International relations over the decades: A memoir

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Canberra & Hamburg, July 2019

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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).

CHRONOLOGY

1933–50	Childhood in north Queensland, South Australia, and Sydney
1951–54	University of Sydney
1955–56	Year in London
1956–58	Magdalen College, University of Oxford
1958–61	Nuffield College, University of Oxford
1961–63	Center for International Affairs, Harvard University
1963–65	Balliol College, University of Oxford
1965–66	Foreign Office, London
1967–74	Department of Government, University of Sydney
1975–85	Department of Political Science, Australian National University
1986–98	Department of International Relations, Australian National University
1999–	Retirement in Hamburg

INTRODUCTION

When I look back over sixty years of writing and teaching about international relations, what most stands out is the magnitude of the changes – in the world, in the subject and in the academic context – from a world overshadowed by the conflict between the two nuclear-armed 'superpowers' to one divided between the winners and losers of globalisation; from a subject seemingly dominated by a single paradigm, 'realism', to one characterised by a remarkable degree of theoretical pluralism; and from a situation in which, in the UK and Australia, International Relations was taught only in a handful of universities to one where it is an established discipline with its hierarchy of specialist journals.

Over the years I taught and wrote on many aspects of International Relations: the Cold War and detente, arms control, Australian foreign policy, and issues in International Relations theory and in international politics. But here I shall focus on the three projects which were my primary interest during successive phases of my work – the central thread, as it were. My first project, a study of West German rearmament in the 1950s, led me to struggle with the dilemmas posed by nuclear weapons, which were bringing about an entirely new form of strategic studies. Second, during the long years of the Cold War, I worked on a major topic which was insufficiently understood: international crises. Here, it seemed to me, a historical approach had much to contribute. In the third and final phase I took a new direction, entering into the debate on the nature of the international system, or order, after the end of the Cold War; here I examined the attempt by the United States and its allies to construct a certain kind of liberal order.

COMING TO INTERNATIONAL RELATIONS

My choice of International Relations was not prompted by any single event nor by a course of study, but developed gradually over a long period. I began to take an interest in world events in the later years of the Second World War, especially in the daily maps showing the advancing allied forces. In the following years the propaganda battles in the United Nations and the Berlin blockade left the strongest impressions. There was nothing in my family background or school to lead me to question the standard perception of communist aggression. Along with the memory of Hiroshima there was the sense that nuclear weapons brought new perils into any war. But the dangers seemed remote, even after the outbreak of the Korean war, and in spite of the alarmist rhetoric in certain circles, I had no sense that Australia itself was under threat.

My student years at the University of Sydney (1951–54) led me to question the conventional wisdom on all topics, including the Cold War, but such was the conservative environment that it felt faintly subversive to read heretical opinions in the *New Statesman* and *Nation*. More seriously, I became aware of my lack of background knowledge to form my own judgement on contested international issues. But this was not a major preoccupation; my main concern was with my courses in history and philosophy. The course in international relations in the inter-war period was only one of those that aroused my special interest; so did those on British rule in India and the rise of Indian nationalism, and in historiography. There was very little graduate study in Australia at that time; my friends and teachers all looked to the University of Oxford, and thus when awarded a scholarship I applied there and was admitted to Magdalen College to read PPE (Philosophy, Politics and Economics), a second BA degree, quite normal at that time.

During a year in London (1955–56) before proceeding to Oxford, I attended some classes in International Relations at the London School of Economics (LSE). Here world events seemed much closer. The first summit meeting between the Western and Soviet leaders, in Geneva in June 1955, achieved no breakthrough but nonetheless reduced the fear of nuclear war: the powers could again communicate with one another. But the violent suppression of the Hungarian revolution in the following year brought back the Cold War with a vengeance; my hitherto taken-for-granted support for the Western cause firmed up into a more conscious commitment.

Oxford offered opportunities for gaining insight into what was behind the events of the day. With a small group of students I listened to Wolfgang Leonhard, a former member of the East German government, describing his experiences, very openly and without the bitterness more typical of ex-communist exiles. At another level, a lecture series by George Kennan, the author of the containment doctrine, proposing a radical alternative to the existing NATO strategy – disengagement – prompted intensive debates. But perhaps the most significant development inclining me to seeing international relations as my primary interest was writing a prize essay in the summer of 1957 on the question why the East-West negotiations on nuclear disarmament remained deadlocked. My essay was commended but not awarded the prize, perhaps because it was limited to an analysis of the negotiations but lacked a theoretical framework. Nonetheless, it convinced me that the security concerns of the powers ruled out any radical disarmament, which became a major premise of my thinking.

Formal courses in International Relations, however, played relatively little part in my decisions. In Oxford, as in Sydney, the course was on the inter-war period, though with different emphases: in Sydney, on Anglo-German relations with special attention to primary sources, in Oxford on wide reading on the different perspectives of the various actors. This course came late in the degree and served to confirm my opting for International Relations. Not surprisingly, I took this to mean recent international history; my brief experience at the LSE had not given me a sense of the theory-oriented conception of the subject, as taught in the US. It is evident that my preparation for graduate study or a career in International Relations was remarkably incomplete, but how could it be otherwise at a time when the subject was scarcely known in British or Australian universities?

