

With a New Introduction by the Author
and Foreword by Gordon Mackerron

Science in Society Series

RATIONALITY AND RITUAL

PARTICIPATION AND EXCLUSION
IN NUCLEAR DECISION-MAKING

BRIAN WYNNE



Rationality and Ritual

Participation and Exclusion in Nuclear Decision-making

Brian Wynne



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**Dedicated with love and hope, history looking forward,
to Granny Wynne, to Matt, Tom, Grum – and Rufus**

Foreword

Gordon MacKerron

The republication of Brian Wynne's book *Rationality and Ritual* is entirely timely. The book uses the Windscale nuclear public inquiry of 1977 as a prime example of problems in the science-policy interface. Only a few years after the appearance of the book in 1982, the fortunes of the nuclear industry took a downward turn in both the UK and, more widely, under the successive influences of the Chernobyl accident in 1986 and the pressures of electricity industry liberalization. It was the liberalization that dealt the most telling blow: without the protection of monopoly, investors found nuclear power to be both costly and too risky.¹ But times change, and nuclear power is back on the agenda across the world again. The UK government has plans to have at least ten large new reactors built, and countries as diverse – and in some cases unlikely – as the US, Italy, the UAE, Nigeria and Eritrea assert their commitment to building reactors. In these circumstances *Rationality and Ritual* is an important reminder of some of the deeper, largely unchanged processes that underlie the revival of official enthusiasm for nuclear technology. It is impossible to represent the subtlety and complexity of Brian Wynne's argument here, but it may be useful to illustrate the enduring relevance of some of his themes with recent examples. In particular, his themes of the 'public deficit' model of scientific understanding, the 'deletion of publics as independent democratic agents' and, most broadly, the constitution of legitimate authority are just as relevant now as they were in 1982.

My examples come from recent UK history, and concern the management of radioactive waste since 2003 and the revival of strong political commitment to building new reactors, which dates from 2005 or at latest 2006. The radioactive waste story has at least some positive elements. In 2003 the UK government set up an 'independent' Committee on Radioactive Waste Management (CoRWM), which I chaired until 2007. The terms of reference for this Committee showed that the government (or at least its 'environmental arm', the Department for Food, Environment and Rural Affairs (Defra)) had learned some of the lessons that Wynne's book had suggested. CoRWM was told to go back to a 'blank sheet of paper' as far as options for management were concerned: it was required to 'inspire public confidence', to consult extensively with the public and stakeholders and was not a committee of experts, at least not in the traditional sense of containing only specialists in the

natural sciences and engineering (the Committee contained a number of social scientists as well as a founder member of Greenpeace).² These starting conditions sent a clear message that the orthodox scientific consensus in favour of geological disposal was to be ignored, that public trust was at least as important as safety and that public engagement was to be central and not an 'add-on'.

The Committee by and large stuck to these terms of reference and was as a consequence roundly condemned by a good deal of the scientific establishment – notably the House of Lords Science and Technology Committee, which described CoRWM's meetings as a 'satire on bureaucratic process' and the Committee itself as unfit even to act as an intelligent customer for scientific advice.³ The implicit (occasionally explicit) position of the Lords was that natural science and engineering represented the only legitimate category of knowledge and that all other kinds of knowledge, or more broadly and in Brian Wynne's terms 'legitimate difference' in framing the problem, were froth and distraction from the real problem. Two members of CoRWM took a similar view to the House of Lords and in acrimonious circumstances both left the Committee – one being dismissed and the other resigning, muttering in public about CoRWM's embrace of a dangerous 'relativist post-modernism' and its overall bad faith.⁴ All these parties took the view that once scientific knowledge was not given pride of place, then 'the public', scientifically incapable of understanding the technical arguments, would somehow settle for irrational and unsafe options. In fact the public took it for granted that the best expert knowledge would be brought to bear on the choice of options and their implementation, but were well able to engage fruitfully on framing and contextual issues as well as providing input on the relative weight to be given to different objectives.⁵

The experience of CoRWM was thus fairly positive in terms of trying to engage with wider public framings and knowledge rather than being confined to a risk-based scientific competition between options. However, this was arguably only possible because CoRWM confronted the problem of legacy waste at a time when the option of nuclear new build was off the political agenda. Once the context changed and new nuclear power returned to the agenda, forcefully so in 2006, the government's behaviour changed radically. On the radioactive waste management front, the government began to conflate the recommendation that CoRWM made in favour of geological disposal for *legacy* waste with the idea that the same option was inevitably right for new build waste. Despite CoRWM's vigorous argumentation that the ethical, political and social issues involved in a deliberate decision to create new waste were different from the inevitable need to manage the legacy, the government chose to conflate the two issues on narrowly technical grounds.⁶

