

Record of publications and other papers

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BIOGRAPHICAL NOTE

James L. Richardson was Professor of Political Science (1975–85) and later of International Relations (1986–98), at The Australian National University. He is the author of *Germany and the Atlantic Alliance* (1966), *Crisis Diplomacy* (1994), and *Contending Liberalisms in World Politics* (2001).

INTRODUCTION

This paper has been prepared as background for a memoir of my experiences as teacher and scholar of International Relations (IR) since the late 1950s. Bearing in mind the unreliability of memory as a historical source, it seemed that I could provide some check on this by setting out a precise account of my writings in terms of their principal themes, drawing attention from time to time to my comments on the state of the discipline, as it has come to be termed, since my perception of the changing discipline will be one of the topics in the memoir, and memory here may be especially selective.

I see my publications as falling within five major groupings, three of these relating to my single-authored books: *Germany and the Atlantic Alliance*, *Crisis Diplomacy*, and *Contending Liberalisms in World Politics*.¹ The two further groupings are concerned with Australian foreign policy and IR theory, broadly understood. A chronological account within each of these groupings has a certain coherence which would be entirely lacking in a single chronological account of the writings as a whole. The order of presentation of these sections is not important, but it is convenient to include Australian foreign policy after the first book, and IR theory after the second.

I. GERMANY AND THE ATLANTIC ALLIANCE AND RELATED PAPERS ON STRATEGY AND ARMS CONTROL, THE COLD WAR AND DÉTENTE, AND GERMAN FOREIGN POLICY

The first book began as a prospective PhD thesis at Nuffield College, Oxford University (1958–61), on the rearmament issue in West German politics and foreign policy, but I became increasingly concerned with the novel and puzzling issues raised by nuclear weapons. This not only brought in a different literature but in effect a different intellectual world as a new breed of strategists, mainly in the US, grappled with the issues raised by nuclear weapons, which posed a major problem for structuring a PhD.

While this remained unresolved, the Center for International Affairs² at Harvard University expressed an interest in my outline on the North Atlantic Treaty Organization (NATO) strategy, but not at all in the PhD. I was offered a one-year fellowship, subsequently extended to two years, on the understanding that the Center would publish my work if it proved sufficiently interesting, at the risk of my ending with nothing. Working under intense pressure towards the end, I completed a draft book manuscript in the two years, in late 1963; after minor revisions and polishing the text, the book was published in 1966, along with a German translation.

The Center, under Robert Bowie, Henry Kissinger, and Thomas Schelling, provided a uniquely stimulating environment. In the preceding two or three years, the classic works defining the new strategic thinking in the nuclear age and the role of arms control therein – by Schelling, Bernard Brodie, and Herman Khan and by Hedley Bull, Schelling with Morton Halperin and Donald Brennan – had been published and were still being assimilated. The members of the small group of research associates at the Harvard Center each had his own project (there were virtually no women in the field) but exchanged drafts and obtained immediate feedback. The Joint Arms Control Seminar, linking the Harvard Center with the Center for International Studies at the Massachusetts Institute of Technology (MIT) and including members from a wide range of disciplines, took up issues still at an early stage of formulation over the whole field of strategic studies. This was no ‘ivory tower’ – both Centers

1 Unless stated otherwise, all publications listed have been authored by myself. *Germany and the Atlantic Alliance: The Interaction of Strategy and Politics*, Cambridge, MA: Harvard University Press, 1966; *Crisis Diplomacy: The Great Powers since the Mid-Nineteenth Century*, Cambridge: Cambridge University Press, 1994; *Contending Liberalisms in World Politics: Ideology and Power*, Boulder, CO: Lynne Rienner, 2001.

2 The Center, founded in 1958, was renamed the Weatherhead Center for International Affairs in 1998.

had numerous links with Washington – but there was a certain distance from immediate policy preoccupations. My overriding impression was the intensity of the intellectual debates.

The book took shape in this environment, against a background of the ongoing Berlin crisis and the shock of the Cuban missile crisis, both of which underlined the immediacy of the issues with which the strategists were grappling. My original problem of bringing the story of German rearmament and the dilemmas of NATO strategy within a single framework was not resolved in those terms, but by constructing a work with five parts each addressing a set of issues which I saw as essential for an adequate discussion of the topic, now formulated more broadly to include the range of issues raised by German foreign policy and NATO strategy in this period. I maintained this basic approach in later projects: that is to say, to treat the topic as a whole, depicting what I saw as its essential aspects and the inter-relationships amongst them. The five parts were the rearmament issue in West German politics up to 1963; the Soviet political and military challenge; NATO's *raison d'être* and the debates on NATO strategy; the Berlin crisis (1958–62); and the tensions in German-allied relations, especially under the John F. Kennedy administration. One continuing theme was, as initially, the tension between the perspectives of the strategists and the political leaders, especially in Europe. The second, which came to the fore during my two years at Harvard, was the tension between German and American perspectives on the issues. It was probably this latter theme which led the Deutsche Gesellschaft für Auswärtige Politik (German Foreign Policy Association) to support the publication of a German translation.

The work drew heavily on the new strategic theorising but was not guided by general IR theory. If the underlying assumptions were realist, this remained implicit. Some of its analyses led to policy recommendations but this was not the main purpose – in contrast, for example, to Kissinger's contemporary work on the alliance. The focus was, rather, on differing outlooks and assumptions, on identifying the ensuing problems and, more tentatively, suggesting a way forward. A few years later, in the first phase of detente and of German Chancellor Willy Brandt's reorienting of West German foreign policy, while the issues relating to NATO strategy persisted, the book's discussion of German foreign policy already referred to the past.

A book for its times, then – but while most chapters addressed specific issues in that particular context, two were concerned with more general issues. Chapter 7, 'Deterrence and War: A General Analysis', presented my view of the central ideas of the new strategic thinking and defended its concern with the actual conduct of war in face of the revulsion against this approach, especially in Europe. I maintained that the mere presence of nuclear weapons and the threat of massive retaliation did not adequately ensure deterrence, and that the dangers of misjudgement, the 'escalation' of a seemingly minor incident, or technical accidents or misreadings needed to be taken seriously. Thus military capabilities and planning needed to ensure that if it came to the use of force, decision-makers would not immediately have to choose between capitulation and nuclear devastation. Second, in Chapter 12, on pressure and resistance, I noted that Schelling's seminal account of the logic of the tactics of conflict had tended to give examples of high-risk tactics, which most starkly illustrated his abstract analysis. Yet in practice, examining the Berlin crisis and noting other examples, I could find few examples of such high-risk tactics: both sides preferred lower risks – vaguely worded threats, ambiguities, limiting firm commitments to long-familiar positions, and attempts to manipulate public opinion. Nikita Khrushchev appeared to have achieved some success through such tactics, alarming Western publics and precipitating differences among Western policymakers. I offered an analysis of such low-risk tactics and the problems of devising an adequate response, which I have not seen elsewhere in the literature. In a later era I might have sought to publish a version of this chapter separately, but at the time I was in a mood to celebrate the book's eventual appearance and did not give this possibility a moment's thought. I wanted to move on.

Related writings

Strategic studies

I wrote little further on general strategic issues, as against arms control. Apart from an early review article on limited strategic war,³ I took up the ethical issues raised by nuclear strategy in a paper presented to the Lancaster Symposium on International Conflict in 1965: 'Nuclear Strategy and its Critics'. This begins with a closely argued critique of Anatol Rapoport's *Strategy and Conscience*,⁴ and discusses some issues arising, but although each of the points still reads as quite reasonable, they are not developed into an overall argument. I did not attempt to publish the paper. Like most of the nuclear strategists, I did not go deeply into the ethical issues, beyond a concern for the consequences of strategic choices. I continued to follow the ethical debates but did not further contribute to them.

Arms control

Here there was a stream of publications, for the most part sought by the respective editors, initially on the Nuclear Non-Proliferation Treaty (NPT) and related issues, on which I had worked in London as a member of the Arms Control and Disarmament Research Unit in the British Foreign Office. A few addressed the state of nuclear proliferation and the issue of controls over peaceful nuclear programmes, but more were written in defence of the NPT after my return to Sydney in 1967. The topic was entirely unfamiliar in Australia but the NPT was denounced by the prominent nuclear physicist, Sir Ernest Titterton, and I took up the defence in the *Sydney Morning Herald* and the journal of opinion, *Quadrant*, as well as a more comprehensive argument in one of the early Canberra Papers on Strategy and Defence published by the newly established Strategic and Defence Studies Centre (SDSC) at the Australian National University (ANU).⁵

I saw the NPT as a major contribution to a safer international order. Concern over the prospect of unrestrained nuclear proliferation, as the technical capacity to produce nuclear weapons became more widely available, had been one of the strategic nightmares of the early 1960s, and the laborious Soviet–Western collaboration to secure the NPT was surely one of the most notable achievements of the early détente. In addition to making the general case for the NPT's contribution to international order, I argued that it was in Australia's interest that regional powers join the treaty. Australia could encourage this by supporting the NPT; should it decline, this would strengthen the treaty's opponents elsewhere.

Later, in the 1980s, I turned to general discussions of arms control, contributing to volumes edited by Desmond Ball and collaborators. Starting with the concept – not necessarily arms reductions (disarmament) but restrictions of all kinds on the use or deployment of weapons, or on maximum limits, aimed at reducing the risk of war or its destructiveness, not requiring prior political agreement but tending to improve the political atmosphere ('confidence-building') – the chapters went on to discuss the main problems and the significant record of achievement. But whereas the tone of the 1982 chapter was quite positive, that in 1987 was preoccupied with the critique, indeed the rejection of previous arms control thinking by prominent strategists in the Ronald Reagan administration and the consequences of Reagan's Strategic Defense Initiative. This remained in the 1990 chapter, but as part of an overview of

3 'A Review: Klaus Knorr and Thornton Read (eds), Limited Strategic War', *Journal of Conflict Resolution*, 7(4) 1963: 781–5.

4 Anatol Rapoport, *Strategy and Conscience*, New York: Harper & Row, 1964.

5 'Will the Treaty Halt our Nuclear Development?' *Sydney Morning Herald*, 21 March 1968; 'That Treaty – For and Against', *Sydney Morning Herald*, 23 July 1969; 'A Limit on H-Bombs, or a Scramble?' *Sydney Morning Herald*, 24 July 1969; 'Nuclear Follies', *Quadrant*, 13(3) 1969: 66–73; 'Australia and the Non-Proliferation Treaty', Canberra Papers on Strategy and Defence No. 3, Canberra: Strategic and Defence Studies Centre, RSPacS, Australian National University Press, 1968.

the whole arms control experience.⁶ After this I did not further pursue arms control. The subject had always required a basic knowledge of the relevant technologies, but was coming to require more specialised technical expertise, and I decided to concentrate on other projects.

Cold War and détente: German foreign policy

The dramatic shift in West German foreign policy from Konrad Adenauer's rigid Cold War orientation to Brandt's Ostpolitik amplified the overall movement towards détente in Europe in the later 1960s. I sympathised with Brandt's approach, but the articles published between 1968 and 1972 were necessarily analytical, drawing attention to problems as much as potentialities.⁷ These were exemplified by the cases of Czechoslovakia, illustrating the limits of détente in Europe, and Vietnam. Two articles on the détente emphasised its likeness to the Soviet concept of peaceful coexistence: the ongoing political and ideological contest, the constant striving for advantage.

