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BLESSED AND AWARDED ARE THE PEACEMAKERS? AN ESSAY IN REMEMBRANCE OF HAROLD JACOBSON

*J. David Singer**

As colleagues in the World Politics program for better than forty years, it should come as no surprise that Harold Jacobson and I knew one another's priorities fairly well. While we were not very close personally, we were nevertheless quite close professionally. We both worried about and lamented the turns that world politics took in the wake of World War II, and while he was never quite as critical as was I in evaluating U.S. policies, we shared the same concern over the extent to which the "superpowers" ignored or even weakened the United Nations system. We also both felt the desirability for greater scholarly and political attention to the needs of the larger global community, embracing not only problems of war and peace and international law and organization, but also those of human rights, sustainable development, and the material environment. In sum, we both believed that scholars need be as concerned with public policy and the state of the global village as with our teaching and our research.

From that concern, it was a short step to consider what we, other scholars, and public intellectuals in general ought to be doing to make the world a more peaceful and humane place. This certainly extended to our joint willingness to speak with and chair student and citizen groups, organize lay conferences, and subject ourselves to interviews with the media. Jake was no "ivory tower" academic nor was I, and one consequence was that we cared how the larger society observed, understood, and evaluated those of us engaged in the study and practice of global affairs. Among the manifestations of that interest was the matter of recognition—friendly or otherwise—of those so engaged. Not surprisingly, then, that with each year's announcement of the Nobel Peace Prize by the Norwegian Storting, we would either bask in the warmth of our shared pleasure, or more frequently, commiserate over the inadequacy or worse of those worthies. I should like, thus, to offer some informed

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thoughts about that issue, and while I do not recall showing my little essay to Jake, I'm sure that he'd be in general agreement.

Given the ambiguities of Alfred Nobel's will, and the varying interpretations assigned to it by the Norwegian Nobel Committee over the years, it is not surprising that the Peace Prize has gone to so diverse a range of individuals and groups, and in recognition of so staggering an array of acts that allegedly "have conferred the greatest benefit on mankind."¹ And as Abrams suggests in his excellent paper on the transformation of the prize, it probably is "all to the good that the Committee has held no one theory of peace . . ."² At the same time, it is difficult to disagree with Albert Einstein's comment in 1935 that the list of recipients would make "good old Nobel . . . turn in his grave."³

The basic theme in this paper is that while no single "theory" on the causes of, or the remedies for, war or other acts of collective violence is necessary, we ought to have some general notions as to: a) which social ills the Prize is intended to ameliorate; and b) what sorts of actions are most likely to further that objective. I proceed from the premise that: a) the most persuasively destructive social ill of the past two centuries has been and remains collective violence, with war the most salient; and b) that until the legitimacy of, preparation for, and conduct of war have been effectively challenged, all too many of humanity's afflictions will remain irremediable. Space limitations preclude an adequate defense of these premises, but it should be admitted that even with a systematic marshalling of empirical evidence, logical reasoning, and ethical exhortation, they would remain mere assumptions, and controversial ones at that.

Returning to my agreement with Einstein, it seems that the criteria invoked and the decisions taken by the Storting Committee since 1901 have led to two undesirable outcomes. First, the prize has all too often gone to those who have either been instrumental in the perpetuation of international war or at least acquiescent in the recurring process. This would include virtually all of the so-called statesmen, from Theodore Roosevelt and Bourgeois through Kissinger and Tho to Begin and Sadat in the first group, and the Red Cross as indicative of the second group. At a minimum, these selections have helped perpetuate the dubious distinction between just and unjust war making, with the corollary that most wars are wars of aggression, a most dubious set of propositions. Equally serious, the awards, with few exceptions, cannot but have helped to per-

1. The Nobel Foundation, Statutes of the Nobel Foundation § 1, available at <http://www.nobel.se/nobel/nobel-foundation/statutes.html> (last modified Feb. 28, 2003).

