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Politics is too important to be left to political scientists: A critique of the theory-policy nexus in International Relations

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(...) they that have no science are in better and nobler condition with their natural prudence than men that, by misreasoning, or by trusting them that reason wrong, fall upon false and absurd general rules. For ignorance of causes, and of rules, does not set men so far out of their way as relying on false rules, and taking for causes of what they aspire to, those that are not so, but rather causes of the contrary.

Thomas Hobbes, *Leviathan* (V.7)

Introduction

A considerable number of International Relations (IR) theorists have recently lamented the discipline's lack of practical relevance and urged political scientists to address fundamental real-world issues (Nau, 2008; Nye, 2008; Sil and Katzenstein, 2010a, 2010b; Jentleson and Ratner, 2011; Walt, 2012; Mearsheimer and Walt, 2013; Reus-Smit, 2013). Although scholars largely disagree over what constitutes valuable knowledge and on how to generate it, a common theme runs through their studies: one of the most important justifications of IR lies in the production of knowledge with concrete bearing on foreign policy issues and international problems. Put differently, political scientists have an obligation to engage in work that is not merely intellectually interesting, but also implies questions that are important for human affairs.

While the urge for a politically relevant scholarship seems reasonable and is perfectly consistent with the traditional study of politics – indeed all great works of political theory, from Aristotle's *Politics* to Hobbes' *Leviathan*, were not intended as knowledge for its own sake but aimed to intervene 'in a concrete political situation with the purpose of change through action' (Morgenthau, 1970: 257) –, the crucial question is concerned with the nature of the relationship between scholarship and policy. In particular, there is a major problem with the idea that IR theory should directly influence policy – that is, with what I call the theory-policy nexus: the belief that scholars have a privileged epistemological standpoint in devising the correct policy for the social and

political problem of the day. Challenging this idea, I would argue that such an understanding of the theory-policy relationship is patently mistaken.

The argument proceeds along three main steps. Firstly, I will outline the epistemological approach underlying the theory-policy nexus, what in the context of the philosophy of social science is termed naturalism¹. Because the so-called 'critical' approaches generally reject the option of problem-solving theories and look for more radical emancipatory changes, the present discussion will be mostly confined to naturalist-oriented scholarship². This section will also show that, though a naturalistic notion of social science does not imply a scientist or technocratic view of politics, the theory-policy nexus is still a typical feature of many of those scholars who share the naturalistic orientation. Secondly, I will contend that the privileged epistemological standpoint of IR scholars is based on shaky foundations. In fact, supporters of the theory-policy nexus are not in a position to advise decision-makers on what action to take. Finally, this paper suggests that the fact that IR theory should not be directly applied does not mean that scholars in this field are of no political use. Their work can be justified not in terms of the direct application of their findings, but rather by virtue of the indirect and unintended consequences on the policy-making process. In particular, IR may play two important roles. The first one concerns the function of theory in the intellectual development of policy-makers. The second one, instead, refers to IR's influence on those ideas that shape the policy and public debate on foreign policy decisions. Here, the role

of theory is not the ‘heroic’ one of providing scientifically derived knowledge by which foreign policy may be guided, but rather the ‘ironic’ one of ensuring that rival explanations can be heard for each problem at issue.

My criticisms of IR as an applied theory is not intended to refute it conclusively, but rather to create an intellectual space in which alternative ways to study international politics can breathe. Thus, the present paper is not a nihilistic philippic against IR theory, but rather a defence of theoretical, methodological, epistemological and ontological pluralism. Although there is widespread agreement on the notion that only multiple perspectives can account for the complex phenomena characterizing international relations (Jackson, 2011; Dunne et al., 2013: 415; Lake, 2013: 580; Mearsheimer and Walt, 2013: 449), I would contend that theoretical pluralism is desirable for reasons related to policy rather than to the growth of knowledge. For, only a plurality of theoretical perspectives can protect practitioners from the temptation of premature closure in the formulation of foreign policy.

Naturalism and scientism

Since the publication of the final three volumes of Auguste Comte’s *Course on Positive Philosophy* (1830-1842) – those dealing with social science – there has been an incessant

debate as to whether the methods of the natural sciences can be fruitfully employed in the study of society and politics.

Advocates of naturalism claim that the scientific method should be employed to study human affairs (Steuer, 2003), including politics in its domestic and international dimensions. In particular, naturalism assumes the unity of the physical and the social worlds (Wright, 2004: 4), and argues for the use of similar modes of inquiry for both domains³. Actually, according to naturalism, the only effective process to achieve valuable social and political knowledge is the use of the methods employed in the study of nature.

It follows that the naturalist-oriented social scientist employs observation, deduction and empirically testable hypotheses to find regularities and propose explanations for political phenomena in much the same way as natural scientists do. As a result, IR theory should be an empirical science, whose main goal is to formulate causal interpretations grounded on observable events and regular patterns.