This is perhaps the point at which to refer to my relationship with Hedley Bull, which played its part, albeit indirectly, in my coming to International Relations. We shared a common intellectual background in history and philosophy in Sydney and a commitment to a particular view of scholarly values derived from what the Sydney philosopher John Anderson termed the 'life of inquiry'. Hedley had been two years ahead of me in Sydney and, after completing the new BLitt. in Politics in Oxford, had been appointed to a lectureship in International Relations at the LSE without any prior study of the subject. Becoming acquainted for the first time in London, we exchanged impressions of England, events and personalities; later International Relations loomed larger, but always informally, impressionistically. There was no mention of the problems I was to experience with the PhD, and very little of his writing *The Control of the Arms Race,* published in 1961 and soon to become a standard work. Conversation with Hedley was exhilarating: there were surprising observations, ironic asides, and one came away with the sense that the world looked a little different, its contours a little sharper. I looked up to him, but did not regard him as a mentor. It was rather a case of travelling along the same road – reassuring although he was far ahead.

ABORTIVE PHD AND FIRST BOOK

In 1958 I was awarded a two-year studentship (1958–60) at Nuffield College, Oxford, which specialised in research on economics and politics and was open to the still novel idea of graduate research on controversial contemporary issues. After failing to find a promising topic in the inter-war period, for which I had more adequate background, I opted for the topic West German rearmament, from the German perspective. I was attracted to work on Germany by the many questions raised in my earlier courses. But as the work proceeded I became increasingly preoccupied with the strategic context, in particular the role of nuclear weapons which, it was reported, were likely to kill many millions in the first days of any major war. Unfortunately the College had no supervisor in International Relations and it gradually became clear that my external supervisor had no more idea than I how to organise the work for such a PhD thesis. Thus after assembling copious source materials in Bonn and narrowing the topic down to foreign policy and strategic aspects – setting aside the issues raised by incorporating the Bundeswehr into the still new and untried democratic state – I could envisage a narrative of the German debates and policy decisions, but I could not see how to combine this with an analysis of the strategic issues, which were now being discussed in the US in a conceptual language entirely unfamiliar to European politicians.

Time passed, and I could find no other potential supervisor at Oxford. The College allowed me the use of a study after the expiry of my Studentship and I began a draft on the strategic issues, though still unclear about the structure of the whole. At this point I benefited from a rare coincidence. One of my student contemporaries knew Thomas Schelling, at the Harvard Center for International Affairs, the most original of the American strategic theorists, and intervened on my behalf. I sent my existing draft and was offered a one-year research fellowship, subsequently extended to two (1961–63), on the understanding that the Center would assist the publication of my work if they found it justified, but had no interest at all in the PhD.

This was a unique opportunity. Contemporary Strategic Studies was still in its formative phase, the implications of nuclear weapons, the requirements for deterrence and the concept of arms control were still being thrashed out by policy-oriented theorists in a handful of universities and research institutes. The Center was both a home for intensive research and debate and a meeting place for leading figures in this small community of scholars and policymakers. One worked under intense pressure but in a relatively relaxed atmosphere, not least thanks to Schelling's unobtrusive intellectual leadership: a word of encouragement, a critical question, and frequent (but not too frequent) informal lunches. The small group of researchers working on strategic issues reviewed one another's drafts and participated in high-level weekly seminars in the Center as well as the fortnightly seminars of the Harvard–MIT Joint Seminar on Arms Control, which brought together a multi-disciplinary group from both campuses.

The Cuban missile crisis as well as the protracted Berlin crisis underlined the urgency of the issues we were addressing. Some Harvard academics were involved in policymaking, which was to become highly controversial during the Vietnam war; at the time, however, one could benefit from this inside knowledge without feeling compromised by closeness to Washington. Some colleagues more critical of US policy were uncomfortable in this atmosphere, but having independently arrived at a policy orientation within the parameters of the accepted public debate, I did not feel constrained by the prevailing climate of opinion.

In this context I resolved the problem of relating the narrative of the politics of German rearmament and the analysis of the strategic issues by expanding the scope of the study, now titled *Germany and the Atlantic Alliance*. It addressed the main issues in West Germany's relations with its NATO allies at that time. I retained the narrative of rearmament in German politics, included an assessment of the Soviet challenge, followed by a lengthy discussion of the NATO strategic issues, adding a section on the Berlin crisis and concluding with a section on the diplomatic tensions in German-allied relations due to latent differences e.g., on the issue of German reunification.

Two recurring themes were, firstly, the tension between the intellectual worlds of the strategists and the politicians and secondly the tension between the West German and the Anglo-American perspectives on contested issues, especially under the John F. Kennedy administration. I did not refrain from policy recommendations: for example, I supported the strategy of strengthening NATO's conventional forces such that, in an acute crisis, Western leaders would not be faced with the choice between the immediate recourse to nuclear weapons or capitulation. My main aim was not policy recommendation, but rather to seek to clarify the issues that often remained opaque in the public debate. It may have been this general approach, and in particular its attempt to identify characteristic German and American perspectives, which led to the work's being translated into German with the support of the *Deutsche Gesellschaft für Auswärtige Politik* (German Foreign Policy Association).

Working under intense time pressure, I completed a draft manuscript by the end of my second year. This being my first book, I was surprised by how much time would be required for minor revisions and tidying up to prepare it for publication. The Center's editor could not find a commercial publisher for it, so I was greatly relieved when the Center decided to recommend it to the Harvard University Press.[1]

In this environment I inevitably became aware of the typical American view of the subject as a whole and began to read more widely, albeit selectively; I struggled with critiques of strategic studies, often stemming from psychology. I did not attempt to develop a general theoretical approach, but found that of Stanley Hoffmann congenial. I was persuaded by E. H. Carr's critique of classical liberal theory and by default accepted a realist perspective which, like not a few others, I did not attempt to define more closely. Of more immediate interest was to decide on my next project. I was looking to an academic career but did not feel ready to apply for a lectureship. I had acquired a specialist knowledge scarcely available in England and was appointed to a research fellowship in peace studies at Balliol College, Oxford (1963–65).

