Lecture 7

The Causes of War

In lecture 1 I sketched an orthodox account of International Relations, its disciplinary history, and its chief concerns. Since then I have been largely occupied in casting doubt on the need for such a discipline, suggesting possible restrictions, questioning what its subject matter might consist in, and challenging the standard disciplinary history. But there is no doubt that IR does in fact exist: that there are numerous scholars who identify with this title, many doing excellent work, and that there have, since the 1960s, been departments, journals, conferences and debates that between them constitute something that has many of the characteristic of an academic discipline, mainly concerned with current events.

In this lecture and the next and final one, I want to run over a variety of approaches to the study of war that have been adopted within that thought-world. Settling on this topic, regarded as central by those who constitute the discipline, allows us to think about methodological eclecticism, because this characteristic feature of IR has been particularly marked with respect to war. Not only inductivism, neo-realism and rationalism, but also *a priori*, normative and post-structuralist approaches can be surveyed, and each in turn subjected to immanent critique, before finally considering their mutual consistency: all this without ever leaving the battlefield. In this spirit, I will begin, this week, by looking briefly at some early suggestions about the causes of war from social and biological scientists before the emergence of professional International Relations and then examine in rather more detail three subtly different theories of the general causes of war from within academic International Relations: inductivist, systemic and rationalist. These, it should be said, merit receive further attention in discussions of the democratic peace debate and balancing.

Historians have generally been interested in the causes of specific wars. Why did war break out in Europe in 1914, or 1870, or 1866, or 1793, and so on. Social scientists have asked why wars happen at all. I will begin with accounts of war advanced by biologists and anthropologists. Some twentieth-century socio-biologists have argued that the deep structure of international politics lies not in the distribution of power in an anarchic states-system, but rather in a process of natural selection operating not upon individuals but upon groups, so that war is to be explained not by individual aggression but by the genetic characteristics of groups or *ethnes*.

E. O. Wilson and others, since the 1970s, have maintained that self-sacrificing behaviour, observed in animal and human populations, can only be explained if it ensures its own perpetuation, that is to say if it is selected. This seems paradoxical. If you lay down your life for another individual, you lose all possibilities of future reproduction and nurture of offspring. But provided your sacrifice is the means of ensuring the survival and reproduction of individuals who are closely related to you, and therefore carry the genes of your ancestors, the effect will be to select, for survival and reproduction, the group to which you belong, with its shared gene-stock, including the 'selfless' gene that led you to act altruistically. This process is referred to as 'inclusive' or 'kinship' selection.[1]

Now it will be objected that this theory might just about account for the cooperation of members of a kinship group in organized hostilities as a functional adaptation so long as fighting takes place

[1] . Edward O. Wilson, *Sociobiology: the new synthesis* (Cambridge MA: Harvard University Press,1975). Wilson has been subjected to much criticism.

between discrete kinship groups; but since kinship groups cannot grow very big without losing their genetic distinctiveness, it would be no more than a theory of the distant origins of warfare. It is hard to maintain the genetic homogeneity and distinctiveness of populations within modern frontiers. A sociobiological theory of war would therefore need to be supplemented with an explanation for the retention and later development of the institution of war beyond the point at which genetic differentiation between warring groups ceased to be significant, and such a supplement would have to come from outside biology. Biological reductionism therefore seems doomed.

Even this modest claim of modern socio-biology about the origins of war is challenged by social anthropologists, who have generally preferred to claim that war, like language, is a cultural phenomenon, as different from aggression as is articulated speech from the trumpeting of an elephant. Alexander Lesser (1902-1982), an anthropologist who studied under Franz Boas, argued that it was inappropriate to speak of hostilities between simple kin groups as war. There might be fighting, surely, and this may have resulted in inclusive selection. Yet war was something different and essentially political. He defined it as a struggle for scarce resources: for territory, women and cattle. War, Lesser argued, arose out of the same circumstances that gave rise to the state, and was indeed conceivable only as a relationship between states. Long-distance trade and permanent settlement of land were the interlinked conditions for the independent development of state and war. Lesser concluded, optimistically, that war was not natural, but could be ‘changed or eliminated, like any other political or cultural institution, by human planning or action (145).[2]

Another student of Boas, Margaret Mead, advanced a different theory of war. Lesser had thought that war was like language: cultural rather than natural, but not invented. Mead thought it more like the wheel: cultural and invented (but not everywhere: there had been no wheels in pre-Columbian America). Mead attacked a particular variant of the security dilemma, which supposed that war was unavoidable in all but the most primitive cultures because the very political institutions needed to supervise long-distance trade and agriculture, while ensuring order within the community, could not but pose a threat to out-groups. Instead, she argued that war was a functional response to specific circumstances and could in

[2] Sidney W. Mintz (ed.) *History, Evolution and the Concept of Culture: Selected Papers by Alexander Lesser* (1974) 145.

principle be got rid of once those circumstances no longer obtained. She found cultures, including cultures with high levels of expression of personal aggression (in the Arctic) that lacked the concept and institution of war, and others (in the Anderman Islands) too primitive to have developed war as a response to social complexity in the manner hypothesized by Lesser, yet that did engage in warfare.