One early article stands apart from the rest: 'The Concept of Atlantic Community'. Reviewing a batch of books on this theme, I sought to make the best of this rather tenuous concept, but was not surprised that it never became more than the aspirations of a small elite.⁸ The most substantial paper in this grouping was the *World Politics* article, 'Cold War Revisionism: A Critique' – not an overview of the revisionist school, but an examination of three of its more radical authors: Gar Alperovitz, Gabriel Kolko, and David Horowitz.⁹ It presents a highly critical reading of their treatment of six major issues in the early Cold War, from the initial disputes over Eastern Europe to the founding of NATO. Looking back, I would not want to modify the critique as such, but would seek to balance it by referring to strengths as well as weaknesses of the revisionist school, and by acknowledging its stimulus to Cold War studies.

II. AUSTRALIAN FOREIGN POLICY

It is convenient to take note of my writings on Australian foreign policy at this stage, before moving on to the more theory-oriented sections that follow. This never became a major research field for me – I was not deeply read in its history – but it was inevitably an interest, through teaching, seminars, and conferences, which I would have pursued further, had time permitted. I welcomed the opportunity to develop my views on current issues in journal articles and book chapters. There was no overarching theme, but I frequently found myself urging a more measured approach where the public debate was, in my view, marked by exaggerated security fears. Since defence issues were so prominent in Australian foreign policy thinking, I shall include the few papers on defence policy under this heading.

During my years at the University of Sydney (1967–74), I wrote far more constantly on Australian foreign policy issues than later. This was the period when the US withdrawal from Vietnam cast doubt on the assumptions which had guided Australian policy since the onset of the Cold War, prompting extensive discussion.

6 'Arms Control', in Desmond Ball, ed., *Strategy and Defence: Australian Essays*, Sydney: Allen & Unwin, 1982, pp. 235–49; 'Arms Control in the 1980s', in Desmond Ball and Andrew Mack, eds, *The Future of Arms Control*, Sydney: Australian National University Press, 1987, pp. 70–97; 'Arms Control', in Desmond Ball and Cathy Downes, eds, *Strategy and Defence: Australian Essays*, 2nd edn, Sydney: Allen & Unwin, 1990, pp. 281–6.

7 'Germany's Eastern Policy: Problems and Prospects', *The World Today*, 24(9), 1968: 375–86; 'West Germany since Adenauer: Stresses and Strains of the late 1960's', *Australian Outlook*, 24(1), 1970: 51–60; 'Europe since Czechoslovakia', *Current Affairs Bulletin*, 48(July) 1971, 34–49; 'East–West Relations in Europe: The Politics of Détente', *Current Affairs Bulletin*, 50(January) 1974: 3–13.

8 'The Concept of Atlantic Community', *Journal of Common Market Studies*, 3(1) 1964: 1–22.

9 'Cold War Revisionism: A Critique', *World Politics*, 24(4) 1972: 579–612.

Three papers discussed the debates on the consequences of this new strategic environment.¹⁰ Their starting point was the uncertainty over the American stance after Vietnam: how far would the disillusionment with military involvement in Asia extend? What did the political unrest in the US portend? How great a threat would China pose? The domino image was not yet dead. And what was to be made of Russia's expanding military power, and its new presence in the Indian Ocean? An articulate minority held that nothing less than its own nuclear capability would secure Australia in the future. These papers, written to meet the requirements of a particular book or journal, presented an overview of the range of viewpoints rather than developing my own, but it was clear that I did not share the more alarmist projections. I did not see the US commitment to NATO in danger, nor its alliances with Japan, or for that matter ANZUS (Australia New Zealand United States), although it might interpret this more restrictively than Canberra would like. The idea of a new balance of power in Asia had some appeal, in the sense that if one power (China or possibly the Soviet Union) were to become too dominant, or threatening, others were likely to combine against it.

Two articles contributed to the series, 'Problems in Australian Foreign Policy', appearing twice yearly in the *Australian Journal of Politics and History*, for the periods July–December 1969 and January–June 1976.¹¹ Neither saw major events, but both included a major speech on the government's general foreign policy orientation. The articles focused on these. In August 1969, Australian Foreign Minister Gordon Freeth sought to modify Australia's rigid Cold War orthodoxy long after other Western governments had adopted a more nuanced orientation. His reward was to encounter a storm of criticism and the loss of his parliamentary seat in the election shortly thereafter. In June 1976, the newly elected Prime Minister Malcolm Fraser gave his first major speech on foreign policy, contrasting his approach with that of the previous Australian Labor Party (ALP) government. He expressed great concern over the Soviet military build-up and Indian Ocean deployment, but this was not a simple return to Cold War orthodoxy. He fully accepted the new opening up to China, and grounded his approach on classic Realist national interest doctrine, not ideological conflict. I commented that his view of the Soviet Union was at one extreme of the current Western debate, far more alarmist than most respected commentators.

The article most critical of Australian foreign policy was on the extreme lateness of its signing of the NPT, after all other Western states, and only just before the treaty entered into force.¹² Despite its professions of support for non-proliferation, its comments on the treaty had been uniformly sceptical; it appeared unaware of the reasons why so many states supported it, and the disadvantage of its approach were widely adopted. I was unaware of how seriously the John Gorton government was considering the option of Australia's actually producing nuclear weapons – an approach abandoned by his successors.

My only article on the Whitlam government, written for a Scandinavian journal, did not attempt an overall assessment but, in addition to outlining its innovations, argued that continuities, although less emphasised in its rhetoric, were no less significant, especially with respect to the American alliance.¹³ All in all, the articles on foreign policy do not amount to a major contribution but provide an indication of how the issues were perceived at the time.

10 'A View from Canberra', in Bruce Brown, ed., *Asia and the Pacific in the 1970s: The Roles of the United States, Australia, and New Zealand*, Canberra: Australian National University Press, 1971, pp. 137–58; 'Australian Strategic Perspectives', *International Journal*, 26(4), 1971: 725–34; 'Australian Foreign Policy in a Changing World', *Pacific Community*, 3, 1972: 475–86.

11 'Problems in Australian Foreign Policy, July–December 1969', *Australian Journal of Politics and History*, 16(1) 1970: 1–10; 'Problems of Australian Foreign Policy, January to June 1976', *Australian Journal of Politics and History*, 22(3) 1976: 327–37.

12 'Australia Signs the Non-Proliferation Treaty', *Australia's Neighbours*, 4th series, 69(March–April) 1970: 1–4.

13 For example, 'Australian Foreign Policy under the Labor Government', *Cooperation and Conflict*, 9, 1974: 9–18.

On Ian Bellany's suggestion, I collaborated with him on a paper on Australian defence procurement.¹⁴ This was entirely new ground for me and I never felt quite at home with the topic, but in retrospect it appears quite reasonable as the first academic study of the issues. We compiled all the available data on expenditure since 1950, and traced the history as technology became more complex and weapons systems more costly. We discussed criticisms of the policy process and attempts to improve the organisation, and the issue of Australian versus overseas procurement.

The second contribution on defence was a chapter for a volume in the series *Australia in World Affairs* (1966–70), still under its first editors, Gordon Greenwood and Norman Harper.¹⁵ This series, each volume on a five-year period, has proved remarkably durable. Contributors were expected to provide a reliable detailed record rather than an original interpretation, although inevitably there was an element of interpretation, if only in the selection of what was deemed significant. The most prominent issues were the war in Vietnam and the decision to maintain the Australian military presence in Malaysia and Singapore after Britain's withdrawal in 1970. The debates on Vietnam and the larger policy issues were discussed in other chapters, so I limited myself to the military aspects – successive phases of the operations of the Australian Task Force. The conclusions reflected the frustrations of the war: a degree of military success but no clear political outcome. The story of the Malaysia–Singapore commitment focused on the policy itself, not the debates or commentaries. Shorter sections dealt with weapons procurement, defence reorganisation, and the NPT, concluding with the government's view of the changing strategic environment.

I wrote nothing further on Australian foreign policy until 1990, when I contributed a chapter on debates and options to Coral Bell's study, *Agenda for the Nineties*. The foreign policy debate had been relatively muted during the 1980s, and the contributors to the volume had little to say on foreign policy options. The main challenges appeared to be in the economic domain. I took up the issue of whether, as Ross Garnaut's report on Australia and Northeast Asia¹⁶ was taken to be recommending, priority should be given to this region, arguing rather for an 'export culture' more alert to opportunities wherever they might open up. I deplored the narrowness of the discussion of economic issues, and expressed some scepticism over the prevailing economic orthodoxy.¹⁷

The next paper was a chapter on Australia and Western Europe in the 1980s published in the *Australia in World Affairs* series. Relations with Britain were treated in other chapters, so this was a relatively straightforward account of relations with the European Community, mainly the steadily expanding economic relationship. Despite the enhanced interest in Asia, there was no turning away from Europe; personal and cultural links remained strong.¹⁸

A more ambitious undertaking was a chapter on 'The Gulf War and Australian Political Culture', exploring a little-charted subject matter. One theme was the way in which recent attempts to promote a new sense of identity – Australia as an independent actor oriented to its region – were submerged as familiar roles were re-enacted; Australia hastening to assist its great power ally. Another was the poverty, or provincialism, of the public debate,

14 Ian Bellany and James L. Richardson, 'Australian Defence Procurement', Canberra Papers on Strategy and Defence No. 8, Canberra: Strategic and Defence Studies Centre, RSPacS, Australian National University, 1970.

15 'Australian Strategic and Defence Policies', in Gordon Greenwood and Norman Harper, eds, *Australia in World Affairs, 1966–1970*, Melbourne: Cheshire for the Australian Institute of International Affairs, 1974, pp. 233–69.

16 Ross Garnaut, *Australia and the Northeast Asian Ascendancy*, Report to the Prime Minister and the Minister for Foreign Affairs and Trade, Canberra: Australian Government Publishing Service, 1989.

17 'Debates and Options for Australia', in Coral Bell, ed., *Agenda for the Nineties: Studies of the Context for Australian Choices in Foreign and Defence Policy*, Melbourne: Longman Cheshire, 1991, pp. 289–306.

18 'Australia and Western Europe', in P. J. Boyce and J. R. Angel, eds, *Diplomacy in the Market Place: Australia in World Affairs 1981–90*, South Melbourne: Longman Cheshire, 1992, pp. 198–207.

especially striking in comparison with the US, where it was led by former presidential advisors and secretaries of state and the issues were seriously discussed in Congress. Australia had no figures of standing to structure a public debate, the issues remained over-simplified, or simply not raised. For example, there was little concern whether diplomatic options had been adequately tested before rushing to war, or whether the level of violence and destruction was justified.¹⁹

There followed a series of papers on regional security, considered here for convenience, instead of later as an aspect of the post-Cold War order. The first was a paper, originally published as an article in *The National Interest* in 1994/95, and subsequently as a chapter in a volume on Asia-Pacific security.²⁰ This paper began by contesting the kind of thesis that had been presented by Barry Buzan and Gerald Segal in a prominent article which, *inter alia*, deplored the absence of European style institutionalisation in the face of a plethora of unresolved longstanding conflicts.²¹ I argued that the standing of governments in East Asia depended on their maintaining rapid economic growth, which would be risked by military confrontations; that the existing regional institutions made for regular communication; and that the Association of Southeast Asian Nations (ASEAN), in particular, had been remarkably successful in defusing conflicts among its members. There was no ground for complacency, but surely not for alarmism.