2. Irwin Abrams, *The Transformation of the Nobel Peace Prize*, 10 PEACE AND CHANGE 1, 21 (1984).

3. EINSTEIN ON PEACE 266 (Otto Nathan & Heinz Norden eds., 1960).

petuate public confusion as to where we might begin to look for the origins and antecedents of war. A concerned citizen of the world, contemplating the identity of almost every year's recipient, could be forgiven if he or she concluded that the roots of international war lie in hunger and poverty, militaristic governments, religious differences, racial bigotry, and suppression of human rights, to name a few. To the extent that these two interpretations are more or less reasonable, the prize has probably done little to enhance fraternity among nations, no less commence the long struggle to banish war from the global village.

Where does peace research fit in this line of argument? Is there anything in the rationale—or ultimate payoff—of the peace research movement that might help to convert the prize into a more effective instrument for the abolition of war? While recognizing that this movement is far from unified in its normative premises, theoretical predilections, and methodological preferences, there are nonetheless certain fundamentals upon which most of its practitioners would tend to agree. The first of these is that war arises out of a complex combination of conditions and events, some of which occurred long before the first shot is fired and others of which obtain in the hours and weeks just prior to the bloodletting. The second is that some classes of war arise out of dynamics that are different from those preceding other classes of war. Third, they concur that we still have very little knowledge regarding the etiology of war.

Beyond these essentials, there is the expected range of disagreement, on both substance and procedure. As to the former, one's theoretical and ideological position largely determines the types of variables worth attending to. Those who suspect (fear) that governments tend to be driven by considerations of *realpolitik*, will focus on military and industrial capabilities, alliance configurations, and the geopolitical implications of weapons technology and transport. Here is the classical notion of "national interest" defined in terms of physical security, territorial integrity, and spheres of influence.

Those who are closer to the Marxian perspective see governments as more preoccupied with markets, raw materials, cheap labor, investment opportunities, trade routes, and are inclined to focus on these elements in their search for the explanation of war. Adherents of this school often, but not always, go the next step and examine differences in domestic economic and political systems as well as levels of industrialization in their search for explanation, on the plausible premise that for socialist economies such incentives would be very weak and that for underdeveloped ones, there would be insufficient capabilities.

Yet a third theoretical school would tend to downplay either the geopolitical or the economic-industrial variables and attend instead to the

central role of professional bureaucracies driven, as they seem to be, by their own career and agency interests and the extra-rational interplay of these parochial interests. A fourth orientation might focus on mass culture and either the stability or the fragility of public opinion vis-à-vis the dimensions of xenophobia, isolationism, expansion, domestic prosperity, white man's burden, racism, and the like. A fifth example might be psycho-dynamic in its orientation, frequently with a strong interest in the personalities of national leaders.

The literature of world politics in general, and peace research in particular, is chock full of these and additional outlooks, as well, of course, of varying combinations. Speaking for what appears to be the dominant perspective in the West over the three decades or so since peace research became a discernible academic activity, replete with professional associations, journals, conferences, and curricula, I lean toward an eclectic effort to synthesize many of these variables, most promisingly organized around the realpolitik model.

In contrast to the wide range of *theoretical* perspectives, one finds only two basic *epistemologies*. One, often embraced by those of a Marxian bent, finds expression in the philosophy and sociology of Mannheim, who was so impressed with the potency of nation, class, and gender that he dismissed an objective social science of world politics or anything else as a psychological impossibility.⁴ Surprisingly—and despite the obvious fact that all serious scientists recognize the impact of social niche and ideology, while counting on the explicitness of our procedures and the reproducibility of our observations to minimize (but never wholly eliminate) such biases—one still finds adherents of this point of view. The more vehement disciples of the two Karls have even gone so far as argue that they have their own epistemology, which, though it rests upon and leads to assertions that can never be empirically tested, provides the only avenue to social, economic, and political truth.

