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The Economics of Crisis Innovation Policy: A Historical Perspective†

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The COVID-19 pandemic has highlighted the value of innovation in a crisis. Economics has a rich tradition of research on innovation policy, with intellectual roots in Nelson (1959) and Arrow (1962), who identified three features of innovation—indivisibility, inappropriability, and uncertainty—as reasons why research and development (R&D) are likely underprovided in open markets relative to the socially optimal level. In doing so, Nelson and Arrow provided the foundation for 60 years of scholarship on the economics of innovation and innovation policy.

The decades of research that followed would seem to make economics as a field well positioned to advise crisis innovation policy. Yet with its focus on market failures in the supply of (generic) private R&D in normal times, the canonical Nelson-Arrow paradigm that has guided economic thinking on innovation policy may not be the right framework for crisis policy. How are crisis innovation problems different? What can we learn from past crises about optimal crisis policy? Does the economics of innovation policy need to expand its conceptual tool kit to confront the distinctive features of crisis innovation?

In this paper, we use recent and historical experience with crisis innovation to argue that crises put unique demands on the innovation system, some of which are beyond the scope of the Nelson-Arrow paradigm. As a result, optimal crisis innovation policy is likely different from that in normal times. Few examples illustrate this more than the World War II research effort, led by the US Office of Scientific Research and Development (OSRD), which developed technologies as far ranging as radar, mass produced penicillin, malaria treatments, and the atomic bomb. We begin by describing how the OSRD approached crisis innovation. We then use the OSRD example to orient a discussion of the specific features of crisis innovation problems. We conclude by discussing the implications of crises for R&D in regular times and identify questions for future research.

I. The OSRD's Approach to Crisis R&D

The World War II effort was organized by the OSRD, which was created in June 1940 by order of President Franklin D. Roosevelt (originally as the National Defense Research Committee, NDRC) to apply scientific research to military problems. Led by Vannevar Bush (then president of the Carnegie Institution of Washington, formerly vice president at MIT), the OSRD was explicitly a crisis research policy agency, comprised of two branches (NDRC and the Committee on Medical Research, CMR), whose mission was to support R&D in technologies and medical treatments to further national defense as well as to lead the mobilization of US science, coordinate the research efforts of other agencies, and advise the president. In describing its work, the OSRD's executive secretary, Irvin Stewart, later wrote that "the need for speed hung like a sword over the head" of the organization (Stewart 1948). The urgency of its work shaped all of its policies, from research-funding choices to indirect cost recovery, patents, coordination of research efforts, and decisions to invest in not just research but also production and diffusion. We describe five main features of the organization

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that may be useful to consider in contemplating a theory of crisis innovation.[1]

A. *Emphasis on Applied Research*

The OSRD funded applied research, with priorities set in collaboration with end users. The applied focus was a result of not only the nature of military R&D problems but also necessity: its leaders recognized that the time for basic research is before a crisis, and urgency meant that "the basic knowledge at hand had to be turned to good account" (Conant 1947). This stands in contrast to the emphasis of Bush (1945) and Nelson-Arrow on funding basic research.

B. *Prioritizing Results*

In choosing what to fund, whom to fund, and how to incentivize their work, the OSRD was primarily focused on getting results. It favored R&D contractors "with the facilities and the manpower which promised the best results in the shortest possible time" (Stewart 1948) over cost or distributional concerns. Its indirect cost policy, novel at the time, provided generous reimbursement for overhead (50 percent for universities, 100 percent for firms) to make it worthwhile for these organizations to loan out their top talent. This priority was also seen in its patent policy, which specified whether the government or contractor received title to inventions. Bush determined it was important to secure firms' cooperation and often awarded them patent rights on OSRD-funded inventions, reserving only a royalty-free license for military use.

C. *Coordination*

The need for speed also led the OSRD to take a heavy hand in coordinating research efforts beyond simple funding. In the penicillin effort, this involved facilitating information flow among researchers without compromising potentially proprietary data. In the malaria program, where thousands of compounds were studied, coordination entailed surveying the portfolio of candidates and distributing them to make sure all bases were covered and to avoid unnecessary duplication. Still, other forms of coordination were prominent in the OSRD's effort to develop radar technology: throughout the war, radar development was centered at the MIT Radiation Lab, where the OSRD co-located scientists and engineers working on interrelated problems. In several of these efforts (e.g., radar and penicillin), coordination also took place with researchers in Allied countries.

D. *Redundancy*

In contexts where there was solution uncertainty, the OSRD often funded multiple rivalrous efforts. In medicine, the CMR supported not only a large effort to make synthetic penicillin but also, in collaboration with other government agencies, a distinct effort to scale up the production of natural penicillin. In atomic energy, unclear which method of separating uranium isotopes would be viable for large-scale production and with urgency due to concurrent German pursuit of the bomb, the OSRD invested in multiple methods until one proved successful.

