Can Operation Warp Speed Serve as a Model for Accelerating Innovations Beyond COVID Vaccines?

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Executive Summary

Operation Warp Speed (OWS) was a U.S. government-led program to accelerate the development, production, and administration of COVID-19 vaccines. The program cut the typical ten-year timeline needed to develop a new vaccine down to ten months and began vaccinating vulnerable populations within a year after launch. OWS's success has led to calls for a similar mission model to accelerate innovations addressing other pressing social needs, including a cure for Alzheimer's disease or atmospheric-carbon removal to combat global warming. We provide a framework to understand which innovations call for a mission approach and apply economic principles to identify key design features that contributed to the success of OWS.

I. Introduction

Operation Warp Speed (OWS) was a program led by the U.S. government to accelerate the development, production, and administration of COVID-19 vaccines. The program cut the typical ten-year timeline needed to develop a new vaccine down to ten months and began vaccinating vulnerable populations within a year after launch. Steele et al. (2022) estimate that 1.6 million hospitalizations and 235,000 deaths were averted in the first ten months of rollout of OWS-funded vaccines in the United States.

OWS's success has led to calls to apply a similar mission model to accelerate innovations addressing other pressing social needs. This paper provides a framework to understand which problems are the best candidates for a special OWS-style mission and which are better addressed with more conventional policies. Based on journalistic accounts corroborated with an interview with the program designer, we lay out OWS's logic and key design features. We then apply economic reasoning to identify which features helped OWS succeed as an innovation mission.

The final sections of the paper examine case studies of other needed innovations that might be accelerated by an OWS-style innovation mission. We first analyze the procurement of diagnostics and therapeutics during the COVID-19 pandemic, the extent to which the procurement programs employed OWS-style tactics, and the outcome of these efforts. Moving beyond the COVID-19 pandemic, we analyze the potential for future OWS-style missions to accelerate innovations to mitigate a broader array of social harms, focusing on a treatment for Alzheimer's disease and a carbon-removal technology to combat global warming.

Our paper draws on the economic literature on the design of procurement mechanisms both in general (e.g., Laffont and Tirole 1993) and in the special case of innovations (e.g., Wright 1983, Weyl and Tirole 2012). We draw on Hopenhayn and Squintani's (2021) idea that when multiple technological approaches to an innovation are possible, certain approaches may be systematically congested even with free entry among competitive firms. This journal has published scores of relevant papers on innovation policy, the closest of which include Gross and Sampat (2022) on historical use of innovation missions to address crises and Azoulay et al. (2019) on the use of new innovation-funding models (the ARPA model in their case) to address future challenges. Our paper contributes to the growing number of histories of the OWS program by experts both inside the program (Slaoui and Hepburn 2020, Mango 2023) and outside (Nocera and McLean 2023, Zelikow et al. 2023).

II. Background on Operation Warp Speed

In May 2020, the U.S. Government launched Operation Warp Speed (OWS) to develop and deliver 300 million doses of a safe and effective COVID-19 vaccine for the U.S. population, with the first doses available by January 2021. Dr. Moncef Slaoui, former director of vaccines at GlaxoSmithKline, served as OWS's scientific director; and U.S. Army general Gustave Perna served as its chief operating officer.

Many informed observers thought this was an impossible goal. Developing new vaccines is typically a long-term, high-risk proposition. Fewer than 7% of vaccine candidates sponsored outside of industry progress from development to licensure (Lo et al. 2020), and the entire development process usually takes ten years or more (MacPherson et al 2021). Yet the program achieved remarkable successes. Two of the candidates sponsored by the program, Pfizer and Moderna's mRNA vaccines, completed clinical trials in ten months (Government Accountability Office 2021), allowing vaccination of some high-risk populations to start as early as December 2020. Ultimately, four of the six OWS-sponsored candidates received regulatory approval in the United States. Remarkably, three of these used novel technologies: Moderna and Pfizer's candidates used a messenger RNA (mRNA) platform, and Janssen's candidate used a viral-vector platform.

While development proceeded ahead of schedule, scaling up supply proved more challenging. Moderna and Pfizer each had been contracted to deliver 200 million doses by March 2021. They struggled to meet this deadline due to limited manufacturing capacity, supply-chain disruptions, and workforce shortages. By the end of January 2021, as OWS transitioned from the Trump to the Biden administration, only 63 million doses had been delivered to the U.S. population (Government Accountability Office 2021). The U.S. government took several measures to address these scale-up challenges: the Army Corps of Engineers oversaw efforts to expand manufacturing facilities, the Department of State expedited visas for technicians and engineers to perform critical functions at manufacturing sites, the Department of Defense worked with industry to coordinate supply, and the Departments of Defense and Health and Human Services prioritized supply contracts for vaccine companies under the Defense Production Act.

OWS was modeled on the Manhattan Project, the World War II program racing Nazi Germany to build the first atomic bomb, famously led by research coordinator Robert Oppenheimer and operations coordinator General Leslie Groves. The Manhattan Project

undertook not one but three approaches to enriching uranium to provide the fissile material for the bomb, constructing large-scale production plants for each even before testing was complete for these never-before-used technologies (Metcalfe 2023). Similarly, OWS pursued multiple vaccine candidates using various vaccine platforms and funded large-scale capacity to produce these candidates "at risk" (that is, before any received regulatory approval).

Like Oppenheimer and Groves, Slaoui and Perna were given emergency authorities and an unrestricted budget and asked to execute a whole-of-government response to deliver on a large-scale development project under enormous time pressure. Slaoui and Perna did not need permission for most of the decisions required to run OWS—they had the authority to act and to coordinate across agencies. OWS also gave them the budget and policy tools to incentivize and coordinate industry. They used advance purchase orders to guarantee demand and they bypassed cumbersome federal acquisition regulations using Other Transaction Authority to negotiate contracts with pharmaceutical companies for limited supply. They pursued multiple candidates in parallel, investing in a diverse portfolio of technological platforms and producers to mitigate development and manufacturing risks. They collapsed workstreams, pursuing some in parallel rather than sequentially, a decision that incurred financial risk, but that accelerated development without compromising safety. They streamlined clinical trials by validating and equipping sites. They expanded the pool of subjects for phase-3 clinical trials for each vaccine candidate to improve safety and efficacy data. They provided logistical support for phase-3-trial preparations even as phase-1 applications were submitted. They worked with industry and the U.S. Food and Drug Administration (FDA) to harmonize the endpoints of phase-3 trials and began scaling up manufacturing capacity and stockpiling vaccine doses before candidates received FDA approval (Slaoui and Hepburn 2020). They subsidized manufacturing capacity at risk, mapped supply chains, and leveraged the Defense Production Act to prioritize essential supplies for frontrunners.[1] It was an extraordinary effort.

III. Innovation Missions

A. Missions in U.S. History

Our July 11, 2024, interview with Robert Kadlec (Assistant Secretary for Preparedness and Response during the pandemic and one of the original architects of OWS) confirmed that the

program was modeled after the Manhattan Project. The program also followed in the footsteps of other publicly funded, large-scale innovation missions, such as the Apollo Program to send U.S. astronauts to the moon in a Cold War display of technological superiority, and the Strategic Defense Initiative to develop a space-based missile defense system. Each of these missions shared several key features with OWS; they had a clearly defined innovation objective with a challenging timeline, a large budget that allowed the program to pursue multiple objectives in parallel, an integrated command structure to enable timely decision making and cross-sector coordination, and a committed sponsor in the U.S. government.

Table 1 reports the results from a more systematic survey of programs in U.S. history that can arguably be classified as missions. The subjectivity involved in defining a "mission" and the difficulty in searching an inchoate historical record will invariably lead to some questionable programs being included or excluded. Changing the criteria for inclusion will alter the balance between including too many or too few programs but will not eliminate potential errors.