Here is a quite different account of the relationship between natural selection and warfare to that offered by the socio-biologists. War, spontaneously invented, spreads through contact until it is nearly universal. This cultural artefact creates the circumstances in which biological selection can operate at individual level in a culturally biased manner, since war brings prestige, wealth, wives and offspring to those individuals who most excel in battle, while the mobility, disruption and miscegenation occasioned by war act to fragment kinship groups, making it impossible for warfare to act as an instrument of group selection.

Mead concluded that, while inventions cannot be un-invented, some pass out of use because they are superseded by improved ways of performing the same function, opening the door to thoughts about peaceful conflict resolution and, borrowing the title of a celebrated essay by Harvard psychologist William James, what might serve as 'the moral equivalent of war'.[3]

Moving on to explanations within the disciplinary framework of IR, I want to begin with the account that, while general, is closest to that of the stereotypical historian, concerned to work from observation of the facts. This is the inductivist approach. To save confusion let me say straight away that this style of research is better known in IR as behaviouralism, and that its application to war has closely associated with the long-running Correlates of War project, to which I will refer, for ease of expression and with no desire to disparage it, as the COW. I call this method inductivist because that is the standard term used to describe an approach that seeks to identify law-like relations between variables by the accumulation of large sets of observations, which is what the COW project has been doing for the past forty-five years or more.

Founded by J. David Singer in 1963 at the University of Michigan, the COW continues today under Paul Diehl, who became director

[3] . For relevant passages from Mead see Leon Bramsoin and George W. Goethals (eds.) *War: Studies from Psychology, Sociobiology, Anthropology* (Basic Books, 1964) and Charles R. Beitz and Theodore Herman (eds.) *Peace and War* (San Francisco CA: W. H. Freemsan, 1973).

in 2005. It has a web-site which gives a good sense of the project, its objectives and its current foci, and I will draw on this before going on to examine the project's assumptions, drilling down to look briefly at a couple of typical exchanges between scholars conducting normal science within the parameters of the COW and expressing some misgivings about it. Given the limited time you are able to devote to this course, I have assigned just one reading on this approach, the sympathetic critique offered by David Dessler.[4]

The COW website declares the primary objective of the project to be 'the systematic accumulation of scientific knowledge about war'. The project leaders take pride in having 'promoted cumulative science … when the scientific study of politics was in its infancy'. (Note this infancy and maturity metaphor. It is commonly used by self-styled social scientists to account for failure: give us more time; give us more money.) The project description runs on: 'By helping to establish a clear temporal and spatial domain for research, promoting the use of clearly defined concepts and common variable operationalizations, and allowing

[4] . David Dessler, 'Beyond Correlations: Toward a Causal Theory of War' *International Studies Quarterly* 35 (1991) 337-355.

replication of research, the project has been a mainstay of rigorous international relations scholarship.' In practical terms, the COW has developed data sets relating to all armed conflicts between states (and some within) since 1816, and has also systematically collected data about candidate causal variables such as alliances, escalation, arms racing, polarity and regime type.

The COW has slowly but steadily responded to some of the more obvious criticisms of its methods. One issue, to which I shall return, has been coding. What is a war? You can't count wars until you've defined them. This was dealt with early on, and in 1972 Singer and his co-author, Melvin Small published *The Wages of War*, which established a standard definition.[5] I am not alone in entertaining serious misgivings about this definition, but am aware that this is being addressed. The current website notes two current foci of research: dyadic war data and geopolitical units. The second of these addresses the coding or definitional problem by 'tracing the status of non-state territorial units in the system, including colonies, mandates, autonomous territories, and so on,

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[5] . J. David Singer and Melvin Small, *The Wages of War, 1816-1965: a statistical handbook* (New York: Wiley, 1972).

their relationship, and their eventual incorporation in/transformation to states.' Yet this does not address a second problem, which is the extent to which dyadic wars (wars between a pair of states) should be aggregated into general wars. (Think of the number of pairs of states at war with one another in 1944.) We will touch on both of these problems in due course.

The first stage in the COW project was therefore to clarify concepts in order to ensure that all participants are talking about the same things. This required definition of a range of terms: not just 'war' but 'state', 'stability', 'democracy', and so forth.[6] Next came the collection of large numbers of comparable observations and the search for regularities: the correlations of the project's title.[7] Once this process had been completed it became possible to form tentative explanations expressed in the form of models yielding implications or hypotheses – which ought ideally to be counter-intuitive – able to be tested against the available evidence.

The method is self-consciously based on procedures common in some of the natural sciences. Vasquez and Henehan suggest that

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[6] . John A. Vasquez and Marie T. Henehan (eds.) *The Scientific Study of Peace and War: a text reader* (Lanham and Oxford: Lexington, 1992) 10-11, 13, 378-9.

science characteristically starts with a search for law-like generalizations and only then leads on to a process by which these may be 'refined, qualified, and made more precise,' often through mathematical expression.

Only at this point is it possible to move to the fourth and final stage of investigation, which is the formulation of a general theory or explanation or what Vasquez and Henehan (rather oddly and interestingly) call an *interpretation*. It is instructive to read their own interpretation of the material collected in their text-reader, which brings together thoughts about alliances, arms racing, escalation, contiguity, and crisis management.