THE CRISES PROJECT

International crises had been much discussed since the Cuban missile crisis, although there had been little research on the topic. Policy-oriented discussions, I thought, tended to over-emphasise the presumed lessons of the Cuban crisis; there were concerns over 'escalation' and crises spiraling out of control, and a few social-science studies of crisis behaviour of limited scope. It seemed to me that here was the most challenging set of issues in strategic studies, and that I might be able to make a contribution by bringing in a historical dimension which was downplayed in the existing discussion. I found a kindred spirit in Glenn Snyder, who was already considering a theoretical approach to historical case studies.

1 *Germany and the Atlantic Alliance: The Interaction of Strategy and Politics*, Cambridge, MA: Harvard University Press, 1966; *Deutschland und die NATO: Strategie und Politik im Spannungsfeld zwischen Ost und West*, Cologne and Opladen: Westdeutscher Verlag, 1967.

It took some time in Oxford to orient myself to the topic, but I eventually prepared an extensive working paper, an outline of issues to be considered and an approach, historical sociology, understood as a systematic, comparative approach to historical cases. It was first necessary to counter the sceptical view that crises were too idiosyncratic, too embedded in distinct historical processes, to lend themselves to such an approach, but I argued that they could be likened to other political topics – wars, alliances, empires and the like. In contrast to certain approaches such as content analysis, which focused on one dimension of crises – formal diplomatic communications – historical sociology could be multidimensional, seeking to identify the many interacting variables which impinge on the course of events and the outcome of crises.

I identified a number of issues which I took to be important in all crises. For example, is it possible to identify the conditions under which crises lead to war, as against being resolved through diplomacy? Is the outcome predictable in terms of the initial situation, or does it depend essentially on decisions during the crisis? Are decisions typically rational, or do factors such as stress, time pressure, misperception and pressures from domestic politics overwhelm rational judgement? Taking account of all these, is it possible to make suggestions to policymakers seeking to bring crises under control? This was not yet a research project – rather a 'think-piece', a preliminary agenda for a particular approach to crisis studies. The next step would have been a specific proposal, a research design. I was undecided between a general study, seeking to bring together the central aspects of the topic, or a partial study, more narrowly focused on one particular aspect.

However, at this point I was diverted from the project by an unusual opportunity, to join a newly formed group in the Foreign Office, the Arms Control and Disarmament Research Unit, established by the recently elected Labour government under Harold Wilson (1965–66). A particular attraction was that Hedley Bull had been appointed its director. At this point there was no bar to an Australian working in the UK government! In retrospect, although there were many benefits – experience of the inside working of government, and of working on policy issues, and of course working as a colleague of Hedley – this may have been a diversion which cost me dearly in coming to terms with my crises project.

The Unit's function was to prepare studies which would enable Britain to take initiatives on arms control, now a highly specialised area of policy unfamiliar to members of the Foreign Office. Hedley and certain other colleagues worked on 'Minimum Stable Deterrence', in preparation for the later negotiations on strategic arms limitation. My topic was controls over peaceful nuclear programs in the context of nuclear non-proliferation. We were initially viewed with some scepticism in the Foreign Office, but gained acceptance as it became clear that we were not attempting to influence the ongoing negotiations. Hedley was at a level to participate in wider policy discussions, where his contribution was obviously well-regarded; the rest of us stayed with our specific projects. Much time was spent in assimilating the vast amount of information daily flowing across our desks – vital for the regular officials, but for us rather peripheral. After eighteen months I was ready to move on, and was fortunate that there was a vacant lectureship in International Relations in the Department of Government at the University of Sydney, to which I was appointed, starting in January 1967.

THE UNIVERSITY OF SYDNEY, 1967–74

I now had to undertake the long-postponed task of formulating my general view of International Relations, as the opening section of an introductory lecture course – a challenging but rewarding experience. I did not want to present a fixed theory, in the manner of Hans Morgenthau's well-known text, but felt it necessary to offer more than open-ended questions. I indicated why it was difficult to accept the standard liberal view which, however appealing, made unrealistic assumptions about what actually took place, hence the appeal of a hard-headed 'realism'; but, Hoffmann-like, I raised questions about its adequacy, and in this context went on to topics such as war, international law and international organisation. In later years I found Martin Wight's three traditions a more congenial starting point. I have to admit that, influenced by the prevailing intellectual climate in the social sciences, I failed to give due weight to normative issues. The greater part of the course dealt with contemporary world politics, starting with the Cold War and going on to the main regions, on which I had to fill in many gaps in my reading. I sought to encourage the students to read critically – by example, by raising questions and by emphasising differences of interpretation of many issues. The publication of 'revisionist' works on the Cold War at this time provided a striking example of such differences. It seems to me that the basic approach to introductory teaching in the subject has not changed, but now there is greater emphasis on the theoretical framework, given the diversity of theories now in contention.

One question at this time was how much emphasis to give to the Vietnam war – for many of the students the central issue. Of course I included it, but thought it important to make them aware of the range and significance of other issues. I was reticent about taking up a public position on the war, on the ground that I had no special expertise to contribute – some colleagues had been there or had studied it deeply, as I had not. I took a more active part in the subsequent debate on the Nuclear Non-Proliferation Treaty, where I had unusual background knowledge.