Table 1
Innovation and Infrastructure Missions over U.S. History

Mission	Timeframe	Goal description	Cost (billion 2023 dollars)	Source
(a) Innovation missions				
Manhattan Project	1942–1946	Develop first atom bomb	37	Metcalfe (2023)
Apollo Program	1961–1972	Send humans to Moon and back	177	Dreier (2022)
Space Shuttle program	1972–2011	Develop reusable spacecraft for orbital missions	266	Borenstein (2011)
GPS development	1973–1995	Create global satellite navigation system	10	Page et al. (1995), Appendix B
Strategic Defense Initiative	1984–1993	Develop space-based anti-missile systems	63	Abrahamson and Cooper (1993)
Human Genome Project	1990–2003	Map and sequence human genome	7	National Institutes of Health (2024)
Operation Warp Speed	2020–2021	Accelerate COVID-19 vaccine development and distribution	21	Congressional Research Service (2021)
(b) Infrastructure missions				
Transcontinental Railroad	1863–1869	Connect East and West coasts by rail	2	Klein (2024)
Panama Canal	1904–1914	Create shipping passage between Atlantic and Pacific Oceans	12	McCullough (1978)
Interstate Highway System	1956–1992	Create national highway network	700	Neuharth (2006)
Y2K preparation	1995–2000	Prevent computer failures from year-2000 transition	183	Chandrasekaran (1999)

Notes: Consult sources in last column for description of mission and estimate of expense in nominal dollars, which we converted into 2023 dollars using the Consumer Price Index. The cost of the Panama Canal reflects only U.S. spending, adding to a similar amount spent in prior years by a French project before it went bankrupt.

To aid in our search for missions in the inchoate historical record, we resorted to artificial-intelligence tools. We asked Claude 3.5 Sonnet, the most advanced chatbot currently provided to the public by Anthropic (Anthropic 2024), to provide a list of important projects led by the U.S. government over history having a tangible goal of producing or inventing something. We also specified that the project have a deadline. The chatbot returned a list from which we culled projects that were obviously not missions (e.g., Reconstruction, New Deal, Cuban Missile Crisis response, Great Society programs). To focus on large-scale missions, we retained those exceeding an expenditure level of 1 billion in 2023 dollars.

The final list of projects can be classified into two groups. The first includes OWS and other innovation missions on which this paper focuses. The chatbot returned a second set of projects that we label "infrastructure missions," including three large transportation projects (the Transcontinental Railroad, Panama Canal, and Interstate Highway System) as well as Y2K preparation. Infrastructure missions share some features with innovation missions: both aim to achieve an ambitious goal on a short deadline. But infrastructure missions apply existing technologies to a national project, while innovation missions develop first-of-a-kind technologies that the market cannot be relied on to deliver.

B. Features OWS Shared with Other Innovation Missions

When work streams are tightly integrated, heavily incentivized, and highly prioritized, as they were for all of the innovation missions in Table 1, entirely new systems can be built rapidly, allowing teams to achieve remarkable outcomes in record time. The success of past missions can be seductive to public officials that want to draw attention to new programs to effect change. But not all problems are amenable to begin solved by a mission. The War on Drugs and the Cancer Moonshot are good examples of initiatives that meet urgent national needs, but that address multifaceted social problems that cannot be reduced to a technological solution. Missions, as we define them, address concrete, time-sensitive, technological challenges of national importance that require market shaping and an uncommon degree of cross-sector coordination to solve.

When public officials declare a mission, they are placing a high-risk, high-cost bet with public dollars. It is important, therefore, for them to know when a mission offers the right tool for the job. OWS was an expensive venture, placing large bets on six vaccine candidates. Despite

taking this many shots on goal, delivering a new vaccine within one year was an unprecedented objective, carrying extreme risk, with no guarantee any of the six candidates would succeed.

The nature of missions makes them difficult to sustain. When missions succeed, it is often in part because participants believe they are facing an existential threat such as a war or a pandemic. No one conducts business as usual. Entirely new systems are developed in record time, but often because participants are making one-time exceptions that are unsustainable over the long run. This level of coordination and public entrepreneurship carries a high "metabolic" cost for all participants. Irvin Stewart, Deputy Director of the office that coordinated World War II research and development, observed that "once the pressure of war lifted… not all the king's horses nor all the king's men could hold the group together" (Stewart 1948, p. 320). Similarly, OWS did not survive more than a year. Slaoui left in February of 2021, and the operation rapidly devolved to a more standard bureaucratic process. OWS has struggled to coordinate follow-on innovation to address variants of concern and remains slow to react to ongoing diagnostic and therapeutic needs.

Historically, missions have performed best when there is a sense of urgency. High levels of coordination and control are most easily achieved under emergency—or time-sensitive-conditions. Some issues are considered chronic emergencies to certain stakeholders, and senior government leadership can still set priorities and objectives to address these issues. For example, as reported in Dlouhy and Jacobs (2024), advocacy groups have called on President Biden to declare a climate emergency, which would unlock new authorities that could be used to reduce offshore drilling, block oil and gas exports, or limit distribution via pipelines and railways. Although Biden did not formally declare a climate emergency, he made climate change a top priority, securing the passage of the 2022 Inflation Reduction Act, a legislative package primarily focused on subsidizing clean energy.

When an innovation target is not urgent, is an argument for slowing the rate of spending and sequencing investment projects. The most promising ideas can be pursued first, only spending on less promising ideas if the initial ideas do not pan out. Investing sequentially rather than in parallel also has the benefit of allowing later research to incorporate learning from earlier work.

The slower, sequential approach is closer to that taken by the Biden Administration with its Cancer Moonshot initiative. While the initiative appropriated the "moonshot" label, it does not have the key characteristics of innovation missions considered in this paper. It is a far cry from, for example, mobilizing swaths of the U.S. government and pharmaceutical industry to develop a

cure for cancer within a year. Instead, the program called for a modest budget ($1.8 billion) spread over an extended period (seven years) (National Institutes of Health 2023).

While commentators have decried the failure of the Cancer Moonshot to discover a notable cure (Mastroianni 2024), a case can be made for the program's limited nature. The enormous scientific and commercial rewards awaiting whoever finds a cure for cancer may curtail the need for public funders to contribute additional resources. While saving a year of cancer deaths by accelerating the arrival of a cure has enormous social value, it is arguably of less value than ending a pandemic a year earlier. Cancer causes around 600,000 annual deaths in the United States (Rahib et al. 2021), close to the annual deaths from COVID-19 during the pandemic. But since it is not infectious, cancer does not result in the shutdown of the economy and schools experienced during the COVID-19 pandemic. Most importantly, cancer research may not be at a technology-readiness level to make it conducive to a time-limited mission. Required cancer research may be so fundamental and broad that it is still better to proceed sequentially, with incremental discoveries building on each other rather than via one of a number of technologies independently generating a cure in short order.

The Advanced Research Projects Agencies housed in various U.S. government departments (for example, DARPA housed under the Department of Defense and ARPA-H housed under Health and Human Services) manage peacetime portfolios of high-risk investments in new technologies. Unlike big-budget missions with multiple shots on a single development goal, these portfolios tend to represent a more diverse range of objectives with more modest development goals. For example, DARPA sought to prove the principle that nucleic-acid vaccines could be rapidly developed in response to new outbreaks. By contrast, OWS sought to make and deliver 300 million doses of a safe and effective COVID-19 vaccine from any one of three different platforms by January 2021. The OWS mission goal is much more ambitious, requiring a practical, time-sensitive, outcome, while being less prescriptive about the technological path.

Both types of innovation investments (urgent and non-urgent) are essential. Long-term, steady investments in basic and applied science are a precondition for mission success. The United States became a front runner in the global race for a COVID-19 vaccine in no small part due to robust public support for science. Early in the pandemic, Pfizer, Moderna, and other commercial pharmaceutical firms were able to build on publicly funded research undertaken during the previous decade on coronavirus biology, mRNA-delivery platforms, and protein-stabilization

techniques. Absent these scientific advances, OWS would not have been able to achieve such a rapid delivery schedule. The protracted and changing nature of disease threats and environmental challenges calls for maintaining consistent investments in pandemic preparedness and climate security. A strong foundation in scientific research will only facilitate the use of innovation missions to address acute problems that arise in the future.

IV. Which Innovations Call for a Mission?

How can public funders know when the social problem they face is a good candidate for the next OWS to accelerate an innovation to address it? This section uses basic principles of economics and public policy to answer the question of "which." We identify five characteristics that make a social problem amenable to an innovation mission. We discuss each of the characteristics in the following subsections in turn.