> War between relative equals – they suggest – seems to result from a series of steps, each associated with the onset of war. Alliances appear to be a way of preparing for war and usually result in such. More dangerous are arms races. Crises that occur in the absence of some form of military buildup rarely escalate to war, but crises that do occur in the presence of military buildups frequently escalate, especially if they involve disputes over territory that is contiguous to one of the parties. The first crisis is not likely to escalate, but if they persist, the probability of escalation goes up, with the third crisis very likely to end in war.[8]

[7] . Ibid. 9-11, 375.

[8] . Ibid, xxv. Sometimes, the conclusions from COW work are almost ludicrously tautological. Vasquez and Henehan summarise the work of Wallenstein (xxvi) as suggesting that 'when states can come up

Now if this set of generalisations were not merely plausible but able to be well established by careful, replicable investigation, then it would at first sight appear to represent an advance over the plausible musings of the deductive or *a priori* classical realists against whom the COW-herds were reacting. Yet, stated in the way adopted by Vasquez and Henehan, it doesn't seem a very substantial pay-off for thirty-three years of labour by hundreds of men and women. And some sense of why progress has been slow can be gleaned by drilling down to look in a little more detail at how debates are conducted within the COW paradigm.

For this purpose I have taken a disagreement between Michael Wallace and Paul Diehl (the latter later to become chief COW-herd) about arms racing as a cause of war. The two articles, originally published in academic journals in 1982 and 1983, were later anthologised by Vasquez and Henehan as exemplary illustrations of the research methods of the COW project.[9]

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with a way to resolve issues on the basis of some agreed-upon principles … they might be able to avoid war among themselves'. How Carl Schmitt would have laughed!

[9] . The two articles together with commentary from Vasquez and Henehan, occupy pp. 75-108 of the text-reader (see above, n.2). The original citations are as follows: Michael D. Wallace, 'Armaments and Escalation: two competing hypotheses' *International Studies Quarterly*, 26 (March 1982) 37-56; Paul F. Diehl, 'Arms Races and Escalation' *Journal of Peace Research* 20:3 (1983) 205-212.

Wallace argues that arms racing is likely to exacerbate existing conflicts between states. Diehl disputes his method and findings. Wallace opens by citing what he regards as an almost universal belief among policy makers and academics alike that military preparedness makes war less likely since it has a deterrent effect. He complains that this ‘doctrine is almost entirely anecdotal and idiosyncratic,’ relying on the ransacking of history.[10] It is also in stark contrast to the equally plausible suggestion that increases in armaments by one country may be matched by others, and that positive feedback may lead to arms racing. Only scientific inquiry can decide between two intuitively plausible accounts.

Wallace takes the set of 99 serious great-power disputes between 1816 and 1965 in the COW data set, 26 of which culminated in all-out hostilities between the pair of states concerned. He eliminates high relative expenditure by the rising or revisionist power as an explanatory factor. Relative changes in the capability of the two opposing states doesn’t seem to be especially significant. He next develops a specific measure of arms racing, defined as sustained growth of 10% or more in military expenditure by a pair of opposing states. He then finds that, of his 99 serious disputes, 28

[10] . Wallace 76.

were preceded by arms racing, thus defined, and 23 of these ended in war. Merging cases in which two or more allied states commence a war with a common foe simultaneously, Wallace introduces a note of reality. This procedure reduces the number of conflicts from 99 to 80. Out of these, 15 resulted in war, and 11 of these were preceded by arms racing. Conversely, only 2 of the 65 remaining conflicts resulted in war. It appears that 74 out of 80 cases are accounted for by Wallace's theory, which can be loosely summed up in the following terms. Transitions in the relative power of status quo and revisionist states need not lead to war, but arms racing is liable to exacerbate existing conflicts. It's not that arms racing causes war, but that arms racing between states with existing disputes is likely to spill over into war.

Diehl is not so sure. He is unhappy about Wallace's arms-racing definition and his research design. A first point is that the process of aggregation used by Wallace to reduce 99 belligerent dyads to 80 is insufficient for Diehl. Wallace merged disputes where two allies go to war simultaneously against a third party. This is not enough for Diehl, who points out that other allies often join in on one side or the other as a conflict proceeds and that reliance on formal alliance is unsatisfactory. The USSR and Britain were not allies when Germany attacked the former in June 1941! In a rather different way, both the USSR and Japan built up their armed forces very rapidly in the years preceding their going to war with one another in 1945, but this plainly ought not to be counted as a case supporting Wallace's hypothesis. More generally, it is not easy to tell whether increases in military expenditure are directed against a specific opponent or against several potential enemies. Diehl concludes that the 15 distinct wars that result from Wallace's procedures ought really to be further reduced to seven or eight integrated conflicts and points out that an implication of this refinement is that 'the strength of the arms race – war relationship stems not from an abundance of distinct cases of dispute escalation, but is merely a function of a coding decision' (95). His conclusion is that Wallace's case rests very heavily on the two world wars. The relationship between arms-racing and war 'gains statistical significance only through an artificial division of an integrated situation' (102).