A second paper, in a volume commemorating T. B. Millar, went over some of the same ground but also argued for much greater attention to dangers in other domains of policy, in particular the economic, where only a very narrow range of dangers was recognised, in particular a return to protectionism. Potential dangers in the international financial system, whose workings were poorly understood, called for much greater attention, as did environmental dangers which, if not addressed in time, might well pose the greatest danger to contemporary societies.²²

An article in 1997 focused on the challenge to diplomacy posed by the security issues, arguing for greater attention to long-term relationships, less preoccupation with immediate jockeying for advantage.²³ A chapter on the regional relevance of the popular ‘declining probability of war thesis’ argued that certain strands in this thesis – interdependence, globalisation, and the changing costs and benefits of major war – were indeed relevant but would not automatically ensure peace. I again called for more attention to diplomacy, both in crises and in long-term relationships.²⁴

Finally, in late 1997 I completed an ‘interim review’ of the foreign policy of the Bob Hawke–Paul Keating governments (1983–96), which remained a working paper as the journal for which it was intended did not leave time for revisions that I thought necessary.²⁵

19 ‘The Gulf War and Australian Political Culture’, in Michael McKinley, ed., *The Gulf War: Critical Perspectives*, Canberra: Allen & Unwin in association with the Department of International Relations, RSPAS, Australian National University, 1994, pp. 72–96.

20 ‘Asia-Pacific: The Case for Geopolitical Optimism’, *The National Interest*, 38(Winter) 1994/1995: 28–39; ‘The Asia-Pacific: Geopolitical Cauldron or Regional Community?’ in Gary Klintworth, ed., *Asia-Pacific Security: Less Uncertainty, New Opportunities?* Melbourne: Addison Wesley Longman Australia, 1996, pp. 6–21.

21 See Barry Buzan and Gerald Segal, ‘Rethinking East Asian Security’, *Survival*, 36(2) 1994: 3–21.

22 ‘Asia-Pacific Security: What Are the Real Dangers?’ in Coral Bell, ed., *Nation, Region and Context: Studies in Peace and War in Honour of Professor T. B. Millar*, Canberra Papers on Strategy and Defence 112, Canberra: Strategic and Defence Studies Centre, RSPAS, Australian National University, 1995, pp. 91–120.

23 ‘Asia-Pacific Security: Challenge to Diplomacy’, *New Asia-Pacific Review*, 3, 1997: 30–4.

24 ‘The Declining Probability of War Thesis: How Relevant for the Asia-Pacific?’ in Stuart Harris and Andrew Mack, eds, *Asia-Pacific Security: The Economics–Politics Nexus*, Sydney: Allen & Unwin in association with the Department of International Relations, RSPAS, Australian National University, 1997, pp. 81–100.

25 ‘The Foreign Policy of the Hawke–Keating Governments: An Interim Review’, IR Working Paper 1997/4, Canberra: Department of International Relations, RSPAS, Australian National University, October 1997.

The overall assessment was positive: in particular, the policy of active engagement in the region was a constructive response to the new situation after the end of the Cold War. But I questioned the wisdom of the extreme shift from traditional protectionism to complete free trade, along with thoroughgoing deregulation of the economy, which could pose risks and overlooked the active role of the state in most other instances of economic restructuring. The ‘good citizen’ international role was welcome, but there was a risk of rhetorical overkill.

III. CRISIS DIPLOMACY AND RELATED PAPERS

My second book, *Crisis Diplomacy: The Great Powers since the Mid-Nineteenth Century*, first envisaged in 1963–64, occupied me from 1973 until 1994 – a major project, virtually a life’s work. It is therefore described in greater detail. The successive phases of the project may be outlined as follows:

- 1963–64: choice of topic and initial outline;
- 1973: research design, papers on definition and the state of the literature, two draft case studies;
- 1978–79: two further case studies, two conference papers;
- 1982: further case study, paper on works by Glenn Snyder and Paul Diesing, and by Richard Ned Lebow;
- 1986–88: draft analytical chapters, complete manuscript for US visit in 1988–89, two published papers;
- 1989–94: revisions and additions following US visit; two further papers; search for a publisher; final revisions for Cambridge; book published 1994.

First phase, 1963–64

As I completed the first manuscript, I saw the problems of international crises as the most challenging in strategic studies. There was much writing on individual crises, but virtually no systematic study of the problems confronting decision-makers and how they could best be addressed. I was not impressed by the most widely used social science approach, content analysis, and saw international history as a more promising starting point. I had some discussions with Snyder, who was already thinking in terms of historical case studies. As a Junior Research Fellow in Oxford I began preliminary reading and prepared an initial statement, ‘International Crises: A Research Project’. This was not yet a research design, but rather set out major questions – such as the prior conditions making for war, the interests, goals, and strategies of the major participants, international political structures, commitments and pressures making for escalation – and proposed a method of approach, historical sociology, in the sense recently formulated by Stanley Hoffmann: the search for regularities and distinctions among historical cases, in contrast to the historian’s typical concern for the individual case.²⁶ Hoffmann referred especially to the study of the international system, but international crises seemed to call for a similar approach, as well as requiring the particular skills of the historian in bringing together the various aspects of complex processes and relationships – whereas content analysis limited itself to one aspect of the complex whole, viz., formal diplomatic communications. I did not at this stage envisage historical case studies, but considered drawing examples from a broad range of crises to illustrate hypotheses relating to each of the questions outlined.

However, my appointment to the Arms Control and Disarmament Research Unit in the British Foreign Office prevented me from further pursuing the project at this stage, and after my appointment to a lectureship at the University of Sydney I was mainly concerned with preparing courses for the first time as well as the various publications on Australian foreign policy referred to above. But I took account of the emergence of more systematic studies of

²⁶ Stanley Hoffmann, *Contemporary Theory in International Relations*, Englewood Cliffs, NJ: Prentice-Hall, 1960, pp. 174–85, 191–208.

crises and worked on the questions to be addressed, especially on the actors' intention and perceptions, and on bargaining.

Second phase, 1973

In 1971–72 I turned to the question of the research design. A short outline, probably from early 1971, reads as a condensation of the original 1964 paper: a set of inter-related questions, and a note on method. I proposed to examine all great power crises since 1815. However, prompted by Hedley Bull and perhaps by other reactions, I began to consider a limited number of case studies in greater depth and opted for this approach by the time of my study leave, divided between Washington and London, in 1973. Hedley saw a need to pose a central question and suggested selecting cases most relevant to it. Apart from the sheer mass of material, he doubted the comparability of all cases. Also prompted by Hedley, I came to see the project as focusing on the outcome of crises, and its central question; under what conditions do crises lead to war, and when are they resolved peacefully, and its corollary, how significant are decisions during crises?

An outline from 1973 or early 1974, presents the project in what was to prove its final form, both the case studies and the analytical chapters. In London in mid-1973, I began immediately to work on the case studies in the specified format.

At the Washington Center of Foreign Policy Research, I wrote two papers and read extensively on diplomatic history since 1815 with a view to selecting the cases, to be divided evenly between those that led to war and those that did not (although not always 'resolved' peacefully through a formal settlement). The first paper reviewed what I saw as the more significant works in the literature, which had expanded beyond recognition since 1964.²⁷ In 1972 Charles Hermann had published a valuable collection of articles by the most prominent behavioural science authors, and Ole Holsti drew together the findings of the Stanford Project on July 1914,²⁸ a year earlier Alexander George and Coral Bell had published notable contributions.²⁹ Hermann himself drew attention to the many issues which had not yet been addressed, and I noted that the issue of crises leading to war, so frequently raised, was not addressed squarely in general terms. I acknowledged that Holsti had presented much evidence of perceptual distortion in the 1914 crisis, but argued that content analysis could not show how significantly this affected the final outcome. George's innovative use of comparative case studies served as a good example of the historical-sociological approach.

This stocktaking enabled me to plan my project in relation to the current 'state of the art'. It remained unpublished: a journal expressed interest but wanted me to extend the coverage of the literature. I saw little benefit from this and preferred to move on.

The second paper took up a preliminary question, the definition of crisis, a topic which then appeared more problematic than later.³⁰ A wide range of definitions was being canvassed, whereas later, for example, the attempt to define crisis as a general problem in human or social life was abandoned, and most settled for a concept of international crisis as an acute conflict with a perception of a high risk of war.

In London I drafted the first case studies: on the Eastern crisis, 1839–41, and the crisis preceding the Crimean war, crises with many similarities but contrasting outcomes. The library of the British Museum provided ample sources; I used mainly secondary sources, supplemented by primary sources such as diplomatic documents or correspondence for

27 'International Crises: Some Recent Studies', unpublished, 1973.

28 Charles F. Hermann, ed., *International Crises: Insights from Behavioral Research*, New York: Free Press, 1972; Ole Holsti, *Crisis, Escalation, War*, Montreal: McGill-Queen's University Press, 1972.

29 Alexander L. George, D. K. Hall, and W. R. Simons, *The Limits of Coercive Diplomacy: Laos, Cuba, Vietnam*, Boston, MA: Little, Brown, 1971; Coral Bell, *The Conventions of Crisis: A Study in Diplomatic Management*, London: Oxford University Press, 1971.

30 'The Definition of "Crisis"', unpublished, 1973.

further evidence on intentions and, especially, on perceptions. Later literature sometimes provided new insights, but the drafts would require no major revision.

In London I read the manuscript of Robert Jervis's major work on perception³¹ and offered some comments, but the most important contact during the year was Snyder, who showed me some of the case studies which his graduate students had drafted for his book, now in collaboration with Paul Diesing. I was already clear that mine would pursue different, although related, questions. We shared many interests, but in the last analysis he was more the theorist, whereas I wanted to give equal weight to the history.

Third phase, 1978–79

I made little progress until the next study leave in the second half of 1978. By this time I had taken up a post in the Department of Political Science, The Faculties, at the ANU. I worked with research assistants on the following case studies, but it became clear that the project required more background in the relevant theoretical literature, which only those with a graduate training not then available in Australia could have. Apart from a valuable detailed account of the Franco-Prussian crisis there was little to show.

In Munich in 1978 I drafted the Russo-Japanese case study and the narrative of the Sudeten crisis; the German sources provided more insights than I could include in the eventual chapter. I kept in touch with the theoretical literature, now developing more incrementally, and in the following year presented two conference papers. The first, 'Crisis Management versus Crisis Diplomacy' criticised the way conclusions for policy were being drawn from certain theories together with a certain reading of the Cuban missile crisis in a way that could lead to over-confidence in an over-generalised doctrine; a concept of crisis diplomacy would better indicate the uncertain choices more likely to arise.

A second paper, 'Crisis Diplomacy: Some Problems in Historiography' presented at a conference to commemorate the historian Ernest Bramsted, took up less familiar issues which I had noted in working on the case studies. Approaching the diplomatic histories with a set of general questions, it was clear that they did not address these directly but provided incomplete materials for an answer. Not infrequently, one became aware of certain limitations, for example an over-emphasis on a specific issue over which there had been controversy which continued to reverberate disproportionately, overshadowing more significant issues. More significantly, one became aware how far historians could reflect uncritically the perspective of their own nationals on a crisis. It had been hoped that these conference papers would find a publisher, but the prospective editor was unsuccessful.

Fourth phase, 1982

My next study leave, six months of 'inside studies' in 1982–83, offered less opportunity because I could not sufficiently disengage from the Department of Political Science, but I worked on the Pearl Harbor case study, outlining its analysis but not yet a full draft. I also wrote a substantial paper in the form of a review article of Snyder and Diesing's book, which had been published in 1977 and the more recent work by Lebow, *Between Peace and War* – landmarks in the study of crises, both based on historical case studies, which raised for me the question of clarifying what my specific contribution was to be.³² Snyder and Diesing remains the most significant theoretical analysis of crises, whereas Lebow offered an original and imaginative reading of certain cases highlighting political and emotional compulsions which led certain actors to disastrous miscalculations.