A. National Importance

To merit a mission approach, a social problem must be large-scale and of national importance. In the case of the COVID-19 pandemic, the United States needed a vaccine to reopen schools and businesses, to protect the health care system from collapse, and to stem the enormous loss of life and livelihood. Forecasts that the United States would suffer $16 trillion of harm from combined mortality, morbidity, and economic-output losses even under optimistic assumptions (Cutler and Summers 2020) translated into losses of $26 billion each day (Baker et al. 2021). Saving less than a day's harm could justify the entire OWS budget, which totaled $18 billion according to most accounts (Congressional Research Service 2021).

Retrospective estimates of the benefit of COVID-19 vaccines provide evidence that they were extremely cost effective. As mentioned in the introduction, 1.6 million hospitalizations and 235,000 deaths were averted in the first ten months that OWS-funded vaccines began to be rolled out in the United States (Steele et al. 2022). Using the official $11.3 million value of a statistical life (Federal Emergency Management Agency 2022), the mortality reduction alone is worth $2.7 trillion. Including the benefit of reduced hospitalizations and faster reopening of the economy and schools would only improve the vaccines' measured cost-effectiveness.

Modest social problems do not call for special missions. The benefits of solving the problems may not justify the cost of inducing innovations to solve them. Even if they pass a

benefit-cost test, at most a modest investment at a measured pace would be called for. The budget may be within a single agency's capacity. It is only large-scale problems that call for large budgets, multi-agency efforts, and an extraordinary organizational endeavor.

It is not sufficient that the social problem be of large scale. To run a mission, the government must be willing to recognize the scale of the problem and designate it among its top national priorities. Prioritizing the problem is necessary for the leadership team to achieve the authority, budget, and political cover required to achieve coordination within government and among intra- and extramural partners. This will also facilitate efforts to integrate policy tools that align with mission objectives.

B. Time Sensitivity

Missions address time-sensitive technological problems that cannot be met on a socially acceptable timeframe, even with the best current approaches. Left to market forces and standard government practices, the United States would not have built an atomic bomb before Nazi Germany, nor would it have developed COVID-19 vaccines within a year. An entirely new system of organization and public-private partnership was required to achieve the goal.

Whether arising during a war or pandemic or outside of these, in general, a social problem calling for a mission should have the urgency of a crisis. Even a large-scale problem, if it is not urgent, may be better addressed with a more measured approach than a mission. An urgent problem is of current concern rather than looming on a distant horizon; an urgent problem entails losses that mount on a weekly basis. Less urgent problems may be better addressed by consistently spending a smaller amount over a longer period of time—standard operating procedure for many agencies. The program may fund different innovative approaches in sequence rather than using a large budget to fund a variety of approaches in parallel. A sequential approach can be more economical: if the approaches explored first (presumably the most promising) work out, investment in subsequent approaches can be saved. Even when initial approaches fail, learnings from the background research can be digested and applied to expedite subsequent approaches.

Time sensitivity can be both a blessing and a curse for a mission. Time pressure can help a mission by focusing the energy and resources of government and industry and reducing resistance to changes in standard operating procedure necessary to accomplish the mission. Agency directors, regulators, and business managers are more likely to tolerate short-term

subversions of authority and exceptions in standard operating procedures if they are temporary. On the flip side, mission-enabling changes and accommodations are inherently unstable. A runner can sprint for a few minutes but soon has to fall back to a jog. The heroic level of coordination and accommodation required to achieve a mission cannot be sustained without building new bureaucracies that institutionalize a new normal. Absent a commitment to institutionalize new systems, missions become time-limited out of necessity.

"Time sensitivity" can go beyond simple "urgency." Social problems that call for a mission often involve a deadline after which the innovation loses much of its value. The U.S. may have been forced to surrender (abandoning the Manhattan Project among many conditions) had Nazi Germany developed an atomic bomb first. COVID-19 vaccines would have been much less valuable coming after the pandemic. The relevant deadline may correspond to the end of a crisis, to a date beyond which a growing problem threatens to spin out of control, or a date beyond which heightened public interest and earmarked resources can no longer be maintained.

C. Uncommon Coordination

Missions are inherently entrepreneurial, often requiring an entirely new system of organization and partnership across industry and government. This condition places special emphasis on the need for an integrated command structure to turn the wheels of government and to adjust incentives to resolve bottlenecks in multiple sectors. Missions employ a raft of policy tools and incentives to facilitate industry participation. These include devising risk-sharing and reward schemes, addressing intellectual-property and liability concerns, and alleviating supply-chain constraints.

D. Well-defined Technological Goal

An innovation mission is more likely to succeed the more concretely its technological goal can be specified. OWS's mission was to develop, produce, and deliver 300 million doses of a safe and effective COVID-19 vaccine from any one of three different platforms by January 2021. This goal was ambitious, time-delimited, and tangible. OWS worked with the FDA and industry to develop and communicate a range of target-product profiles with common clinical endpoints and clear guidance on requirements to receive regulatory approval.

It would be difficult to write a target-product profile for an innovation for which much basic science is still to be worked out. Innovation missions are most suitable to technological areas

for which the underlying scientific principles are fairly well established. The DOD and NASA use a classification system to assess the maturity of a technology called technology readiness levels (TRLs), ranging from 1 for technologies at the basic-research stage to 9 for technologies used in actual operations or commercialization. For example, on the eve of the COVID-19 pandemic, mRNA vaccines were arguably around a TRL of 6 or 7 (Government Accountability Office 2021).

To qualify for a mission, an innovation should not be too low on the TRL scale. Crash programs can consolidate and apply new science, but they rarely generate it. Basic science takes time to mature and does not conform to deadlines. In a manner similar to OWS, a World War II flu commission developed the first licensed flu vaccine in the United States in less than two years. The commission was able to accelerate development because scientists had been laying the groundwork for a flu vaccine since the 1930s. When war broke out, the commission pulled together knowledge about how to isolate, grow, and purify the flu virus, devised methods to scale-up manufacturing and to evaluate the vaccine for safety and efficacy. By contrast, when the underlying science is less well understood, as it was for anthrax and other more exotic pathogens of military interest in the 1940s, crash development programs failed (Hoyt 2012). One difficulty using a mission for a low-TRL innovation is that it may be too early to specify enough details of a target-product profile (TPP). A well-defined TPP can focus the mission's drive toward a goal and help constituents assess the goal's value. Funding mechanisms that reward firms based on successes rather than attempts (called pull funding, discussed at length below) may be infeasible without reference to a detailed TPP. Without drive and accountability, missions can wander and fail.

At the opposite end of the TRL scale, an innovation that is already close to commercialization would not call for a full-blown innovation mission to complete it. (If any sort of mission is called for it would be not an innovation mission but more like the infrastructure missions in Table 1, supporting the wide adoption of the innovation rather than its creation.) In sum, technologies intermediate on the TRL scale are the most suitable for innovation missions.

E. Commercial Market Inadequate

For the vast majority of innovations, the prospect of profits earned from private sales provide the needed incentives to bring the innovation to market. While some sort of intellectual-property protection (copyright, trademark, patent) may complement the private market to prevent copycats

from diluting the innovator's profits, the main incentives come from customers, whose willingness to pay signals what products they demand.

An innovation that would have a ready commercial market does not raise a problem for a mission to solve. To call for a mission, there must be an abnormally large gap between the social need and the returns provided by the commercial market. Externalities provide one source of such a gap. Vaccines that inhibit disease transmission exhibit a positive externality. Consumers may be unwilling to pay extra for this benefit to strangers, so the commercial returns from selling vaccines may not reflect the full social value without an additional subsidy (Goodkin-Gold et al. 2022, Goodkin-Gold et al. 2024). A technology that helps win a war or end a pandemic provides a potentially much larger externality that permeates and stabilizes a nation's entire macroeconomy. Castillo et al. (2021) conservatively estimated that capacity for COVID-19 vaccines installed early in the pandemic had a global social value (in terms of reduced mortality, morbidity, and economic stagnation) of $5,800 per course, eclipsing the $40 the United States paid for the highest-priced courses. Technologies mitigating climate change also provide enormous positive externalities and may be considerably undersupplied for similar reasons.