Although most naturalist scholars may not aim to influence the policy-making process and might even disagree on the goals of theory and on how to do research, many of them share the problematic assumption that scholars have a privileged epistemological standpoint in understanding how international politics works and how it should be managed and changed. According to this approach, scientific knowledge is not produced for its own sake, but in order to improve social and political reality. The naturalist mantra

savoir pour prévoir, prévoir pour pouvoir implies that – as usually happens in the natural sciences – knowledge makes it possible to understand how the social world works and then how to control and address social phenomena according to specific political preferences (Panebianco, 1989: 564-567). ‘In this view’, Hans J. Morgenthau (1957[1946]: 4) explained, ‘the problems of society and nature are essentially identical and the solutions of social problems depend upon the quantitative extension of the method of the natural sciences to the social sphere.’

Before proceeding, it should be noted that it would be perfectly consistent to accept naturalism while rejecting the scientist and technocratic view of scholarship. For, the naturalist conception of social science only makes claims about the nature of scientific knowledge, which does not entail the additional contention that such knowledge should be applied to the political world. Moreover, it is imperative to avoid conflating naturalism and scientism because of an enduring cleavage among naturalist-oriented scholars separating those who believe that scientific and applied knowledge are mutually compatible from the “purists” who claim that the scientific study of politics should not be “contaminated” with issues concerning policy relevance.

It is, however, true that many proponents of naturalism have also explicitly or tacitly endorsed the scientist view. Indeed the scientist or technocratic understanding of political research was not just an *infantile disorder* of a young political science, as such an attitude is still very much present in the discipline. While there used to be no hesitation

in referring to 'A Scientific Test of Values,' 'A Scientific Theory of Democracy,' 'Politics as Pure Science' (Easton, 1950)⁴, the scientist views that still survive in the discipline are now generally supported with less emphasis. A few notable examples, which come from many theoretical quarters of the IR community, will illustrate this point.

Although Kenneth Waltz (1979: 6) warned that his theory should not be mistaken for a theory of foreign policy, in a telling passage of *Theory of International Politics* he plainly admits that the 'urge to explain is not born of idle curiosity alone. It is produced also by the desire to control, or at least to know if control is possible.' In a similar vein, after clarifying that 'We study international affairs not because war amuses us, but because we want to learn how to avert conflict,' Peter C. Ordeshook (1995: 180-182) contended that in order to 'improve the operation of political processes and institutions (...) the "engineering" component of the discipline' needs to take on 'a central role.'

Jeffrey Frieden and David Lake (2005: 138) even went so far as to argue that 'Only when International Relations brings science to the discussion does it have anything of enduring value to offer, beyond informed opinion.' Likewise, in spite of his findings on the limits of 'expert political judgment', Philip Tetlock (2010: 476) explicitly supports the old ideal of scientific politics when he defends 'technocratic assistance democracy' from 'populist democrats.' Implicit here is the notion that the only genuine type of political knowledge is the one provided by technocrats and experts⁵.

An appeal for applied political science can also be found in Fred Chernoff's (2014: viii) recent review of the field of security studies, where he explicitly endorses the theory-policy nexus: 'The ability to develop better explanations of international politics provides us with an enhanced ability to identify which causal factors are most strongly connected to which effects. This is essential for policy making.' A brief note should also be devoted to the scholar who claims to have found the right formula for predicting the occurrence of future events. Bruce Bueno de Mesquita (2010) not only asserts that game theory is an accurate predictive tool, but in his *Predictioneer's Game* (2009) he maintains he can provide the analytic instruments 'to see and shape the future,' as the subtitle of the book clearly puts it.

It may be objected that Bueno de Mesquita's unequivocal, strong support of the theory-policy nexus is the peculiar, idiosyncratic position of a particular scholar. However, in a recent article (2014) contending that America needs a Council of International Strategy, a similar view is put forward by David Laitin as well. Not only does he support the idea that theory should guide policy but – for the benefit of the policy-making process – he also urges the US government to take advantage of 'the gigantic policy community'. While discussing the work of the Political Instability Task Force (PITF) – a panel of analysts that use 'statistical methods to forecast instability events across the world', Laitin identifies 'a gap in American foreign policy-making that demands filling'. Here is a passage that is worth quoting at length:

While the statistical models produced by the PITF have garnered praise from high-level policy makers, they seem to play little or no institutionalized role in assessing expected returns from differing policies when an instability onset is forecast or actually occurs. This gap should be filled by a team of strategic analysts capable of using the statistical models of the forecasters and applying that knowledge for purposes of modeling how differing treatments (aka policies) could lower the probability or reduce the impact of an instability event that could threaten U.S. interests. What about a government agency staffed by academic analysts experienced with quantitative data and also with strategic thinking about foreign policy, modeled after the Council of Economic Advisors, whose job it would be to advise the president on probabilistic effects of differing policy courses?

The rest of Laitin's commentary provides two empirical examples to elucidate how academic studies of crisis bargaining and compellence could offer 'the president [...] policy alternatives that combine theory and statistical probabilities'.

Although Laitin and the other IR theorists quoted above differ over the methods employed, over the goals of theorizing, and over what counts as knowledge, they all still share, with different degrees of emphasis, the idea that the scientific study of politics should influence policy. Let me be clear about what I am not arguing. I am not arguing that these theorists are representative of the entire naturalist-oriented scholarship in IR. Nor do I contend that erudite and highly sophisticated scholars such as Laitin, Lake,

Tetlock and others advocate a naïve scientist conception of the theory-practice relationship. In the work of these eminent students of international politics there is no trace of what Lester Frank Ward (1893: 313) termed *sociocracy* (i.e. the belief that policies should be in the hands of social scientists). My more modest claim is twofold: firstly, I wish to emphasize that the belief in the direct application of ‘scientific’ knowledge to political reality is more widespread than expected; secondly, that the assumptions and limitations of such a view need to be scrutinized in detail.