But more broadly when nuclear power had become defined as a state imperative once again, the government's approach to the public radically changed from the 'progressive' stance it had allowed CoRWM to take. The initial consultation on nuclear new build was so narrow and partial (in both senses) that a judicial review stimulated by Greenpeace found the consultation

to be seriously flawed and the government was forced to undertake a further round of consultation. This was bad enough, but on the day that the judicial verdict was announced, the then Prime Minister Tony Blair went on the public record to say that, while there would have to be further consultation, the policy would not change – as clear an indication as there could possibly be that the consultation was to be merely window-dressing and as powerful a disincentive as could be imagined for the public to engage in the new consultation exercise. So when the government perceived the stakes to be high enough it reverted to a model of treating the public as a passive bystander, unable to have any effect on the ‘need’ to press ahead with nuclear power.

The revival of official commitment to nuclear power alone makes a rereading of *Rationality and Ritual* an important contribution to understanding the issues. But while Brian Wynne’s book is based empirically on nuclear power as a particularly powerful exemplar, it has wider resonance in its deep dissection of the moral, political and cultural issues that the relationship between scientific expertise and political process – more recently in debates about genetics and biotechnology – involves. The book was a pioneering study in its depth and capacity to illuminate. It remains so to this day.

Notes

- 1 MacKerron, G. (1996) ‘Nuclear power under review’ in J. Surrey (ed.) *The British Electricity Experiment*, London, Earthscan, pp138–163.
- 2 Committee on Radioactive Waste Management (2006) *Managing our Radioactive Waste Safely: CoRWM’s Recommendations to Government*, CoRWM Doc 700, July.
- 3 House of Lords Committee on Science and Technology (2004) *Radioactive Waste Management*, Committee Session 2003–2004, Fifth Report, December, London, The Stationery Office, pp14–15.
- 4 Baverstock, K. and Ball, D. (2005) ‘The UK Committee on Radioactive Waste Management’, *Journal of Radiological Protection*, Vol. 25(3), pp313–320.
- 5 Committee on Radioactive Waste Management (2006) *Managing our Radioactive Waste Safely: CoRWM’s Recommendations to Government*, CoRWM Doc 700, July, Chapter 7.
- 6 DTI (2007) *The Future of Nuclear Power: The Role of Nuclear Power in a Low Carbon Economy*, May, London, The Stationery Office, Chapter 8.

Preface to New Edition

When several long years ago I accepted Steve Rayner's courageous suggestion that Earthscan republish this book, I did not begin to imagine just how difficult this would turn out to be. All of the obstacles were self-inflicted, including the fact that since the mid-1990s my attention had been almost wholly given to science and technology studies in genomics and biotechnology, innovation, public concerns and environmental risk. This despite some significant work between about 1999 and 2004 with the newly refashioned UK Nuclear Industry Radioactive Waste Executive, Nirex, and its more independent CEO Chris Murray and Head of Communications David Wild. There are some important continuities between these different domains when viewed from my own academic field of Science and Technology Studies (STS), but the thoroughgoing practical engagement which is necessary for my kind of STS cannot be done for both together. Since I was centrally involved from the 2002 start of the UK ESRC's Genomics Research and Policy Network, in the Lancaster and Cardiff Universities Centre for Economic and Social Aspects of Genomics, Cesagen, my intellectual and policy attention has been virtually exclusively taken up with these fast-moving, globally absorbing and sublimely complex fields relating to genomics and bio-knowledge innovations, ambitions and risks. Nuclear has thus been forced to take a back seat for me – even while my awareness of similar fundamental issues between the technical, economic-political, social and philosophical dimensions of these fields has only grown with time.

No one should therefore read this updated edition expecting an up-to-date analysis of the nuclear rebuild issue.¹ It was never remotely such an analysis even in its first edition. Yet the less direct issues which I raised in this book about the attempted construction of public authority through particular constructs of science and rationality (and thus, of politics and publics), remain relevant even as they continue to be neglected. This is true even whilst the specifics of public inquiries and decision processes, of the Windscale-Sellafield nuclear site, its technologies and their wider connections, have been transformed.

