
Overlooked Costs of War-Related Public Research

*A Comment on “Early RAND as
a Talent Incubator” by Nicholas
Rescher*



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In “Early RAND as a Talent Incubator: An Extraordinary Experiment,” Nicholas Rescher argues that the first decade of the RAND Corporation (1948–1958) was a unique and extremely successful experiment in talent cultivation. Rescher’s article is focused largely on the distinct structure of RAND and how it created an environment where young, talented scholars were able to pursue their interests with few constraints. The main indicators of the success of this experiment, he argues, are the innovations made by some of those working at early RAND as well as the professional accolades they received for their contributions. Rescher’s article offers not only the opportunity to consider the early days of the RAND Corporation from a purely historical perspective but also the chance to reconsider the common beliefs that

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war-related public science advances academic disciplines and that it advances the broader “public interest.”

The logic behind the first belief is that war-related innovations advance the disciplines of those carrying out the research. This theme is central to Rescher’s narrative, as illustrated by his list of early RAND researchers who made contributions to their respective fields of study.

The reasoning underpinning the second belief, that war-related research advances societal welfare, is as follows. Preparation for war focuses government efforts—investment, planning, and so on—and in the process generates significant social benefits beyond the immediate war effort itself (see, for example, Ruttan 2006 and Cowen 2014). Tyler Cowen captures the essence of this argument when he writes, “Fundamental innovations such as nuclear power, the computer and the modern aircraft were all pushed along by an American government eager to defeat the Axis powers or, later, to win the Cold War” (2014). Rescher never makes this argument explicitly, but it is in the background throughout his article. The reader is told, for example, that the early RAND’s organization provides a “striking demonstration of how to develop and energize the creative impetus of able young minds in the service of the public interest,” indicating that the research generated widespread social benefits for American society.

We do not deny that the RAND Corporation brought together an extremely talented group of minds in its early days. Further, we do not deny that many of these scholars made various contributions to their fields and received professional accolades for doing so. What is striking to us is that for all of the talk of the great advances in knowledge made by war-related research, including supposed contributions to the discipline of economics, there is often a failure to attempt to understand the overarching economics of the matter.¹ Doing so requires an appreciation of Henry Hazlitt’s insight that “[t]he art of economics consists in looking not merely at the immediate but at the longer effects of any act or policy; it consists in tracing the consequences of that policy not merely for one group but for all groups” ([1946] 1979, 17).

Our purpose is to extend this core insight to understand some of the overlooked costs of war-related public research. We focus on two unseen consequences of such research: the first is the distortionary effect of government-influenced research on academic disciplines; the second is the opportunity cost of the resources employed in public science.

It is often assumed that war-related research results in innovations that advance the researchers’ fields. Rescher writes, for example, that the early days of RAND “produced revolutionary innovations in a wide spectrum of critically important areas: not only in military matters such as strategic planning, force development, weaponry, and intelligence management but also in mathematical economics, resources management, computational mathematics, and the theory of games and competition.” Focusing on

1. Exceptions are Kealey 1997, Butos and McQuade 2006, and Klein 2013.

the observable outcomes of war-related research, however, neglects the unseen, distortionary effects of that research on the disciplines funded and influenced by government for national security purposes.

This “distortionary thesis” can be traced back to Paul Forman (1987), who studied the effects of military influence on quantum electronics (see Hounshell 1997, 238–39). Forman found that military spending and influence shaped both the institutions that carried out research on quantum electronics as well as the field of study itself. According to his analysis, government funding led to a reorientation away from research to advance fundamental knowledge in the field and toward meeting the government’s national security goals.

In a subsequent study, Stuart Leslie (1993) traces the influence of military funding on the trajectory of the science departments at MIT and Stanford University. He argues that the military influenced the structure of these departments, the questions they asked, and the research they undertook. In his estimation, these changes were not for the better in that the military influence diminished the “[academics’] capacity to comprehend and manipulate the world for other than military means” (9).

The distortionary effects of government influence are not limited to the hard sciences. Philip Mirowski (2002) documents how the relationship between the military and economists shaped the evolution of the discipline during World War II and throughout the Cold War. Specifically, Mirowski argues that the economics discipline was influenced by those working in the cyborg sciences—operations research and systems analysis—leading to the modeling of economic actors as automatons and markets as a type of computer. These influences on the trajectory of the economics discipline remain today.

As this body of research suggests, government intervention into academic disciplines can have long-lasting, distortionary effects. These effects can occur on a variety of margins, including the structure of the discipline, the type of questions that are asked by those in the discipline, the methods used to address those questions, and the policy implications derived from research. Whether these effects are, on net, positive or negative is open for discussion. The key point is that it cannot be assumed, *ex ante*, that innovations emerging from war-related research, whether during the early days of RAND or at another time elsewhere, are a net benefit in terms of their influence on scientific disciplines themselves. Moreover, disciplinary advances that occur at a specific time—for example, the first decade of RAND—cannot be considered in isolation from their long-term effects on the fields that are influenced. Like other types of unintended consequences, the distortionary effects of government influence on scientific disciplines are often long and variable.