A double critique of the theory-policy nexus

Various objections have been raised to the very possibility of an applied social science. At least two of these critiques should be mentioned here. In the 1940s Friedrich A. von Hayek (1945) labelled this belief in applied social science as ‘constructivist rationalism.’ Hayek’s critique of central planning was not directed solely against communism, but also against all rationalist illusions about the power of human reason (Hayek, 1979). At the very time that Hayek was writing, Hans J. Morgenthau (1957 [1946]) condemned the scientist attitude to the study and conduct of foreign policy. In particular, he criticized the belief that politics could be understood and, then, possibly controlled scientifically. Both of these eminent intellectuals were critical of the ideal of organizing politics upon the principles of social scientific knowledge.

There are two main reasons why the notion of applied social science still appears to be unsound. The first one has to do with the need for policy-making to rely on some degree of prediction (Merton and Lerner, 1951: 304; Chernoff, 2009; Mearsheimer and Walt, 2013: 436). As Herbert Simon (2001: 32, 60) explained, while ‘basic’ science describes the world and makes generalizations about collected phenomena, ‘applied’ science is grounded on the predictive power of the knowledge we possess⁶. Indeed, choosing one policy rather than another means having some expectations about the effects of the policy itself. As Bueno de Mesquita (2009) rightly contends, you can shape and engineer the future only if you are able to make accurate predictions.

However, even if one overlooks the fact that IR scholars have systematically tried (Gaddis, 1992-1993: 10)⁷ but failed to predict major events such as the Iranian Revolution, the peaceful end of the Cold War, the Iraqi invasion of Kuwait, and, more recently, the Arab Spring, available studies show that experts are not much better at predicting future outcomes than common people. In his *Experts Political Judgment*, Philip Tetlock (2005) has shown that experts, largely political scientists, area study specialists, and economists working in academic and non-academic institutions, are not very good at predicting future developments. They are better than the ‘unwashed masses’ (i.e. Berkeley undergrads), but no better than relatively simple statistical procedures and attentive readers of newspapers. Moreover, Tetlock found that knowledge of a specific issue might make one a better forecaster, but being a specialist can actually reduce the

ability to predict future developments: ‘we reach the point of diminishing marginal predictive returns for knowledge disconcertingly quickly’ (2005: 59). That leads to a paradoxical situation in which more knowledge means a lower capacity of being a reliable forecaster. Tetlock’s conclusion is quite depressing for those scholars who wish to influence policy-making: ‘In this age of academic hyper-specialization, there is no reason for supposing that contributors to top journals – distinguished political scientists, area study specialists, economists, and so on – are any better than journalists or attentive readers of the *New York Times* in “reading” emerging situations’ (2005: 233)⁸.

If the ability to predict is a necessary requirement for an applied science but IR scholars are only slightly better at predicting than the general public, why should they be credited with a privileged epistemological standpoint in the policy-making process? Here, I am not suggesting that policy-makers can master political subjects better than scholars; as Henry Nau (2008: 636) points out, ‘neither profession can make a superior claim to social knowledge.’ Nor do I contend that prediction is impossible. In fact, some limited prediction is possible but, again, experts of international politics are not much better at forecasting than practitioners and laypeople.

Although failure to predict might not pose any problems for a scientific study of politics – as explaining (Singer, 1990: 74; Leggold and Nincic, 2001: 89; Keil, 2010) or scenario analysis (Bernstein et al., 2000) can be sufficient goals⁹ –, it does have detrimental implications for a social science that aspires to advice, drive and change

politics. Advising, designing and planning are all based on the assumption that social scientists are much better at prediction than the general public. Yet, such a claim is, at present, unsubstantiated.

The second reason why the argument for an applied IR theory seems misguided is concerned with the issue of the knowledge generated within the discipline. In particular, there is no agreement among scholars about what we know and how international politics works. Even if we neglect the problems concerning prediction and go for a pragmatic attitude to political knowledge – an attitude by which the latter is simply what investigators agree upon (Peirce, 1992: 138-139; Friedrichs and Kratochwil, 2009) –, scholars do not agree about what we know and about the best means to reach political goals. Interestingly, disagreement on these important areas can be found not only between different epistemological approaches, but among like-minded scholars working within the same theory of knowledge and even within the same school, tradition, research program or paradigm.

The problem does not lie solely in the fact that there are several theories aiming to explain one and the same political phenomenon, but rather in the fact that it has become impossible to establish the scientific validity of the knowledge produced in the discipline. Despite the presence of agreed-upon rules to determine theory acceptance (Vasquez, 1997; Christensen and Snyder, 1997; Hopf, 1998; Legro and Moravcsik, 1999; Elman and Elman, 2002; Bueno de Mesquita, 2004; Lake, 2013), there is still sharp disagreement

on the progress of IR. Indeed, the widespread (though not universal¹⁰) adoption of the criteria suggested by Imre Lakatos (1970) – *the methodology of scientific research programmes* – has not generated consensus on the progress in terms of knowledge achieved by the discipline.