On first inspection, the book and its object of concern seems woefully dated, as the opening of my update Introduction explains. With looming potential climate catastrophe as its alibi, nuclear energy is back ostensibly on the front foot, even if no one is ready to pay its extravagant bills. Whether

nuclear energy does overcome its practical financial problems and become a significant new-phase energy supply industry, reprocessing of the ensuing spent nuclear fuel which was the issue for the 1977 Windscale inquiry, and which has still never proven itself to be commercially viable, may well be discontinued – except under unadvertised military ambitions to produce nuclear weapons-grade plutonium and enriched uranium.

The book's main *raison d'être* was not the particular inside story – which it does give – of such an intensely demanding, absorbing and unprecedented technical and policy process. It was to give what was a different *analytical* perspective on the politics of knowledge-processes shaping big science-technology ambitions and commitments. This remains salient to today's very different world, including to huge proliferating new fields far beyond nuclear energy-weapons, like genomics and biotechnology, which barely existed as *public* issues at the time of the Windscale inquiry. In distinct ways these fields of promise now carry the collective emotional and political charge, through idolatry of 'science' in policy expert cultures, which nuclear once did; and their measured, reasoned and reflective social appraisal is at least as difficult as it was for nuclear in the days when it 'enjoyed' that similar quasi-religious status. I believe that this exaggerated status imbued by sycophantic politicians and media is against the proper interests of the associated sciences and technologies, not to mention against a robust, reason-informed democratic society. Here I can only hint at those resonances between nuclear technoscience as public focus in the 1970s, and contemporary 'big' technosciences such as genomics; but in the absence so far of space and time to pursue that task herein, I hope that through this seemingly parochial and dated case-study, readers may see some of these for themselves.

For example, I had chosen to focus analytically on the rituals of public authority in which scientific reason has been the defining actor for some considerable time. Since then, later classics of my own academic field of STS, and History of Science, such as Steven Shapin and Simon Schaffer's (1985) *Leviathan and the Air Pump*,² about 17th Century science, culture and politics, have pointed in such directions from their very different ostensible starting points. Also, in a book published in the same year as my own, about cosmology and science in late modernity, Stephen Toulmin³ described the various historical articulations of scientistic attempts to stretch beyond the strictly limited – if valuable – authorities of rigorously disciplined scientific reason and observation, into societal mythologies as distinct from (in some way) validated explanations of the world. While some sociological and anthropological perspectives might wish to challenge the implication that myths cannot serve social functions, and may also be thus-validated in their own ways, Toulmin noted that, while modern society claims to have escaped the tendency to invest accounts of the world with tacit *human* concerns such as a sense of security and belonging, the corresponding habit of artificially naturalizing familiar or comfortable (imagined) societal orders and trajectories is alive and kicking. Since Toulmin wrote, those scientistic presumptions involved in defining public meanings as matters of scientific discovery (hence scientific authority), as distinct from

democratic societal ones, have only intensified in many public policy fields involving scientific knowledge.

Thus for example, just as occurred for nuclear power and public concerns in the 1970s and 1980s, in the controversies over GM crops and foods in the UK and EU, scientific spokespersons given policy authority have pontificated confidently – just as they did in the nuclear case – about the sovereignty of scientific knowledge over public concerns, by defining the *public* issue as a ‘risk issue’, thus a ‘scientific issue’. This is even claimed to encompass benefits questions too. Regrettably, social scientists and humanists have failed to challenge such distortions of public meaning, and their underlying political form.

This is a similar basic logic to that which I identified as the ritual of rationality in and around the Windscale inquiry’s constructed factual-discovery quasi-judicial ethos. With benefit of hindsight, the 1982 Windscale book might be seen as an inadvertent forewarning of the immersion of contemporary politics of science and technology, innovation and risk, in ‘science’ as presumptive foundation of (instrumental) public meaning. This does signal a deep and troubling confusion besetting our institutions of policy and their mutual agents of scientific-technical expertise. This is a confusion of scientific reason with anxious moral and emotional concerns about order and authority for specific political-economic commitments. Just because these – like nuclear power too – have been made *in the name of science*, does not justify their proponents’ presumption of any such unambiguous, and undemocratic, normative endorsement. ‘Is’ (as claimed) has been allowed to become ‘ought’ yet again, and contention over the former should not be allowed to fool us that finding the proper ought is just a matter of finding ‘the’ correct ‘is’. Here, Toulmin’s more reflective observations are salient to the immediate challenge of how we might exercise discriminating collective reason over genomics-related technologies when these are insistently imagined and projected as a *necessity discovered by science*, and rarely if ever as functions of the more fallible collective human responsibility which they are. Hanna Arendt⁴ had relevant thoughts on this.

