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International Relations Scholarship and the Tyranny of Policy Relevance

Lee Jones

Professor Joseph Nye recently complained in the Washington Post that the reason why no International Relations (IR) scholars were picked to serve in the Obama administration was that the profession has withdrawn to the ivory tower. Nye (2009) chided his colleagues that they must make themselves more policy-relevant in the future.

On the one hand, Nye's complaint addresses a specifically US culture, where there has historically been something of a revolving door between the academy and policy circles. Nye himself was Deputy Undersecretary of State for Security Assistance, Science, and Technology under Carter, chairing the National Security Council Group on Nonproliferation of Nuclear Weapons, and was later Assistant Secretary of Defense for International Security Affairs under Clinton. He has now been appointed US ambassador to Japan.

However, the general complaint is a common enough refrain, not just in the US but in the UK – that academics must be 'policy relevant' in their choice of research topics, their research methodology, and the way they disseminate their findings to various so-called 'stakeholders'. In the UK, this imperative is reinforced by the bureaucratic *diktat* of the various government-backed funding councils. It fits with the viewpoint enunciated since the 1980s that (a) the primary task of academics is research and (b) academics must be 'accountable' for the way they spend public money. It naturally follows from this that academics should do 'relevant' research (who would defend 'irrelevant' research?) which addresses itself to the contemporary requirements of policy (or the economy – a subject for another time).

Despite acquiring a taken-for-granted quality, these ideas are a very recent innovation. For centuries, the job of people staffing universities was primarily to teach their students, not to conduct research. Then, only a few decades into an age where ordinary people could aspire for the first time to become university students, suddenly the mission of universities underwent radical transformation as governments refused

to allocate the necessary money to fund this expansion in higher education, forcing universities instead to compete for funding through exercises designed to assess their research output – what became known in the UK as the Research Assessment Exercise. The growing subordination of the academy to political and economic imperatives was recently expressed in the abolition of the government department for 'innovation, universities and skills', with power over universities being transferred to the department for business.

The history is somewhat different in the US, where the expansion of higher education to include the working classes has largely involved the grossly underfunded expansion of largely state and community colleges. Elite, Ivy League schools have expanded their undergraduate numbers but much undergraduate teaching is hived off to graduate students whose own studies are correspondingly extended; only those who can afford to take a Masters degree may be taught by the academics whose 'big names' attracted them in the first place. It is a good thing that research is now carried out in specialised institutions by people enjoying (supposed) academic freedom. Along the way, however, it might well be argued that the academy's initial mission has actually been usurped.

Putting those questions aside for now, what of the broader relationship between the state and the academy? Let us concede that Nye has a point, even if he is taking aim at the wrong targets. We should concede that academics are often poor at communicating their ideas to society at large. All too often we are content to write for each other, knowing full well that our work will never be consumed beyond a narrow, disciplinary clique. We need to do much better. Equally, however, there is no need to reduce this to the banal process of 'knowledge transfer' being promoted by the UK research councils, which seeks to justify research by appealing to its value to British firms. We should be trying to communicate our work to our students, trade unions, citizens' groups, non-governmental organisations, and citizens who are simply curious about the world – not to improve companies' balance sheets but to play a positive role in 'cultivating our humanity' as Seneca once urged (Nussbaum, 1997).

Nye also complains that the *American Political Science Review* never publishes policy-relevant articles – or even articles that policymakers can comprehend. That is undeniably true, but the real problem here is actually one of methodology – which he hints at by swiping at scholars obsessed with modelling. What he really criticises is the mania for quantitative methods and formal modelling that has gripped the US academy and which is gradually infecting the British academy. The most influential IR journals, like *Foreign Affairs*, contain absolutely no articles of a quantitative bent.

There is no doubt that the average *APSR* article is impenetrable to any normal person lacking econometric training, but this is not the only reason such research is not policy relevant: it is also because very few questions of major import

actually lend themselves to being studied properly in this fashion. Hedley Bull once commented that a commitment to quantitative methods is a commitment to the 'marginalia of the subject that are susceptible of measurement and direct observation' (Bull, 1966, p. 363). The real problem is that marginalia are increasingly the core of our discipline. This is clearly linked to the overall conception of social science to which we subscribe – whether we believe people are like particles and can be studied using similar methods to those used in Physics.