Even if the commercial incentives are adequate to bring a new product to market eventually, incentives to bring it to market quickly may be inadequate. Cutting development time may multiply the cost of more measured investment. The innovating firm may not get much return from accelerating development, obtaining a similar price whether it sells to consumers today, in a year, or in five years. Commercial firms with financial resources and industry expertise may be the most promising innovators but may require a boost from a public funder to bridge the gap between the social value of innovation and their market return. Complementing commercial incentives to innovate with external funding, perhaps in the form of grant funding, subsidy, prize, or advance procurement contract has been dubbed "market shaping" (see Rachel Glennerster's overview reported in Rodriguez and Harris 2024).

To qualify for a mission, a social problem necessarily requires some market shaping. Otherwise, the commercial market could solve it without intervention. Thus, it is important to identify the presence of a gap between social and private returns due to externalities and other frictions before calling for market shaping. However, if that gap is identified, market shaping may be just the starting point. A mission is not an ordinary market-shaping exercise, but market shaping "on steroids." Instead of picking a single form of market shaping, a mission may call for "all of

the above." Instead of erring on the side of economizing on expenses, a mission errs on the side of getting results fast. The presence of the other four characteristics discussed in this section will help determine whether a needed innovation calls for modest market shaping or a full-blown mission. The extensive resources of a mission may help bridge an abnormally large gap between social and commercial incentives; its focused timeline may provide the impetus to accelerate the innovation.

V. Economic Analysis of Key OWS Features

The previous section answered the question of "which": which social needs call for an innovation mission like OWS? In this section we turn to the question of "how": how did OWS work? We apply economic principles to identify the design features that in theory contributed to its success. Future missions may consider incorporating some or all of the design features.

A. Prodigious Funding

There is disagreement over total spending under OWS. The budget was $18 billion according to some official accounts (Congressional Research Service 2021). Some officials including Robert Kadlec (July 11, 2024 interview) and Paul Mango (Mango 2022, p. 175) suggest even higher spending, ranging from $26–30 billion. Even the lower-end estimate represents prodigious spending for a short-term vaccine program, and reflects the national importance accorded measures to escape the COVID-19 pandemic. Such prodigious spending is perhaps the single key feature of the OWS program since it is a prerequisite for most (if not all) of the other program features discussed in the rest of the section, which simply do not work without substantial funding.

The danger in designing a prodigiously funded program is that it is hard to distinguish the optimal level of prodigious spending from overspending. There is a tradeoff between the loss from failing to reach a goal because too little was spent and the loss overspending to achieve the goal. We argue that in crisis situations, the bigger risk is failure not overspending.

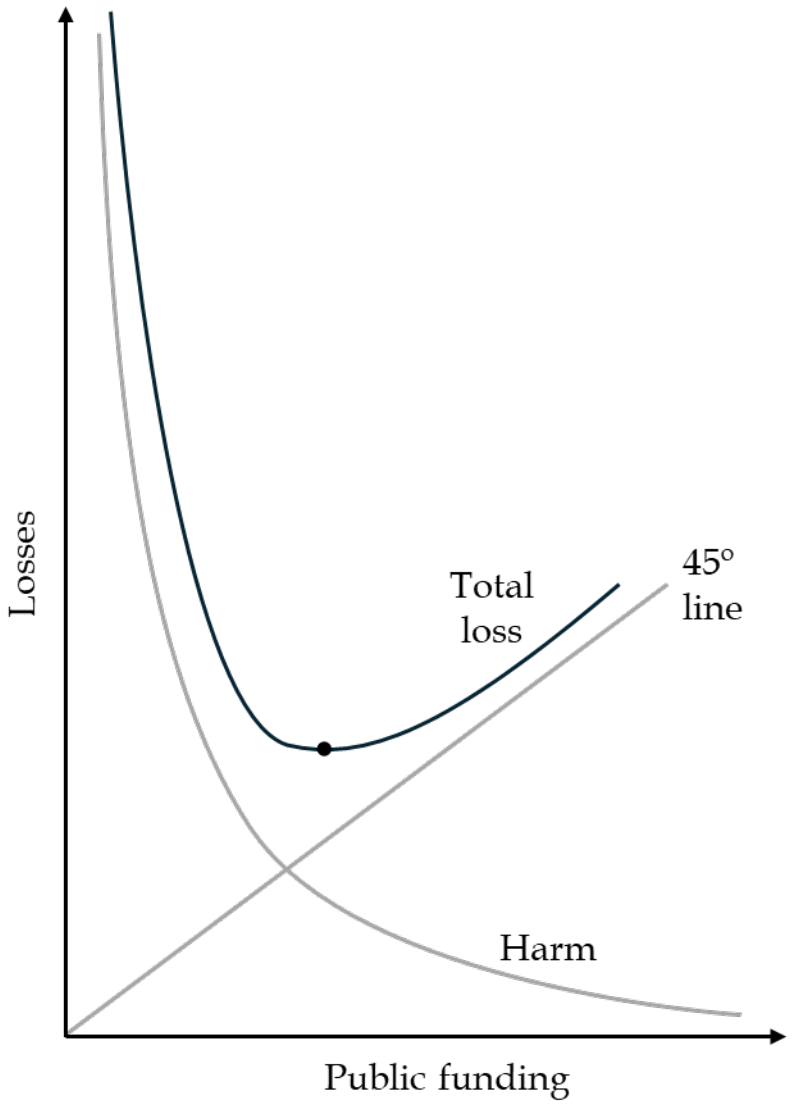

Fig. 1. Schematic diagram of asymmetric loss function in crisis situation. When public funding is reduced below the optimum (indicated by the dot at the bottom of the total-loss function), losses can rise steeply as the risk of mission failure eclipses the savings in public funds.

To see this point more systematically, refer to the schematic diagram of a loss function with program expenditures on the horizontal axis in Figure 1. The expected harm from a representative social problem is graphed as the hyperbolic curve, which slopes downward because more spending increases the chance of developing an innovation that addresses the problem. Program expenditures follow the 45-degree line. The U-shaped curve above the two gray curves is their sum, thus the total-loss function. This function trades off the harm from not achieving an innovation goal because too little was spent against the loss of overspending to achieve the goal. In a pandemic, or any emergency calling for an innovation mission, the loss function becomes severely asymmetric, with the loss from failing to reach the goal eclipsing the loss from overspending. For a program that, in the words of Athey et al. (2022), is "spending billions to save trillions," economizing on a billion dollars of program expenditure is less important than saving a trillion dollars of harm.

B. Portfolio of Approaches

The goal of OWS was to obtain a safe and effective vaccine that could be widely rolled out to the population. While having multiple successful vaccines to choose from is "nice to have," most of the benefit—the "must have"—comes from having some vaccine to roll out in the population. OWS did not pursue just one candidate to obtain that one success. To increase the chance of scoring quickly, it is worth taking multiple shots on goal. Historically, the probability of success for vaccine candidates sponsored outside of industry is less than 7% (Lo et al. 2020), too low to bet the health of the nation on one shot.

OWS included three vaccine technologies: mRNA (Moderna and Pfizer), viral vector (Janssen and AstraZeneca), and adjuvanted protein (Sanofi/GSK and Novavax). Failure risks are correlated within technologies: a given technology may simply not be suited to combat the given pandemic pathogen, and so if one fails it may be an indication that every candidate in its technology class might also fail. Pursuing different technologies reduces the correlation in the failure risk and improves the probability that some candidate succeeds. The principle harkens back to portfolio theory in finance (Sharpe 1964), whereby a financial asset with a given expected return is more valuable the less correlated its return with the overall asset market.

