars within the university to grow apart. An increasingly specific separation of fields of interest was apparently inevitable to maintain an overview of the territory of academic knowledge. This is the principle of determination on which the development of the university has been based since the Middle Ages. Major steps forward in academic conduct became possible. However, this differentiated world has caused the divergence of the individual academic disciplines. The differences have become so significant, and the nomenclature and procedures employed by scientists so specific that it has become very difficult to exchange knowledge across the whole spectrum of the university, let alone engage in interdisciplinary or transdisciplinary co-operation. For example, astronomers from a particular university may now have more contact with astronomers on the other side of the world than even with physicists from their own university, let alone with classicists! The university has become fragmented. Each field of science and scholarship has formed its own paradigm within the university, with proprietary approaches and methods, research practices and publication conventions, and in the course of their development they have increasingly differentiated themselves from other disciplines. Professorships constantly become more and more specifically defined. Overlap with other fields is avoided rather than encouraged. As a result, the benefit of having all fields of academic knowledge represented within one university remains unexploited.

Reductionism and holism

Since Kant, many authors have considered the relational patterns which fields of academic knowledge have or should have with one another. Edward O. Wilson wrote a remarkable book on the subject in 1998 under the telling title of *Consilience, The Unity of Knowledge*.[2] He dismisses post-modernism and takes on a very reductionistic view as the basis of all forms of academic knowledge. Although he wishes to unite all these forms in order to be able to ultimately grasp reality, he believes all forms of science and scholarship, including the creative and performing arts, ethics, religion and everything else should apply one single perspective on thinking and acting: reductionism. The world should be divided into ever smaller elements and issues to be able to attain the truth. According to Wilson, the creative and performing arts play an important role: The most significant reason for the emergence of the creative and performing arts was the need to create order out of the chaos caused by intelligence. Only unified knowledge shared universally can help us progress on our journey of discovery. Art represents the ultimate expression of our being and gives us access to the manifold cultures and their related notions. According to Wilson, this is why the creative and performing arts are part of the 'unified knowledge' incorporating all science and scholarship. Starting from the coherence of all science and scholarship, Wilson endeavours to comprehend all reality with his rigid reductionistic viewpoint, and to find explanations for human behaviour, the cosmos, religion, art, ethics, etc. This raises the question of whether an opposite view would also be possible. As a contrast to the reductionistic approach, the holistic ideal could be considered. Would a holistic perspective within which all scientific disciplines, creative and performing arts, ethics and religion and the specific approaches assumed by them are considered in one integrated structure, not be more appropriate for describing our reality? Would such a perspective not be a better route towards the consilience Wilson advocates? Or do we have to go one step further and see reductionism and holism not as an antithesis of one another, but as ways of thinking and behaving that are parts of the same equation?[3] This question introduces the dilemma that is being discussed in this article.

On the one hand, new discoveries require an ever-increasing specialisation in science and scholarship, while on the other hand it has become

apparent that disciplinary boundaries will have to be crossed if we want to develop new perspectives of discovery. Would the best way to reach this connection be to adhere strictly to one type of approach – the reductionistic viewpoint – or is a consciously pursued combination of reductionism and holism required?

In fact, there are two questions being raised here, the one following on from the other. First of all there is the question of the relationship between the creative and performing arts on the one hand, and science and scholarship on the other. Once they were united. Wilson tells us that the creative and performing arts are required in combination with science to comprehend all dimensions of our reality. With the help of the creative and performing arts, the four-dimensional time-space continuum acquires other observational perspectives, and thereby new points of view and forms of expression. The human sensory capacity, upon which all knowledge is based, is augmented in the creative and performing arts with experiences and ways of observation that diverge completely from what is usual in science. Through the medium of the creative and performing arts new possibilities for discoveries are engendered. But, just as has been the case with the various disciplines of science and scholarship, the creative and performing arts and science have grown apart, and this process is still continuing. This brings us to the second question: is the trend towards increasing reductionism that we have observed in science and scholarship, also apparent in the creative and performing arts? We will now explore this question. It is becoming obvious that in the creative and performing arts a nomenclature, observational perspective and mode of expression exist that are unique to the domain of the creative and performing arts. We see the same dependency in the various academic domains. The dependency is so dominant that, more often than not, scientists are wary of the practices of their colleagues in other domains.