Our beliefs about social science are influenced strongly by the overall political economy of research funding – Physics attracts the most funding as a 'hard' science; Economics models itself on Physics to attract the most funding in the social sciences; and the other, envious social sciences model themselves on Economics. Physics undeniably generates a great deal of very useful research (although the constant push for immediately 'useful' research 'outputs' may well now be undermining 'blue skies' research that expands the limits of human understanding). Whether Economics has done likewise is clearly open to debate, especially in light of the current global economic crisis. Ironically, IR scholars still labour beneath the shadow of the work of Kenneth Waltz, who sought explicitly to import models used in Economics to understand the emergence of a balance of power between states. At root this is a problem of all human knowledge being judged by a set of extremely idealised, monolithic notions of how 'science' works.[1]

The more abstract and bizarre these models become, the more removed they become from reality, the less impact they are likely to have on reality in turn. But the problem here is not 'policy relevance' – it is rather a two-fold issue: a far deeper philosophical question as to what social science can accomplish and what it is for, combined with a funding structure which assumes the wrong answer to these questions. Policy-relevant material will not be produced by the sort of exhortations Nye issues, but only by addressing these two broader issues.

Having conceded where Nye has a point, let's now consider the ways in which he may simply be wrong. His assumption is that the academic should be, needs to be, policy-relevant. As indicated above, this can be a very pernicious assumption. As an invitation to academics to contribute to discussions about the direction of society and policy, no one could reasonably object: those who wished to contribute could do so, while others could be left to investigate topics of perhaps dubious immediate 'relevance' that nonetheless enrich human understanding and thus contribute to the accumulation of knowledge and general social progress (and, quite probably, to those scholars' research communities and their students). As an imperative, however, it creates all sorts of distortions that are injurious to academic freedom. It encourages academics to study certain things, in certain ways, with certain outcomes and certain ways of disseminating one's findings. This 'encouragement' is

more or less coercive, backed as it is by the allure of large research grants which advance one's institution and personal career, versus the threat of a fate as an entirely marginal scholar incapable of attracting research funding – a nowadays a standard criteria for academic employment and promotion.

Furthermore, those funding 'policy-relevant' research already have predefined notions of what is 'relevant'. This means both that academics risk being drawn into policy-based evidence-making, rather than its much-vaunted opposite, and that academics will tend to be selected by the policy world based on whether they will reflect, endorse and legitimise the overall interests and ideologies that underpin the prevailing order.

Consider the examples Nye gives as leading examples of policy-relevant scholars: Henry Kissinger and Zbigniew Brzezinski, both of whom served as National Security Advisers (under Nixon and Carter respectively), while Kissinger also went on to become Secretary of State (under Nixon and Ford). Kissinger, as is now widely known, is a war criminal who does not travel very much outside the USA for fear of being arrested *à la* General Pinochet (Hitchens, 2001). Brzezinski has not yet been subject to the same scrutiny and even popped up to advise Obama recently, but can hardly be regarded as a particularly progressive individual. Under his watch, after Vietnam overthrew the genocidal Khmer Rouge in 1978, Washington sent tens of millions of dollars to help them regroup and rearm on Thai soil as a proxy force against Hanoi (Peou, 2000, p. 143). Clearly, a rejection of US imperialism was not part of whatever Kissinger and Brzezinski added to the policy mix.

In addition to them, Nye says that of the top twenty-five most influential scholars as identified by a recent survey, only three have served in policy circles (Jordan et al, 2009). This apparently referred to himself (ranked sixth), Samuel Huntington (eighth), and John Ikenberry (twenty-fourth).[2] Huntington, despite his reputation for iconoclasm, never strayed far from reflecting elite concerns and prejudices (Jones, 2009). Nye and Ikenberry, despite their more 'liberal' credentials, have built their careers around the project of institutionalising, preserving and extending American hegemony. This concern in Nye's work spans from *After Hegemony* (1984), his book co-authored with Robert Keohane (rated first most influential), which explicitly sought to maintain US power through institutional means, through cheer-leading post-Cold War US hegemony in *Bound to Lead* (1990), to his exhortations for Washington to regain its battered post-Iraq standing in *Soft Power: The Means to Succeed in International Politics* (2004). Ikenberry, who was a State Department advisor in 2003-04, has a very similar trajectory. He only criticised the Bush administration's 'imperial ambition' on the pragmatic grounds that empire was not attainable, not that it was undesirable, and he is currently engaged in a Nye-esque project proposing ways to bolster the US-led 'liberal' order.