To illustrate these points, it is instructive to work through the thought experiment of choosing the optimal portfolio of vaccine candidates to invest in from those in Table 2. This table

is reproduced from Athey et al. (2022), providing a list of 20 of the most promising candidates in development when OWS was making its decisions. The authors estimated probabilities of success of individual candidates and the correlation in failure rates among them based on the modeling work of Ahuja et al. (2021). Their model generates correlation in failure by assuming that failures can be generated at various levels, and failure at any one level spells failure for the product, as in Kremer's (1993) O-ring theory of product development. The model specifies a chance that no vaccine will work for COVID-19, then a chance that a given technological platform might fail (inactivated virus, viral vector, mRNA, DNA, etc.), then subcategories within a platform (omitted from Table 2 for brevity). Even if no failure is experienced at higher levels in the hierarchy, individual candidates within a subcategory can always fail, more likely earlier in the development pipeline.

Table 2
Vaccine Candidates Used in Thought Experiment of Constructing Optimal Portfolio

Platform	Clinical stage	Probability of at least one success in portfolio (%)		Standalone probability of success
		Cumulative	Increment	
Inactivated	Phase 3	28.8	28.8	28.8
Viral vector	Phase 3	48.4	19.6	28.8
mRNA	Phase 3	58.4	10.0	21.6
Inactivated	Phase 3	65.8	7.4	28.8
Protein subunit	Phase 2	70.8	5.0	18.4
Protein subunit	Phase 2	74.5	3.7	18.4
Protein subunit	Phase 2	77.0	2.5	18.4
mRNA	Phase 3	79.0	2.1	21.6
Inactivated	Phase 3	80.7	1.7	28.8
Viral vector	Phase 2	82.1	1.4	18.4
Virus-like particle	Phase 1	83.3	1.2	13.2
Viral vector	Phase 2	84.1	0.8	18.4
Viral vector	Phase 1	84.7	0.7	13.2
Protein subunit	Phase 1	85.3	0.6	13.2
DNA	Phase 2	85.8	0.5	9.2
Protein subunit	Phase 1	86.2	0.4	13.2
Live attenuated	Preclinical	86.5	0.3	8.1
DNA	Phase 2	86.8	0.3	9.2
Live attenuated	Preclinical	87.1	0.3	8.1
Protein subunit	Phase 1	87.3	0.2	13.2

Notes: Excerpted from Table 2 of Athey et al. (2022), which is based on the model estimates of Ahuja et al. (2021). Their model also accounts for additional correlations induced by technology subcategories, but those subcategories are omitted from this table for brevity. Candidates are ranked in order of increment to the probability of at least one success in the portfolio.

As a simple thought experiment, consider forming a portfolio of four vaccines out of the 20 in Table 2 to maximize the probability of at least one success. According to the model, four candidates had the highest probability of success (28.8%) because they were already far along in clinical trials (in phase 3) and used proven delivery technologies for vaccine delivery (inactivated virus and viral vector). If one assumes, counterfactually, that failures are independent events across the four candidates, the probability of at least one success equals the complement to the probability that all four fail: $1 - (1 - 0.288)^4 = 74.3\%$. But the failures are not independent events. Three of the candidates used the same inactivated-virus delivery platform. All would fail if that delivery platform happened not to work for COVID-19. According to model estimates, which take into account the correlation in failures, the probability of at least one success in the portfolio of the four candidates that have the highest standalone probabilities of success is not 74.3% but only 63.1% (Athey et al. 2022, Figure 2).

Selecting the candidates with the highest probability of success does not lead to the optimal portfolio in this thought experiment. The portfolio of four candidates could be improved by substituting the most promising mRNA candidate for one of the three using the inactivated-virus delivery platforms. Even though the model ascribes a lower standalone probability (21.6%) to the mRNA candidate because the delivery platform had never successfully been used before in vaccines, a case can be made for its inclusion because its failure is less correlated with the other vaccines in the portfolio than a third using the same delivery technology as others. Including the mRNA technology increases the probability of at least one success in the portfolio of four vaccines from 63.1% to 65.8%. An increase of a few percentage points may seem small, but when multiplied by the trillions of dollars of surplus from mitigating pandemic harm with a successful vaccine, this is a nontrivial improvement.

The OWS portfolio did include two mRNA vaccines despite pessimism among some scientists about whether the technology would ever prove practical, based partly on a disappointing experience with the related DNA technology a decade earlier (Hwang 2024). It turned out to be fortunate that mRNA vaccines were included. Pfizer and Moderna's mRNA vaccines ended up being the dominant vaccines distributed in the United States. Some of the other technologies that received approval were found to have side effects among especially younger patients, which, despite being extremely rare, led to their use being curtailed. Taking multiple shots on goal not only increases the chance of some success but also increases the chance of multiple successes.

These generate additional benefits when some of the apparent successes do not pan out or when there is heterogeneity in effects so that different candidates may produce more benefits in different situations (say different candidates are better for certain populations by age, gender, race, or location).

Competitive forces on their own may not lead to the optimal spread of firms across technologies, and there may be systematic reasons why the optimal portfolio should include an array of technologies including some that initially appear less promising. Hopenhayn and Squintani (2021) develop a theoretical model in which a fixed stock of researchers allocate themselves across project areas with different private returns. The free flow of researchers ends up equalizing average returns across different areas. Owing to congestion externalities arising from researchers cannibalizing some returns from others in the same area, marginal social returns may remain quite unequal across areas despite the equalization of average private returns. Put simply, researchers tend to overcrowd the most promising areas, to the detriment of social welfare and overall innovation.

Adapting Hopenhayn and Squintani's (2021) model to the OWS setting, in which the same revenue would be obtained by a safe and effective vaccine whatever the underlying technology, the difference in firms' private returns would be driven by differences in cost and probability of success. The logic of the model would suggest that firms predictably overcrowd traditional technologies with more initial promise, leaving more difficult/speculative technologies such as mRNA underexploited. The marginal social returns for mRNA candidates may be higher than traditional candidates, providing a rationale for at least giving a second look to non-traditional candidates in the portfolio, not *despite* but precisely *because* they are more difficult/speculative. Of course there is a limit to the argument: while difficult/speculative technologies may deserve a second look, some could have so little promise as not to be worth funding.

OWS could have saved money by taking its multiple shots on goal sequentially rather than simultaneously. Once one succeeded, further investment in other candidates could be saved. The urgency involved in pandemic response precluded a sequential approach. Supposing optimistically that each candidate could be developed within a year from start to finish, the last candidate in a portfolio of four would take four years to get to, the last in a portfolio of five would take five years to get to, and so forth, while the population would suffer for lack of a vaccine if earlier candidates did not generate a success. On the other hand, if developed in parallel, a portfolio of arbitrary size

could be accomplished in the same year (or whatever development period is required for a single candidate). Parallel development might even experience economies of scope from conducting clinical trials of several candidates together, since they could use the same control group, and so require fewer total subjects.

Our thought experiment above considered constructing the optimal portfolio of four vaccine candidates. OWS was more ambitious, sponsoring not four but six candidates. As ambitious as the program was, some commentators urged even more shots on goal. The Athey et al. (2020) op-ed called for the United States to invest $70 billion in 15 to 20 candidates. The marginal improvement in probability of some success obtained by including the 20th candidate is just a fraction of a percentage point according to Table 2, but, again, a small chance of saving trillions of dollars of harm is worth additional investment, even in the billions of dollars.

C. Taking Long Shots

The previous section hinted at some reasons for undertaking what under ordinary circumstances would be considered wasteful long shots but become justifiable in the urgent setting of a mission. In the COVID-19 pandemic, as just mentioned, the Athey et al. (2020) op-ed called for a much larger portfolio than OWS ultimately included, as many as 15 to 20 vaccine candidates rather than six. Presuming that candidates with the most promise would already have been selected for smaller portfolios, marginal candidates added to form increasingly larger portfolios are increasingly longer shots. According to the estimates shown in Table 2 of Athey et al. (2022), the marginal candidates added to the optimal portfolio of size 20 include a vaccine with an 9% standalone probability of success (owing to its use of the speculative DNA technology platform) and another with an 8% standalone probability of success (owing to its being very early in preclinical development). Those marginal candidates contribute less than a half a percentage point to the overall probability of at least one success in the portfolio. Yet a case can be made for expanding the optimal portfolio to include those candidates because even a tiny increase in the probability of program success, when multiplied by the enormous losses that might be averted by a successful vaccine, justify the expenditure of billions of dollars of investment in the marginal candidates.