If some of the main theorists of some of the main schools of IR claim that their research programmes are all progressive (Di Cicco and Levy, 2003; Keohane and Martin, 2003; Lee Ray, 2003; Moravcsik, 2003) and, therefore, all scientific and cumulative, then one could infer that none of them is probably truly progressive, at least in Lakatos' terms. For, judgment on progress is for Lakatos not simply a contest between theory and empirical evidence, but rather a three-cornered contest between competing theories and empirical evidence. Moreover, besides awarding a promising future to their research programmes, some of these scholars have also claimed that their rivals' theoretical programmes are 'degenerative' and, thus, outside the scope of science (Vasquez, 1997; Legro e Moravcsik, 1999).

Thus, not only meta-theoretical debates about epistemology and ontology appear irresolvable (Monteiro and Ruby, 2009; Sil and Katzenstein, 2010a: 417), but also empirically testable questions are rarely resolved. Indeed there is no consensus on what causes war, on the economic and political sources of democracy, on when states should intervene abroad, and on other major issues concerning international politics. The debate over these questions seems interminable¹¹.

The fact that strong disagreement exists in the hard sciences too is not good news for IR theory. Although scientific research always implies the assumption of fallibility and the knowledge produced is often subject to change, that does not mean that the natural and social sciences are essentially identical. Dismissing the differences in accuracy, prediction, and control between natural sciences and social sciences as ‘mere matters of degree’ is an old but untenable strategy in order to defend the scientific study of politics (Crick, 1959: 218). Indeed, differences in degree might soon turn into differences in kind.

The modest success of IR theory in developing a body of policy-relevant knowledge barely comparable to the one generated by the natural sciences suggests that the so-called scientific method has not yet been able to produce remarkable results when employed in the study of international politics. Hayek’s and Morgenthau’s critiques to applied social science still appear to be valid, though many students of international politics think and write that such an unsuccessful record does not question the possibility of an applied political science.

If no theory or method can deliver any truly predictive knowledge of international politics, then policy must be insensitive to theoretical and empirical findings. This is why, despite being concerned with relevant political issues and emphasizing any potential dangers and mistakes, IR theory should not directly inform policy. French statesman Georges Clemenceau famously remarked that ‘war is too important to be left to the

generals.' Paraphrasing Clemenceau, I would argue that *politics is too important to be left to political scientists*.

Suggesting that policies are to ignore scientific conjectures, however, does not imply that there should be no role for IR theory and no point to theorize about international politics and foreign policy. Being practically relevant does not equate to directly affecting policy. The argument developed here does not deny the importance of IR scholarship which, far from claiming any direct influence of a scientist kind, can have practical relevance in two different functions.

The first one refers to the role of theory in political judgement. In particular, I would argue that theory is an important tool for the intellectual development of policy-makers. From this viewpoint, no broad line of demarcation should be drawn between the practitioner and the scholar. The second function, on the other hand, is concerned with IR scholarship as a whole and involves its unintended effects on policy-making. Such influence is indirect, yet no less important. Here I contend that a broad line of demarcation should be drawn between decision-making and IR theory. Despite these two implications might appear to be inconsistent at first sight, as I will try to show in the next two sections, they tend to reduce the scholar's role but not the function of scholarship.

International Relations as a tool of self-education

While at present point prediction appears impossible in international politics, what about the most common type of forecasting in social science – that is, probabilistic predictions? Apart from Bueno de Mesquita, perhaps, no scholar would claim to have developed the right formula for forecasting future outcomes. Proponents of statistical models, for example, would argue that the predictions they make are probabilistic and the variables they employ are ‘probability-raisers’ (Grynaviski, 2013: 824).

In relation to the theory-policy nexus, however, facts and figures cast in probabilistic terms cannot solve the dilemmas of policy. Although scholars might be content with knowing that there is a certain relationship between variables, policy-makers cannot act according to probabilistic propositions in the particular, individual cases they have to face. In matters of war and peace, for instance, where many lives may be at stake, the error term is something that cannot be ignored: ‘How, for example, can the cost of thinking rather too well of a particular speculation within pure theory be compared to the pain, sufferance and death which follow errors in the application of theory?’ (Collingridge and Reeve, 1986: 34).

Practitioners are not interested in distinguishing between approximations and exact results not because they fail to understand the epistemological limits of social sciences and the complex nature of political reality, but because they often face issues and circumstances that are unique. Since a variable ruled out of a theory on account of its

rare or scarce influence on a particular phenomenon might have a major effect under specific circumstances, generalizations are of little help to practitioners.

Likewise, knowledge of causal mechanisms and processes, highly valuable for understanding how variables are connected to one another, does not solve the problem of whether the case a policy-maker is facing is either a particular case of a general class of events or a contingency characterized by unique features¹². Thus, general propositions about causal chains and mechanisms are also of limited use for policy-making. As George and Bennett (2004: 277) acknowledge, 'No theory or systematic generic knowledge can provide policy specialists with detailed, high-confidence prescriptions for action in each contingency that arises. Such policy-relevant theory and knowledge does not exist and is not feasible.'