I also taught a second-year course in French and (West) German politics, and an honours seminar on international conflict. The honours seminars were always a pleasure: small classes permitting real discussion, the students highly motivated. At this time there was tutorial teaching only in the first year, and thus there was very little contact with students in large lecture courses. In addition to teaching, since I was returning to Australia after so many years abroad, there was the matter of 'tuning in' again to the local scene, and meeting expectations that one would contribute, e.g. by occasional adult education lectures and contributing to local journals and edited books. This was a happy period for me: I had good colleagues, I was fully occupied but there was no intense time pressure.

However, the time left for my long-term research, the crises project, was very limited. New literature was appearing, none of it close to my own approach. I began to draft outlines for a research design, firming up a set of questions to be addressed. I had been pondering over the inter-relationship of the various aspects of crises, and had tacitly abandoned the option of focusing on a single aspect. Initially I envisaged including a large number of crises, but was persuaded, inter alia by comments from Hedley, that it would be more practicable to choose a small number of cases for study in depth. By the time of embarking on study leave in 1973 I had formulated the questions to be examined in the case studies, and in chapters based on them, but had not yet selected the cases. These questions remained, to provide the structure of the eventual book. I could not have imagined that this would take more than twenty years to complete.

CRISIS DIPLOMACY, FIRST PHASE

During the study leave I worked entirely on the project, in the first half-year, at the Center for Foreign Policy Research, Washington. I read extensively on nineteenth-century diplomatic history and selected the crises for the case studies, divided evenly between those ending in war and those ending peacefully, at least in the short run. I also wrote a paper on what I saw as the most significant works in the rapidly expanding literature: a study of the July 1914 crisis by Ole Holsti, summing up a major project at Stanford, and a collection of prominent contributions edited by Charles Hermann.[2] These were mainly behavioural studies or content analyses of perceptions of hostility or indications of stress. The only exception, addressing the strategic dimension of crises, was Snyder's chapter on crisis bargaining. I was surprised by the lack of political studies or those seeking to inter-relate the many variables that surely were at play. A journal to which I submitted this paper wanted a more exhaustive review of the literature, but I preferred to press on with my project. Thus I missed an apportunity to make my approach known to the discipline.

In London in the second half-year, working largely in the British Museum, I drafted the first two case studies, on crises with many similarities but opposite outcomes: the Eastern crisis (1839–41) and the Crimean war crisis (1853–54). These followed the plan determined earlier: the setting, the parties' values, interests and objectives, their perception (or misperception) of the situation, the bargaining process, the role of internal politics and finally the determinants of the outcome. Bargaining, a key concept, was understood broadly to include negotiation, threats and offers, but also manoeuvring for position, military deployments, and the like. My focus was not on the detailed reconstruction of the bargaining process but on the question, to what extent the outcome depended on the actors' bargaining strategies. I felt confident that I had made a good start on the project, but concerned over the time it might take: it would be difficult while teaching to undertake the intensive work which was required.

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

THE AUSTRALIAN NATIONAL UNIVERSITY, 1975–98

This problem was complicated by my move to Canberra, where I had been appointed head of the Department of Political Science in the Faculty of Arts in 1975. This was a time of transition in the universities: the older heads of department, appointed for life, were gradually being replaced by limited-term headships. I was appointed for five years, extended to seven. In retrospect, I think that I coped quite well with the multiple demands of the headship of what was regarded as a 'difficult' Department, but the last two years were strenuous and the Department was ready for a change. The pattern of my teaching was now different. The basic International Relations course was in good hands; I taught courses in Australian and in US foreign policy, and an honours seminar on Third World issues. In the later years I introduced a new honours course, Modern Political Analysis, to meet several needs: to provide something of an overview of the discipline, to introduce some significant topics not covered in the regular courses and, rather in the manner of courses in historiography, to introduce philosophical/methodological issues in the study of politics.

The crises project had to wait until my half-year study leave in Munich in 1978, where I drafted a case study on the Russo-Japanese crisis (1903–04) and a narrative of the Sudeten (Munich) crisis (1938). Meanwhile the literature was expanding rapidly, mostly in the behavioural mode, but two works concerned with politics and strategy, and also based on historical case studies, raised the question whether my work had been pre-empted. In one case, Richard Ned Lebow's highly original work, *Beyond Peace and War,* this was clearly not the case: it was focused on a narrower range of theories and issues. Snyder's study, in collaboration with Paul Diesing, *Conflict among Nations*, was another matter.[3] This remains the most significant theoretical work on crises. The authors construct novel game-theoretic models of bargaining, the central theoretical strand, relate these to psychological analyses of perception and 'information-processing' and place these in a context of systemic theory, drawing on summaries of thirteen cases. But their approach is entirely different from mine. Whereas they erect an elaborate theoretical construct, I seek to evaluate the range of theories that have been employed to explain crisis phenomena. Some of their findings were similar to those I was beginning to reach, others not. I could build on their work, now a major source, but would not be overshadowed by it.

By the mid-1980s I was searching for a research appointment or grant which would give me a realistic chance of completing the work. This was complicated by my having published little since moving to Canberra: I had not wanted to spend scarce research time on new topics, and apart from a conference paper for the International Political Science Association I did not have findings on crises that I was ready to publish. At the end of 1985 I was able to move to the Department of International Relations in the ANU's Research School of Pacific Studies (subsequently Pacific and Asian Studies). There was a difficult appointment process, but fortunately for me, the head of the Department, Bruce Miller, had confidence in my abilities.