Given my doubts that a particular 1982 STS case-study could be relevant to issues in *contemporary* science, technology and policy, it is deeply gratifying that someone as experienced and insightful as Gordon MacKerron sees the book’s enduring relevance – this also when even what is meant by ‘public’ has been transformed over the intervening years. I was fortunate to have the original book reviewed by the most eminent science policy thinker of that era, the Godfather of the US light-water nuclear reactor – Alvin Weinberg, who generously described my analysis as ‘brilliant’ – but flawed by what he saw as its intransigently anti-nuclear commitments!⁵ This initiated a sharp but amicable correspondence in which I tried to explain his mistaken account of my avowedly sceptical view of the nuclear project – not because of its (routine emissions or accident) radioactive risks, nor of its ultra-long term radioactive wastes, but because of its production of nuclear weapons material, and the continuing failure to demonstrate that in this respect at least, nuclear is a successfully working technology. What counts as ‘working’ – the validation

question again, just like the scientistic mythologies I question – is always subject to the primary question: for what purpose(s)? As STS has taught us through work such as that of Donald McKenzie,⁶ ‘validity’ is (or should be) always open to negotiation, in terms of which human purposes (and needs) are we assuming to be sovereign, and under what conditions. Thus in terms of keeping nuclear weapons capacity separate from civil nuclear energy, as distinct from the worthwhile social function of producing electricity, nuclear energy technology has *never* worked. A further point of the original analysis which I also complained that Weinberg seemed unable to grasp, was the crucial one that rationality can work as ritual, hence my title (Weinberg was not alone – too many social scientists have confused this distinction), *Rationality and Ritual*, not *Rationality or Ritual*. It remains a profound cultural handicap of science-technology policymaking, that its practitioners are still day-in, day-out reinforced in the propensity to Weinberg’s false reading of my analysis, that it was asserting ‘the Windscale Inquiry as ritual *rather than* rationality’ and as ‘*mere* ritual’ (my emphasis). When this remains so deeply ingrained as given in scientific and social and policy institutions, STS has barely yet scratched the surface of the challenges it faces.

Since the original publication, my professional and personal debts have proliferated in countless directions. All I can do here is to express my immense gratitude for having enjoyed such good and generous colleagues across so many fields, including ones with whom I have been in critical relationship. I have just about managed to keep my Earthscan editors – Alison Kuznets, Claire Lamont, Camille Bramall and Nick Ascroft – within viable bounds of exasperated criticism of my woeful failures and excesses of doubt. They have shown heroic patience with me, as well as admirable professional focus and dedication.

I strongly appreciate the need for disengaged reflective and disciplined thinking, but I have always felt the need to pursue academic work not only across disciplinary boundaries, as my own disciplinary trajectory shows, but also according to questions and problems which arise from human worlds beyond the academy. I have only ever managed that essential reflective disengagement for short periods, and in recent years not at all, with severe effects on productivity. My academic work has always been a function of concerns arising from the social, political and ethical worlds of science, technology, their actual and imagined directions, driving forces and implications. Whatever academic value it has provided has been a function of those agonistic engagements, and the opportunities for real-time as well as post hoc reflection upon them. I doubt whether the privilege which I have enjoyed in being able to try to bring practical action together with academic reflection is any longer available in the transformed political economy of academic life over recent years.

This book was the product of the earliest of those continuing active engagements for me, attempting to develop and test an extension of the highly esoteric academic work of The Science Studies Unit at Edinburgh University, in which I was involved as an unreconstructed postdoctoral scientist newcomer –

the strong programme in sociology of scientific knowledge as conducted in non-public scientific research arenas like laboratories. My original move was to see what happens when we apply the same analytical perspectives to scientific knowledge in important public arenas such as The Windscale inquiry and nuclear controversy. To the extent that I have made any contribution here, this is thanks to friends and colleagues then at the Edinburgh University Science Studies Unit, especially to its late Director, my mentor and friend David Edge, who as usual saw the point and defended my experiment.