Creative and performing arts

The sector of creative and performing arts cannot be placed in the catalogue of science and scholarship without further explanation, despite the fact that musicology, art history and the like have in the past been placed in the university domain; however, they deal with the creative and performing arts from the outside, they look at what the arts have

produced; the creative and performing arts sector itself *is* the arts, in the way of creation, presenting. The great majority of art forms call for tremendous knowledge and expertise; the material, the skills to re-invent and/or to apply, the history, the context and the fact that an artist always communicates and knows that there is a considerable group of those who are going to perceive the creation or re-creation.

This aspect is not always understood correctly by visitors to a museum, a concert or a dance performance who may immediately form all kinds of opinions without even the slightest knowledge of the respective discipline; judgements based upon what one thinks is beautiful or ugly are presented as serious interventions by connoisseurs. Nobody would dare to speak about great achievements or innovative discoveries in such fields as pharmacy or archaeology, but in the creative and performing arts obviously anything goes: 'I am not a specialist, but this is a lousy work', or 'That was a wonderful concert'. A significant aspect of the essence of the creative and performing arts is the context, which is important to both the creation and the presentation or performance practice. Given that the creative and performing arts cannot fully rely on expression in words to the same extent as the scientist can, context in this area is more important than in the traditional areas of science and scholarship. As soon as works of art, such as paintings, are presented without or in another context their meaning changes, as well as their significance, their strengths or weaknesses.

The concept and the making of a work of art resemble the act of giving birth. Three dimensions are involved. First of all there is the conception, the 'real' creative process: to fill in the silence with (concepts of) sound, to colour the white page: acts of creativity on the basis of in-depth knowledge. Some create immediately, inspired by a very clear internal vision or hearing of what they must make visible or audible, with others a painstaking process is involved. The second dimension is when the work of art starts its life, i.e. when it is presented or re-created. And the third dimension is the role of the perceiver, who encounters the work and reinterprets it in his or her own way, in so doing becoming the co-creator and co-re-creator.

If we want to bring all these aspects of the arts into the universe of discourse, an important handicap remains, however, that words do not suffice.

The difficulty in trying to express the meaning of music in words can

be proven by the following example: a discussion between two early music specialists, one of them an instrumentalist, the other also an amateur singer, specialising in music from the Renaissance. The first states that in his opinion, the sacred music of this period is static, evoking the feeling of a rigid block, like a glacier slowly shifting across an icy landscape. The other specialist makes a completely opposite analysis: having sung many cambiata parts in Motets and Masses by Josquin, Gombert and Obrecht, he has the feeling that the voices around him are constantly in motion, that this music is lively, fluid, flexible, transparent.

Who is right? Both are, but how can the same type of pieces, not on written paper but 'heard', produce such completely different expressions? This is the aspect of music as one of the disciplines in the creative and performing arts that is almost impossible to express in words. Sounding static is caused by motion, motion causes density and thickness, and this is all caused evidently because the role of the listener-non-performer, and the performer and at-the-same-time listener leads to a seemingly unsolvable paradox at the moment when the medium of 'language' is used. The incredible movement in this music can be demonstrated in the rhetoric when, for example, in certain pieces as it were the clouds appear in front of the sun and a whole piece turns from light into dark. In Lassus' 7th Psalmus Poenitentialis the text colouring and the texture change in less then 10 seconds when the text arrives … 'descendentibus in lacum'. The open vowels with -è- and -i- change into the dark -aa- and -oo- sounds; on top, the whole texture tumbles one-and-half octaves down. To be part of this process, changing your own colours, but also feeling the darker paints dripping around you into the black lake of death is an experience which is impossible to express in words.

Here we are in good company with Pim Levelt, former director of the Max Planck Institute for Psycholinguistics in Nijmegen, who in a recent interview explained a few interesting facts about 'using language'. 'On the one hand,' he says, 'truth plays a central role: people who communicate commit themselves to the truth and "the being right" of what they say. When one says "it is raining" it should really rain. If not, the partner in the discussion feels betrayed. The function of truth is the binding element. On the other hand, there is the misunderstanding that people think in language. When I play music, I think like mad, but without making any use of language'. Making music, listening to it, looking at a painting: these activities cause a great deal of thinking and we believe that we use

language to clarify the thinking, but that is just not true. This explains the difficulty when we start to verbalise what we have created, have performed or have perceived.