31 Robert Jervis, *Perception and Misperception in International Politics*, Princeton, NJ: Princeton University Press, 1976.

32 Glenn H. Snyder and Paul Diesing, *Conflict Among Nations: Bargaining, Decision Making, and System Structure in International Crises*, Princeton, NJ: Princeton University Press, 1977; Richard Ned Lebow, *Between Peace and War: The Nature of International Crisis*, Baltimore, MD: Johns Hopkins University Press, 1981.

The paper was a closely argued analysis and critique of the two works – written, it is now clear to me, more for the crisis specialist than for the general reader in this discipline. Thus it was not surprising that the chosen journal did not accept it, giving no reason beyond the number of manuscripts they were receiving. I praised the contribution of both works, but while acknowledging that Lebow had revealed an important source of crisis mismanagement, and offered many insights, I saw his approach as theoretically one-sided, indeed failing to take account of bargaining and the whole gamut of game-theoretic and rational-choice approaches. Snyder and Diesing was far more substantial, a work of theoretical construction and integration: constructing new game-theoretic models prompted by the subject-matter and showing how theories could be fitted together in a multi-perspectival explanation of crisis behaviour. Where I became unhappy was with their concluding chapter, which became deterministic in a way that surely went beyond the detailed accounts in the earlier chapters.

I was now more concerned over how long my project was taking, and the problem of finding enough time that could be fully devoted to it. I found little prospect of a grant for long enough, but then the opportunity of applying for a Professorial Fellowship in the Department of International Relations, Research School of Pacific Studies at the ANU presented itself, and after a difficult application process I took up the appointment at the end of 1985. I had remained confident that I could write a study which would address a range of issues not adequately treated in the burgeoning crisis literature, but in the absence of relevant publications it was difficult to convince others. It was my great good fortune that Bruce (J. D. B.) Miller, the head of the Department of International Relations, had confidence in my abilities.

I had also become concerned over losing contact with other scholars in the field, almost entirely American, and therefore welcomed the opportunity to participate in a panel on ‘New Issues in International Crisis Management’ at the conference of the International Political Science Association in Paris in July 1985. My paper was a substantially expanded version of the earlier paper, ‘Crisis Management versus Crisis Diplomacy’, which provides some indication of my approach to the overall topic. The panel’s papers were published, but only three years later.³³

Fifth phase, 1986–88

In planning for the new appointment I needed to work out how to keep the manuscript to a manageable length. Four lengthy case studies plus a theory chapter and at least six analytical chapters would already be a long book. I now intended to reduce the remaining cases to brief summaries, following the model of Snyder and Diesing. The four would be sufficient to show readers how I derived the arguments in the analytical chapters from the case studies. All the remaining chapters would be very substantial. I had preliminary drafts only for the theory chapter and the analytical chapter on objectives. Thus I had to allow for a two to three-year timeframe for a complete draft, which proved realistic. I would not have any other duties during the remaining three years of Bruce Miller’s headship.

The first task was to complete the analytical sections of the Sudeten crisis. It was necessary to come to terms with the multitude of new publications, especially on British policy, occasioned by the opening of the archives which brought a new emphasis on the magnitude of the financial constraints limiting the government’s choices. Historians had long recognised that the traditional stereotypes – Neville Chamberlain the naïve appeaser, Winston Churchill the farsighted realist – amounted to a gross over-simplification. The new literature offered a new perspective: a ‘structural interpretation’ advanced by diplomatic historian Paul Kennedy and a number of German authors. Changes in the overall context, in particular the strategic and financial constraints on British policy, drastically limited the options such that there were no good outcomes, only a choice of evils. Both sides of the debate were correct as regards the

33 See ‘Crisis Management: A Critical Appraisal’, in Gilbert R. Winham, ed., *New Issues in International Crisis Management*, Boulder, CO: Westview Press, 1988.

negative consequences of the alternative, but equally over-optimistic on the prospects of their preferred approach. I found it useful, in order to clarify the significance of the new literature, to prepare a seminar paper on it, and its challenge to the IR discipline, which had not moved beyond the traditional stereotype. A revised version was published in 1988.³⁴

In the same year I published a second article, 'New Insights on International Crises', a review article on the first five volumes in a series of case studies in the International Crisis Behavior Project, edited by Michael Brecher. The five books, each by a distinguished specialist, scrupulously following Brecher's research design, focused on a single major issue, the effects of crisis-induced stress on decision-making – an issue that had been prominent in the early behavioural crisis studies, which Brecher proposed to examine rigorously and exhaustively in a wide range of contexts, the initial expectation being, in line with earlier findings, that there would be a serious decline in the quality of decision-making. The surprising conclusion of the five volumes was that this was not the case; the rigorous methodology that was followed left no margin of doubt. I discussed the significance of this finding and related the Project to other studies such as Snyder and Diesing, and Lebow.³⁵ I found it quite valuable to vary the even flow of work on the major project with short digressions such as this, close to the main project but on a different issue.

To return to the book, following the Introduction, the chapter on 'Theories of Crisis Behaviour' outlined the theories which, in the light of existing research, I took to merit serious consideration. They could be classified under five headings, each representing the general perspective shared by a group of theories. These were: theories of rational choice, or presupposing rationality; psychological theories of impaired rationality; political and organisational theories; theories of adversary interaction; and systemic and deterministic theories. The evaluation of such theoretical perspectives is not a matter of rigorously testing specific empirical hypotheses but rather, as Snyder and Diesing suggest, a question of relevance and 'fit'.

The first analytical chapter considered international systemic perspectives. The abstract systemic theory best known in IR, of theorists such as Morton Kaplan and Kenneth Waltz, I saw as being at too great a level of generality to be relevant to the onset, the course of events, and the outcome of crises. But a version of historical sociology, the approach of certain theory-oriented historians such as F. H. Hinsley, Paul Schroeder, and Kennedy, offered a valid framework for examining the changing features of the international system as they shaped the context in which crises took place. The constellation of the international system might be relatively 'stable', conducive to the peaceful settlement or to the limitation of crises; or the opposite might apply: crises would be more likely to lead to major war. I discussed the interaction of three variables: the changing arena (from a European to a global system); patterns of alignment; and changing norms.

Rational-choice assumptions, explanations in terms of the actors' goals and intentions, structure most narratives of crises, but the study of goals has been played down in crisis studies: they tend to be taken as given, the attention focusing on strategies and tactics. The chapter on the choice of goals seeks to repair this omission. I distinguished between the immediate objectives of the actors and the larger values and interests at stake, treating it as an open question whether the objectives were well grounded in a conception of these larger interests – i.e., whether in this sense the choice of goals was rational. The conclusion, perhaps surprisingly, was that they normally appeared to be, although the reasoning often remained largely implicit. The exceptions, however, were important – cases where the actor pursued no clear objective, either because decision-makers were unable to resolve their differences or because their perception of the situation was so faulty that they could not define a relevant

34 'New Perspectives on Appeasement: Some Implications for International Relations', *World Politics*, 40(3) 1988: 289–316.

35 'New Insights on International Crises', *Review of International Studies*, 14(4) 1988: 309–16.

objective. I used the term 'informal rationality' to refer to this situation, whereas formal rational-choice theories required a precision which could not be established.

The chapter on selective perception and misperception could draw on a literature more focused on the relevant issues. Perceptions were always influenced by established images and expectations, thus a degree of misperception of new or uncertain situations was inevitable. The main question addressed in this chapter was: when does misperception have serious consequences for the course of events and outcome of a crisis? I distinguished the misperception of capabilities, of intentions, of the consequences of one's own actions and of local actors. The misperception of capabilities was less frequent than the others but tended to have the most serious consequences. Many misperceptions were inconsequential or were corrected in the course of the crisis, but there were two important exceptions: at a crucial moment a decision based on a misperception could create a new situation which could not be reversed; or a faulty belief might be so strongly embedded as to be unshakeable by evidence to the contrary. In such a case, misperception would be a necessary condition of the ensuing outcome. I found support in the case studies for both cognitive and political explanations of significant misperception – i.e., the tenacity of long-established images, and the political compulsion to remain committed to a course of action.

The case studies had suggested that bargaining was the most prominent form of interaction in crises, but not the only form. The chapter on bargaining – understood as the attempt to influence the decisions of others through strategies of coercion and accommodation – inquired into its overall significance in crises, and into alternative modes of interaction. It postulated a four-fold typology: pure bargaining; strategy without bargaining (insistence on demands, or purely unilateral moves); bargaining negated by misperception; and bargaining negated by internal differences. The latter two, not surprisingly, coincided with the exceptions to rational decision-making. The cases provided no example of the radical alternative model of interaction, the hostility spiral of escalation, but quite frequent examples of elements of this model, and of the expression of fears that it could become the reality: the situation could get out of control. Most of the cases exemplified a combination of types, but usually one was predominant. The cases where strategy and bargaining were negated tended to have the more dangerous outcomes. The chapter engaged in extensive discussion of Snyder and Diesing's study, following its exposition of game-theoretic models, but qualifying its assumption that bargaining was the only significant form of crisis interaction.

It was evident from the case studies that internal politics could have important consequences, often disruptive, for crisis diplomacy, but the relationship between the internal and the external has proved resistant to theorising, so the chapter proceeded inductively. First, it noted how the historically 'given' structures and institutions as well as the political circumstances determine the nature of the decision-making elite and the strength or uncertainty of their control over policy. Second, it noted the major consequences of unresolved differences among the decision-makers but also that, while policy differences were always present, they were usually resolved, seldom through intellectual argument. Typically, one view prevailed, and was ratified by the final authority. Where unresolved differences coincided with political instability – a situation where a government or regime is in danger of losing power – advocates of more risky policies could appeal to aroused public emotions, increasing the probability of war. But political instability is one of those historical 'givens' beyond control. This may suggest a question seldom raised in crisis studies, viz., whether there are political prerequisites for 'good' crisis diplomacy: in the light of views advanced by Lebow, the chapter concludes with some tentative suggestions.

Finally, the chapter on the outcome of crises and the risk of war argued against the view that these were pre-determined by the nature of the conflict between the main rivals. Crisis decisions and diplomacy were always significant, and involved a complex interaction among the variables studied in the previous chapters. The final outcome depended on the preference orderings of the relevant decision-makers, but the options and preferences became clear only in the final phase. Whether the outcome would be war or its avoidance could not be discerned

at the outset, but it could be judged whether a conflict was so acute as to render war probable, or whether it offered a reasonable chance of a diplomatic solution. This made it possible to distinguish four situations:

1. War initially probable, war the outcome: the Franco-Prussian crisis and the US–Japan crisis were deemed to be of this type.
2. War not initially probable, war the outcome: the Crimean and Russo-Japanese crises were deemed to be cases which offered a reasonable prospect of diplomatic resolution, although this was not achieved.
3. Resolved peacefully, but more acute than initially probable: the Eastern, Agadir, and Sudeten crises were seen as having moments closer to war than an initial assessment would suggest.
4. Peaceful resolution the more probable, and achieved: both Berlin crises, the Berlin blockade providing the only example, thanks to the airlift, of a crisis that proved less acute than was reasonably to be expected.

The conclusion that ‘crisis diplomacy matters’ was significant but not surprising; more striking, and disturbing, was the conclusion that crises tend to become more acute and dangerous than might reasonably be expected at the outset.