The heavy investment by OWS in the mRNA vaccines can be viewed as a long shot ultimately crucial to the program's success. That mRNA vaccines became the "go to" candidates for COVID-19 primary series and boosters can lead one to forget the initial doubt expressed by

some scientists and industry experts whether the technology was viable, having never been licensed for a vaccine (Hwang 2024). The estimates in Athey et al. (2022) put the standalone probability of success of the most promising mRNA vaccines at 21%, but more skeptical experts thought the estimate was closer to 0%. Outside a pandemic, it is not hard to justify avoiding speculative technologies. During a pandemic, the tradeoff changes, and it becomes harder to justify *not* investing in speculative technologies if they have a chance of working, all the more if those technologies are less correlated with traditional approaches.

The enormous losses experienced during the pandemic rationalized another measure that would rarely be undertaken under ordinary circumstances: at-risk capacity building. Normally, a pharmaceutical firm would wait until its product received regulatory approval before undertaking any large capacity investments. But as we have repeatedly said, in a pandemic, the social returns to having the vaccine faster are potentially enormous. Whether the capacity is coming from repurposing existing contract manufacturers' facilities to produce pandemic vaccine or the construction of new facilities, installing vaccine capacity is complex, expensive, and time consuming. Regulators have to verify that the facilities are using good manufacturing processes (GMP). The lag from plan to production can take months or years. Time can be saved if firms expand capacity in parallel with clinical trials before regulatory approval. The drawback is that the expenditures on scaling up capacity will turn out to have no return (wasted in that sense) if the product fails to be approved. But the gain from accelerating availability by having vaccine ready to rollout not long after the approval date may be worth the risk of that wasted expenditure. At-risk capacity investment is a tactic that is hard to justify under ordinary circumstances but hard not to justify in a pandemic. Even if billions of dollars end up being expended on capacity for failed candidates, that expenditure is well worth even a modest chance of accelerating the availability of vaccines in a pandemic.

According to the estimates in Ahuja et al. (2021), if one credits the at-risk investment strategy with accelerating the availability of the OWS capacity by just three months, that credit translates into a $390 billion reduction in pandemic harm in the United States. The benefit from at-risk investment (measured in level terms) scales with the amount of capacity involved. Had OWS installed the capacity found by the Ahuja et al. (2021) analysis to be optimal—about twice that installed under OWS—the benefit to the United States of using the at-risk strategy to accelerate the availability of that capacity by three months would have been $560 billion. The

benefit to the world from accelerating optimal world capacity by three months was estimated to be $3.4 trillion.

OWS did not pursue all available long-shot opportunities. OWS chose not to pursue variant-resistant vaccines. Officials were aware of the distinct possibility that mutations could cause waves of mortality, as experienced during the Alpha, Delta, and Omicron waves during the pandemic. OWS could have taken some shots at candidates with a second holding epitope, which reduces the chances of vaccine escape because a virus mutation would have to mutate to evade that second target as well (Magazine et al. 2024). The economic analysis in previous sections provides some support for OWS's limiting the target to just the ancestral variant. A more ambitious target could have delayed in the arrival of first doses if not resulted in complete failure, risking the loss of trillions of dollars of social value from the rapid availability of even an imperfect vaccine. Further analysis is required to evaluate the tradeoff more formally, but suggestive support for the OWS's more limited target is provided by Więcek et al. (2022), whose epidemiological simulations show that using fractional dosing to overcome supply constraints that limit the population vaccination rate could have saved lives relative to full doses even if fractional doses were considerably less effective.

D. Combining Push and Pull Funding

The innovation process is sometimes envisioned as a pipeline leading from inventors' initial ideas through many stages including the development of prototypes, engineering refinements, through scale up of capacity for widespread production. Various policies can try to boost innovation incentives by adding funds at either end of the pipeline. The policy of picking promising innovators at the start and funding their costs as they proceed through the pipeline, typically via grants, is called "push" funding. An alternative funding mechanism, "pull" funding, dangles a reward for success at the end of the pipeline, whether in the form of a patent promising a lucrative commercial market, a lump-sum prize for solving a puzzle, or a purchase contract for producing a certain quantity of a product that meets a technical profile. Push pays for attempts; pull pays for success. Each form of funding has pros and cons. The conditions under which push or pull is optimal is still under investigation, but the answer will undoubtedly build on the prior theoretical literature of optimal innovation policy, including such seminal work as Wright (1983) and Weyl and Tirole (2012).

In a pandemic, when the risk of not developing a vaccine is much more damaging to society than spending a billion dollars too much, the perspective is not so much "either or" as "both and." Uncertain as to whether push or pull would provide the best incentives, OWS used both. With one exception, the firms funded by OWS received some push funding, covering their R&D expenses as well as expenses involved in scaling up at-risk capacity. Pfizer was alone in rejecting initial push funding. All the firms funded by OWS, Pfizer included, received pull funding in the form of advance procurement contracts, signed before firms even had approved products, promising a per-dose payment for a specified quantity upon FDA authorization.

Expanding on the insights in Ahuja et al. (2021), Athey et al. (2022) discuss the virtues of mixing push and pull funding for innovation in crises. As we have discussed, crises call for multiple shots on goal, some of which may be long shots. Using pull funding to induce the investment of marginal candidates—the long shots—can be quite expensive, and the need to offer that contract to inframarginal candidates as well only multiplies the expense. Push funding can economize on some of that expense, as can be demonstrated by a numerical example.

To make the calculations as simple as possible, suppose there are only two vaccine candidates that can be included in the program's portfolio, one with a 50% probability of success and one with a 10% probability. Suppose that bringing a candidate through the development pipeline to approval and at-risk capacity investment amounts to $1 billion per firm. To simplify, normalize production costs and the required rate of return on capital both to zero, so that we will assume a firm is willing to participate as long as the revenue it expects from the pull contract exceeds $1 billion, covering its up-front investment. Since firms only obtain the pull-funding revenue if they succeed, to break even, revenue must equal the up-front cost times the reciprocal of its probability of success. The firm with 50% probability of success must earn at least $2 billion conditional on success. The firm with a 10% chance must earn at least $10 billion. To incentivize the investment of both firms, a uniform pull program would have to pay $10 billion to any successful candidate. The expected outlay from that pull-funding program is $6 billion (equal to the $10 billion paid to any successful firm times the sum of the probabilities that firms succeed: 50% + 10% = 60%).

Consider an alternative program that funds firms' investments via push. Assume that a firm is willing to invest as long as grant funding covers its $1 billion up-front expense. A push-funding

program inducing both firms to participate would cost $2 billion, a third of the expense we computed for the pull-funding scheme.

Push also transfers some of the risk of sunk investment from the firm to the funder. This may have an advantage if the funder has some ability to control the risk (say the government can relax the approval guidelines to increase the probability of success or can follow through on its procurement promises). It may reduce the firm's need to raise capital, which may be quite costly if it reflects an unusually large risk premium that may have to be covered in a crisis situation with at-risk investment in the presence of a low probability of success and uncertain norms about overcharging in a pandemic. There may be a limit to how much capital a small pharmaceutical company like Moderna can access in a short time, which might fall short of the substantial amount needed to take a vaccine through large clinical trials and to spin up the capacity to cover a substantial fraction of the U.S. population.

The previous section argued that at-risk investment in large-scale capacity has the potential to generate large social value in a pandemic but is not how commercial firms ordinarily behave. Push funding is a direct way to overcome this reluctance: the funder can ask the firm to invest at risk and agree to cover the associated expenses from doing so.

Thus, push can incentivize speed simply by directing firms to make early investments that the funder pays for. Incentivizing speed with pull raises some difficulties. If the contract does not specify delivery dates, firms could save money by installing limited capacity and delivering doses over an extended period. Incentives for speed could come from specifying a target date and adding bonuses for early delivery or penalties for late delivery. But the ability to meet a target date may depend on events outside of the firms' control. Bonuses and penalties may increase risk and consequently the firms' cost of capital. As Castillo et al. (2021) note, a penalty reflecting the social harm from delayed vaccine availability in a pandemic may be higher than most firms are willing to pay.