By clarifying the problems and risks involved in certain situations, theory can contribute to informing policy. But scientific knowledge cannot replace political deliberation; many instances in international politics are so unique that the idea that generalizations can be employed to conduct foreign policy appears misguided¹³. What 'makes men foolish or wise, understanding or blind, as opposed to knowledgeable or learned or well informed', writes Isaiah Berlin, 'is the perception of these unique flavours of each situation as it is, in its specific differences' (1999: 24). To these 'unique flavours' IR conceived as a scientific enterprise has not much to offer. Good political judgment

might not be illusory; the illusion is to think that judgment can be replaced with rational calculation or probabilistic inference.

Acting on the highest probabilities available, indifference to the 'particular', and blindness to the individual circumstances are a very dangerous path to take in international politics. Thus, it is necessary to understand the nature, the structure, and the issues of a particular context regardless of universal formulas and general rules. As Isaiah Berlin argued 'What makes statesmen, like drivers of cars, successful is that they do not think in general terms – that is, they do not primarily ask themselves in what respect a given situation is like or unlike other situations in the long course of human history' (1999: 45). Hence, every situation must be understood in its own distinctiveness and a particular decision should not be the rigid application of a mathematical formula, but rather a deliberation based on critical reflection over the specific situation in which one needs to act.

If the single case is largely insignificant for the social scientist armed with probability distributions, for the policy-maker it is the only thing that really matters. As Sheldon Wolin (1972: 45) put it 'Context becomes supremely important, for actions and events occur in no other setting.' No theory, even when it is well-supported and methodologically well-grounded, can tell statesmen how they should conduct their foreign policy. Such a condition of uncertainty should preclude, as Jean Bethke Elshtain (1995: 277) maintained, 'doctrines of action and rules of thumb. The best diplomats and

presidents and prime ministers do work through a loosely structure conceptual grid but, hopefully for all our sakes, they are not bound up in the rigidity of law like “behaviours”.’ While decision-makers must receive a great deal of information and advice from experts, their final decisions should chiefly remain a matter of judgement which, in turn, is to be informed with a hardly measurable combination of experience, intuition and political reasons, not by supposedly scientific discoveries.

Not coincidentally – I think – Tetlock (2005: 2) found that the best forecasters are those experts who think like Isaiah Berlin’s foxes, those eclectic thinkers who ‘know many little things’, base their predictions on a variety of traditions, and ‘accept ambiguity and contradiction as inevitable features of life’, rather than hedgehogs ‘who “know one big thing,” toil devotedly within one tradition, and reach for formulaic solutions to ill-defined problems.’ ‘Foxes’ are just those thinkers and actors who reject formal generalizations and adapt their knowledge to the conditions of particular situations.

What, then, is the role of theory for practitioners? Rather than prescribe policies, IR scholarship should have a pedagogical role; it should help decision-makers to familiarize with the complex matter of politics. It should be an instrument of self-education meant to widen policy-makers’ mental horizon. IR should play the role that Clausewitz assigned to theory in the education of the military commander:

Theory should be study, not doctrine (...) It is an analytical investigation leading to a close acquaintance with the subject; applied to experience (...) it leads to thorough familiarity with it. The closer it comes to that goal, the more it proceeds from the objective form of a science to a subjective form of a skill, the more effective it will prove in areas where the nature of the case admits no arbiter but talent (...) Theory is meant to educate the mind of the future commander, or, more accurately, to guide him in his self-education, not to accompany him to the battlefield (1976: 141).

Theoretical knowledge is an instrument for intellectual development, not a substitute for political judgment. Thus, policy-makers, while practitioners, should also be students of politics and, paraphrasing William Butler, *no broad line of demarcation should be drawn between the practitioner and the thinking man*¹⁴. IR theorists, on their part, should therefore be reminded about the beginnings of American political science, which was born not primarily as a technique for direct application but as a critical and formative discipline meant to educate political leaders and citizens (Crick 1959: 19-36).¹⁵

More may be better: the political importance of theoretical pluralism

The second function of IR refers to its influence on those general ideas that shape the discussion on foreign policy decisions. In particular, even if scholarship is not supposed to guide the conduct of foreign affairs directly, it can still perform the role that

Collingridge and Reeve (1986), in their analysis of the role of experts in policy-making, labelled as 'ironic.' Rather than provide solutions to problems, IR helps policy-makers to look at problems from different angles, to question causal assumptions, and to challenge simplistic doctrines employed by practitioners in their foreign policy conduct. IR scholarship can contribute to policy and public discussion on how to consider a specific issue, even when there are no precise answers to offer (Wilson III, 2000: 122).

Debates and disputes on international affairs have the great advantage of ensuring that rival explanations and policy advice can be taken into account for each problem (Panebianco, 1999): 'The role of scientific research and analysis is therefore not the heroic one of providing truths by which policy may be guided, but the ironic one of preventing policy being formulated around some technical conclusions' (Collingridge and Reeve, 1986: 32). Indeed, while the general public is very likely to ignore academic debates, there is strong evidence that policy-makers follow research and scholarship on international affairs on a regular basis (Avey and Desch, 2014: 229).