CRISIS DIPLOMACY, SECOND PHASE

I could now plan to finish the work in three years, assuming the remaining case studies (the Franco-Prussian crisis 1870, the Agadir crisis 1911, Pearl Habor 1941, the Berlin blockade 1948–49 and the Berlin crisis 1956–62) were limited to summaries, as in Snyder and Diesing. The four completed cases would show how I drew conclusions from the individual cases. The Sudeten (Munich) chapter needed to be rewritten in the light of extensive new literature, the theory chapter and the chapters analysing key aspects of the crises needed to be drafted. Before this I needed to read extensively on the five remaining cases. The analytical chapters set out the study's findings on the state of the international system at the time of the crisis: the actors' goals, selective perception and misperception, crisis bargaining and impediments to it, the role of internal politics and the determinants of the outcome and the risk of war.

The existing theories of the international system were at too general a level to offer relevant insights, but a historical account of changing power configurations and prevailing norms could provide the essential systemic context of each crisis. The actors' goals were often neglected in the literature, or taken for granted, yet they were crucial in shaping the course of events. I found that for the most part, governments pursued

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

reasonably clear objectives, but the exceptions were important: cases where policymakers were too divided to agree on an objective, or where their perception was too distorted to enable them to formulate a relevant goal. Similarly, crises could often be interpreted in terms of the actors' bargaining strategies, but not in the case of serious internal divisions or gross misperception of the situation.

Perception was necessarily selective; misperception was pervasive but often inconsequential because corrected by events, but it mattered if it was deeply embedded or was present at the time of important decisions. Internal politics impeded rational policymaking if differences among decision-makers could not be resolved or if political instability prevented continuity of government. Thus I could not offer any grand theories about crises. The conclusions took the form of contingent generalisations. For example, there could be no general statement that under certain conditions, crises would lead to war. The initial conditions might render war probable, or the contrary. But the actual outcome would depend on decisions during the crisis. In two of the cases ending in war, an assessment of the initial situation would not have suggested that this was probable. Similarly, while warning against over-confidence in current notions of crisis management, I could not offer novel suggestions for success. Rather, it was a matter of judgement: which of the various maxims recommended to policymakers were best suited to the particular circumstances?

I completed the draft manuscript within the three years and then sought reactions in the US. These were generally positive, but I was advised by a prospective publisher to present full case studies, not just summaries, of the case that involved the US. I decided to write briefer, but still substantial narratives of the remaining five cases, and to add a chapter on the theoretical implications of the study. I had presented critical assessments of the theoretical literature, but something more seemed indicated. These substantial additions, plus 'polishing' the whole manuscript for publication, took me well into 1990, and then came the usual waiting for the publisher's response.

This came as a shock. Although Princeton University Press had expressed interest in including the work in a new series on history and theory, it followed the advice of the reader who rejected it. I did not feel that the critique was justified: the reviewer did not expose weaknesses in the argument, but wanted a different work, one which would rigorously test one or two theories, whereas I was asking which of the many theories were relevant and contributed to explaining the cases. I came to see this reaction as characteristic of the present state of the American discipline – insisting on a particular methodology rather than the demands of the topic – and turned to the UK, where a well-established Cambridge series was more open to varied approaches. The Cambridge readers offered valuable comments, but the Press demanded significant shortening of the manuscript – no doubt for the better, but the least stimulating task imaginable. The book appeared in early 1994.[4]

Was it all worth it – to spend the best part of a working life on a single book? It is the only work of its kind, a systematic historical and theoretical study of crises, with the emphasis on the historical, illustrating an approach, historical sociology, which has been taken up in a variety of other contexts in International Relations. Snyder and Diesing's study remains the outstanding theoretical work on the topic, but in my view does not allow sufficiently for historical contingency. The most massive study of crises, the multi-volume product of Michael Brecher's International Crisis Behavior Project, includes quantitative studies of patterns of crisis behaviour and book-length case studies of individual crises, but has a limited theoretical focus, entirely omitting the strategic bargaining dimension.[5] I see my work as comprehensive enough to be placed alongside these. They could serve as a foundation, if at some time in the future crises among the major powers should again attract attention.

However, the time required was costly. At the outset there was virtually no systematic study of crises. By the end a substantial literature existed and the main positions had been staked out. With the end of the Cold War interest declined dramatically. In retrospect, it might have been better if I had resisted the attractions of the Foreign Office appointment and had pressed on with the project, especially if I had pursued a limited aspect of the topic rather than so comprehensive an approach. This would have brought

4 James L. Richardson, *Crisis Diplomacy: The Great Powers since the Mid-Nineteenth Century*, Cambridge: Cambridge University Press, 1994.

5 For an overview of the findings of the Project, see Michael Brecher, *Crises in World Politics: Theory and Reality*, Oxford: Pergamon Press, 1993.

the historical sociology approach into contention while crisis studies were still developing, instead of when the discipline had largely moved on. At the time, of course, I was not aware of such potential consequences.

MEANWHILE …

Even during the three years of intensive work on the book I was able to do other writing, in particular on closely related topics such as a review article on several volumes of the International Crisis Behavior Project.[6] Subsequently I continued to find topics arising from the book, in particular on problems associated with rationality.