Push funding has its own drawbacks, leading Ahuja et al. (2021) to endorse the use of a mix of push and pull, as OWS did, not purely one or the other. Pure push funding might run into an adverse-selection problem if the funder does not have a good idea of who the serious innovators are. The funder may end up wasting grant funding on researchers' pet projects rather than serious attempts and may miss some serious attempts that it is unaware of. Push funding might also run

into moral-hazard problems, with firms overstating their funded costs or moving overhead and expenses from other lines of business into funded-program expenses.

Perhaps the key benefit of pull relative to push is the powerful incentive it provides to achieve an ultimate goal, as seen with OWS. Development and approval of a vaccine were just milestones along the way to the ultimate goal of the widespread rollout of a safe and effective vaccine to the population. Specifying a generous payment per dose provides powerful incentives to develop a practical vaccine that can be produced at scale and to operate facilities that carry out that production. Push funding and milestone payments do not provide as powerful incentives to achieve that clear commercial goal. In certain cases, it may be impossible to sign procurement contracts before the product even exists. There may be too much uncertainty to determine a suitable target product profile. In the case of OWS, the product was known (vaccines), the pathogen was identified (COVID-19), and the suitable dosage, safety, and efficacy could be specified to accord with FDA regulatory standards.

Ahuja et al. (2021) recommended defraying most of the firms' program costs, say 85%, leaving the residual 15% to be incentivized via pull funding. By tying the bulk of the payment to firms' audited expenditures, push funding can limit what Laffont and Tirole (1993) call the "information rent" accruing to inframarginal firms. The residual of 15% or so of pull funding may leave the firm with enough "skin in the game" to mitigate adverse-selection and moral-hazard problems.

E. Leadership and Coordination

In "business as usual," public and private organizations have protocols and bureaucracies in place that allow them to function well enough without exceptional leaders. Missions are different. Missions represent a form of public entrepreneurship that requires leaders who do not merely have a vision for how to do things differently, but who are able to persuade a wide range of individuals and institutions to break through existing power structures and disrupt established protocols to participate in this vision. Missions succeed when they can incentivize participation and streamline coordination within and across departments and sectors. OWS relied on a toolkit of diverse policy instruments to support mission objectives. This included the use of instruments such as advance purchase agreements to incentivize the speed and scale of industry production, Other Transaction

Authority to facilitate contracting, the Defense Production Act to alleviate supply constraints, and Emergency Use Authorization to accelerate regulatory review.

Mission leaders must also have the vision and authority to make go, no-go decisions in real time. This is necessary to be able to manage a portfolio for speed and efficiency. Leaders require timely access to information to be able to pare down their portfolio and to redirect resources to remaining candidates. Leaders can acquire the needed information to make critical decisions by coordinating industry, government, and academic partners under the mission objective.

One example of where coordination was achieved across government agencies and between government and industry was the integration of regulatory review into vaccine development. OWS worked with the NIH and the FDA to establish common standards and protocols to accelerate the assessment of candidates. The FDA was able to give industry real-time feedback to enable timely, evidence-driven decision making. The FDA issued timely public-guidance documents on vaccine criteria to align industry and to demonstrate objectivity and transparency. In one instance, it took two weeks to move from a letter to guidance, a process that typically takes a year. The FDA allowed concurrent and combined clinical-trial phases, enabling preclinical animal studies to occur while conducting human trials and the FDA redirected staff resources to evaluate clinical-trial data in real time.

VI. Procuring Other Products under OWS

While vaccines were heavily favored as the fastest and surest way to end the emergency, OWS invested in diagnostic and therapeutic development as well. The search for drugs and diagnostics shared some characteristics of the vaccine mission; they each operated under a two-person leadership team that split scientific and operational oversight, invested in a portfolio of candidates, coordinated clinical trials, invested in manufacturing at-risk, coordinated supply chains, and employed the same raft of policy tools and incentives used for vaccines. But none of these approaches were pursued with the same degree of funding or coordination as the vaccine program. They each operated under smaller budgets, a less well-defined investment portfolio, and more loosely coordinated management structures and incentive systems.

Commentators have denigrated these programs “sputtering record of success” (Zelikow et al. 2023, p. 238). While we agree that their record is mixed, we believe that more research is

required to provide a precise assessment of how well these programs delivered on their stated mission objectives.

For example, while the United States initially rolled out diagnostic testing more slowly than South Korea and some other countries, the Rapid Acceleration of Diagnostics (RADx) program ultimately contributed to Emergency Use Authorizations for 32 products and the commercial availability of over 840 million tests (National Institutes of Health 2021). Similarly, the therapeutic team initially prioritized the development of monoclonal antibodies because they were relatively easy to manufacture and because they could be used to prevent infection and serve as treatment in both inpatient and outpatient settings (Slaoui et al. 2020). The therapeutic team took multiple shots on goal, implemented pull funding, and supported scaled manufacturing of monoclonal antibodies before the completion of clinical studies. Unfortunately, evolution of the virus soon reduced the effectiveness of these interventions.

The therapeutic team also worked with the National Institutes of Health's ACTIV (Accelerating COVID-19 Therapeutic Interventions and Vaccines) initiative to assess therapeutics under development. ACTIV assessed therapies using a total of nine master protocols from April 2020 to May 2021. These protocols allowed ACTIV to evaluate multiple drugs across different studies against a single control arm. By November 2020, the program had four therapeutics receiving Emergency Use Authorization: remdesivir, COVID-19 convalescent plasma, bamlanivimab, and hydroxychloroquine (Government Accountability Office 2020).[2]

VII. Additional Opportunities for the OWS Approach

A. An OWS for Alzheimer's?

The growing burden of Alzheimer's disease and lack of a promising cure has prompted many to call for an OWS-style effort. Across high-income countries, Alzheimer's is projected to be the first or second leading cause of years of life lost by 2040 (Foreman et al. 2018). The social value of improved quality of life, increased lifespans, and reduced cost of care projected into future decades likely amount to trillions of dollars, leading for calls for a global mission to develop an Alzheimer's preventive or treatment (e.g., Vradenburg 2015).

Alzheimer's disease arguably fails to satisfy key criteria to justify a full-blown, OWS-style mission. That said, certain OWS design features could be used to improve the efficiency of a funding program for it.

An Alzheimer's preventive or cure would be a blockbuster pharmaceutical providing enormous commercial returns even in the absence of government support. The disease exhibits no infectious or other obvious externality that would impair the commercial market.[3] In addition, curing Alzheimer's lacks some of the urgency of stopping a pandemic since a large swath of the population is not suddenly at risk from immediate infection. The steady flow of new cases can be treated without needing to spend prodigiously to scale up manufacturing and map out deployment, and without needing a direct line to the West Wing or a whole-of-government response.

Features of the OWS approach could be beneficial. More basic research may be required to move the problem up the TRL scale before commercial firms dive into research. The learning spillovers involved in basic research may limit commercial incentives to pursue it, calling for public support. To the extent there are adequate commercial incentives for innovation, the most promising approaches may be congested, possibly leaving other approaches underfunded. There could be value in the government providing push funding for basic research and for understudied approaches. Given the scale of the harm from Alzheimer's disease, multiple of these approaches could be considered.

Of course, the large potential value of push funding for Alzheimer's research has not been lost on the federal government, which appropriated 3.8 billion for this in 2023 alone (Alzheier's Association 2024). However, these resources did not require a mission, but were appropriated by standard law making, which could presumably be counted on for future funding. Whether sufficient funding is being spread to approaches that are less correlated with the most congested approaches is beyond the scope of this paper to answer, but the logic of OWS would suggest funding according to that strategy.

Another OWS design feature that would be promising for Alzheimer's disease is to accelerate clinical trials and regulatory review. Cummings et al. (2016) outline a series of steps that could streamline recruitment of clinical-trial subjects and prioritize Alzheimer's treatments.

B. An OWS for Carbon Removal?

Climate change is a major social challenge. The Intergovernmental Panel on Climate Change (2023) estimates that continuing policies in place at the end of 2021 will result in 3.2 ℃ warming by 2100. For the United States alone, according to government estimates, climate change is projected to increase annual federal expenditures by $134 billion and result in the loss of up to $2 trillion in revenue annually by the end of this century (Office of Management and Budget 2024). The Environmental Protection Agency (2022) estimated a social cost of carbon of $190 per ton (assuming a discount rate of 2%).