I am not arguing that different theories freely compete in the marketplace of ideas. Nor do I support the claim that theoretical competition is a key requirement for the growth of political knowledge. As we all know well, settlement of political disputes in the public arena are rarely based only on the quality of the argument proposed, but often on the values and the political and economic power behind those arguing. In the political arena arguments rarely confront each other in a fair manner with the purpose of reaching truth

and improving understanding. Actually, they are often used as ammunition for political competition. Theoretical proliferation, the clash of competing arguments, and mutual criticism can neither ensure that the best theories will survive nor prevent major mistakes, but they can avoid errors to become uncontested truths.

The positive contribution of IR, then, does not result from the intentions and purposes of single political scientists, but rather from the involuntary effects of all scholars' contribution. Although social scientists are aware of unintended consequences on political reality (Merton, 1936; Boudon, 1977), they seem to forget that they are part of the same reality they are studying and, as social individuals, they produce unintended effects too. The paradox is that the work of scholars plays this ironic role especially when theorists disagree about the decisions to be made (Panebianco, 1999). As Ralf Dahrendorf (1968: 240, 247) maintained, the 'only adequate response to the human condition of uncertainty (...) is the necessity of maintaining a plurality of decisions patterns, and an opportunity for them to interact and compete (...) Uncertainty demands variety and competition (...) and political conflict, and institutions that provide suitable conditions for this conflict.' In so doing, different theories enable us to look at present and future conditions in different ways in all the three major stages of the policy process: problem recognition, the formation and refining of policy proposals, and politics (Kingdon, 1993).

The existence of a multitude of contradictory theories in the discipline should not be seen as a problem, but rather as a positive and desirable condition. Although pluralism

tends to fragment the IR community, only a scientist conception of political knowledge can regard theoretical diversity as a hindrance to healthy policy-making. Thus, variety of approaches and methods is not only beneficial for intellectual reasons (Smith, 2004; Mearsheimer and Walt, 2013), but for political reasons as well. Indeed, the ‘critical debate is the sole adequate procedure in view of the fundamental limitations on our knowledge’ (Dahrendorf, 1968: 251).

In this light, the role of so-called reflexivist approaches appears crucial for the political and policy debate. Although Fred Chernoff (2009) argues that reflexivism is incapable of producing policy-relevant theories because reflexivist scholars are either uninterested or unable to make predictions, policy relevance should not be mistaken for policy advice. As shown in the previous discussion, policy relevance is a broad notion that cannot be defined just in terms of applied science. Indeed, should we argue that Michel Foucault’s (1965) study on madness was not policy relevant? Actually, there is no doubt that Foucault’s analysis paved the way for a new understanding of the mentally ill leading to more humane practices towards them and to the end of their traditional confinement in society (Bracken and Thomas, 2010). Likewise, considering Alexander Wendt’s idea (1999) that anarchy has no fixed nature but is a social construction produced and reproduced by states practices, shall we label it as a policy irrelevant theory?

Policy relevance should not be restricted to advising decision-makers on what to do or not to do, as it also includes studies of the origin and nature of our value systems as

well as criticism of existing institutions and practices. And in this area of inquiry reflexivist approaches like critical theory, post-structuralism, feminism and some brands of constructivism can greatly contribute to the political debate that precedes decision-making and policy implementation.

The argument of this paper is a call for tolerance and diversity not only for epistemological reasons, but especially for the well-being of the policy-making. IR scholars have still more to offer by asking interesting and important questions, than by coming to final conclusions. As Roger Smith (2002: 43) argued, political scientists should consider themselves ‘as Socrates described himself, more like ‘gadflies’ to the body politic than its hired therapist or servant.’ Despite scholars’ frustration with the theory-practice gap (George, 1993; Jentleson, 2002; George and Bennett, 2004: 263-285; Walt, 2005), this argument implies that theory and policy should remain two separate activities.

It would not be fair, after criticizing the naturalist-oriented attitude to the theory-policy nexus, to conclude this essay without recognizing the merits of this approach. Since Robert Lynd’s *Knowledge for What?* (1939), it has been a recurrent theme in the discipline that excessive emphasis on methods and research techniques has turned political science into a sterile academic activity (Moore, 1953; Crick, 1959; Bull, 1966; Flyvbjerg, 2001). In particular, Lynd was critical of trying to solve the tension between pure and policy relevant knowledge in favor of the former, and passionately urged for an activist social science (Smith, 1994: 157). Similar claims were reiterated not long ago

during the debate prompted by the Perestroika movement (Smith, 2002; Flyvbjerg, 2004; Sanders, 2005).

It is worthy of notice, here, that an important virtue of the naturalist-oriented scholars who endorse the theory-policy nexus – as compared with the theorists who mainly deal with pure knowledge – is their commitment to an IR theory with practical concerns and significance. Indeed, despite the critique raised in this paper, I agree with this body of naturalist-oriented scholarship on two major points: that key political and social problems should be studied and that scholars should not hesitate to play an active role in the public debate. Nevertheless, I believe that scholars entering the policy-making process or the public sphere should not expect their scientific theories to be applied directly.