I travelled with the draft completed to this point to the US in late 1988, gave several seminars, and discussed the manuscript with one of the editors of a new series on history and theory at Princeton University Press. He wanted more than summaries of the further cases, especially those concerning the US, but was willing to consider it for their series. I opted for a mid-way solution for the remaining cases: a narrative highlighting the analytical themes but much shorter than the original case studies, and also to add a Conclusion, drawing together the evaluations of the theories and formulating some implications for policy. Along with my duties as head of the Department, following Miller’s retirement, these additions and normal revisions occupied much of the next two years.

Sixth phase, 1989–94

In the Conclusion I presented the concept of structuration derived from Anthony Giddens as a convenient framework for ordering the theoretical conclusions. But for those seeking a novel theoretical approach the outcome was rather deflating. The conclusions for the most part took the form of contingent generalisations, pointing to the limitations of each theoretical approach as much as its explanatory potential. For example, both rival theories of misperception could best explain certain cases, but not others, and in some it was not necessary to look beyond the incompleteness and uncertainty in the available evidence.

The discussion of policy thinking was perhaps more innovative. Its starting point was the concept of diplomacy rather than crisis management, avoiding the over-confidence of the latter, emphasising the centrality of the awareness of the other. The currently favoured principles were not misguided but needed to be qualified and supplemented, regarded not as maxims or rules but as considerations which needed to be taken into account. And considerations such as the importance of timing – not over-looked in the literature but under-emphasised in the policy discourse – needed to be given more attention. The discussion did not seek to offer specific policy suggestions but to enlarge the stock of ideas circulating in the policy community.

I sent the completed manuscript to Princeton. The response was not only disappointing, but something of a shock. While Snyder, who identified himself as one of the two readers, wrote a highly positive assessment – generously accepting my critique of his study as part of the scholarly debate – the second reader rejected it outright, finding its theoretical approach lacking in terms of the current state of the art. I had encountered a similar reaction in one of my seminar presentations, but had expected that a reader would recognise that I was not engaged in the standard task of structuring the case studies in terms of a single, well-defined theory, but seeking to evaluate the various theories in relation to the set of cases, for which

there was no established methodology. And this whole undertaking resulted from my central question concerning the outcome of crises, which likewise he did not address. Although Snyder was the acknowledged leader in the field, the Press simply followed the second reader – probably, I thought, a member of their editorial board. I approached Cornell University Press, although with little optimism as their series tended to follow the standard American expectation on the role of theory – that is to say, working within parameters of an established theoretical approach, not taking stock of the adequacy of that approach in relation to the chosen topic. My expectation was correct: they quickly decided it was not suited.

Andrew Mack, the new Department head, encouraged me to try Cambridge University Press, with its well established series open to greater diversity of approach. I wrote a careful letter to Steve Smith, the General Editor, to explain the purpose and character of the work; he agreed to consider it, and I could not have hoped for better readers, both of whom subsequently identified themselves. I was particularly impressed by the report by Michael Nicholson, the leading UK exponent of the quantitative approach, who showed himself fully aware of the demands and problems of my unusual historical study. The second, Kalevi Holsti, offered some criticisms which I could acknowledge as needing attention, and also found the chapters on perception and the Sudeten crisis disproportionately long. The manuscript had indeed grown to a length which might deter many readers. The publisher accepted it on the condition that I shorten it, albeit more than I preferred. The revision, which occupied too much of my study leave in Frankfurt in 1992–93, proved very arduous, because in addition to reducing the over-long chapters, lesser cuts had to be made throughout. I have no doubt that the book was better for the revisions, but my main reaction was relief that the labour of so many years had been rewarded, indeed had found a place in so well-regarded a series.³⁶

During the later stage of the publication process, I presented two conference papers arising from the book. The first, ‘Informal Theories of Rationality’, delivered at the International Studies Association (ISA), remained a departmental Working Paper.³⁷ After an extensive critique of the application of formal models of rationality in the context of foreign policy decision-making, it spelled out rather too discursively my concept of informal rationality. Publication as an article would have drawn greater attention to this key concept in my analysis, but my priority was to complete the book manuscript for publication.

The second paper, ‘Rationality in Foreign Policy’, presented at the inaugural Pan-European Conference of the European Consortium for Political Research in 1992 and selected for inclusion in the published conference volume, surveyed the debates in IR in the context of the wider literature on rationality, moving on to consider the recent reassertion in IR of the claims for rationality, concluding with a brief exposition of informal rationality. The paper was more systematic and clearly structured than the former but, although taking a positive view of the informal approach, was not primarily an argument for this concept but rather a survey of the state of the debate.³⁸

By a strange coincidence, two other long-standing projects on crises, begun like mine in the early to mid-1970s, were completed at much the same time, in books published in 1993. Both employed behavioural-science methodologies: aggregate studies addressing different issues from mine, but the overall conclusions broadly congruent, such that the three might be seen as complementing one another.

36 The Cambridge Studies in International Relations series.

37 ‘Informal Theories of Rationality’, IR Working Paper 1990/8, Canberra: Department of International Relations, RSPacS, Australian National University, 1990.

38 ‘Rationality in Foreign Policy’, in Frank R. Pfetsch, ed., *International Relations and Pan-Europe: Theoretical Approaches and Empirical Findings*, Munster: Lit, 1993.

Russell Leng's *Interstate Crisis Behavior, 1816–1980*, written within David Singer's Correlates of War Project, analyses data from 40 crises to evaluate competing theoretical assumptions – rational/strategic and behavioural/psychological – testing hypotheses drawn from both and going on to consider the implications for policymaking.³⁹ He concludes that the former – realist considerations of interest and power – explains much crisis behaviour, but that at higher levels of intensity cognitive emotional factors may be more significant. He argues that policy prescriptions need to be drawn from both approaches. In behaviouralist style he limits the number of variables by 'black-boxing' decision-making and internal politics; thus while he can explain the general pattern of crisis behaviour he cannot explain the course and outcome of specific crises.

Second, Michael Brecher's study draws together the conclusions of the multi-volume International Crisis Behavior Project, initiated in 1975. Following a pluralist research strategy, the Project had published book-length case studies of ten crises and three volumes of aggregate data and analyses of all crises between 1918 and 1988. The former focus on the effects of stress on crisis decision-making; the latter employ a model based on systems theory to identify patterns in the onset, escalation, de-escalation and impact of crises.

The aggregate findings are probabilistic. For example, crises are most likely to occur in a polycentric (hybrid) system structure between actors that are both heterogeneous and contiguous. Factors making for violent escalation include power asymmetries, authoritarian regimes, newly acquired independence, and internal instability. If the systemic findings confirm widely held assumptions, those on decision-making, as indicated in my 1988 review article, disconfirm many theory-based hypotheses on the deleterious effects of stress.⁴⁰ He finds partial support for hypotheses postulating negative effects of conceptual closure and 'groupthink', for example, but finds that crisis decision-makers are not, typically, closed to new information, rather seeking it more actively than usual, and considering different options more carefully.

His work exemplifies the behaviouralist commitment to the painstaking accumulation of empirical data, but he does not engage with the subsequent 'wave' of deductive theory-driven research. There is no place in his framework for bargaining or strategic choice. In the end he is surprisingly sanguine about crisis management: having rejected the more far-reaching hypotheses on the disabling effects of stress, he does not discuss further difficulties with the notion of crisis management.⁴¹

IV. INTERNATIONAL RELATIONS THEORY

The papers in this category are extremely diverse: there is no central thread, partly because my interests were wide-ranging and developed over time, and quite a number were requested by editors or seminar convenors for a specific topic. Liberal theory became a major interest in my later years in Canberra and this is treated separately in the final section. There are more unpublished papers than usual in this section, reflecting the ad hoc character of many of the seminar or conference presentations.

The earliest is a seminar paper in the Department of International Relations, London School of Economics (LSE), in late 1964 or 1965, on 'Psychology and the Study of International Relations'. Although acknowledging that there were some areas where it had much to offer, the paper was mainly a severe critique of current psychological writings on conflict and war – quite cogently argued, but too narrow a reading of the topic. Thus it was

39 Russell J. Leng, *Interstate Crisis Behavior, 1816–1980: Realism Versus Reciprocity*, Cambridge: Cambridge University Press, 1993.

40 'New Insights on International Crises'.

41 Michael Brecher and Jonathan Wilkenfeld, *A Study of Crisis*, Ann Arbor, MI: University of Michigan Press, 1997.

open to the critique it received from F. S. Northedge, who had a particular interest in psychological approaches to IR.

Next was a paper on 'The Soviet Union and International Order', presented at a conference which Hedley Bull organised at the ANU in 1968 – the only occasion for many years where the international lawyers and IR scholars met together. Taking Martin Wight's essay on 'Western Values in International Relations'⁴² as its starting point, the paper argued that although the Soviet Union had moved some way in the direction of supporting the existing order, this was essentially a matter of practical accommodation and the avoidance of war, not of shared norms. The Soviet doctrine of 'two systems', based on fundamentally antagonistic ideologies, amounted to a rejection of the concept of an international society, the unstated assumption behind Western thinking on international order. The attempt to find a publisher for the conference papers proved unsuccessful.

In 1971 I contributed a chapter 'Super Powers and Secondary Powers' to a book edited by Carsten Holbraad.⁴³ The suggestion came from the editor. There was no term in current usage to refer to states between the superpowers and middle powers, such as Canada. Most of them were former 'great powers'. The question was what role, if any, these powers played in the current international order. I saw them as those which played a major role in one of the principal regions of tension, Western Europe and Asia. It was clear that they were not cohesive: some remained close to the US, others resisted superpower hegemony. I argued that their existence rendered it unlikely that the bogies of hegemony or condominium would materialise. The concept has not caught on, and indeed as defined, it lapsed with the ending of the Cold War.

Two papers in the following years were concerned with dependency theory. The first, at an ANU seminar on world society in 1975, examined 'The Structural Dependence Model'. This was essentially exploratory; I had only recently come to the topic. The second, 'Dependency as a Theory of International Relations', prepared in 1981/82, was a draft chapter for a book which a colleague across campus planned to edit, but unfortunately this came to nothing. This was a much more substantial paper, addressing a question which had recently begun to be raised. Dependency had been discussed in the context of development, in a literature quite separate from IR. But there had been suggestions that it tacitly combined two systemic structures, both essential to IR – the socio-economic and the states system. This raised the question how these were inter-related. One overly ambitious view, developed in the US, was that there could be a grand theoretical synthesis, but in practice, as expounded, this involved subsuming the political, states-system, within the socio-economic, capitalist framework. I took a sceptical view of attempts at synthesis and saw more promise in the alternative approach, seeking to inter-relate the two domains, each with its distinctive theoretical logic. Alternatively, contemporary dependency could be treated as a partial theory of IR (patterns and constraints in North–South relations).

When the plans for publication collapsed, I submitted the paper to a journal, but the readers were divided, probably looking for a more forcefully argued conclusion, whereas I was mainly concerned to clarify the logic of a confusing theoretical debate.

There followed two rather time-bound papers, on the teaching of IR in Australia and on peace research. The first, for an edited book on the discipline in Australia, surveyed the courses offered and discussed some immediate teaching problems, but did not raise more

42 Martin Wight, 'Western Values in International Relations', in Martin Wight and Herbert Butterfield, eds, *Diplomatic Investigations: Essays in the Theory of International Politics*, London: Allen & Unwin, 1966.