E. *Diffusion*

The OSRD's work did not end with R&D. Conant (1947) emphasized the OSRD's role in "connect[ing] effectively the laboratory, the pilot plant, and the factory with each other and with the battlefront." It had specific offices to aid in getting new technology into the hands of soldiers, supporting small-batch initial production runs, field tests, and even battlefield deployment. Tight links between the OSRD and the military enabled rapid feedback and continual tweaking to ensure the technology met the needs in the field. The CMR was also active in testing and promoting utilization of therapies and procedures developed by the researchers it funded and (with other government agencies) funded clinical trials, scale up, and diffusion of treatments.

Each of these features—the focus on applied research, prioritizing results, supporting coordination, investing in parallel efforts, and funding diffusion—reflected the focus on crisis resolution. Whether by default or design, the fact the OSRD was a new organization and preceded the postwar research bureaucracy might also have allowed it to be agile and adaptive.

[1] For a more complete account of the OSRD, its administration of the World War II research effort, and the resulting lessons for crisis R&D management, see Gross and Sampat (2020b).

Reflecting growing belief that science and technology helped win the war, before the war was over, Roosevelt asked Bush to draw lessons from the OSRD. Bush's response, *Science*, *The Endless Frontier*, famously makes the case for government funding of basic science in peacetime. It does not explicitly draw lessons for crisis innovation beyond emphasizing how important prewar basic science was for the OSRD effort and the need to redress disruptions to science caused by the war. Bush instead anticipated the market failure view of innovation policy that would later become the bedrock of the economics of science.[2]

II. Crisis Innovation and the Nelson-Arrow Paradigm

For our purposes in this section, we define a crisis as an immediate, extreme threat to human life, prosperity, or freedom. Not all crises can be tackled with innovation, but in some—including disease, environmental catastrophe, and even war—science and technology can be powerful tools in the arsenal. Two basic features of crisis innovation problems are the large social returns to R&D and a need to act quickly, before losses mount or the threat grows more difficult to contain. Crises often present an asymmetric loss function, where overinvesting in R&D is less costly than undershooting. Equally notably, crisis innovation problems are not resolved by invention alone: production and diffusion, including human factors, can be just as important as the innovation itself and may need to be accounted for from the earliest stages of R&D.

We can begin by connecting crisis innovation to the Nelson-Arrow paradigm and its descendants, drawing both parallels and distinctions. Though it shares an interest in welfare, to a first order, the goal of crisis innovation is crisis resolution rather than generic technological progress. This will usually require attacking specific innovation problems with applied (versus basic) R&D and prioritizing speed over other considerations. Urgency is not necessarily outside of the scope of the Nelson-Arrow framework: time value is a standard concept in economic models. Likewise, exceptionally large social benefits of innovation are not beyond the scope of traditional models per se. Crises may even magnify traditional Nelson-Arrow causes of market failure such as uncertainty or appropriability, especially when R&D investments are made early on in a crisis, before the scale of the problem or technical feasibility are fully understood and before it is known whether or how much private R&D efforts will be rewarded. Through the lens of Nelson-Arrow, a crisis merely underscores the importance for a generous government funder.

Even so, sharp changes in the basic parameters of traditional economic models of innovation can have material effects on optimal innovation policies. For example, urgency and very large returns may justify funding (overlapping) parallel research efforts and collaboration among R&D performers to increase the rate of arrival—activities that could be considered inefficient in regular times. Urgency may even *necessitate* trading off long-run efficiency for short-term impact. In the most extreme cases, the intertemporal elasticity of substitution is infinite: without short-run impact, there may not be a long run, a tension not contemplated by Nelson and Arrow but that was arguably the case in World War II, a major armed conflict against a global hegemon. At a minimum, ambiguity over what an efficient policy is—due to the uncertain and fast-moving nature of crises and intertemporal trade-offs—may require accepting the risk of allocative inefficiency and a recognition that ex ante efficient policy may not be ex post efficient.

As the OSRD discovered, appropriability is an especially complicated subject in a crisis. On the one hand, private participants in crisis innovation efforts bring their R&D capabilities to bear on innovation problems, including physical and human capital; must be assured return on investment; and often expect to retain patents. On the other hand, public funders of crisis innovation may want to guard against profiteering, and many observers argue that patents produced with public funding should be public property or freely licensed—especially in a crisis (Kilgore 1943). Even worse, patents can slow down technological progress. Yet there is also a possibility that the appropriability problem is softened rather than amplified in a crisis, when private actors may be motivated by altruism, social responsibility, or reputational benefits.