I have examined this exchange at some length for two reasons. First of all, it is a fair indication of the care with which work in the COW tradition is conducted and subjected to critical scrutiny within a quite clearly self-defined research community. The second factor, however, is that these two papers illustrate the way in which this ostensibly scientific and objective research programme takes place in a political context.

The end of the 1970s witnessed the fall of the Shah of Iran, the Polish crisis, and the Soviet invasion of Afghanistan. Reagan was inaugurated early in 1980; Margaret Thatcher had become Prime Minister in Britain the year before. Decisions were being taken that would result in renewed confrontation between the two Cold War superpowers, and these included major increases in military expenditure, not least by the United States on anti-missile defence systems. So if we set aside the science for a moment and look at the politics of the Wallace/Diehl exchange we find Wallace explicitly supporting arms control as against what he refers to as the ‘peace through strength’ position. He writes: ‘[D]espite the ideological predispositions of those in leadership positions, it is vitally important both to avoid the resumption of an uncontrolled strategic arms competition and to resume efforts to achieve arms control’ (85).

So the energy with which Diehl and others challenged Wallace did not derive from purely scientific zeal, but also from political disagreement about United States policy. Diehl explicitly identified Wallace's paper as favouring the START (Strategic Arms Reduction Talks) position. His own finding, that only a quarter of great-power wars were preceded by arms-racing was, conversely, calculated to offer comfort to those in the Reagan administration arguing for increased military expenditure.

Moving now from specific examples of COW-herding to a more general critique, I intend to concentrate almost exclusively on the first of the four steps outlined in the sketch I offered earlier: conceptual clarification and coding.[11] My concern is that the inductivist approach is in deep trouble if it can't get past this hurdle, less because of any difficulties caused by *lack of agreement* on operationalizable definitions, but because any *agreement* has strong implications for the process of theory-testing and, in consequence, for the policy-relevance of any generalizations that may emerge. And while the COW website is silent on normative matters, there is little doubt about the intention of the COW project, which is to provide policy-makers with means

[11] . Dessler concentrates rather more on heterogeneity of variables and the reluctance of the Cowherds to think about causation because of their metaphysical commitments.

of anticipating and averting war. Not for nothing has a good deal of COW work been published in the *Journal of Peace Research*.

This particular cat is let out of the bag by Charles Kegley in an essay collected in the Vasquez and Henehan reader. Kegley insists that ‘until war has been systematically described, it cannot be adequately understood, and with such understanding comes the first meaningful possibility of controlling it, eliminating it, or finding less reprehensible substitutes for it’.[12] Description, then, is pre-theoretical, a necessary step before scientific observation can take place.

Let us therefore examine the standard COW definition of a war. It must result in no less than 1,000 deaths of military personnel in battle. It must be fought between and not within nations (*sic*), and these are in turn specified as states consisting in organised sovereign polities with a population of 500,000 or more and diplomatic missions from Britain and France. Vasquez and Henehan conclude: ‘[a]n international war is a military conflict waged between (or among) national entities, at least one of which

12 . V&H 379.

is a state, that results in at least 1000 battle deaths of military personnel'.

Let me take each of these criteria in turn, starting with the minimum 1,000 deaths of military personnel in battle. The object is to exclude skirmishes, border incidents, minor interventions (gunship diplomacy) that are small in relation to the resources of the participant states. This seems sensible, but note that any inference from the resultant data sets is limited by this decision, once taken. If the kind of gunship diplomacy and military intervention routinely practiced by the USA in Central America and the Caribbean in the early twentieth century, or worldwide by Britain in the nineteenth century, is excluded from the figures, this will prejudice any investigation that might be undertaken, for example, into the question of whether democracies or Great Powers are more or less belligerent than other kinds of states.[13]

Next comes the requirement that, to be counted as a war, an armed conflict must be between and not within 'nations' (p.385). The first thing to note here, in passing, is the cavalier way in which, in an account of the importance of precise and scrupulous definition we are told ‘we use state, nation, and nation-state interchangeably’. Now the primary object of this rule is to exclude civil wars, wars of secession (at least if they are unsuccessful) and colonial wars. That is to say, the objective is to focus on wars between states as defined by the founders of the project. However there is a world of difference between empires, early-modern composite monarchies, unitary states and federal states, as also between national and multi-national states, however constituted. It would be absurd – because quite counter to the usage of the time and the current usage of historians today – to claim that the temporal authority of the Emperor over German states meant that wars fought by states within the Holy Roman Empire in the sixteenth and seventeenth centuries were not ‘real’ wars, while the engagement of forces fighting on behalf of the Emperor or interventions by Denmark, France, Sweden were.