Since moving on from the Science Studies Unit, my most inspiring and rewarding collegial home has been Lancaster University, and I owe immeasurable personal and intellectual debts to many Lancaster friends and colleagues, especially to those who created and sustained CSEC in the 1990s, and into current times. Here I cannot fail to mention my closest CSEC partner, Robin Grove-White – a reluctant academic whose non-conventional enterprise, commitment and honesty gave academic thinking more than it was able to recognize.

In the spirit in which I prepared and wrote it, historical reflection looking forward in hope, I dedicate this book with lasting thanks to my Mum and Dad, who gave so much for me, and to my sons Matt, Tom, and Grum – and to Rufus.

Brian Wynne
October 2010

Notes

- 1 Readers will notice references made at the time of the original writing to issues which have since changed, but which I decided for the new version it was better in the text, to leave true to their time. I could have composed endless footnotes with appropriate commentary connections and updates, but with the exception of the Introduction 'update' chapter, I have left the original to stand for itself.
- 2 Schaffer, S. and Shapin, S. (1985) *Leviathan and the Air-pump: Hobbes, Boyle, and the Experimental Life*, Princeton NJ, Princeton University Press.
- 3 Toulmin, S. (1982) *The Return to Cosmology: Postmodern Science and the Theology of Nature*, Berkeley, University of California Press; Toulmin, S. (1990) *Cosmopolis: The Hidden Agenda of Modernity*, New York, Free Press.
- 4 Arendt, H. (2005) *The Promise of Politics*, New York, Schocken Books.
- 5 Weinberg, A. (1983) 'Science and Trans-science in the Decision to Reprocess Uranium', (review of Wynne), *Minerva*, xviii, pp337–332.
- 6 McKenzie, D. (1990) *Inventing Accuracy: A Historical Sociology of Nuclear Missile Guidance*, Cambridge MA, MIT Press.

Preface to Original Edition

Although it describes in new ways the already widely analysed Windscale inquiry, this book also has ambitions stretching far beyond specific events. The importance of the imminent Sizewell 'B' Inquiry – the first major focus of conflict since 1977 – has not been dimmed by the recent refusal of many would-be participants to be drawn in. Many procedural problems were raised by the Windscale inquiry; Sizewell has already exhibited some of these, and no doubt there will be plenty more. However, there are wider questions to be faced than those of formal procedure and participation, especially if Sizewell turns out to be the death knell of the 'stretched' public inquiry. I hope to have expressed them in this book. I should also explain the more general motives behind this analysis.

At least since my own introduction to the field of science, technology and society over a decade ago, it has been lamented that the more abstract sociological, historical and philosophical branch of science studies was divorced from the policy-oriented side. As the field has developed, despite some exceptions, each branch seems to have segmented even more into its own specialist concerns and reference groups. Valéry's dictum that 'everything which is complex is useless, everything which is simple is false',¹ seems to speak to this predicament; one side appearing esoteric and self-indulgent to the other, in its turn appearing superficial and co-opted. Despite the usual temptations to embrace one side or the other of this cultural schism, I find it impossible to avoid the middle ground.

With the growth of political concern about resolving scientific disputes on all manner of important commitments, the emphasis in policy-oriented work has shifted towards methodologies such as 'decision analysis', 'risk assessment' and related possibilities for combining 'proper' evaluation of factual uncertainties with the expression of social values. Even more strongly than in previous incarnations such as 'technology assessment', these developments in the policy sphere have opened the door to the sociology and history of science, disciplines which have focused extensively on the social processes whereby scientific knowledge achieves consensus and authority. This shift in policy concerns has opened up questions that sociologists have been studying for years about scientific method, rationality and consensus; for what is at stake in any specific issue – though some more than others – is authority and governability as such. And the myths of science which have for so long supported authority are perhaps losing their social purchase.