Conclusion

This paper has not suggested that, since we cannot fully understand political and social phenomena, we should give up the attempt to comprehend them through the use of scientific methods. It would be mistaken to abandon the effort to turn the study of politics into a science; no one is in a position to deny that in the future both nature and politics will be successfully studied with the same research techniques. In the absence of cumulative knowledge about international affairs, however, it would be premature to

suggest today that there should be convergence towards a particular method of analysis. Likewise, it would be mistaken to argue against the practical relevance of those approaches that refuse to study politics with the methods of science, such as normative theory. Actually, there might be more than a grain of truth in Reus-Smit's suggestion (2012: 538) that the 'persistent bifurcation of analytical and normative inquiry is arguably the greatest impediment to IR being practically relevant.'

The fact that disputes over what values and goals policy-makers should pursue are intensely political and are likely to be endless does not mean that such exchanges should not take place within the discipline. The strength of reflexivist approaches, which inquire into and challenge what we take for granted in terms of values and goals to be pursued, is just an explicit commitment to normative concerns. Ultimately, the main problem with naturalism is not its limited success in generating a genuine scientific knowledge of politics, but rather with what it excludes: an investigation on those moral presuppositions that can give a rational significance and purpose to empirical findings.

I am not sure if these arguments support Feyerabend's (1975) maxim 'anything goes,' but they certainly support the epistemological and political liberal view that pluralism must flourish. Hence, theoretical and methodological diversity should be encouraged. The only acceptable and healthy protection from poor IR theory and, most importantly, from bad policy is not the silencing of particular approaches or the convergence towards a particular methodology, but a mutual criticism that avoids ending

up in intellectual standardization. Although a dialectic link between theory and practice should characterize the interaction between the two, the relationship should not be resolved in terms of synthesis. In other words, the gap between scholarship and policy-making is not to be bridged.

IR theorists' public duty – starting and contributing to public debates – can be better developed when scholars are aware that they have no demiurgic role (Panebianco, 1999). Whether scholars are naturalists or not, the most important contribution that IR theorists can make to foreign policy and international politics is neither 'speak truth to power' (Morgenthau, 1966; Wallace, 1996) nor 'discipline power with truth' (Krasner, 1996: 125), but rather *discipline power with a variety of truths* and encourage the self-education of practitioners. These are the two main functions that IR theory can perform for the policy-making. Whether these arguments are applicable to political science more generally is an issue that has not been debated here and that is left open for discussion by other scholars in their respective intellectual domains.

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Notes

¹ In the context of IR, such an approach has often been described as positivism and the approaches that reject the possibility to study politics with the methods of science have been termed as *post-positivist*. However, the term naturalism appears to be preferable for two main reasons. Firstly, the label positivism is extremely ambiguous: it might refer to the original French positivism of Saint-Simon and Comte, to logical positivism, or to neo-positivism. Secondly, many scholars who are described as positivists refuse, for good reasons, such a label. For example, Waltz (1997: 913-914), who is generally portrayed as a positivist, rejects the typical positivist distinction between theory and observable facts. Moreover, Waltz (2003: ix) describes the conception of science found in King, Keohane,

and Verba's *Designing Social Inquiry* (1994), based on Popper's reworking of logical positivism (McKeown, 2004: 140), as 'of the medieval variety.'

² It should be noted, however, that critical theories might also have both explanatory (Dunne *et al.*, 2013: 410) and predictive goals. For example, Chris Brown (2013) advocates the development of a 'critical problem-solving' theory.

³ However, since the research techniques in the natural sciences might differ significantly from one area to another, naturalism does not prescribe one single method but rather a variety of methods. For example, social science theories might be tested in different ways: from controlled experiments to observations, which can either amount to quantitative or qualitative analyses or both (i.e. the multimethod approach). Finally, it should be noted that a naturalist attitude is compatible with both formal and non-formal theories.

⁴ David Easton (1950) used these expressions to describe Harold Lasswell's work.

⁵ I will return to Tetlock's analysis on expert political judgment later, but it is convenient to note here that Tetlock's conclusions from his findings on the limits of expert political judgment differ from mine, as clearly shown by his aspiration to a 'technocratic assistance democracy' (2010: 476).

⁶ Simon (2001: 32) provides the following definition of applied science: 'Laws connecting sets of variables that allow inferences or predictions to be made from known values of some of the variables to unknown values of others. Inferences and predictions can be used, in turn, to invent and design artifacts (*e.g.*, arches) that perform desired functions

(support the weight and other stresses placed on them), or to anticipate and adapt to future events on the basis of knowledge about the present and past.’

⁷ In a review of scholarship on the Cold War, the eminent historian John Lewis Gaddis (1992-1993: 10) emphasized the importance of prediction for IR theory: ‘the major theoretical approaches that have shaped the discipline of international relations since Morgenthau have all had in common, as one of their principle objectives, the anticipation of the future (...) The role of theory has always been not just to account for the past or to explain the present but to provide at least a preview of what is to come.’

⁸ Bueno de Mesquita (2009: xix) contends that his game theoretical model, based on the inputs of area specialists, proved accurate in real forecasting situations about 90 percent of the time. He quotes a ‘declassified CIA study’ (see Feder, 1995) to support this data. What this rate really signifies is, however, unclear. Indeed, we do not know how the result was measured and what exactly was predicted. This is a serious problem for any scientific analysis since, as Tetlock (2009: 66) argued, ‘A 90 percent hit rate is, for example, no great achievement for meteorologists predicting that it will not rain in Phoenix. And it is no big deal even to achieve a 100 percent hit rate of predicting X—no matter what X may be—if doing so comes at the cost of an equally high false-alarm rate. Anyone can predict every war from now until eternity by simply predicting war all the time.’