This particular example lies outside the time-frame of the COW project, but it can plausibly argued that the Confederate states in nineteenth-century North America had a right to secede because their sovereignty had not been fully dissolved into the Union, and that the American *Civil* War was only thus named by the victors,

[13] . It is a point taken up and investigated in great detail by David Mares in Violent Peace

as one mark of their victory.[14] The United States continued to be grammatically plural for many up to the 1860s, among them the English writer Antony Trollope.[15] It does not seem immediately obvious that states that became protectorates of the British Crown thereby became subordinate parts of a single British imperial state. Quite the contrary: the ostensible and often the original objective of protectorate status was to preserve the distinct identity of such polities. This feels like a contractual arrangement in which the sovereignty of the protected state is suspended but not extinguished. That was how the Afrikaaner boers understood their relationship with the British; but once again it was the victors who got to write the history.

This, as I remarked earlier, is a problem that the COW-herds are addressing currently, but it has taken forty years to get around to it, and even now the wording of the summary description of this endeavour suggests a bias toward formality likely to exclude indigenous polities in Africa and the Americas such as the Iroquois confederation, the Zulu nation, or the Matabele, Dahomey or Ashanti polities. As with the limitation to 1,000 or more battle

[14] One small but chilling residue of that struggle is the refusal of Harvard to commemorate the small number of its graduates who died fighting as officers of the Confederate armies.
[15] . Antony Trollope, …

deaths, the real question is how application of this criterion affects the scope of the whole project: what it can and cannot do? How does it limit the scope of the discipline of IR? My broad claim would be that, notwithstanding the ongoing revision of geopolitical-unit definition, the 'between not within' criterion depends on a clear, modern, euro-originated distinction between inside and outside – a sharp concept of sovereignty – that is historically appropriate only within a quite limited range of time and space. It is therefore worth remarking that we seem to be moving once again, as last week, toward generalisations about warfare that are implicitly much more restricted than traditional realism, with its claims about the continuity of conflict between polities from ancient times to the present. The methodology entails (and this will be made more explicit through an examination of the third criterion) the conclusion that modern war between modern states is not comparable with war within earlier or non-European states-systems. It also marginalizes wars between indigenous polities, and between them and European and Creole states, which were numerous and widespread in the nineteenth century. Think, for example, of New Zealand, where the Maori wars took place after formal treaty relationships with the British, acknowledging Maori sovereignty, had been established; think of Brazilian expansion into Goias and Mato Groso. This is consistent with the view I expressed in an earlier lecture, that IR is a 1960s discipline dealing with contemporary events, a position supported by one of the objections lodged by Vasquez and Henehan against both Wallace and Diehl: that nuclear deterrence may have fundamentally changed the nature of the game. Perhaps unipolarity at the end of the Cold War changed it again?

Next we come to the insistence that, in order to be counted, wars must be between states, defined as organised sovereign polities with a population of 500,000 or more and diplomatic missions from Britain and France. The effect of this restriction is to exclude conflicts between 'nationlike entities that are so small and/or so obscure (at least to the historians whose writings are available today) that we must exclude them'. This need to exclude is based on two reasons The first of these is that 'it is safe to assume that they (American or African tribes or European microstates) are so different from entities we usually think of as sovereign states that it is safe to assume that the process through which they get into wars is probably quite different from the process in which larger and more modern entities become involved in such conflicts' (V&H 300+). The second reason is that 'the plain fact is that many small and/or obscure entities are not within our grasp historically … no records were kept or [the records have been] lost for ever'.

Now my concern about this restriction is partly methodological and partly 'factual' or practical. On the methodological front, the assumption of difference seems in principle no more or less safe than the typical realist assumption of comparability. I don't know how many battle deaths there were in the Peloponnesian wars or the wars that took place between North Italian states in Machiavelli's day prior to the French intervention; nor do I know whether all these states meet the 500,000 population criterion. (When Richard Cobden visited Greece in the 1830s (Morley, p.80) 'he was amazed to find the mighty states of Attica and Sparta within an area something smaller than the two counties of Yorkshire and Lancashire. "What famous puffers those old Greeks were! [Cobden exclaimed] Half the educated world in Europe is now devoting more thought to the ancient affairs of these Lilliputian states … than they bestow upon the modern history of the South and North Americas…"'.[16] Still on a point of

[16] . John Morley 80. An even better illustration of the great liberal's matchless provincialism and small-mindedness is to be found in Morley's account of his 1847 visit to Rome (433): 'Walked with Mrs Jameson into the Sistine Chapel to see Michael Angelo's frescoes; the Last Judgment at one end, and the whole of the ceiling from his pencil. It is a deplorable misapplication of the time and talent of a man of genius to devote years to the painting of the ceiling of a chapel, at which one can only look by an

methodological principle, the idea of simply dismissing a class of phenomena and entities because of the difficulty of direct observation seems odd. Surely the logical thing to do before dismissal is at the very least to search for ways of indirect observation: observable traces made in cloud chambers by small sub-atomic particles that cannot (or used not to be able to) be directly observed come to mind.

Secondly, on the practical side, it is perfectly true that, as recently as 1963, Hugh Trevor Roper famously declared that Africa had no history because it lacked diplomatic archives. But Trevor Roper was already behind the game in the 1960s, and the historiographic position is not nearly as gloomy as Vasquez and Henehan claim. To begin with, it is surprising to find that early nineteenth-century Indian states, still independent of the British Raj, fail to meet the criteria set by the COW project, which lists the wars between Britain and the Mahrattas (1817-18), Burma (1823-6), Afghanistan (1838-42 and 1878-80), the Sikhs ((1845-6 and 1848-9) as extra-systemic (likewise the Boer War of 1899-1902).