A few other quotes: Daniel Barenboim, Charles Elliott Norton guest-professor at Harvard University: 'The best we could do is to try and make a connection between the unfathomable content of music and that of life in general. The best definition of music is *sonorised air.* Of pianist Murry Perahia it was recently said that 'He feels with his head and thinks with his heart'. The late Hans Vonk, on being asked what is the secret of conducting, said: 'To guard the time'. Many other examples could be mentioned, but one might arrive at the preliminary conclusion that the most sublime that one can achieve in the creative and performing arts is to say something that cannot be expressed in words.

This, of course, raises the question whether in science and scholarship it is possible to express 'everything' in words. If one attends a relevant number of PhD sessions, one starts to doubt whether the answer could be 'yes'. It is more likely to be 'no', if one listens to the variety of questions and answers and the layers in the discussion. In the realm of academia one would expect an 'exchange of thoughts' across all the layers of the subject involved, but quite often this clearly does not happen. Particularly if the answers consist mainly of citations from the dissertation or of wordings which are synonymous with the text of the dissertation, although the questions were meant to deepen the context, one gets the feeling that there is more landscape and vista present, but that language is too deficient to express this.

However, some candidates succeed in lecturing around the text, and use it as a point of departure for further expatiation in a 'creative sense'; they compose, as it were, on the basis of given material. But even then: is everything that science delivers eligible for expression in words? Recently there was a photo of the Victoria crater on Mars. One can describe what one sees, all thanks to a number of scientific miracles, but the photo is a piece of art. The edges of the crater with the Opportunity lander as a tiny ink spot on the map, the colours and the 'kind of Schoonhoven' pattern in the middle of the crater evoke thoughts, feelings, emotions which leave even the best PhD candidate speechless. Like Pim Levelt, one thinks like mad but language has its shortcomings.

Photography in any event is not merely a skill; it is a discipline within the creative and performing arts that expresses much more than words can do. The body of former president Milosevic is unloaded from a plane in Belgrade: 500 of his followers wait for him on the tarmac. The photo in the newspaper shows part of his group: 50 faces of grim-looking men, all dressed in the same attire, with the same posture, the same expression. When we try to describe what the photo says, we cannot express the full impact of this picture. This is the power of this art form where language has limitations. The anger, the history, the aggression, the disappointment, the humiliation: all these feelings and much, much more are expressed in one single piece of paper of 30x20 cm.

Discipline-dependence

This exemplary description given above shows us that in the creative and performing arts, reductionism has progressed as it has in academic knowledge. In both cases the same sort of development has occurred over the same period of time. Of course, it is true that the increased determinism in the methods and techniques of observation and expression of the creative and performing arts does not manifest itself in quite the same way as it does in the various sectors of scientific practice. There is nothing unusual here: academic knowledge originally started based on theology and law. With this came particular modes of observation and expression. This set was completely different from the approaches in medicine, mathematics, astronomy, technical sciences and social sciences, all of which came and claimed their rightful position in the universe of science after the Middle Ages. It is certainly true that many scientists have difficulty seeing the specific properties of practising the creative and performing arts as an enhancement of the academic practice of teaching and research. The deterministic paradigm they have been educated into and by means of which they have often become very successful seems to deny them that. The chasm between the practising of the creative and performing arts and the practising of academic science is comparable with the gaps between the various disciplines. In both cases the differences between methods, techniques, nomenclature and perspectives of observation get in the way of a meaningful communication, exchange of knowledge and co-operation. To put it more precisely, if each discipline tries to claim superiority in possessing the best

approach, this leaves precious little room for considering approaches from different areas. Wilson, for example, claims superiority for reductionism.