Over the years I had developed an interest in many other issues on which there has been no opportunity to write, thus there were now a number of topics on which I was keen to organise my ideas in written form. I wrote a chapter on the changing discipline and one on normative issues relating to the Third World. In addition, I edited or co-edited three of the Department's publications – on International Relations theory, on the Garnaut Report on Australia and Northeast Asia, and on the nature of the international system that was taking shape after the end of the Cold War. In the last few years, before my retirement at the end of 1998, I wrote on an issue that was preoccupying some of my colleagues, the challenges to security in the Asia-Pacific region.

The paper which I most enjoyed writing was a chapter on Hedley Bull's view of the academic study of International Relations.[7] His early death in 1985 had greatly saddened all who knew him. Although he had achieved so much, there was a sense of incompleteness. I sought to draw together his many comments on the topic: he warned against a narrow conception of the discipline, insisting on a thorough grounding in history, on distancing oneself from the preoccupations of the moment and questioning prevailing assumptions, on the dangers of a prior commitment to the perspective of government or of any political movement, on the need for reasoned argument over values, his criteria for evaluating scholarly work, and his ambivalence towards the contemporary social sciences.

After Bruce Miller's retirement at the end of 1988 I served as head of department for two and a half years. This was of course much less onerous than in the case of a large teaching department such as Political Science, but nonetheless it came at the beginning of a stressful period. Resources were beginning to be cut back, the 'managerial' style of university administration was coming in. There was tension between departments over funding and the ensuing disputes over priorities. International Relations was in an exposed position. At the outset of his headship, Bruce had been assured that, although the Department was in a regional School, its mandate would include the global and theoretical. Now there were persistent demands that we be confined to the region. I was able to resist these, but could not obtain a reaffirmation of our original mandate, and the continuing uncertainty was bad for morale.

My experience with teaching, however, now at the graduate level, was entirely positive. I taught the History course in the MA Program, many of the students being from East Asia. I sometimes regret that I did not develop a more original interpretation of the subject/matter, but, given that most of the students had little background knowledge, it was necessary to cover very basic material, hopefully stimulating interest in reading further. I now had more PhD students than previously, both a pleasure and an intellectual challenge, especially in the case of the few who chose theoretical topics and were committed to one or other of the new approaches that came to the fore in the discipline in the 1980s, several of whom wrote outstanding theses. I had not previously concerned myself with post-modernism (also termed post-structuralism), and was impressed by its critique of the standard literature for its disregard of the underlying philosophical issues, even though I resisted many of its claims. I was essentially a friendly critic of the students' drafts, whereas some of my colleagues were totally antagonistic to this style of theorising and its seemingly obscure jargon. For a time I even considered for my next project to attempt an assessment of this 'new' theorising (new in contemporary International Relations, but not unknown elsewhere), but decided it was preferable to work out my own point of view on a substantial topic rather than commenting on the existing literature.

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

7 James L. Richardson, '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: Oxford University Press, 1990, pp. 140–85.

A POST COLD WAR INTERNATIONAL ORDER?

Such a topic had begun to take shape as I was finishing the crises manuscript. After the end of the Cold War it was realised that the superpower rivalry had, rather paradoxically, provided a framework and even a certain kind of order within which international conflicts had been in some sense contained. There was much speculation over what kind of international system, or hopefully order, might take its place. A return to power politics as usual, as some realists proclaimed? Or would the spread of democracy usher in a 'democratic peace', as some liberals dared to hope? Two competing visions captured the popular imagination: Francis Fukuyama's 'End of History' and Samuel Huntington's 'Clash of Civilizations'. We took up these issues in the Department in informal discussions, seminars and conferences. I found the realist thesis unconvincing: war had always been the final arbiter in serious international conflicts, but was this credible when the major states were nuclear armed? Thanks to economic interdependence, the increasing role of international institutions and the spread of democracy, the liberal alternative had become more plausible, but the new democratic institutions often appeared insecure; for many reasons the thesis of the democratic peace was open to question. Thus the liberal vision had its problems, especially in the case of Fukuyama's notion of the triumph of liberal democracy. I wrote papers outlining the questions to be addressed, a critique of the realist thesis, on liberalism as a problematic paradigm, on American preponderance in the new international system, and co-edited with Richard Leaver a book which the publisher optimistically titled *Charting the Post-Cold War Order*.[8]

I then saw that these might form the nucleus of a book, not indeed on the whole gamut of issues, but focusing on the role of a certain kind of liberal theory, which came to be termed neoliberalism, in shaping the current political-economic order. This took me into new literature, in several directions. I had had no more than a passing acquaintance with international political economy, which had developed as a major strand in the discipline since the 1970s. And I wanted to give a historical grounding to my premise that there were competing doctrines within liberalism, in contrast to the assumption that the current American version was liberalism per se. I wanted to complete the work before my retirement in December 1998, but there was no sense of urgency, and I was also writing on other topics, in particular regional security. Such diverse material was difficult to bring under control, so I took a step back and prepared a detailed summary of the argument as a whole and, now working under time pressure, completed the draft by the end of 1998.

The draft had five chapters. The first two developed the theme of contending strands within liberal thought: initially, the liberalism of the propertied elites versus the dissident voices calling for majority rule; later, the laissez-faire orthodoxy challenged by the 'new' social liberalism of the late nineteenth century. The division was not so clear-cut in liberal international thought, but the liberalism of the powerful could be seen to lend legitimacy to the existing order, and to discourage claims for justice in the name of liberal values such as equal rights. All this presupposed a conception of liberalism such as that of Isaiah Berlin, in which there were tensions among liberal values which could pose moral and political dilemmas.