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effort that costs too much inconvenience to the neck to leave the mind at ease to enjoy the pleasure of the painting …']. It may have been this quality in the Manchester radicals that may have led Lord Palmerston to complain that (Bright … add up the cost to Manchester on one hand and …)

Moreover, if we turn to the practical objection put by the COW-herds, that we simply don't and can't know much about these states, the response must be that we know a good deal from archaeological evidence, from oral tradition, and from the reports of travellers about pre-colonial polities in sub-Saharan Africa and the indigenous plains nomads of Patagonia and the American prairies and pampas; and this evidence suggests two things: that some of the polities concerned were highly organised, populous, and in many respects comparable to European-style states. I have already referred to Dahomey, Ashanti and the Buganda kingdom; to these may be added the Araucanian or Mapuche polity in what is now southern Chile, which held out until the 1880s. The initial reaction of Portuguese mariners visiting the Congo was to recognise the people there as constituting a polity and invite their leader to send an envoy to the Pope in Rome, which he did (Winthrop Jordan). We noted earlier that the initial reaction of the Spanish Dominican theologian, Francisco de Vitoria, in the sixteenth century, was to recognise the indigenous polities of the recently discovered Americas as (perfect) political communities and therefore subjects of international law, so that war against them had to be justified in the same way in which war between

Christian states had to be justified. These first reactions soon faded, and by the nineteenth century the kind of dismissal embedded in the COW project was routine; but while this explanation accounts for this dismissal, it does not justify it. It is simply exposed as a very local judgment.

To sum up, it is useful to read Cynthia Weber's book about intervention, where positivist work raises many of the same problems that beset the COW approach to war. Weber concedes that '[it] is possible to ask meaningful questions … by employing behavioral techniques'. Then she drives the knife in.

> [I]t is not possible to do so – she points out – without simultaneously settling some questions about meaning. Put differently, behavioral projects which aim to 'uncover' or 'discover' the nature of interventionary behaviour succeed only in that they create or invent the nature of interventionary behavior through the conduct of their analyses. Intervention commonly has been operationalised as "the movement of troops or military forces by one independent country, or a group of countries in concert, across the border of another independent country (or colony of an independent country), or actions by troops already stationed in the target country' (quoting Pearson, 1974:261). But where do indicators, signifiers, and operationalizations come from? It is theorists who impose definitions of intervention and propose indicators to capture interventionary behavior, and they do so from outside of history. That is, these definitions are not generated by historical conditions or by the cases analysed themselves; rather they are decided prior to analysis by theorists. … In other words, meaning is generated by the theorists so that it may be tested' (18).

I have expressed some concerns about the definition of war used in the Correlates of War project and I believe that they apply more generally, creating problems for any attempt to define war, the state, or other concepts, in an essentialist manner, by specifying minimal criteria. The obvious alternative - think of the later work of Wittgenstein - is to define by observing terms in use within specific communities or in W's terminology 'forms of life'. The famous example of 'game' shows no single common characteristic of all games. This does not mean the term is meaningless or especially hard to use. Perhaps in the same way the search for shared characteristics of all wars, all polities, etc. is futile.

All this would by no means sink the COW approach. But it would make clear that it cannot tell us much about armed conflict or polities in general, only about inter-state wars as it defines them. So what presents itself as a universal scientific inquiry turns out to be a rather parochial exercise.

I want now to move on from the inductivist approach to agent-based approaches in the rationalist tradition. Once again, I have had to choose a single reading from an extensive body of work, but before glancing at James Fearon's 'Rationalist Explanations for War' I want briefly to consider an interesting paper by James De Nardo, which stands between the cruder exercises of the Cow project and Fearon's bargaining approach methodologically.[17] I include it, because it demonstrates nicely, and in relation once again to the early 1980s missile-defence controversy, the danger to which Dessler draws attention – as much for model builders as for Cowherds – of an ideological tainting of social science. Singer himself, founder of the Correlates project, had claimed hat 'the whole point of scientific method [was] to permit us to investigate whatever problems interest and excite us, while largely eliminating the possibility that we will come out where we *want* to come out.'[18]

The failure of behaviouralism to eliminate ideological disagreement was examined through the Wallace–Diehl debate about arms-racing, which evidently bore upon, if it did not actually arise out of the Strategic Defence Initiative of the Reagan administration in the 1980s. The paper by De Nardo, while

[17] James De Nardo, 'Complexity, Formal Methods and Ideology in International Studies' in Michael W. Doyle and G. John Ikenberry (eds.) *New Thinking in International Relations Theory* (Boulder CO: Westview, 1997). **When revising this, perhaps make this the place to consider complexity more generally, notably from Axelrod**.

fundamentally sympathetic to modelling, notes the same problem of scientific research that just happens to validate the political views of the scientists. He compares three attempts to model the implications of effective, though less than perfect anti-missile defence systems for strategic stability. The stability of nuclear deterrence, it is assumed, depends on the ability to retaliate after a first strike. Anti-missile systems are likely to increase the ability of a state to retaliate without wholly deterring attack so long as the attacker can confidently expect that some missiles will get through. However, the process of moving from no effective defence against missiles to something close to a fully-effective system is a dynamic one, and is fraught with uncertainties.