What about the human capital employed in generating war-related public research? As Rescher documents, extremely talented individuals were involved in early RAND. These individuals’ observable successes are presented as indicators of the overall success of the early organization: “RAND employed some 135 research professionals,

and, as the list [the appendix] indicates, about 50 of them achieved truly outstanding success in their varied fields of activity—a truly extraordinary record.”

From an economic perspective, however, the question is not whether war-related research generates observable outcomes but rather the opportunity cost of the resources used to produce those outcomes. The creation of RAND and other war-related research efforts redirected resources—both human and physical capital—away from how they otherwise would have been used. These forgone alternatives need to be taken into account. Listing observable innovations and awards says nothing about the opportunity costs of the resources employed to produce those outcomes.

Rescher attempts to address this point in a footnote (note 6) by suggesting that the discussion of opportunity cost misses the point because his focus is on early RAND’s organizational structure. Given the structure’s effectiveness in this context, he asks, “[W]hy should it not work in others?” But this question dismisses the crucial issue of opportunity cost too quickly. The reason for considering different organizational arrangements is precisely because they offer alternative ways to employ scarce resources to achieve the desired ends. As Rescher notes, early RAND’s unique organization matters because it allowed highly talented employees to leverage their abilities in an unconstrained environment to innovate in the public interest.

But how do we know that the value of their scarce talents was maximized compared to alternative uses? How do we know if the same innovations that took place at RAND would have taken place in the absence of its founding? How do we know that the innovations that took place at RAND did not crowd out superior innovations that would have otherwise emerged? And how do we gauge the waste associated with war-related research? After all, those resources could have been employed in other uses.

The central issue is one that plagues all government-sponsored, public research—there is no market test to determine the true value of resources employed and outputs produced. What is valuable to decision makers in government and the military or to one’s academic peers is not necessarily valuable to private citizens. This is problematic if one assumes that the purpose of public research is to contribute to the general well-being of the citizenry.

Public research falls prey to the same issues that plague attempts to centrally plan economic activity. Decisions need to be made regarding the allocation of scarce resources. Absent the market mechanism, however, these decisions are made based on a set of arbitrary bureaucratic rules, including proxy benchmarks of what is considered success. This is not how innovation works in other areas of life. In market settings, for example, talented individuals experiment with alternative uses of scarce resources (including different organizational forms) and subject their output to the market test of profit and loss, which is governed by consumer choice. This process reveals which resources are, from the perspective of consumers, being used in a value-added manner and which are not. Based on these judgements, resource owners make adjustments accordingly. It is this process that underpins numerous innovations that involve successfully leveraging the unique talents and insights of a multitude of individuals.

There is no analogous mechanism in war-related public research. This is problematic because, as Terence Kealey (1997) has documented, government-sponsored research can be ineffective, wasteful, and counterproductive to the general well-being of ordinary citizens. And as the aforementioned literature on the distortionary thesis highlights, government influence can be potentially harmful to the trajectory of academic disciplines, making the absence of feedback and incentive mechanisms even more costly in the long-term.

Rescher is fully aware of the impossibility of determining which inputs will produce value-added outputs, noting that “one cannot predict which individuals will prove to be statistical outliers.” But this is precisely why the issue of opportunity cost cannot be dismissed. Any discussion of alternative organization forms in the context of advancing the “public interest” must appreciate and analyze the mechanisms for gauging alternative resource uses on two margins. First, mechanisms must exist for determining the value of resources and outputs from the perspective of private citizens, not planners. Second, mechanisms must exist to incentivize the reallocation of scarce resources to correct for errors and changing circumstances in order to maximize the value of these resources as determined by private consumers.

In conclusion, the creation and organization of the early RAND Corporation is an interesting historical episode worthy of study. As Rescher notes, RAND lured young, intelligent people from a variety of disciplines and created a unique intellectual environment for them to engage in research. Some of them achieved great success in their respective fields and received significant professional accolades. In doing so, early RAND and the broader war-related, public-research environment of which it was a part influenced the trajectory of numerous disciplines on a variety of margins as well as the foreign policy of the United States. We argue, however, that the overall effect of this disciplinary influence is ambiguous because of the distortionary effects of military-related research, which are not necessarily positive. Likewise, the effects of the research emerging from RAND on overall societal welfare are indeterminate given the absence of a mechanism to gauge the opportunity cost of resources involved. This goes both for the value of specific innovations and the value (or lack thereof) in terms of making American citizens safer. Finally, as Rescher recognizes, the structure of early RAND was sustainable for only a short period of time and is unlikely to be mimicked today.

For these reasons, we are unable to share his enthusiasm for replicating early RAND. At the same time, it is our hope that his paper will spark an ongoing discussion about the history, nuances, and implications of war-related public research, which remains a significant influence in universities, think tanks, and governments today.

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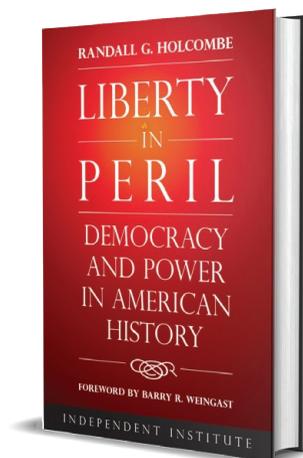
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