43 'Super Powers and Secondary Powers: Western Europe and Japan', in Carsten Holbraad, ed., *Super Powers and World Order*, Canberra: Australian National University Press, 1971, pp. 90–104.

fundamental issues concerning objectives nor challenge prevailing practices.⁴⁴ The short article on peace research, occasioned by the forthcoming establishment of the Peace Research Centre at the ANU, noted the lack of an established discipline or area of study and commented on the desirable qualities of the peace researcher, above all intellectual independence in face of the pressures to which the Centre would be exposed.⁴⁵

The next paper was more unusual: a contribution to a seminar series organised by John Vincent on 'Neglected Thinkers in International Relations'. I chose the diplomatic historian Paul Schroeder, arguing that diplomatic (or more broadly) international history was neglected in IR, and Schroeder one of the more original historians with theoretical interests. I referred to some current debates among diplomatic historians and Schroeder's distinctive approach.⁴⁶ Although it had been a lively seminar series, Vincent did not consider the papers substantial enough to pursue publication.

In 1986 I presented a paper on 'Psychological Theories and International Relations' at the conference of the Australasian Political Science Association – a long paper, noting 'some historical landmarks' and mainly concerned with cognitive psychology and foreign policy decision-making. In marked contrast to the early paper at the LSE, it passed very quickly over the school of thought there condemned. I took the general findings on cognitive psychology as well established and their application in IR as significant but not free from problems. In effect, this was a stocktaking of the literature on which I was drawing in the crisis study, while noting that there were several other areas of interest in IR: it served its purpose in bringing the literature to the attention of colleagues as well as clarification for my own project.

The next paper to be written, although published a little later, was 'The Academic Study of International Relations', a chapter to the volume commemorating Hedley Bull, and one which I most enjoyed writing.⁴⁷ Notwithstanding some reservations over his celebrated critique of behaviouralism, I had felt that he often articulated better than I could a shared view of the academic enterprise that originated in the thought of the Sydney philosopher, John Anderson: a view which insisted that the scholarly ethic take precedence over other commitments, and the need to struggle to maintain it in the face of social and political pressures, especially when supported by the authority of church or state. The first part of the chapter discussed Bull's views on the study of IR in this philosophical context. The second, noting the brevity of his account of the 'classical approach', attempted to spell this out more fully, taking account of clues in his work as a whole, including his book reviews, which sometimes offered insight into all aspects of his thinking. The final section discussed what was distinctive and especially valuable in his own approach. The chapter was essentially expository, but not uncritical, concluding that the best tribute to him was not the uncritical acceptance of his views but to approach them with the kind of critical but constructive spirit he had brought to the assessment of others.

As something of a postscript to the chapter, I presented a seminar on 'The Two Cultures: The Case of Bull versus Waltz'.⁴⁸ This contrasted the American and British disciplines, the former with its extensive development of theory in many substantial works with the British tendency to conduct theoretical debates in collections of essays, individual statements but

44 'International Relations Teaching in Australian Universities', in Coral Bell, ed., *Academic Studies and International Politics*, Canberra Studies in World Affairs No. 6, Canberra: Department of International Relations, RSPacS, Australian National University, 1982, pp. 171–86.

45 'Peace Research and the Peace Researcher', *Peace Studies*, 3(March) 1984: 21–3.

46 'Paul W. Schroeder, Diplomatic Historian: A Neglected Worker in a Neglected Field', unpublished, August 1983.

47 'The Academic Study of International Relations', in J. D. B. Miller and R. J. Vincent, eds, *Order and Violence: Hedley Bull and International Relations*, Oxford: Clarendon Press, 1990, pp. 140–85.

48 'International Relations Theory and the Two Cultures: The Case of Bull versus Waltz', unpublished, September 1987.

lacking focus and with few exceptions failing to develop substantial studies. Kenneth Waltz's *Theory of International Politics* had provided such a focus of debate and theoretical development;⁴⁹ at that time, Bull's work had not. The paper argued for the superiority of Bull's work to Waltz's, but on re-reading this section is unsatisfying, the critique of Waltz not sufficiently developed.

It was also the time for a book in honour of Bruce Miller, *The West and the Third World* – not directly commenting on his work but concerning his major interests. My chapter, 'Ethical Issues in North–South Relations', took his position as its starting point: that there was greater scope for acting on moral principles in this area than in most other policy domains, because security imperatives were not at stake.⁵⁰ I distinguished several approaches: the social-democratic, typified by the Brandt Commission Report⁵¹ – and several variants of conservatism. These ranged from the rejectionist (the denial of any obligation to assist) through the managerial (for example, the World Bank under Robert McNamara), 'the market as panacea' (the view that the free-market economy was not only necessary but also sufficient to promote development), to social conservatism – in practice close to social democracy in its policy recommendations. I concluded that the market as panacea was amply disconfirmed by the evidence and that the ethical arguments favoured, indeed required, a much more generous Western response. The obstacle was evident: this did not rank high enough in the priorities of Western governments – and voters.

In early 1989 at a conference organised by the Department I presented a paper on 'The State of the Discipline: A Critical Practitioner's Approach'. After reviewing the historical background and taking a positive view of the intellectual challenge posed by the current theories, the main section discussed the discipline's strengths and weaknesses. Its most notable achievement was its response to the challenge posed by nuclear weapons, devising appropriate concepts which had shaped the public discourse. Certain issue areas had been intensively researched and one could point to some significant findings (I referred to examples with which I was familiar, such as crisis decision-making). But a major weakness was the limited attention being paid to the greatest present challenge – the environment. I criticised the undue prominence of rational-choice theory and – what was to be a recurring theme – saw the history discipline, and in particular historical sociology, with its multi-perspectival approach, as a more appropriate model.

Two articles took up a related theme: the relationship between History and International Relations Theory. In the first, 'Paul Kennedy and International Relations Theory: A Comparison with Robert Gilpin' – a contribution to a symposium on the 'Kennedy thesis', the decline of American power – I pointed to remarkable similarities between Gilpin's theory of international systemic change and the assumptions underlying Kennedy's *The Rise and Fall of the Great Powers*.⁵² They also presented certain common problems: ambiguity concerning the primacy of economic factors in explaining historical change, and the primacy of 'structures' as against 'agents'. In the latter context I brought in another recurring theme: the neglect of diplomacy/statecraft in the IR discipline as in political practice.⁵³

The second was a review article of Jack Snyder's *Myths of Empire*, widely acclaimed at the time, which presented case studies of 'over-expansion' of great powers in the industrial era to test competing theories: neorealist, cognitive, and his own preferred domestic-political

49 Kenneth N. Waltz, *Theory of International Politics*, Reading, MA: Addison Wesley, 1979.

50 'Ethical Issues in North–South Relations', in Robert O'Neill and R. J. Vincent, eds, *The West and the Third World: Essays in Honor of J. D. B. Miller*, Basingstoke: Palgrave Macmillan, 1990, pp. 242–59.

51 Independent Commission on International Development Issues, *North–South: A Programme for Survival: Report of the Independent Commission on International Development Issues*, London: Pan Books, 1980.

52 Paul Kennedy, *The Rise and Fall of the Great Powers*, New York: Random House, 1989.

53 'Paul Kennedy and International Relations Theory: A Comparison with Robert Gilpin', *Australian Journal of International Affairs*, 45(1) 1991: 70–7.

theory of ‘cartelisation’.⁵⁴ Although acknowledging the originality of his theoretical construct, I argued that his treatment of the cases was seriously flawed. Because he approached them from a particular angle of vision they appeared to support his theory. But they were lodged within scarcely acknowledged interpretations of the cases that were questionable and indeed the concept of over-expansion was often highly problematic. In effect, the article was a plea for greater attention to the demands and methods of the history discipline on the part of IR theorists.⁵⁵

In February 1995, I presented a paper at the University of Manchester on ‘Historical versus Economic Modes of Analysis in International Relations’, a revised version of which was subsequently published (discussed below). I also presented a paper at the University of Sussex on ‘Michael Mann, the Longue Durée and International Relations Theory’. I had been considering a larger project on the longue durée but decided to proceed first with the liberalism project. The starting point was the weakness of the established international relations theories in explaining structural or systemic change, exposed by the end of the Cold War and their inability to address the question whether the states system was undergoing fundamental change – the issues raised by globalisation. I saw contemporary historical sociology as better equipped for this task, and Michael Mann as of particular interest because his major work addressed the question of structural change since the beginning of recorded history.

The paper gives an extensive summary of his complex theoretical framework, emphasising his insistence that generalisations are always limited to certain periods, and his respect for historical particularity, all of which enhanced the persuasiveness of his explanations of the great historical transformations. But these strengths made for major difficulties in seeking to apply his approach to the contemporary situation. It offers a wealth of relevant questions but, as in his historical accounts, one also needed a well-grounded analysis of the particulars of the present. In itself, the framework pointed to numerous possibilities. Thus the conclusion comes across as unsatisfyingly indeterminate. A larger study might have been able to tease out more from his framework, but for the time being I did not see a way of developing the paper into a more convincing account of Mann’s potential contribution to the discipline.

In 2001 I finally published a methodological ‘manifesto’ in a volume on the discipline – a revised version of the paper in Manchester on modes of analysis. In retrospect, this probably attempts to cover too much ground too rapidly. (The editor, in his reference to the chapter, misses a key issue.) It begins with a moderate statement on the limitations of formal rational-choice theory and one-dimensional economic modes of analysis in the IR discipline, then presents historical sociology as a more promising approach to systemic theory, especially for systemic change. The central contribution comes in the final section, an outline of the mode of theoretical analysis the ‘historical’, which I recommend as more appropriate for the discipline in place of parsimony, structured complexity; treating Mann as exemplar; methodological pluralism; the limited scope of most theories; and criteria for testing/evaluating historical/political interpretations.⁵⁶

V. CONTENDING LIBERALISMS AND THE POST-COLD WAR ORDER

In the early 1990s, like many colleagues, I was taking a keen interest in the debates on what new international system, and hopefully order, would replace the familiar Cold War superpower rivalry as the framework – increasingly stable, as it appeared – for the conduct

54 Jack Snyder, *Myths of Empire: Domestic Politics and International Ambition*, Ithaca, NY: Cornell University Press, 1991.

55 ‘History Strikes Back: The State of International Relations Theory’, *Australian Journal of Political Science*, 29(1) 1994: 179–87.

56 ‘International Relations and Cognate Disciplines: From Economics to Historical Sociology’, in Robert M. A. Crawford and Darryl S. L. Jarvis, eds, *International Relations – Still an American Social Science? Toward Diversity in International Thought*, Albany, NY: SUNY Press, 2001, pp. 277–98.

of international relations. I did not initially envisage this as the area for my next project. I felt that I had completed what I had to say on international crises and considered several options for a new direction. One was to work out my position on the issues raised by post-modernism, a major interest of our graduate students. But I came round to the view that it would be better to develop my own framework than, as in the crises study, critically reviewing theories advanced by others. A second option was to pursue historical sociology in quite a different area: instead of the short-term focus of crisis studies, the opposite extreme: a study of the *longue durée*, the changing international system from the outset to the present. After some preliminary work in London in 1994–95, I decided to give priority to what had become the liberalism project, which was further advanced.