Absent a carbon tax, this high social cost is not fully internalized by consumers and firms, leading to socially excessive emissions contributing to further global warming. The Intergovernmental Panel on Climate Change (2023) suggests that 6 billion tons of annual carbon removal will be needed by 2050 to stay within the 1.5 ℃ target for warming above pre-industrial levels. The world is not currently on track to develop sufficient carbon-removal capacity to meet those targets, which makes carbon removal a possible candidate for an OWS-style mission.

The United States leads the world in the development of carbon-removal technology, despite the scattered nature of policy investments to date. In 2021, the Department of Energy launched the Carbon Negative Shot, setting a specific technical goal of reducing the cost of carbon removal below $100 per ton by 2032 (Department of Energy 2024a). Thus far, the initiative has committed up to $100 million to support pilot projects (Department of Energy 2024b). The Carbon Negative Shot is pursuing a diverse portfolio of potential technologies, including direct air capture with storage, soil carbon sequestration, biomass carbon removal and storage, enhanced mineralization, ocean-based carbon removal, and reforestation (Department of Energy 2024a).

An additional $3.5 billion of federal funding was appropriated for the Regional Direct Air Capture Hubs programs. The Inflation Reduction Act enhanced the tax credit for direct-air-capture projects involving geologic sequestration to $180 per ton. Even with all these programs combined, the U.S. government is still operating in the same modest range as private-sector efforts such as Frontier, a public-benefit corporation started by the payments-company Stripe in 2022. Frontier initially raised $925 million from various corporations to develop an advance market commitment for carbon removal (Calma 2022).

Accelerating innovation in carbon removal has the hallmarks of a problem amenable to an OWS-style mission. We already discussed the enormous social benefit that estimates suggest a

low-cost technology could provide. Avoiding harrowing temperature increases at the upper end of projections makes the need urgent. There is potential to specify concrete goals, say removing a target amount of atmospheric carbon annually at some price point (say less than $100 a ton) by 2030, scaling up the target amount each decade to 2050. Perhaps the key characteristic of the problem beyond its huge scale that lends itself to an OWS-style mission is that the benefit of carbon removal is almost completely external to the purchaser. Absent a carbon-tax credit, net-zero mandate, that appear difficult to enact, the commercial market provides essentially zero incentives for innovation in carbon removal.

An OWS-style mission could follow Carbon Negative Shot's approach to incentivize a diverse portfolio of carbon-removal technologies. But it could be much more ambitious in its goals, for example, targeting the removal of one billion tons of carbon annually by 2030 for less than $100 a ton, scaling up to 3 billion tons by 2040 and 6 billion tons by 2050. A mission of this magnitude would require much more investment, on par with the more than half a trillion dollars in estimated climate spending authorized by Congress this decade across the Inflation Reduction Act, Infrastructure Investment and Jobs Act, and CHIPS Act (Meyer 2022). The spending could be spread across push and pull mechanisms, including advance market commitments, innovation prizes, milestone payments, loan guarantees, direct research grants, and cost-sharing agreements for demonstration projects.

Following the OWS model, the carbon-removal mission would designate it as a national priority with an integrated command structure that could direct federal agencies to focus on the carbon-removal technology development and deployment mission over other government equities. Th mission could coordinate the cooperation among dozens of offices and bureaus within multiple departments: the Department of Energy (e.g., OCED, FECM, ARPA-E, and the National Labs), the Department of Agriculture (e.g., USFS and ARS), the Department of the Interior (e.g., BLM, BOEM, OSMRE, and USGS), the Environmental Protection Agency (EPA), the National Science Foundation (NSF), and the National Oceanic and Atmospheric Administration (NOAA). An OWS for carbon removal could include regulatory fast-tracking: expedited environmental impact assessments, faster permitting processes, and dedicated review teams to quickly evaluate project proposals and follow up on monitoring, reporting, and verification data.

VIII. Conclusion

The success of past missions can be seductive to public officials who want to draw attention to new programs to effect change. But not all problems are amenable to a mission solution. When public officials declare a mission, they are placing a high-risk, high-cost bet with public dollars. It is important, therefore, to know when a mission offers the right tool for the job and which program features are essential for success.

We identify five features that make a pressing social need conducive to an innovation mission. First, the problem should be of such national importance that officials are willing to make it one of their top priorities, meriting spending of extraordinary attention and budget resources. Second, the problem should be urgent, whether a race against a rival nation to reach the moon or a race to develop a vaccine against a surging pandemic. Less urgent problems can be solved by existing policies and procedures at a measured pace. Third, the problem requires extraordinary coordination across government agencies, requiring leadership with cross-cutting authorities. Otherwise, a single agency would be able to address the problem using its standard operating procedures. Fourth, the problem can be addressed with a well-defined goal to bound the problem and focus the mission. The goal for an innovation mission should be the development and application of some new technology. By contrast, infrastructure missions, such as the transcontinental railroad or interstate highway system, involve the wide-scale rollout of pre-existing technologies. Fifth, the problem requires market shaping. Both innovation and infrastructure missions address pressing social needs that the commercial market would not supply without public funding, perhaps because it has aspects of a public good such as defense or vaccination rather than being a purely private good. The commercial market can be better at aggregating preferences and incentivizing economical investment than the government for typical private goods.

We also identify five features that allowed OWS to succeed as an innovation mission. First, the foundation for the other program design elements is prodigious funding. Funding multiple candidates, signing lucrative purchase contracts, defraying firms' investment costs, building extensive at-risk capacity prior to approval, coordinating multiple government agencies, providing real-time regulatory guidance, prioritizing these over other products for regulatory review, all these steps require extraordinary spending, adding up to the figure for OWS's budget, variously reported to be anywhere from $18 billion to upwards of $30 billion.

Second, OWS took a portfolio approach, pursuing multiple vaccine candidates simultaneously, some using different technology platforms that might be less likely to fail together, in hopes that at least one succeeded.

Third, the program was willing to undertake what might be considered long shots in ordinary times but were worthwhile in an emergency context. The program supported two vaccine platforms that had not been previously approved by the FDA. The program funded large-scale expansion of capacity for these vaccines even before regulatory approval (called an at-risk capacity strategy) even at the risk of these funds being wasted if the vaccines failed in clinical trials, to avoid lags in spinning up capacity after approval.

Fourth, the program employed a mix of push and pull incentives. The program funded some firms' costs of development and capacity up front, but it also signed purchase contracts in advance of regulatory approval.

Finally, the program had strong and effective leadership, allowing it to coordinate resources across government agencies in partnership with industry under a single unified mission.

We do not claim that OWS was a perfect program. Some experts argued that the program was being too conservative in applying its own principles. In their op-ed, Athey et al. (2020) called for spending not $18 billion on six candidates but $70 billion on 15 to 20. Systematic analysis by Snyder et al. (2020) and Ahuja et al. (2021) reinforced the optimality of greater expenditures. OWS was focused on the nation, not the world at large. There is a danger of a single country monopolizing emergency supplies for its own citizens to the detriment of the globe, especially low-income countries which may not be able to afford a spot up in the contract queue. Done properly, advance investment in R&D and capacity by a world leader such as the United States can provide public goods for the rest of the world by being careful to honor contracts and avoid export controls. But international coordination is easier to achieve if the framework is worked out in advance by setting principles, treaties, and financing programs (Agarwal and Reed 2022).

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Endnotes

1. See Lupkin (2021) for an account of the use of the Defense Production Act under OWS and Siripurapu (2021) for general background on the Defense Production Act.

2. Blah Emergency Use Authorizations for convalescent plasma and hydroxychloroquine were revoked months later when subsequent evidence revealed insufficient therapeutic benefit.

3. Perhaps the most important limitation to the commercial market is that clinical trials of treatments that slow the progress of the disease take a long time to complete (Cummings et al. 2016), reducing the effective patent life. Extension of patent life would be a more direct policy response to this limitation than an OWS-style mission.