In this context the Western powers, led by the US, were seen as seeking to realise a certain vision of the liberal-democratic order, primarily an economic order, characterised by the free movement of capital as well as free trade, deregulation, privatisation and minimising state 'intervention' in the economy. This, it was maintained, would maximise human well-being. Other liberal ideals such as the promotion of democracy and human rights were pursued much more circumspectly.

The third chapter examined the practical consequences of this ideology – Western societies experienced increasing social inequality and a narrowing of opportunities for the disadvantaged. The consequences were more far-reaching in the developing world, in particular for countries dependent on international assistance. The imposition of a single model by the international financial institutions, excluding the state from development policy, made for mixed outcomes: a few successes but many falling back. I was especially critical of the inflexibility of these institutions in the face of adverse experience, which was to be modified, but only up to a point, in the following decade.

The fourth chapter inquired into the forces sustaining the paradigm. I followed Robert Cox in pointing to a ruling coalition of corporations and financial institutions – internationally oriented, capital – which had gained the upper hand over internally oriented business, more prepared to reach compromises with other social groups. Secondly, I saw the US as having greater scope than in the Cold War to impose its model

8 Richard Leaver and James L. Richardson, eds, *Charting the Post-Cold War Order*, Boulder, CO: Westview, 1993.

worldwide – elevating the private sector and diminishing the role of the state. Thirdly, I suggested that the immediate source of the ideology was to be found in the economics discipline – or rather, in the way it could be readily distorted and its authority claimed for an over-simplified free-market ideology, which was reinforced by the new hegemony of economic terminology in the public policy discourse.

The final chapter turned to the search for alternatives. It was not difficult to outline an alternative ideological orientation: an updating of social liberalism under the heading inclusive liberalism, just as neoliberalism was essentially an updating of laissez-faire. The difficulty was to show how this might be realised in practice. I could offer some proposals, but was not more successful then other critics of neoliberalism in showing how these might be rendered politically feasible.

I sent the manuscript to Lynne Rienner Publishers. Again the readers were divided, one very positive, the other unhappy that I had neglected the advantages of neoliberalism. This time the publisher decided to go ahead. I made some revisions and added a short introductory chapter and a conclusion. All this took some time due to our move to Germany after my retirement, and the book was published in 2001.[9] It received favourable comments but attracted less attention than I had hoped. Perhaps the problem was a lack of a strikingly novel thesis. On returning to the book, I think its main deficiency may be that it does not sufficiently develop the theme of international order, which had been its starting point. It is the inadequacy of neoliberalism as a foundation for international order that is its central thesis.

HAMBURG, 1999–

I pursued the theoretical issues in a departmental Working Paper and in an ambitious draft article 'Redrawing the Theoretical Map', which would have brought my view of liberal Internationals Relations theory to an American readership. It was rejected by the two main American journals. I was not persuaded by the readers' reports, which failed to address my central argument. Perhaps I was seeking to compress too much into the limited space available, but nonetheless this experience confirmed my sense of alienation from the American discipline, the feeling that for the most part it was not open to approaches outside its familiar parameters. A more satisfactory outcome, however, was the publication of chapters on liberalism in two contrasting volumes: an Australian introductory text and a reference volume. The former required an introductory overview, the latter a discussion of the ethics of (neo)liberal institutionalism.

These two volumes illustrate very clearly how the discipline had developed since I first encountered it. Both presented multiple theories, old and new: realism, liberalism, Marxism, critical theory, feminism, constructivism, post-modernism and the English School. The first, *An Introduction to International Relations: Australian Perspectives*, went on to a wide-ranging coverage of the old and new agendas: roughly, the issues addressed in the traditional discipline, then international political economy and the new global issues. The second, *The Oxford Handbook of International Relations*, placed the emphasis on theory, with chapters on the normative aspect of each approach, and went on to questions of method and different approaches to research (sociological, psychological and the like). The one gave a good overview of the present state of the discipline, the other an indication of the direction in which it might be heading.[10]

A further late publication is worth noting: a book chapter in which I finally presented a manifesto for historical sociology, which I now contrasted with what I termed the economic mode of analysis. There was a place for both, but I argued against the ambition to elevate game theory, or more generally rational choice theory, to a trans-disciplinary role as the basic model for social science theories. I saw the 'structured complexity' of historical sociology as a more fruitful approach to explaining typical political phenomena. I no longer took crisis decision making to illustrate the argument, but the transformation of the international system, a topic more prominent in current debates.[11]

9 James L. Richardson, *Contending Liberalisms in World Politics: Ideology and Power*, Boulder, CO: Lynne Rienner, 2001.

10 'Liberalism', in Richard Devetak, Anthony Burke and Jim George, eds, *An Introduction to International Relations: Australian Perspectives*, Port Melbourne: Cambridge University Press, 2007, pp. 43–53; 2012, 2nd edn, pp. 48–61; '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.

11 '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: State University of New York Press, 2001, pp. 277–98.

Retirement at the end of 1998 had not diminished my interest in the issues thrown up by international relations, and I was most fortunate that the Department of International Relations continued to support my writing in the years thereafter. Of course, the external circumstances had greatly changed, in particular the lack of contact with colleagues and students, and after the completion of the book there was no pressure of deadlines. My wife, Ursula Vollerethun, had her own intellectual interests, but we both participated in the 2002 and 2004 conferences of the International Studies Association, in New Orleans and in Montreal. After that there was a certain slowing down, until my wife's serious illness in 2010; she died in March 2011.