In contrast to previous books, this third one took shape in the course of publishing a number of papers in the years before it was written. The first of these, ‘Questions about a Post-Cold War International Order’, took stock of the burgeoning literature (broadly, realist pessimism versus liberal optimism), and forces making for the transformation of the international system. It sought to identify some important underlying issues – concerning the concept and criteria for order, the relevance of historical experience, and the potential for statecraft. And while offering no firm conclusions, it made a number of tentative suggestions which foreshadowed some of the themes in the eventual book. And in the context of the issue of legitimacy, it looked back to the earlier paper on ethical issues in North–South relations, which could be seen as presenting the essential normative foundation of the argument in the eventual book.⁵⁷

The second paper, ‘The End of Geopolitics?’, took up one of the central issues in the debates, viz., whether the changes under way were so far-reaching as to amount to a systemic change, such that war among the major powers was no longer the final arbiter of serious international conflicts. The strongest argument for this view was the drastic change in the cost-benefit ratio. The risks posed by nuclear weapons and the costs of economic disruption under conditions of intensified interdependence were so obvious, and the prospective gains so implausible, that the hegemonic wars of the past were no longer imaginable. Increasing international institutionalisation was part of this picture, but some other liberal theories remained problematic: for example, the theory of the democratic peace was debatable, and in any case not all major states were democracies. Power differentials remained important, and liberals still tended to overlook the tensions that could be generated by the liberalism of the powerful.⁵⁸

The theme of the next paper, ‘An American New World Order?’, was the central importance of the US role in the construction of any international order. The need for hegemonic leadership was questionable but the US undoubtedly enjoyed military, economic, and cultural predominance, and others tended to acquiesce or even defer. The US foreign policy community could be divided, broadly speaking, into realist and liberal schools, each with distinctively American characteristics. Most significantly, these defined the limits of American foreign policy thinking. On balance, the liberals had the stronger arguments, but there was a need for a realist corrective. And there was a need for a stronger input from outside the US. As a legitimising ideology American liberalism was too narrow, due to its over-emphasis on market economics and neglect of the economic rights and deprivation of the disadvantaged, and also of ecological issues.⁵⁹

The next paper, ‘Problematic Paradigm’, amounts to an outline of the book’s central argument concerning the role of neoliberal ideology in the shaping of the emerging

57 ‘Questions about a Post-Cold War International Order’, IR Working Paper 1992/2, Canberra: Department of International Relations, RSPacS, Australian National University, 1992.

58 ‘The End of Geopolitics?’ in Richard Leaver and James L. Richardson, eds, *The Post-Cold War Order: Diagnoses and Prognoses*, St Leonards and Canberra: Allen & Unwin in association with the Department of International Relations, RSPacS, Australian National University, 1993, pp. 39–50.

59 ‘An American New World Order?’ IR Working Paper 1993/7, Canberra: Department of International Relations, RSPacS, Australian National University, 1993.

international order. The starting point was the perception of a world without alternatives – to liberal democracy and the market economy. But the narrow parameters of social choice were not the outcome of inexorable forces ('globalisation') but were shaped by ideology – not liberalism per se, but a particular version of it. The significance of differences within liberalism was shown in a brief historical survey. The neoliberal suspicion of government and celebration of the market reflected certain core liberal values but had long been contested, and indeed rejected outside the US. It had led to a drastic narrowing of the post-Cold War agenda and subordination of the concerns of the world's disadvantaged. The paper also drew attention to forces sustaining the paradigm and forces for change.⁶⁰

The article 'Contending Liberalisms' spelled out a historical interpretation of different strands within liberalism as a succession of struggles between elitist and egalitarian doctrines. Neoliberalism, the contemporary doctrine of the privileged, could be expected to be no more able to achieve general acceptance and legitimacy than earlier versions of elitist liberalism, but there was no clear alternative doctrine. Nor was there a clear articulation of an alternative, more participatory form of democracy. In the case of human rights, however, there was a clear distinction: radical liberals strongly affirmed economic and social rights, neglected in official Western liberal discourse.⁶¹

The article did not further examine the forces sustaining neoliberalism but one of these, the dominance of economics in the contemporary Western political culture, was discussed in 'Economics: Hegemonic Discourse', foreshadowing a section in the book. It was not that economics as a discipline endorsed neoliberalism, but in this intellectual climate oversimplified economic claims could assume an authoritative status for which there was no justification.⁶²

In the foregoing papers the examples of the disadvantaged were drawn mainly from the 'Third World', but a more polemical article took up this issue in the Western societies, based on personal observations in London in 1994–95, a time when the sight of homelessness and begging in the streets of London still aroused a sense of shock.⁶³ Another striking instance of the consequences of Thatcherism was the polarisation of the retail market between an upper tier, modish and expensive but not necessarily high quality offerings and a lower tier, unappealing but surprisingly cheap. There was no longer the layer in between: high-quality goods at affordable prices to those who saw themselves as part of the middle class.

The final preliminary to the book was my critique of Francis Fukuyama, 'The "End of History"?' a chapter in a book on images of world politics after the Cold War. It raised questions about two closely related liberal theories endorsed by Fukuyama: the democratic peace and international regimes. They reflected the perspective of the advantaged Western societies and passed over the utterly different perspective of the disadvantaged majority. It concluded with a critique of the downgrading of historical experience implicit in the 'end of history' image and its consequences in the contemporary intellectual and political culture.⁶⁴

Much work remained before these fragments could be brought together within a coherent structure. The issues raised required reading in multiple literatures and disciplines. My aim, as in the previous books, was to provide a coherent overall account of the major inter-related issues. It would seek to keep up with the state of research but would not advance new

60 'Problematic Paradigm: Liberalism and the Global Order', in Joseph A. Camilleri, Anthony P. Jarvis, and Albert J. Paolini, eds, *The State in Transition: Reimagining Political Space*, Boulder, CO: Lynne Rienner, 1995.

61 'Contending Liberalisms: Past and Present', *European Journal of International Relations*, 3(1) 1997: 5–33.

62 'Economics: Hegemonic Discourse', *Quadrant*, 41(3) 1997: 52–60.

63 'The New English Sickness: Beyond the Point of No Return?' *Quadrant*, 39(7–8) 1995: 16–20.

64 'The "End of History"?' in Greg Fry and Jacinta O'Hagan, eds, *Contending Images of World Politics*, Basingstoke: Macmillan, 2000, pp. 21–32.

specialised findings – in some sense a synthesis, unified by a certain perspective, viz., the contending strands within liberal thought – not novel but unfamiliar in this particular context.

The first of the five main chapters, on the history of liberal thought, develops the central theme of the conflict between elitist and egalitarian strands within liberalism. This is most evident in the period from the seventeenth to the nineteenth century, when the traditional ruling strata could limit representation to the propertied, those ‘with a stake in the country’, for long resisting the claims of democracy and universal franchise. It was less evident, but clear to critical analysts, that in the domain of political economy, the laissez-faire doctrine in the mid-nineteenth century favoured those with capital over those without, until in the later nineteenth century a ‘new’, social-liberal approach proclaimed the rights of all citizens to adequate subsistence and education, entailing a greater role for the state than ‘classical’ liberalism had accepted. Similar issues are raised in the contemporary debates between neoliberalism and its critics, although an alternative to the former has not been so clearly defined as by the social liberals.

The history of international liberal theory (the next chapter) does not reproduce this theme, although it re-emerges at the end. Traditionally liberalism rejected realism and the world of power politics. For most early liberals its evils could be remedied only when states became liberal republics, maintaining peace and observing international law. Traditional liberal theory was discredited by the collapse of the League of Nations, leading to the ‘hegemony’ of realist thought thereafter. But by the 1970s, changes in the international system led to a revival of liberal theory, now for the most part incorporating certain realist assumptions. This departed from the tradition by being primarily empirical, not normative, and was divided among several different strands. It is suggested, however, that this family of theories, mostly American, share a common perspective, a conservatism that corresponds to the stance of the predominant power. Normative international theory, developed separately by political philosophers, has little impact in the American public discourse. By failing to address the issues raised by neoliberalism and its critics, liberal international theory tacitly endorses its role in shaping the emerging international order.

The third of these chapters, ‘Neoliberalism in Practice’, sets out the central policy analysis and argument. Many of the criticisms were not new, although they had thus far had little influence. Since then they have been reinforced by the authority of economists such as Joseph Stiglitz and there has been some modification of the approach of the international financial institutions, while neoliberal elites and their political supporters remain committed to their rigid ideology. Thus in some respects this chapter’s critique has been overtaken by events, but not entirely so, and some of its themes remain distinctive. These include the cultural side of the ‘assault on public institutions’: beyond the attempt to minimise the role of the state to remodel public institutions along the lines of the private sector. The chapter makes clear that the neglect of economic and social rights leaves many deprived of the basic essentials for a tolerable existence. It concludes by emphasising the narrowing of horizons: at a time of unprecedented wealth there was nothing of the optimism and sense of limitless opportunities in earlier periods of liberal ascendancy, but a climate of austerity, a preoccupation with financial constraints, and a perception of individual economic insecurity.

The next chapter discusses forces sustaining neoliberalism. First, it takes up the ‘neo-Gramscian’ concept of a transnational business and financial coalition supplanting the previous welfare-oriented coalition as the dominant influence on the policies of the Western governments, noting that other strata also benefit from the neoliberal order and referring to cultural measures to reinforce it and to promote its legitimacy. Second, it endorses familiar notions of the multiple dimensions in which US power outranks that of any potential rival, the military dimension playing a vital role in the background but the economic and cultural dimensions being more relevant to the construction of a neoliberal order –attuned to the specifically American liberal tradition. Third, and less familiar, was the way in which apologists for neoliberalism exploited the economics discipline to claim the authority of that discipline to legitimise their ideology – a distortion which at that time other economists had

not yet succeeded in countering, perhaps because the increasing prominence of economic and financial values in the contemporary political culture was not uncongenial to them. The whole analysis would now require updating but I would defend its basic theses.

The book might have had more impact if the final chapter, on the search for alternatives, had come up with a new perspective, but this has proved to be the most difficult question of all. It offers suggestions that seek to revive the social-liberal framework. With respect to Western societies I saw the concept of the 'third way' as intellectually attractive but in practice too close to neoliberalism, as the passage of time appears to have endorsed. With respect to the 'Third World' and the issues of poverty and development, I saw the approach of the United Nations Development Programme (UNDP) and the ideas of Amartya Sen as offering a genuine alternative to neoliberalism and proposed a new source of funding an upgraded UNDP to enable it to compete with the established institutions, which suffered the same fate as many other suggestions in this general area: to be quietly set aside.

The Conclusion stood back from the argument and raised the question of its implications for liberal international theory more generally. The argument had illustrated the inter-connecting of the empirical and the normative which was typical of the radical strand of liberalism and which would follow from making explicit the normative premises of the existing empirical theories. As it was, their normative assumptions remained implicit; they assumed that the liberal order which they envisaged would be morally good, but did not inquire into the normative character of the order actually coming into being. Engaging with these normative issues could also help to overcome the separateness of the present strands of empirical theory, suggesting how they each contribute to realising a more adequate liberal vision, and where they fall short.