⁹ If the purpose of theory is to understand how a phenomenon works the way it does, then the ability to explain is more important than the success in predicting (Waltz, 1997: 916).

On the importance of explanation, David Singer (1990: 74) maintained that ‘despite the folklore to the contrary, *prediction* is neither the major purpose nor the acid test of a theory; the goal of all basic scientific research is *explanation*.’ This is an important point because one might have the ability to predict a phenomenon without having an explanatory theory (Winch, 1990: 115). This is particularly true about statistics, in which predictive and explanatory models are differently conceived (Shmueli, 2010). Still, there is no way out: ability to predict is a necessary requirement for applied science. As Wendt (2001: 1042) spelled it out: ‘*any* theory, rationalist or constructivist, that only explains past choices will be of limited value in making future ones’.

¹⁰ See, for example, Walt’s (1997) and Schweller’s (2003) rejections of Lakatos’ model of scientific progress. Patrick Thaddeus Jackson and Daniel Nexon (2009) contend that neither approaches, such as realism, liberalism, and constructivism, nor particular areas of inquiry, like the democratic peace, qualify as research programmes. Sil and Katzenstein (2010b: 6-7) have, instead, suggested referring to Larry Laudan’s concept of ‘research tradition’ rather than Lakatos’ methodology. However, as Andrew Bennett (2013: 464) has recently argued, Laudan is quite ambiguous on the problem of theoretical progress and theory choice.

¹¹ There is of course a well-known exception. A variety of scholars, including Robert Keohane (2005) and, more recently, David Lake (2013), suggest that the democratic peace theory is a clear example of progress in terms of knowledge. Doubtless, this theory

is not only a well-supported generalization, but it might also be seen as one of the most interesting findings in contemporary social science. However, the policy value of the theory is not as evident as one might expect. Although even this theory enjoys no consensus among scholars – since it has been criticized and explained alternatively as a historical accident largely produced by Cold War dynamics (Farber and Gowa, 1997; Rosato, 2003) or as a by-product of market economies (Gartzke, 2007) –, the main problem with the democratic peace thesis is that its important scientific findings appear inconsequential for foreign policy. Indeed, do we support and sometimes promote democratic institutions abroad because democracies do not fight each other or rather because we believe that liberal democracy (with all its limitations) is the most humane and just political regime? The desire to expand the number of democracies in the world does not seem to result from the finding that democracies do not wage war on one another. While it is true that in the 1990s the Clinton administration framed the move from a strategy of containment to one of enlargement of democracies also in terms of this empirical law (Lepgold and Nincic, 2001: 114-115), democracy promotion abroad, though erratic and shifting according to other interests at stake, has been part of the American foreign policy agenda since World War I. In fact, the recent proposition that emerging democracies are especially likely to go to war (Mansfield and Snyder, 1995, 2002) has not prevented the United States and part of the international community from trying to establish democracy in places like Iraq and Afghanistan. Moreover, even if we

should find out that democracies are not so peaceful with each other, there appears little ground for doubt that the promotion of democracy, where possible and at acceptable costs, will be likely to continue because we see it as an intrinsic good. That is why, though the democratic peace theory has strong prescriptive implications, its findings are not that relevant for democracy promotion abroad, which is driven by values and political interests rather than scientific discoveries.

¹² Although an explanation based on causal mechanisms is often seen as an alternative to the account provided by the nomological-deductive model, which explains a certain event as a special instance of a general proposition, as Leggold and Nincic (2001: 38) correctly noted, the explanatory value of mechanisms ‘assumes that we have in mind some general and law-like statement relating the mechanisms in question to the occurrence of some event (...) we cannot assume that the mechanisms leading from cause to effect operate in an ad-hoc manner.’

¹³ It is often noted that the use of theory is unavoidable for practitioners too (Leggold and Nincic, 2001: 40; Nye, 2008: 648). When policy-makers decide which facts are important and which events are negligible, and when they choose which policy should be pursued and which instruments should be employed, they do so against some explicit or tacit theoretical background (Walt, 2005: 28). The question, however, is not whether theory is avoidable in practice, but whether IR theory should be employed in the conduct of foreign policy.

¹⁴ The following is Butler's original sentence (1889: 85): 'The nation that will insist upon drawing a broad line of demarcation between the fighting man and the thinking man is liable to find its fighting done by fools and its thinking by cowards.'

¹⁵ In a recent survey on what former senior national security decision-makers expect of IR theorists, Paul Avey and Michael Desch (2014: 237) found that only 54 percent of respondents think that scholars should serve as 'trainers of policy makers.' Although the percentage seems to challenge the pedagogical role of IR theorists, such an attitude, as Avey and Desch (2014: 237) suggest, is likely to result from 'the curriculum and content of much graduate professional education in international affairs,' namely from too much emphasis on quantitative and formal social science. From this viewpoint, a reassessment within the IR community on how students are prepared for careers in governmental and international institutions should definitely take place.

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