The strongest property of a dilemma is that a certain aspect or approach is the prisoner of some other aspect. We see this with Wilson's thoroughly reductionistic approach. The paradigm is keeping him prisoner while he strives for consilience. True consilience will have to come with abandoning superiority claims related to approaches, methods and techniques. A purposeful combination of the different approaches from the various academic disciplines and forms of creative and performing arts can facilitate the multilateral discovery we are aiming for. This means that the methods and techniques from science and scholarship and those from the creative and performing arts will have to be considered as equal. The temptation to weld them together into indefinable forms must be resisted. If each of the two approaches is selected to reach deliberately chosen goals, they will enhance each other. The way out of the dilemma can only be found if and when the specific aspects of the approaches are recognised and deliberately selected. In this way the best results for the particular goal will be obtained, while at the same time it is recognised that no claim to superiority can emerge that will cause the chosen approach to be declared the norm for all disciplines.

In so doing, scientists, scholars and artists will reach an optimum in methods and techniques for observation, analysis, expression and performance that is specific for the academic discipline or form of art concerned. This is true also for disciplines where the methods and techniques have proven hugely successful in the past, such as, for example, certain sciences or literary history: we all know that scientific errors can occur, as they have done at various times in past centuries. Our concept of consilience has two large advantages in this respect: first, it can do a great deal to diminish the risk of scientific errors of judgement. And secondly, as reductionism requires ever more specific and narrower paths of research, it can help scientists to avoid getting stuck in dead alleys.

There are two more points we have to address here. First of all there is the question of words as a viable means of expression. We will have to accept that sometimes, or perhaps on most occasions, words are the dominant expression factor but at other times they are not.

Secondly there is the intersubjective verifiability of results. As we have seen above, this is not something most people involved with the creative and performing arts feel is an inevitable requirement, or even possible

with performances: the creative and performing arts *are* the creative and performing arts, as we stated earlier, there is no intersubjectivity involved there. Here the creative and performing arts can learn something from science and scholarship: in line with our concept of consilience, it has to be possible on the one hand to accept the methods and techniques of the creative and performing arts, while on the other hand one can raise the level of intersubjectivity using the methods and techniques of science and scholarship. This concludes our description of the key issues we are aiming to solve with our concept of consilience.

The legacy of the Enlightenment

The chasm between the practising of the creative and performing arts and that of science is the result of lack of familiarity with the legacy of the Enlightenment. Therefore, the notion of the interconnection of disciplines put forward by Kant and Von Humboldt, necessary for the progress of science as much as for the realisation of the 'Bildungsideale', is neglected within the university of the twenty-first century, in favour of the perfection of disciplinary differentiation. Ralf Dahrendorf recommended after the collapse of the Berlin wall and the end of communism that the thread of Enlightenment should once again be taken up. He reasons that all kinds of European revolutions and wars have made the twentieth century into a 'lost century', causing the development of the ideals of the Enlightenment to stall. He observes that with the transformation processes caused by the 'Wende', everything has taken on a double meaning. His postulation that history started anew in 1989 is a reference to the work of Karl Popper (1902-1993): *Die offene Gesellschaft*.[4] The *civil society* is a new, or to put it more precisely, a renewed interpretation of Popper's notion 'die offene Gesellschaft'. Popper was inspired here by Kant's notion of 'bürgerliche Gesellschaft', a notion that Hegel and Marx conceived in a negative way. The 'bürgerliche Gesellschaft' was to Kant the start of a free world community, while Hegel saw it as an obstruction to the freedom and development of the people. Marx went one step further in this dialectic by granting the people the dictatorship of the proletariat. This led to great stagnation in Eastern Europe, and caused the development of history to grind to a halt. This period of stagnation was followed after the collapse of the Wall by a new beginning, in which the concept of 'bürgerliche Gesellschaft', originally put forward by Kant and