These issues were developed more fully in the following year in 'Critical Liberalism in International Relations', a concept implying that liberal values and their practical consequences needed to be constantly rethought in the light of new international challenges. Longstanding assumptions could not be taken for granted. Liberal theory was not seen as a complete paradigm for international relations, but its emphases on systemic change and on agency provided a corrective to more deterministic structural theories. The shortcomings of current empirical theories, due largely to their uncritical normative assumptions, were spelled out more fully, noting the neglect of the issue of justice and the failure to address the issues raised by the role of neoliberal ideology in the formation of a supposedly liberal order. The concluding section suggested some ways in which this conception would extend the current liberal research agenda, for example with respect to 'human security' and by bringing the question of justice into the theorising on institutions and on commercial liberalism.⁶⁵

In the same year I presented a paper at the ISA conference on 'Neoclassical Economics and the Neoliberal Order', on the question of whether, as the attempts to claim the authority of economics for neoliberal policies suggested, the contemporary discipline actually endorsed these policies – which not a few economists denied. There was no standard definition, but neoclassical economics was usually taken to be a body of theories on the determinants of prices and the working of the market, universalist theories which claimed to present the essential principles, abstracting from local and historical variations: thus the foundation for economic analysis. The actors in this system were individuals – persons or firms – and society, or the economy, were essentially the outcome of their interaction. Viewed as a positive science, the discipline could not imply any particular policies. However, I argued, there was an affinity between the discipline, thus understood, and the neoliberal ideology: a shared individualist and universalist outlook, which could make for certain normative presuppositions – on the virtues of unconstrained choices in the market and the welfare that this would entail. The rejection of state initiative or regulation ('intervention') – the doctrine of the Chicago School – was a natural consequence.

65 'Critical Liberalism in International Relations', IR Working Paper 2002/7, Canberra: Department of International Relations, RSPAS, Australian National University, September 2002.

A further point of congruence is the way in which neoclassical economics reinforces the cultural effects of neoliberalism. While the ascendancy of the latter in public policy directly restricts state support for cultural pursuits, during recent decades, economics has abandoned the broad-based education of economists which saw a need for historical depth and a serious acquaintance with the humanities. These disciplines are now devalued: mathematical and technical expertise are all-important. Even where neoliberal policies are not adopted, financial criteria and ‘managerial’ approaches narrow down the awareness of cultural possibilities. In many spheres of life, quantification replaces judgement as the mode of decision-making. Economics is not the only source of these changes – the prestige of the natural sciences and bureaucratic convenience enter in – but these developments are congenial to neoclassical economics in its current phase – just as they would have horrified John Maynard Keynes and others of his generation.

The next paper, ‘American Hegemony: A Dangerous Aspiration’, presented at the 2004 ISA conference, was very much a product of its times, an intervention in the debate prompted by US President George W. Bush’s foreign policy. A few years later, with President Barack Obama’s insistence on the limits of American power in the context of a changed climate of opinion in the US, the issue had disappeared. The paper defined a hegemonic power as one that plays a leading role in shaping a certain international order, exercising its dominant power in such a way as to win broad acceptance and legitimacy. Its thesis was that many of the disputed issues could be clarified by distinguishing between the main domains of power: military, economic, ideological, and political.

The US came closest to hegemony in the international economic domain, but to a greater or lesser extent in each domain its hegemony was partial and qualified. There was no overall hegemony: US leadership did not enjoy this kind of acceptance. Apart from encouraging rash military interventions, the danger in American hegemonic aspirations and the habitual deference of the Western governments to American leadership lies in the way in which American thinking on international order is shaped by an ideology in important ways unsuited to the challenges of the twenty-first century. There is a need for other states to engage more actively and independently in the construction of a viable international order.⁶⁶

My last publications, at this point, were two book chapters on liberalism. The first, in a new Australian text, presented the challenge of deciding what were the essentials of liberalism that must be communicated to students at this introductory level. A historical treatment seemed unavoidable, first of the essential principles of liberalism, then the traditional liberal approach to foreign policy. Contemporary theories were outlined briefly and shown to be related to familiar themes in the everyday political discourse on democracy, for example, and on interdependence and international institutions. Similarly, I sought to indicate the kinds of philosophical issues that were raised by familiar normative debates, for example on human rights, intervention, or relations with other peoples. My theme of contending views within liberalism was present but not over-emphasised.⁶⁷

The second was a chapter on ‘The Ethics of Neoliberal Institutionalism’ in *The Oxford Handbook of International Relations*. This strand of contemporary liberalism was an empirical theory of the way institutions could promote international cooperation, which accepted certain realist assumptions but saw far greater scope for cooperation than realism allowed. It made no explicit normative claims but necessarily, in my view, made certain normative assumptions and implicit normative claims. The task was to tease out what these were. I first noted how the presence of evaluative terms, for example, peace, order, and co-operation itself, conveying positive connotations, established a general value orientation. More importantly, I argued that the conceptual/theoretical framework promoted certain

66 ‘American Hegemony: A Dangerous Aspiration’, IR Working Paper 2006/2, Canberra: Department of International Relations, RSPAS, Australian National University, May 2006.

67 ‘Liberalism’, in Richard Devetak, Anthony Burke, and Jim George, eds, *An Introduction to International Relations*, 2nd edn, Melbourne: Cambridge University Press, 2012, pp. 48–61.

values while others were excluded, explicitly or through silence. For example, the theory's closeness to economics brought out values such as 'efficiency', while references to the Pareto concept of welfare excluded issues of redistribution. The radical strand in liberal theorising was entirely absent.

I went on to note how Robert Keohane, the most prominent theorist of the School, addressed normative issues explicitly in his more recent writings, taking a particular interest in the problem of democratic accountability in global institutions. Such concerns are essentially Western – an impression which is reinforced when other prominent Institutionalists are also considered. I had briefly referred earlier to the views of John Gerard Ruggie and G. John Ikenberry, presenting more historically grounded analyses of contemporary institutional developments, more explicitly normative but more US-oriented. I concluded that it should be acknowledged that all current liberal institutionalist thought expresses the perspective of the established powers of the day, and finally expressed the faint hope that the trend to greater normative explicitness could prompt scholarly debate across social and cultural barriers.⁶⁸

Before writing these chapters I had completed a paper, 'Liberalism in International Relations: Redrawing the Theoretical Map' (2005), which I submitted unsuccessfully to two of the major American journals in the hope of bringing my view of liberalism to the attention of the American international relations community. This was an ambitious attempt to spell out what would be involved in bringing empirical and normative theory into a single framework. The first section went over familiar ground, the strength of empirical theory in explaining the changes which were bringing the international system an increasingly liberal character, but the divorce from normative theory leading to its failure to address the ways in which it fell short of realising Liberal values. The second section developed this critique in relation to the major schools of empirical theory. The third section, proceeding from differences over the definition of liberalism, outlined two major 'axes of tension' in contemporary normative theory, with implications for empirical theory: the divide between neoliberalism and social liberalism in international political economy, and a division based on John Gray's *Two Faces of Liberalism*,⁶⁹ between a prescriptive and a *modus vivendi* approach, the former based on a unitary conception of liberal values and imperatives, the second on a pluralistic and partially conflicting view of liberal values.

Returning to the paper ten years later, I think that thus far the argument is clearly formulated and should be accessible to readers in the discipline, but the final section, on redrawing the map, may be covering too much unfamiliar ground too quickly, and moreover setting out an agenda for inquiry, no longer stating a thesis. This may be the underlying reason for the negative reader reports to the journals, although they do not put it this way. I found them disappointing, not really addressing what the article was attempting to do. One of those colleagues with whom I corresponded about the draft article suggested dividing it into two parts, one critical, the other constructive, but I thought it unlikely that a journal would want to consider a two-part article.

The final paper remained unpublished. It took the form of a review article of Andrew Hurrell's book, *On Global Order*,⁷⁰ whose publication I had keenly awaited. It is not easily read or assimilated: although clearly structured, its argument is highly complex, drawing on many specialist literatures. Each chapter presents a richly detailed analysis, the empirical and the normative intertwined. Thus the work, the first English School study of international order since Bull's classic work, is of quite a different character from his.

68 'The Ethics of Neoliberal Institutionalism', in Christian Reus-Smit and Duncan Snidal, eds, *The Oxford Handbook of International Relations*, Oxford: Oxford University Press, 2008, pp. 222–33.

69 John Gray, *Two Faces of Liberalism*, Cambridge: Polity Press, 2000.

70 Andrew Hurrell, *On Global Order: Power, Values, and the Constitution of International Society*, Oxford: Oxford University Press, 2007.

While insisting on the inadequacy of the traditional pluralist view of international order, he sees the tension between the pluralist and solidarist approaches as pinpointing the central normative issues in the contemporary order. His Conclusion takes up the issues of justice and legitimacy that are touched on throughout, not going into the debates on the nature of justice but inquiring into the political prerequisites for any order's acceptance as just and legitimate. The book does not put forward a single novel thesis, but offers many original arguments along the way. A major work, one hopes it receives the careful attention it deserves.

I corresponded with a journal editor, who expressed interest, but in a more thematic article on the development of thought on international order from Bull to Hurrell. I considered this, but my wife, Ursula Vollerthun's terminal illness intervened, and subsequently I turned to another project.

POSTSCRIPT

After my wife's death in March 2011, and after consulting some close colleagues and friends, I decided to edit her thesis⁷¹ for submission to a publisher. She and I had discussed this rather abstractly some time earlier. Whereas she would have to revise the manuscript more substantially, I could limit myself to changes of presentation necessary for turning a thesis into a book. She was not against this, but also not enthusiastic: this was *her* work, and she was apprehensive about changes. I thought nonetheless that it was worth seeking to gain for her the recognition she deserved for her scholarly achievement, and also that the work would make a significant contribution to the discipline.

Initially I sought to limit the editing along the lines suggested, adding an Introduction to draw attention to more recent literature, but I was persuaded by the readers for Cambridge University Press that more was now expected in a serious scholarly work: it should take account of, indeed engage with, the relevant current literature and new perspectives on the topic. I resolved to attempt this, still keeping the original text as far as possible, and found the effort extremely stimulating. I had not previously worked on such an early period, and now read the original texts far more thoroughly as well as examining the recent interpretations. Fortunately I found no reason to question Ursula's interpretation of the four thinkers – the core of her contribution – and indeed could reinforce it in relation to alternative readings. I was also prompted by the Cambridge readers to bring out more clearly the relationships between the successive thinkers, and their distinctive contributions: but I saw this as making explicit what was already implicit in the original manuscript: an extension of the author's text, not a new perspective.

The book was published in August 2017.⁷² Its contents are summed up by the publisher as follows:

This book offers the first comprehensive account and reappraisal of the formative phase of what is often termed the 'Grotian tradition' in international relations theory: the view that sovereign states are not free to act at will, but are akin to members of a society, bound by its norms. It examines the period from the later fifteenth to the mid-seventeenth centuries, focusing on four thinkers – Erasmus, Vitoria, Gentili and Grotius himself – and is structured by the author's concept of international society. Erasmus's views on international relations have been entirely neglected, but underlying his work is a consistent image of international society. The theologian Francisco de Vitoria concerns himself with its normative principles, the lawyer Alberico Gentili – unexpectedly, the central figure in the narrative – with its extensive practical applications. Grotius, however, does not reaffirm the concept, but wavers at crucial points. This book suggests that the Grotian tradition is a misnomer.

71 Ursula Vollerthun, 'The Idea of International Society: A Study of the *Via Media* in the Theory of International Relations, with Special Reference to Erasmus, Vitoria, Gentili and Grotius', PhD thesis, Department of International Relations, RSPacS, The Australian National University, Canberra.

72 Ursula Vollerthun, *The Idea of International Society: Erasmus, Vitoria, Gentili and Grotius*, edited by James L. Richardson, Cambridge: Cambridge University Press, 2017.

In conclusion, I cannot over-emphasise the contribution of the publisher's readers to the final product through their searching questions and their constructive criticisms. Likewise my gratitude to the Department of International Relations, Coral Bell School of Asia Pacific Affairs at the ANU for its continuing support of my work after my retirement, and especially Mary-Louise Hickey for her much appreciated assistance.