These scholars' commitment to the continued 'benign' dominance of US values, capital and power overrides any superficial dissimilarities occasioned by their personal 'conservative' or 'liberal' predilections. It is this that qualifies them to act as advisers to the modern-day 'prince'; genuinely critical voices are unlikely to ever hear the call to serve. The idea of, say, Noam Chomsky as Assistant Secretary of State is simply absurd.

At stake here is the fundamental distinction between 'problem-solving' and 'critical' theory, which Robert Cox introduced in a famous article in 1981. Cox argued that theory, despite being presented as a neutral analytical tool, was 'always for someone and for some purpose'. Problem-solving theories ultimately endorsed the prevailing system by generating suggestions as to how the system could be run more smoothly. Critical theories, by contrast, seek to explain why the system exists in the first place and what could be done to transform it. What unifies Nye, Ikenberry Huntington, Brzezinski and Kissinger (along with the majority of IR scholars) is their problem-solving approach. Naturally, policy-makers want academics to be problem-solvers, since policies seek precisely to – well, solve problems. But this does not necessarily mean that this should be the function of the academy.

Indeed, the tyranny of 'policy relevance' achieves its most destructive form when it becomes so dominant that it imperils the space the academy is supposed to provide to allow scholars to think about the foundations of prevailing orders in a critical, even hostile, fashion. Taking clear inspiration from Marx, Cox produced path-breaking work showing how different social orders, corresponding to different modes of production, generated different world orders, and looked for contradictions within the existing orders to see how the world might be changing.[1] Marxist theories of world order are unlikely to be seen as very 'policy relevant' by capitalist elites (despite the fact that, where Marxist theory is good, it is not only 'critical' but also potentially 'problem-solving', a possibility that Cox overlooked). Does this mean that such inquiry should be replaced by government-funded policy wonkery? Absolutely not, especially when we consider the horrors that entails. At one recent conference, for instance, a Kings College London team which had won a gargantuan sum of money from the government to study civil contingency plans in the event of terrorist attacks presented their 'research outputs'. They suggested a raft of measures to securitise everyday life, including developing clearly sign-posted escape routes from London to enable citizens to flee the capital. There are always plenty of academics who are willing to turn their hand to repressive, official agendas. There are some who produce fine problem-solving work who ought to disseminate their ideas much more widely, beyond the narrow confines of academia. There are far fewer who are genuinely

critical. The political economy of research funding combines with the tyranny of 'policy relevance' to entrench a hierarchy topped by tame academics.

'Policy relevance', then, is a double-edged sword. No one would wish to describe their work as 'irrelevant', so the key question, as always, is 'relevant to whom?' Relevance to one's research community, students, and so on, ought to be more than enough justification for academic freedom, provided that scholars shoulder their responsibilities to teach and to communicate their subjects to society at large, and thus repay something to the society that supports them. But beyond that, we also need to fully respect work that will never be 'policy-relevant', because it refuses to swallow fashionable concerns or toe the line on government agendas. Truly critical voices are worth more to the progress of human civilisation than ten thousand Deputy Undersecretaries of State for Security Assistance, Science, and Technology.

Notes

[1] It should now be commonplace knowledge that science does not always advance in the way imagined by positivists like Karl Popper, by attempting to falsify hypotheses and endless rigorous testing, but, as various sociologists of science, starting with Thomas Kuhn, have demonstrated, often by far more human methods, including retroduction, hunches, guess-work, 'massaging' the data, interpreting the data through paradigmatic prisms, etc. Yet the leading proponents of social scientific methodology in IR, for example, suggest rigidly positivist modes of operation that are frequently bypassed in the laboratories of hard scientists (King, Keohane & Verba, 1994).

[2] However, John Ruggie (fourteenth) also worked as an Under Secretary-General in the UN. Moreover, Nye and the TRIPS survey probably understate the degree of low-level academic involvement in policy planning processes through consultancies and advisory positions. This has a long history and Area Studies in the US was deliberately promoted to cultivate such involvement (Simpsom, 1999).

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