developed by Popper, evolved into civil society, says Dahrendorf. In his view, influenced as it is by Hegel and Marx, the developments in Middle and Eastern Europe prematurely halted the process of Enlightenment in the Kantian sense. This makes the twentieth century a lost century. The thread of history is to be taken up again where the Russian revolution began. Dahrendorf calls this the 'zweite Modernität'. It is not his aim to strip society of its values and norms, but instead to invent new ones, to rediscover, to redesign: 'die Neuerfindung der Aufklärung'. The position of the university and the structure of science and scholarship and the creative and performing arts matter here because they are strongly connected with society. Dahrendorf unfolds a thesis of ambivalence: risk is an opportunity and a threat at the same time. According to Dahrendorf, there are a great many similarities between the period after 1989 and the decade before 1914. Being an 'unverbesserlicher Aufklärer', he searches for an explanation: 'Eine Neigung zur Anomie mit all ihren Folgen für Recht und Ordnung ist vorhanden. Doch entstehen auch neue Strukturen, noch zögerend, aber möglicherweise mit einem ausgewogener Verhältnis von Freiheit und Bindung als zu Begin des Jahrhunderts. Solche soziale und kulturelle Wandlungen gehen … Hand in Hand mit der Suche nach neuen Regeln des Zusammenlebens, einer neuen Beziehung von Gemeinde, Region, Nation und Welt. Derlei Veränderungen sind noch undeutlich und oft zweischneidig. Eines indes ist nach 1989 die vorherrschende Realität: das Leben in offenen Gesellschaften. Nicht alle können es ertragen. Es gibt "integristische", "fundamentalistische" Anfechtungen. Doch ist der Systemkonflikt einstweilen vorbei. Damit kann Geschichte endlich aufhören, manichäische oder selbst hegelische Züge zu tragen. Statt Position, Negation und einen dritten Weg gibt es 101 Wege voran, oder 87 oder 163. In der neuen Welt haben viele Kapitalismen Platz, nicht nur der von Chicago, und viele Versionen der Demokratie, nicht nur die von Westminster. Eine Zeit für neue Versuche ist angebrochen; darunter werden gewiss auch Irrtümer sein, aber solche, die sich ohne Blutvergießen korrigieren lassen. Das ist die große Hoffnung des 21. Jahrhunderts'.[5] In this analysis Dahrendorf chooses a multi-faceted approach to the university, science and creative and performing arts, and he invests his hopes not in one single perspective or standard of observation and expression, but instead in an infinite number. Observation, thematisation and verbalisation are central elements for

academic practice in the service of the truth. Many scientists have a rather naive attitude because they overestimate their own observation skills as well as their powers to thematise and verbalise. In the creative and performing arts, it is realised that words and any other forms of presentation always fall short of the ideal. Kant's central notion that the truth can never be seen or found in its entirety, is acknowledged, but there is a lack of awareness that the uncovering of the truth would benefit from using various forms, techniques and methods of observing, thematising and verbalising in parallel. In relation to this, the creative and performing arts can be seen as the highest form of expression of human observation, because the creative and performing arts utilise all forms of observation and thematisation – from differentiation to integration – alongside each other and simultaneously. This is especially so because the pillars on which the architecture of research in the creative and performing arts can be described have obvious similarities to the wide variety of methods and techniques of the various specialisations in science and scholarship. And, it cannot be repeated enough: research in the creative and performing arts (by artists) is something completely different from arts research, or research into the arts by researchers in the humanities.

For research in the creative and performing arts sector, the originality and the deliverance of a contribution to new knowledge and understanding are compulsory. Other pillars are a clear wording of a research question and being specific about the context of the research and the research question. The final results of the research, being a work of art and/or the report of the research trajectory and findings, have to be documented and disseminated to the relevant (international) fora and the public at large. What the creative and performing arts sector adds to that, unlike many academic disciplines, is the condition that there is, or must be, a unique relation between the artist/researcher and the subject. The research can only be done by this specific person because he or she as an artist has the almost exclusive skills, character and abilities to liaise in a unique way with the research subject, which together with the indispensable creativity of the artist researcher guarantees – in principle – equally unique outcomes. Recently, a very famous scientist said during a PhD session: 'If another researcher repeats the same experiment, the outcome of the test must be the same'. This approach is not only contradictory to the great

variety of processes within the creative and performing arts and their products, but it could also be questioned whether such an approach can be representative for in-depth 'scientific' processes. We guess not.

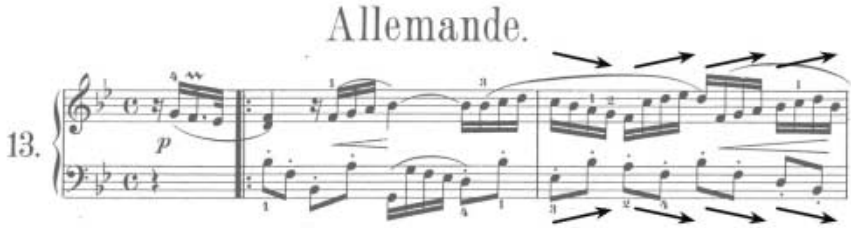
If the above-mentioned quotation were right or could be justified, the number of Nobel prizes would be equal to the number of scientists in the respective areas. Thus, an escape from the dilemma of specificity and cohesion or reductionism and holism is found, because no one specific approach or method is chosen.