THE IDEA OF INTERNATIONAL SOCIETY

I had not planned to write a further book, but after my wife's death I soon decided to attempt to publish an edited version of her doctoral thesis. Completed in 1991, it had been highly commended but was deemed too specialised to find a publisher. Later she considered the possibility of revising it to relate it to more current interests, but decided against this: it would have been very time-consuming and publication still uncertain. We agreed that if I were to edit it, this would not require such extensive revision, but this seemed a rather remote possibility, given the earlier lack of interest on the part of the publishers. Now, twenty years later, there was greater interest in the subject matter – the history of international thought. I saw an opportunity to gain for her work the recognition it deserved as a significant contribution to scholarship.

I had married Ursula in 1976; from this point on my work benefited from her critical reading and her positive advice. She had come to Australia in the mid-1960s and had completed a BA degree at the University of New South Wales in 1973. In Canberra she wrote a PhD thesis on 'The Idea of International Society', looking at the early history of what Martin Wight termed the rationalist or Grotian tradition in international thought. Wight saw this as a *via media* between a Machiavellian realism and a universalism which aspired to transcend the world of sovereign states. The thesis addressed a question which had been raised by Hedley Bull: whether these were genuine historical traditions, in the sense that the ideas of important thinkers actually fell within these patterns, or did they just represent an imagined philosophical debate which Wight had invented? The thesis examined three thinkers often identified with the rationalist tradition: Francisco de Vitoria, Alberico Gentili and Hugo Grotius, and also one who seemed a likely candidate: Erasmus. Her main conclusion was that the thought of three authors – Erasmus, Vitoria and Gentili – was indeed in accordance with the patterns of ideas that Wight had assigned to this tradition, but that, surprisingly, Grotius's works were not. Whereas the others presented a tolerably clear image of international society, Grotius's account was marred by numerous unresolved inconsistencies.

Thus the thesis offered a new perspective on the origins of the 'Grotian' tradition. While Grotius himself does not adequately express the outlook of Wight's *via media*, the earlier three thinkers do so unreservedly, each in his different way. Erasmus, whose world is limited to Europe, sees a world of Christian princes subject – although practice often fell short – to basic Christian norms of mutual support and cooperation. He eloquently expresses his abhorrence of war but is not a pacifist: war is sometimes unavoidable, and the prince must defend his people. Erasmus's thinking is not systematic but, when his numerous comments are pieced together, a consistent image of international society takes shape.

Vitoria, the leading Spanish theologian of his day, is concerned with basic principles of human conduct, applied to the relations among peoples. Their rulers are obliged to observe the norms of natural law – derived not from divine command but from God-given reason – and also the law of nations, norms agreed along themselves. He applies these principles to the justification for war and to Spain's relations with the American Indians. He does not examine political practice, but it is evident that it fell far short of his principles.

Gentili, an Italian jurist in Oxford, writing on diplomacy and on the law of war, followed in the natural law tradition, but was more concerned with spelling out its practical application. His major work on the law of war set out a systematic discussion of the just causes of war, the just conduct in war and just conditions in peacemaking; his treatise on embassies outlines the legal regulation of diplomacy. The least eminent of the four thinkers, it is Gentili who offers the most explicit account of international society. Indeed, he seems closer than the others to contemporary thinking on international relations.

Grotius comes as a great disappointment. Although the Prolegomenon to his famous work, *De Jure Belli ac Pacis*, offers some images which seem to suggest a vision of the world as a kind of society, the text as a whole gives no substance to this notion. His concept of natural law has been much discussed, but he seldom explains what it might entail in practice. The text is rambling and marred by inconsistencies.

My initial idea was to preserve as much as possible of the original text, referring to new literature in notes or in an introduction. But I was persuaded by the publisher's readers that more was required. In order to meet the expectations of today's readership the work would have to be oriented much more fully to the present state of scholarship. This amounted to constructing a new framework for the argument: a new introduction and conclusion, with consequential changes. I retained her interpretation of the four thinkers, mainly in her own wording, and reinforced it against recent rival interpretations – of Vitoria and Gentili in particular. All this required a much more thorough reading of the texts than I had undertaken initially, and the whole process involved three years of intensive work. It had taken me into quite unfamiliar terrain, intellectually rewarding but in the end strenuous. In the last months I came to realise how much energy such labour can require. I was satisfied that the work would make a worthwhile contribution to the discipline and do justice to Ursula's scholarly achievement, and was very happy that it was accepted by Cambridge University Press.[12]

With this I see my scholarly work concluded.[13] I have been fortunate to have experienced the modern university in one of its best periods, a time of expansion in which higher education was made freely available to ever increasing numbers of students. This came to be seen as conferring an unfair advantage as against those excluded, but the pendulum has swung too far, burdening the current student generation with a disproportionate level of debt. It was also a time of great intellectual freedom and university autonomy, when academic values could flourish. All this is now restricted by the unholy alliance of government and business and the managerial style of university governance. This would not have surprised John Anderson, for whom the life of inquiry was perpetually engaged in struggle against adverse social forces. It would never enjoy security, but nor could it be extinguished.

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

13 For a comprehensive account of my publications and papers, see James L. Richardson, 'Record of Publications and Other Papers', Canberra: Department of International Relations, Australian National University, 2017, http://ir.bellschool.anu.edu.au/sites/default/files/uploads/2017-10/richardson_record_of_publications_camera_ready_october_2017.pdf.