I therefore take one decision-making case study, of major international importance, to show how sociological, historical and anthropological analysis of scientific knowledge can enrich our interpretation of such practical matters, especially the problem of legitimate authority for technological commitments. This objective is complex and perhaps ultimately impossible. Academic analysis, however, may tempt us to forget that political life is always partly a process of rationalizing the repeated non-achievement of impossible visions, and although I make no pretence here to have fully bridged the gulf between political realities and analytic abstraction, I also make no apologies for having offered an analysis which is intrinsically political (even if only indirectly). In the words of Jacques Ellul, 'the judgement of uselessness is no excuse for inaction'.² At the very least, I hope to show that the project is worth more effort than it presently receives.³

In order to translate these general aims into substantial analysis, I have had to draw upon several fields which have been absent from previous discussion of the Windscale inquiry (see note on sources). First, I have set it in a historical context – nuclear decision-making over the last 25 years. Surprisingly, only Williams has so far done this (in greater historical detail), but he had the different aim of arguing for greater public accountability, especially for the economics of policy alternatives.⁴ Second, my focus is on the rituals (especially their cognitive dimensions) which secure public authority. I have therefore worked extensively on legal processes and judicial rationality as an institutionalized form of authority in many ways parallel to science, but superior to science in finality and sheer practical productivity. Developments in sociology and history of scientific knowledge in the last decade tie in with the social analysis of law and legal ways of framing reality and logic, and with my own dissection of the Windscale inquiry's discourse. Third, the ritual dimension of highly elaborated forms of rationality in political settings requires an anthropological approach. There has recently been some reorientation of anthropological analysis towards industrial societies, and Douglas and Thompson have been especially original and relevant in examining the underpinnings of different rationalities – even of scientific and technological subcultures.⁵ I have found this work exciting and important, despite its controversial political aspects, but its full development remains a challenge. It is hard to choose between these extra strands in terms of importance, but if I had to do so I would offer the historical perspective. Whether my own interpretation is valid is secondary to the fact that it at least counters the widespread neglect of the historical character of individual 'decisions'. This historical dimension emphasizes how radically history differs for different actors; it is also the only canvas on which we can paint ourselves busily repainting history with a more comforting sense of order than it has in reality. The historical dimension acts as a foundation for the others.

Since this book may have a diverse audience, several points must be made clear:

- It is not concerned with specific outcomes. Thus, although I criticize the Windscale inquiry process, this emphatically does not mean that I think a 'better' process would have 'naturally' produced an anti-THORP decision
- It is not concerned with individual motivations and attitudes, except where these are judged to reflect more general social patterns. Thus, for example, I criticize the reasoning and bias of Mr Justice Parker, the inquiry inspector, but I reject the view that his performance was from the outset a deliberate and carefully calculated defence of the nuclear industry. Indeed, I am pleased to acknowledge his sincere attempt at an open debate during the inquiry and his kindness to me at several difficult moments as a struggling lay advocate on behalf of a local group, the Network for Nuclear Concern (NNC). Equally, my critical interpretation of legal frameworks in political affairs does not dim my respect for the professional qualities which I was able to observe amongst the best of the lawyers at Windscale; nor does it qualify my gratitude for their help and advice, most especially from my chief adversary, BNFL's counsel
- It is not directly concerned with decision-making or inquiry procedures themselves. I have contributed elsewhere to this very necessary debate.⁶ Here I adventure beyond this into the realm of political or moral philosophy of technology. This is important because, as I hope to have made clear, procedural possibilities are inevitably restricted by the definition of procedure itself and the limited level of discourse it can embody. Ultimately, questions of technology assessment are questions to ourselves, about ourselves and our social and moral existence. But this is no glib assertion – it requires detailed analytic effort to structure questions and develop the fitness to reply. This is what I hope to have begun. I can only overcome my instinct to avoid the pretentiousness of this aim by my even stronger conviction of its practical importance
- I have described the inconsistencies and delusions which have prevailed in a central area where politics is entangled with science and technology. In particular I have tried to portray the ritual which has allowed us to live with the gross contradiction between the enthusiastic claims for the definitiveness of the THORP 'decision' and its embarrassingly messy reality. I have thus suggested that the ritual upholds the 'inauthentic politics' of this area, following the terms of the debate around Habermas' work.⁷ My aim, however, has not been to ask how we might sweep this from our political affairs, because my belief is that such falsehoods are endemic and, in a certain sense, necessary to public life.

For example, if taken seriously, rationalist prescriptions for 'realistic' decision-making would unremittingly expose the hidden agendas – the more nebulous but more basic concerns – inspiring formal public arguments on all sides. The key faith is that through this process some common ground will ultimately be reached. But there can be no such guarantee: indeed this process might conceivably arrive at a point where conflicting views confront each other unmediated by any pacifying influences, with possibly violent results. It may