Solving the dilemma

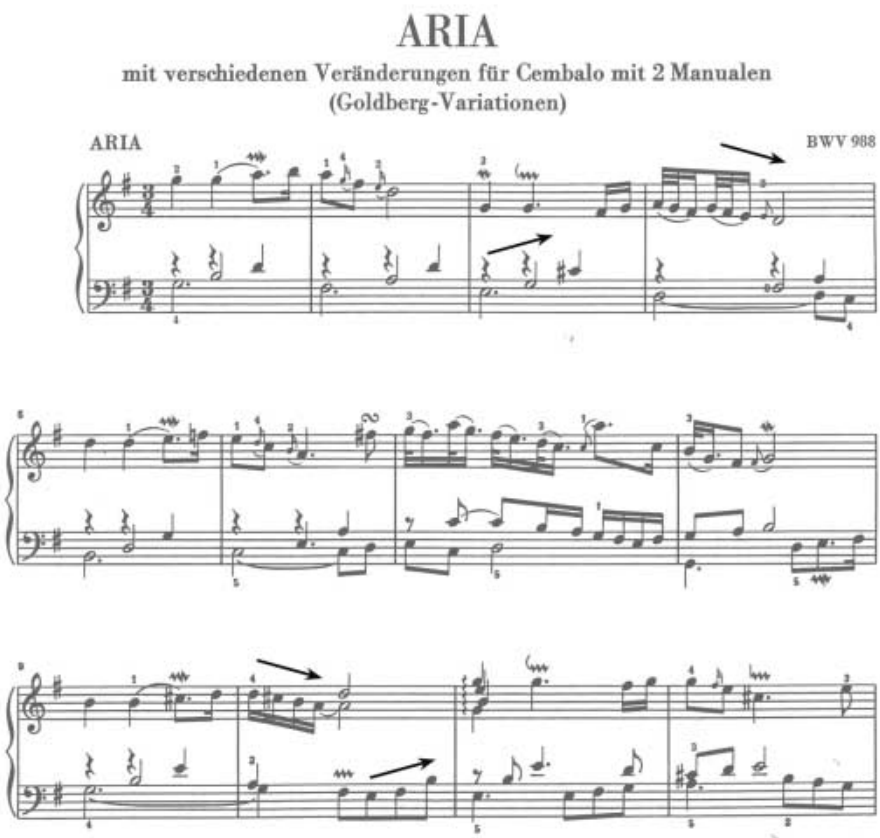
The unity of the creative and performing arts and science and scholarship can be found if one starts from the reality of the diversity of the academic disciplines and the various art forms. It will have to be accepted that each academic discipline and each form of the creative and performing arts has methods and techniques of its own, based on specific axioms that are typical of the discipline, ultimately resulting in methods and communication forms also typical of the discipline. Usually, in academic practice language is seen as a dominant instrument, whereas one has a hard time squeezing formulae, experiments and projects into this form of language. The unity of the creative and performing arts and science and scholarship is attainable only when, starting from the acknowledgement of the necessity of the disciplinary differentiation and the relevant instrumentarium, language is not by default regarded as the most effective way of communication. With some disciplines, languages definitely will fulfil a dominant role, whereas with other disciplines in science, technical sciences and the creative and performing arts formulae, visual designs or 'performances' will have more impact. The unity of the creative and performing arts and science and scholarship is being done a favour if one accepts that a mixture of communication instruments is the preferred method for bridging, expressing and representing all the disciplines. This article provides some experimental thoughts meant to explore an integrated way out of the dilemma: how can we bring about more unity into the various disciplines of science and scholarship and the creative and performing arts without ignoring their differences? At this stage and in this place some central notions will have to suffice.