Throughout his lucid account of a complex debate, De Nardo accepts that models must simplify reality, trying to create 'a simple representation of the problem that contains its essential features'. They don't do this in a search for elegance or because of ignorance of the awkwardness of the world; they do it because they see no alternative. Worse; testing is often impossible. This is because 'specification drives estimation,' because 'extrapolation is

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[18] J. David Singer ''The Incompleat Theorist: Insight without Evidence' in K. Knorr and J. N. Rosenau (eds.) *Contending Approaches to International Politics* (Princeton NJ: Princeton University Press,1969) 80, quoted in Dessler 'Beyond Correlations' 348 (emphasis in original).

treacherous when experience is narrow,' because 'competing models often fit the observed data equally well … even though [their] predictions diverge sharply outside the observed range,' and because statistical models with many parameters often produce unstable estimates' and typically do so in the social sciences.

De Nardo recognises that this inability of the modelers to agree lays them open to political manipulation, a vulnerability that Dessler lays at the door of unresolved taxonomic disputes among the correlators. After surveying three different models of strategic interaction, De Nardo concludes that each supports 'strikingly different conclusions about the effect of missile defences on strategic stability. In the first model, stability increases continuously as the defences improve in quality. In the second, deterrence is stable with very bad and very good defences, and it deteriorates in the middling range. In the third, middling defences produce the most stability, and very good and very bad defences produce unstable outcomes'.[19] This divergence is typical of formal work on big policy issues in the social sciences and is, De Nardo ruefully concludes, what opens the door to 'partisan interference

[19] . James De Nardo, 'Complexity, Formal Models, and Ideology in International Studies' in Michael W. Doyle and G. John Ikenberry (eds.) *New Thinking in International Relations Theory* (Westview, 1997) 149/150.

and ideological controversy, notwithstanding the best efforts of scientific analysts. People of every ideological persuasion can find a model (or a scientist) to suit them' (154). To paraphrase Singer: they come out where they started, which is just where they wanted to be. Notice, however, that for De Nardo the corruption arises from the manipulation of a vulnerability of scientists by non-scientists.

De Nardo is undismayed. Given the huge increases in computing power, even a decade ago, he was able to believe that the proper response to the problems he had identified was to press on and refine the modelling process, staunchly rejecting any traditionalist backsliding into a priori and informal methods. No other field of inquiry, he insisted, (no 'mature' discipline!) had turned back having once adopted formal methods (155).[20]

The Correlates of War project has been concerned with accumulating data relating to a great variety of candidate variables. The kinds of models considered by De Nardo are more concerned with the strategic calculations of political actors. The

[20] . Charles W. Kegley, also in the Vasquez and Henehan collection, pleads that physics needed three hundred years to mature into a truly predictive science (379?).

first may be termed inductivist and the second rationalist. To move to James Fearon's 'Rationalist Explanations for War' before finally turning to Kenneth Waltz's structuralist neo-realism risks confusion, since Fearon is at some pains in the introduction to his essay to identify himself with neorealism. It is certainly true that Kenneth Waltz relies upon states acting rationally under anarchy to generate his theory of international politics, but his emphasis is rather more on the context and structure of anarchy than the rationality of those agents who find themselves within it, while for Fearon, the emphasis is reversed. His central concern is that states in conflict, knowing how uncertain and costly war can be, ought to be able 'to locate negotiated settlements that all would prefer to the gamble of war'. His question is why they cannot 'locate an alternative outcome that both would prefer to a fight'.[21]

Fearon's answer, which once again distinguishes him from the highly abstract structuralism of Kenneth Waltz, is that anarchy, expected benefit, precautionary anticipation and rational miscalculation do not quite fit the bill. Instead he proposes that failure to reach a deal may arise from three problems. The first is

[21] . James D. Fearon, 'Rationalist Explanations for War' *International Organization* 49:3 (Summer 1995), 380.

that leaders may have secret information about relative military capabilities coupled with strong incentives not to disclose this information. This is more than mere imperfect information; it is that there may be asymmetries of information built into anarchy. The second problem is that agreement may be impossible because it is apparent that one of the parties will have an incentive to renege on the agreement. This, Fearon calls the ‘commitment problem’. A third possibility is that states may fail to agree because they cannot separate the issue or issues at the heart of the conflict from other concerns or because what lies at the heart of the dispute simply is not open to compromise: – Jerusalem, perhaps? Fearon’s central claim is that while Kenneth Waltz, to whom I shall turn in a moment, might attribute war, say, to multipolarity, he would need, for a comprehensive explanation, to explain what it was about multipolarity (as compared to bipolarity) that had created insuperable information or commitment problems.

