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Robert Axelrod. 1986. "An Evolutionary Approach to Norms." *American Political Science Review* 80 (December): 1095-1111  
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Source: *The American Political Science Review*, Vol. 100, No. 4, Thematic Issue on the Evolution of Political Science, in Recognition of the Centennial of the Review (Nov., 2006), pp. 682-683  
Published by: [American Political Science Association](#)  
Stable URL: <http://www.jstor.org/stable/27644412>  
Accessed: 15/03/2011 23:20

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theories, but spoke to the political concerns of the broader public as well.

The academic response to the published version of "Trust in Government" started with Jack Citrin's (1974) insightful "Commentary." Citrin acknowledged that a major contribution of "Trust in Government" was that it shifted explanations of political trust from the earlier focus on social characteristics and personality (McClosky and Schaar 1965) to evaluations of government performance. Whereas earlier theories had suggested that trust would remain stable over long periods of time, "Trust in Government" argued that if discontent accumulated across a set of authorities, distrust of the government institutions and regime would increase. This generalization hypothesis continued to be the focus of much subsequent work inspired by "Trust in Government" (e.g., Lipset and Schneider 1983).

Another set of studies inspired by "Trust in Government" raised questions about whether the focus of the National Election Studies (NES) items measuring trust reflected assessments of current incumbents as opposed to judgments about the system or regime (e.g., Citrin and Muste 1999; Muller and Jukam 1977). My initial interest in trust arose partially out of such measurement concerns.

While a graduate student at the University of Michigan, I served on the 1970 NES survey as a research assistant to Angus Campbell. At that time I already had a strong interest in political alienation, diffuse support, responsiveness, and trust. These interests had developed from my sense of the turbulence of the 1960s, from taking seminars with Gamson and Walker at Michigan, and from the literature on alienation (e.g., Aberbach and Walker 1970; Easton 1965; Gamson 1968). As a graduate student I constructed a series of survey questions that I felt would make valid measures of diffuse support. Although the 1970 study did not have enough space to accommodate the new items, I lobbied successfully to have all the trust items included in the study (the 1966 study had only two items) and subsequently used them for "Trust in Government." Although measurement questions continue, time and many other studies (including my own reports in Miller 1974 and Miller, Goldenberg, and Erbring 1979) have demonstrated the importance of the trust items as leading indicators of deeper discontent and support.

As levels of trust continued to decline in the United States, an explosion of studies addressed questions raised in "Trust in Government," such as the implications for compliance and for voting or participation. Other studies raised additional explanations for declining trust, such as the rise of television and its cynical messages about politicians. Similarly, the decline in trust in the United States stimulated comparisons with other countries (e.g., Miller and Listhaug 1990). More recently, the durability of distrust prompted tests of the hypothesis that distrust influences policy preferences.

"Trust in Government" made a contribution to political science because it added to conceptual, empirical, and normative theory building regarding the relationship between citizens and government. It presented the first evidence of what was to become a long-term trend of growing political distrust among U.S. citizens. It shifted the focus of scholarly discourse on explanations of political distrust from slowly evolving social and personality factors to political considerations of government performance. It raised a series of hypotheses relevant to investigation by a wide array of related substantive disciplines including comparative studies, sociology, mass communications, psychology, and law. Many of the conclusions reached by "Trust in Government" were controversial, thereby provoking subsequent scholars to examine these issues from different perspectives, but this is exactly how the science of politics is advanced.

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—Robert Axelrod, *University of Michigan*

I have long been interested in the question of how cooperation can emerge in a world of egoists without central authority. Over a period of five years, culminating in my book on the *Evolution of Cooperation* (1984), I published a series of studies that explored the emergence and maintenance of cooperation in the context of the two-person iterated Prisoner's Dilemma (PD). I was well aware that two-person interactions can tell only part of the story of cooperation in societies, so I thought about various ways of building and sustaining cooperation when one person's actions can affect many others. Unfortunately, the most straightforward way to extend the two-person PD game would not sustain cooperation unless something else was added to solve the collective action problem by preventing free riders.

In looking for mechanisms that actually work in societies, I was impressed with the power of social norms to maintain specific types of cooperation. For example, the norm against cheating can be sustained by individuals who punish cheaters whenever they are detected. The problem is that punishment is typically costly to the punisher even if

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beneficial to the community. In the paradigm of egoists without central authority, no one would want to pay the private costs of punishing a violator, and soon the norm itself would collapse. My article proposed that one way to build and sustain cooperation in such a setting would be for individuals not only to punish the norm violator but also to take vengeance on anyone who did not punish the norm violator. I explored the dynamics of this situation with an agent-based model in which relatively effective strategies would be used more than less effective strategies in the future.