Let us consult Johann Sebastian Bach. Can his modus of the delayed countermovement, for example, help us out of the dilemma? With this construction, Bach diverged from the somewhat predictable Baroque style, or, to put it more precisely, he elevated Baroque music into an art form. With the delayed countermovement, one musical phrase follows the other, not in the symmetrical way we see in 'ordinary' Baroque music, but the one movement follows on from the other delayed in time. This principle inspires us to search for ways to create a useful connection between the different methods and techniques of the various fields of the creative and performing arts and science and scholarship. The disadvantages of one approach should then be compensated by the advantages of the other approach. Thus, reductionism and holism can reinforce each other, just as in a musical countermovement. The one approach will not suppress the other, nor will it be combined with a diametrically opposite perspective. This would lead to confusion on the scale of the Tower of Babylon. The reductionistic approach and the holistic one remain separate persuasions but in research themes they are consciously employed in succession. The research question is thus being approached from two different angles. This multi-faceted approach creates the possibility that the paradigmatic boundaries of the field of science which with a reductionistic approach limit the scope of discovery may still be crossed in the next phase of the research. At the same time, the disadvantages of the holistic approach, i.e. borderlessness, have already been countered by the reductionistic anchor points. In this manner, the specific can follow into the general in the story or research line but they will not suppress one another, or even get in one another's way. This is the method by which Bach, too, was able to rise above conventional Baroque music. With his delayed countermovement, he created a double layer: internal counterpoint but at the same time a paradox with its context and therefore a multi-faceted unique story all of its own. In this way he reached the highest form of musical expression. But Bach needed the specificity of the reductionism of the Baroque to achieve this. The argument about the scientific approach runs parallel to this. Both scientific practice and the performance of the creative and performing arts are impossible without the specific scope of reductionism because they would otherwise lack the methods, techniques and nomenclature necessary to penetrate to the core of their objects or themes, but at the same time the connection with the larger unity of the other disciplines of

Example Handel, Allemande: regularity
When the right hand makes a downward movement, the left hand moves upwards, and vice versa: fully synchronised.



Example Bach, Aria from Goldberg Variationen: irregularity
A downward or upward movement is responded to with its countermovement in the next bar or later in the bar: delayed countermovement.



the creative and performing arts and science and scholarship is needed, to make the most intricate voyage of discovery as well as not to lose sight of the connection with the complete reality. Only a connected, alternating approach using reductionism and holism can bring out the highest form of practice of the creative and performing arts and science and scholarship. In conclusion: the delayed countermovement is the 'missing link' between the specific: reductionism, and the general: holism.

By applying the principle of the delayed countermovement, the highest quality standards of the creative and performing arts and science and scholarship can be achieved, provided that the various forms of academic practice and the practice of the creative and performing arts remain based on the principle of ever-increasing specialisation or reduction, as well as on the deliberate identifying of combinations of approaches, types of observation and forms of expression, whereby a more integrative or holistic approach can be attained. We introduced the principle of the delayed countermovement here to ascertain that holism will not suppress reductionism. These two remain separate approaches or lines that follow and complement, but do not suppress one another. Neither one is superior to the other. This stresses the importance of precision and exactitude, just as in Bach's compositions, in which each (supposed) detail or minor element acquires a prominent place and is connected with the whole. The delayed countermovement causes every small, homogeneous, but at the same time individualised, element to be given a clearer accent.

Conclusion

The continuation of the discovery of the reality around us requires an all-round, holistic approach. All-roundedness, however, can only come into being based on the collectiveness of all the monodisciplines. The great diversity in forms of observation and expression of the various disciplines of science, scholarship and the creative and performing arts is necessary to be able to penetrate the reality around us in various ways. The principle of the delayed countermovement can unite these two seemingly conflicting approaches. Time, words and performance thus acquire a number of dimensions and they fulfil a variety of roles within the university. This gives new impetus to the development of the phenomenon of the university, such that discovery should never need to cease. The

profile of the university becomes more multi-faceted and more exciting.

The advocated approach of plaiting together reductionism and holism provides us in this timeframe of 'die Neuerfindung der Aufklärung' with inspiration for structuring and ordering the fields of science and scholarship within the university and the connection that can be made with the creative and performing arts. The consequences this will necessarily have for the organisation of the university, however, unfortunately fall outside the scope of this article.

Notes

The authors are indebted to R.H. Lussenburg for his help with the final editing of this article.

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