[2] Bush (1945) writes, "We cannot expect industry adequately to fill the gap … basic research is essentially noncommercial in nature. It will not receive the attention it requires if left to industry."

To summarize, the principal questions asked by Nelson-Arrow are (i) what the first-best level of R&D investment is and (ii) whether this optimum will be achieved by competitive markets. For the reasons above, R&D market failures may be amplified in a crisis, making Nelson-Arrow arguments for public investment even more pointed, and urgency and large social returns may justify a flood of public funding. Given that it is difficult to precisely specify the optimal level, it may be better to err toward being overly generous, as the cost of overshooting the target is likely to be far lower than that of undershooting.

Yet as the OSRD story shows, crisis innovation may require activities that often do not feature in economic models of innovation, like coordination, redundancy, and investments in production and diffusion—including in production capacity at risk, before a candidate technology has been fully developed or proven to work. From a systems-engineering perspective, redundancy is valuable as insurance against R&D failure and has been a standard feature of crisis innovation efforts, from penicillin or atomic fission to a COVID-19 vaccine.

Crisis innovation efforts also tend to feature extensive coordination. End users such as the military (in World War II) or hospitals (in the COVID-19 pandemic) can often give valuable guidance to crisis R&D efforts, with an intimate knowledge of the technological challenge and field conditions to which a solution must be adapted—whether it is a radar system that must function in freezing conditions or a vaccine that can be preserved without deep-freeze storage. As the OSRD illustrates, coordination across research efforts can likewise be useful, such as in ensuring that the full range of research strategies are pursued or in the sharing of information (e.g., technical details or interim successes and failures) that can benefit many R&D projects.

Though in normal circumstances considered distinct from innovation, production and distribution are part and parcel of crisis innovation policy: when speed is of the essence, it is important to ensure that finished technology can be produced at scale and deployed as soon as R&D is complete. Economic models of innovation often focus on R&D and take diffusion as given, which in normal times is usually an equilibrium outcome of product markets. In a crisis, however, getting technology deployed is as consequential a problem as R&D itself. The conditions under which to build production capacity at risk, how much, and for how many developing technologies is not one that is addressed by standard economic models of innovation.

III. Implications for Regular Times

What are the implications of crises for R&D in regular times? One lesson is the strategic value of basic research and public investments in other techno-scientific capabilities including growing the stock of scientific human, physical, and institutional capital across a range of fields. As Bush (1945) argued, these are the resources that crisis innovation will draw from—such as COVID-19 vaccines, which are rooted in the basic understanding of mRNA.

Another lesson is the importance of designing R&D-funding and -performing institutions that can pivot quickly in a crisis. As a de novo agency, the OSRD was advantaged in this regard, with little precedent or constraint—not to mention significant funding and access to much of the country's best scientific talent, who made themselves available to the war effort. But other features—like trusted leadership, close relations with other agencies and end users, and a catalog of domestic scientists and facilities for different R&D problems—likely helped and could be emphasized more by science policy institutions today. Perhaps most importantly, there was effectively one agency overseeing government R&D. The OSRD was both a central authority and a clearinghouse of information, with a direct line to the president. Rather than competing or colliding, other agencies were synchronized under OSRD guidance.

Crises can also have effects on innovation that outlive the crisis itself. Though the OSRD was decommissioned in 1947, it laid the foundation for postwar R&D funding, and its work was continued by a constellation of federal agencies. Many research subjects were positively shocked by the war effort, while research in other areas was interrupted; Stewart (1948) writes that some subjects were even "born of war." In recent work (Gross and Sampat 2020a), we have shown that the OSRD had a lasting effect on the direction and domestic geography of US technological innovation, catalyzing long-run growth in fields and locations that were mobilized by the wartime research effort, with downstream effects on entrepreneurship and employment. Anecdotally, the wartime research effort endowed many of the participating firms with intellectual property, absorptive capacity, and other advantages that persisted in the post-war era. It seems possible that the COVID-19 research effort could have similar impacts on biomedical innovation, potentially unleashing a new era of vaccine innovation much as the World War II penicillin program did for antibiotics, among others.

Though economists' understanding of crisis innovation is growing, many questions remain. What is the optimal level of appropriability in a crisis, and how can crisis innovation be supported without bestowing postcrisis market power? How elastic are different organizations, researchers, and fields in their ability to pivot to crisis innovation problems on short notice? How detrimental are crises to fields that are not the focus of crisis R&D? Are there examples of failed crisis innovation programs, and if so, what can be learned from them? In our view, however, the most important question is whether there is a reliable way to determine which fields should be research funding targets in regular times in preparation for (unpredictable) emergencies.

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