Needless to say, those of us who believe that matters of war and peace are too important to be left forever to the endless debate between/among arguments that are extremely subjective, idiosyncratic, and mystical, gravitate toward a more quasi-positivistic epistemology. Few of us would go as far as the extreme positivist A.J. Ayer,⁵ but remain in the traditional Comtean mode, accepting the importance of the inductive-deductive mix and recognizing the limits of operationalism; hence, the consensus around the proposition that empirical "truths" and "causal" inferences are, at bottom, inter-subjective.

4. See, e.g., Karl Mannheim, *ESSAYS ON THE SOCIOLOGY OF KNOWLEDGE* (1952).

5. See, e.g., A.J. AYER, *LANGUAGE, TRUTH, AND LOGIC* (1936).

It follows from these two lines of argument—the theoretical and the methodological—that despite the need to be agnostic as to any particular “theory” of war, the question need not remain eternally open. With the formulation of rival models that are logically consistent and empirically (but not *necessarily* historically) confirmable (or for the purists, disconfirmable), we can arrive, by a mix of incremental inductive discoveries and bold deductive leaps, successively closer to a compellingly accurate explanation of international war, or at least some large subset of the phenomenon.

This brings us, then, back to the task facing those who must choose the appropriate recipients of a peace prize and, minimally, specify some of the criteria guiding that choice. That is, they must embrace, however tentatively, the theoretical model that seems most accurate at the moment, and most relevant to their historical period (if indeed, the parameter-shift problem across time turns out to be acute). But to do that, while being consistent, requires them to evaluate two other dimensions of the contending models. One and the simpler, is the quality of the empirical and deductive evidence proffered in support of the various models; they must become scientifically competent enough, or turn to those who are, to go beyond either plausibility or popularity.

The other, perhaps more difficult, is to find in the model of their choice, the role of knowledge and action. This can be complicated, but is far from insuperable. What is needed, to put it differently, is an acceptable meta-theory, by which we mean a scheme that proposes one or more explanations for war, that prescribes the rules of evidence for evaluating these putative explanations, and at the same time incorporates explicit notions as to how individuals and groups, armed with the “best” explanatory theory, might go about modifying those sets of conditions and patterns of behavior that produced war in the past. Presumably, then, the prize givers could say in good conscience that they think they understand “why” war occurs with such destructive frequency (an average of six international ones per decade since the Battle of Waterloo), why they believe that particular explanation, and how their recipient has acted or might act to enhance the modification process which will reduce the probability of war in the future.

This is, one might say, a tall order, and not one for which the Nobel or other peace prize committees are typically trained. The peace research community has, therefore, a double opportunity to contribute to the reduction or elimination of war. The first and most obvious is the discovery of those conditions and events that best account for the incidence of the several—and as yet unidentified—classes of international war. As our understanding of the etiology of war improves, we can serve

as more knowledgeable consultants and/or critics of the practitioners, and increasingly sensitize them to: a) the quality of the evidence in support of a given model; and b) the need to treat all single cases as members of a *class* of cases, to properly recognize the class to which the case at hand should be assigned, and finally, the adaptive policy response to that case.

The second opportunity is less obvious, but no less important. It is to create incentives for peace prize committees to learn more about the applicability of scientific method to the study of social conflict, to appreciate the connection between knowledge and policy, and thus to use the prize as an explicitly recognized instrument for the eventual abolition of war.

Most scholars take a dim view of academic, literary, and political prizes—and for good reason. The criteria are typically invisible and jumbled, the judges incapable of applying them anyway, the extrarational considerations immense, and the resulting awards a source of embarrassment for many. But peace prizes, and especially the Nobel Peace Prize, offer an interesting challenge to the whole idea of rewarding and/or enhancing merit in one or another sector. By attending to the evidence, and reasoning behind it, the judges could not only rehabilitate the legitimacy of public prizes, but also strike an important blow for turning the nations away from war.

A good place to start the process of political action that could culminate in the abolition of war would be at the point where relevant and critical knowledge so crucial to appropriate political action will be found: the leadership of the peace research community. Clearly, Jake would have been quite comfortable to be included in that category, and his research output has indeed already contributed to achieving that objective. Would that all of our research were as germane to this goal as was his!