But although the inductivists are interested in alliances and the rationalists study strategic interaction between national actors, neither group lays emphasis on system structure in the way that Kenneth Waltz did implicitly as early as 1959 and explicitly in his

Theory of World Politics.[22] It is probable that rationalists, with their emphasis on individual states or ‘units’ in Waltz’s language, notwithstanding the protests of Fearon that rationalist explanations ‘could just as well be called “neorealist explanations,”’ would be dismissed as reductionist.[23]

With characteristically deceptive clarity, Waltz warned, early in *TIP* (12) that ‘much pointless work’ was done in international politics because basic questions about methodology that ought to have been asked at the outset of an inquiry were ignored. My catechism differs only slightly from Waltz’s. Are we dealing with a system? If so, is it one in which relations between the variables are linear or non-linear? Is it one in which an analytical method is appropriate or in which the number of variables and/or the relations of the system with other systems requires, and the population of events makes possible, a statistical or probabilistic approach? Even more ‘systemic’ than Waltz, I insert, and may find time to explain, a dichotomy that he does not employ, between linear and non-linear systems. Also, I see the decision between analytical and probabilistic method as one that still applies even when one is

[22] . Kenneth N. Waltz, *Theory of International Politics* (Reading MA and London: Addison Wesley, 1979).

dealing with (to stick to Waltz's definition of system) an 'object of study [that] is both complex and organised' (TIP 12).

Waltz's answer, to be blunt, is that we are dealing with a system which is fundamentally anarchic and in which only the interactions of the major powers matter. Hence, statistical work is not appropriate, since the only important variable is the distribution of capabilities across the great powers. Note that it is not the capability of each actor that is important here, but the *distribution* of capabilities. 'For the purposes of developing a theory, states are cast as unitary actors wanting at least to survive, and are taken to be the system's constituent units. The essential structural quality of the system is anarchy – the absence of a central monopoly of legitimate force. Changes of structure and hence of system occur with variations in the number of great powers. The range of expected outcomes is inferred from the assumed motivation of the units and the structre of the system in which they act.'[24] Any explanation of systemic outcomes by resort to unit-level characteristics or behaviour is dismissed as reductionist. So for Waltz and those who follow him, the probability of war varies solely

[23] . James D. Fearon, 'Rationalist Explanations for War' *International Organization* 49:3 (Summer 1995) 380.

according to whether the system is multipolar, bipolar or (perhaps impossibly?) unipolar, and the assumption about rationality is that states seek to maximize either capabilities or security.

This is a causal explanation, but at a very general level. It does not predict. It accounts for broad tendencies and suggests that the tendency to war will vary according to the distribution of capabilities across a system and their intensity, Waltz's own preference being for bipolarity coupled with nuclear deterrence as the most stable arrangement.[25]

What are the peculiar virtues of bipolarity? The first, according to Waltz, is that the two powers in such a system tend to trade and invest within their own separate spheres of influence. These become are more autarkic or self-sufficient which, he believes, reduces the economic causes of conflict. If superpowers have no economic relations with each other disputes arising from this source that might cloud the strategic relationship will simply not arise. From this low level of interdependence springs a second advantage of bipolarity. Relative seclusion in a bipolar world makes signalling easier because it's clear who the message is for

[24] . Kenneth N. Waltz, 'War in Neorealist Theory' in Robert I. Rotberg and Theodore K. Grabb eds.) *The Origin and Prevention of Major Wars* (Cambridge: Cambridge University Press, 1989) 42.
[25] . Ibid., 48-52.

and it is less likely to be drowned out by routine traffic. It’s easier to use crises, proxy wars, etc. to balance the system. Third, the lack of more or less equal allies greatly reduces the chances of being dragged into a conflict by a weaker yet indispensable partner (as Germany was by Austria-Hungary in 1914) and also makes it easier for each hegemonic power to provide public goods and develop selective incentives with less fear of free-riding or defection. Finally, the dangers of miscalculation are lessened in a bipolar world (Rotberg 47), and while over-reaction remains a danger in a bipolar system, it is the lesser danger ‘Miscalculation is the greater evil – Waltz suggests – because it is more likely to permit an unfolding of events that finally threatens the status quo and brings the powers to war. Overreaction is the lesser evil because at worst it costs only money for unnecessary arms and possibly the fighting of limited wars’ (Waltz in Rotberg 47).

In the search for general causes of war we have run the gamut from Biology, through Social Anthropology, to various positions in contemporary International Relations, and the questions I want to leave you with are two. First of all, are all these explanations broadly compatible with one another? Does each have something to offer? Are we at liberty to pick and choose? Or must each of us, in the interests of consistency, tie his or her colours to some mast or other? My second question leads directly to the eighth and final lecture. Why should there be so much concern about the causes of war? After all, to uncover its causes is not necessarily to make war any less likely, given the complex, continually self-conscious and reactive nature of society. If we are stuck with war ought we not to be devoting attention to understanding what kind of thing it is. Is it an institution? Is it a breakdown of social order? Is it an instrument of justice or punishment? Ought we not also to be trying to develop some guidelines about how to conduct ourselves in a state of war and how to win the damned things? It to all but the last of these subjects, which I leave to strategic studies specialists and the military, that I will turn in the final lecture, in order to complete this tour of the methodological eclecticism provoked by international relations.

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