The principal result of this evolutionary approach was that if the level of vengeance was initially high enough, selfish acts (such as cheating) would be suppressed. To be successful you had to punish defectors lest you be punished for *not* doing so. The simulations of the agent-based model demonstrated how and under what conditions cooperation would emerge in a setting where large numbers of people are affected by each selfish act.

The article also informally discussed social norms in relation to other mechanisms for sustaining cooperation in groups, such as law, power differentials, internalization, social proof, and voluntary membership. For most readers, this informal discussion of how various mechanisms complement each other was probably far more useful than the formal model. For example, the article discussed how a law is likely to be effective when supported by an existing social norm, whereas the existing norm, in turn, is reinforced by the law.

This article proved far more compelling than I expected it to be. For example, it was mentioned (along with my two-person work) in the citations for two awards I won shortly thereafter, a Five-Year McArthur Prize Fellowship and the National Academy of Sciences Prize for Behavioral Research Relevant to the Prevention of Nuclear War.

It is hard to evaluate the influence of this work. The formal modeling of social norms did pick up markedly after this article was published in 1986, but several earlier works also promoted this trend. What is clear is that my terminology of “metanorms” did *not* catch on, and the concept had to be “reinvented” under other labels such as one of the (many) meanings of “indirect reciprocity.” The influence of the model is perhaps more clear in the growing literature on agent-based models and evolutionary approaches to game theory. I attribute much of whatever influence it did have to my decision to treat norms in behavioral terms, rather than rely on the standard definition of social norms in terms of beliefs and values.

Another aspect of the article’s influence was its relevance to a wide range of problems in political science and beyond. Within two years of publication, it was cited not only in political science journals, but also in journals of economics, sociology, law, philosophy, anthropology, and animal behavior.

The article has apparently stood the test of time. Unlike the typical scholarly article, which receives fewer and fewer citations after the first four or five years, the annual citation rate for this article shows no signs of declining after 20 years. The citations in the second decade after publication actually exceeded the citations in the first decade by 30%.

I attribute much of the attention this article has received to its role as an extension of my earlier work on pair-wise cooperation. More generally, both projects benefited from the widespread desire to provide a “hardheaded” rationale for cooperation.

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**16. Michael Doyle. 1986. “Liberalism and World Politics.” *American Political Science Review* 80 (December): 1151–69. Cited 311 times.**

—Michael Doyle, *Columbia University*

“Liberalism and World Politics” had an impact because it is short and (by my standards) well written. I am sure its brevity has attracted anthropologists. The more substantial reasons for its impact seem to me to be the following five:

First, the article addressed an important anomaly. Traditional theories of democratic foreign policy said that democracies would inherently be peaceful. The mainstream of the literature on war and peace refuted that claim. A group of carefully done empirical studies suggested that democracies were peaceful, but only with one another. This article resolved the anomaly by outlining the logic of how states that were both liberal and representative would maintain peace with one another, but not reliably with nonliberal, nonrepresentative states. It argued that although states throughout history have gone to war with one another to advance both their rational national and nonnational governmental interests, liberal democratic peoples have, by and large, respected one another’s sovereignty and security.

This argument was inspired by Kant’s “Perpetual Peace,” (Kant 1970) which held that liberal representative republics do not wage war against one another (though they sometimes do go to war against nonrepublican peoples). The reasoning was that liberal republics are responsible to the majority of electors, who, unlike monarchs and dictators, cannot regularly displace the costs of going to war on others. If, moreover, those electors respect liberal principles, then they will respect the rights of peoples similarly free to express their rights, and will negotiate rather than fight over differences of interest. Respect for rights of property and the benefits of commercial exchange will reinforce these moral commitments.

But the same respect and self-interest that generates a self-enforcing peace among liberal republics produces distrust of states that oppress their subjects and compound conflicts of interest between liberal republics and nonliberal autocracies that neglect property rights and restrict trade and other contacts. This is what makes for a peace among liberals, but not between liberal and nonliberal states.

Second, it was interdisciplinary. It combined the international relations of war and peace with political and social philosophy. It drew on Machiavelli, Schumpeter, and Kant to distinguish imperial, pacific, and internationalist variants of popular republicanism.<sup>1</sup>

Third, it bridged methodological divides. It reflected my long fascination with Carl Hempel’s (1942) “The Function of General Laws in History” and yet it gave a central place to interpreted, inter-subjective ideas and norms as causal factors. It lent itself to both quantitative and qualitative assessment. It argued that significant insights could be produced by the combination of institutions (representative republics), ideas (human rights), and interests (commerce, investment) that did not produce significant outcomes in isolation.

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<sup>1</sup> I later changed my mind about how to interpret Machiavelli’s views (see Doyle, 1997, Chapter 2).

<sup>2</sup> I sent the latter a paper on how to preserve, protect and expand the community of liberal democratic states, published by Doyle in 1992.