# Operationalizing Social Science for National Security

## Chapter Nine

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CHAPTER NINE

# Operationalizing Social Science for National Security

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In 2005, Defense Advanced Research Projects Agency (DARPA) Director Tony Tether approved a novel program: Pre-Conflict Anticipation and Shaping (PCAS). What made this program novel was that it was among very few explicitly social science–focused DARPA programs since the premature termination of Project Camelot—an ambitious Cold War social science program to study social processes associated with social and political upheaval or destabilization—in the early 1960s.[1] Project Camelot was cancelled because foreign scholars who became aware of the project publicly questioned the intentions of the U.S. government, not because the science was faulty. The legacy of the cancellation served as a moratorium on social science research funded by the U.S. Department of Defense (DoD)—a moratorium that lasted, with a few exceptions, for 40 years. The consequence was a loss of social science expertise. The gutting of social science programs and related personnel led to generations of program managers who were unfamiliar with social science research.[2]

One important question not answered by PCAS or any subsequent effort is the following: What is needed to discover social regularities to refine and properly scope social science questions so that they are useful for real-world prediction? Another, related question is this: Why is finding these regularities so difficult? The first step toward answering the first question is addressing the second.

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[1] Irving Louis Horowitz, "The Life and Death of Project Camelot," *American Psychologist*, Vol. 21, No. 5, 1966; Robert A. Nisbet, "Project Camelot: An Autopsy," *Public Interest*, Vol. 5, Fall 1966; Mark Solovey, "Project Camelot and the 1960s Epistemological Revolution: Rethinking the Politics-Patronage-Social Science Nexus," *Social Studies of Science*, Vol. 31, No. 2, April 1, 2001.

[2] Funding for social science research at the National Science Foundation (NSF) has been a very small slice of NSF's overall budget for research, limiting the extent to which expertise resides within the U.S. government's cadre of research sponsors, managers, and evaluators. From the 1960s to the 2000s, NSF social science research obligations as a percentage of NSF total research obligations hovered between 3.2 percent and 7.4 percent, with an average of 4.8 percent. Funding data from 1960 to 1991 are drawn from Otto N. Larsen, *Milestones and Millstones: Social Science at the National Science Foundation, 1945–1991*, New York: Routledge, 1992. Funding data from 1991 to 2010 are drawn from NSF's fiscal year budget requests (see National Science Foundation, "Budget Internet Information System," webpage, last updated October 2020).

In this chapter, I offer answers to these questions. First, after briefly discussing PCAS in the next section, I make explicit some fundamental, but not insurmountable, obstacles to the serious study of the social world. I focus on the value of a positivist social science approach, not because it is all that is needed to advance social science, but because this social science tradition has *largely* been overlooked by and excluded from recent investments in advanced research programs. The need for social science is recognized by funding organizations, as is the need to formalize or quantify social science to scale to meet the needs of the U.S. government. However, what seems to go unrecognized is that the social science traditions include validated approaches for the quantitative, formal, empirically based study of the social world and that there are hundreds of social scientists trained explicitly in these methods. These researchers, just like their qualitative social scientist colleagues, are trained in theory and substantive areas, as well as in the mathematical and computational skills used today by scientists in other disciplines. The difference is that these social scientists are trained specifically to capture and analyze social phenomena using these formal tools and techniques. They are also well trained to engage with their qualitative counterparts and with researchers from other fields. However, this type of specially honed expertise is underutilized, and this underutilization is detrimental to advancing social science innovation and application. As a final thought pertaining to this discussion, quantitative social scientists should be sought after as leads on advanced research programs that specifically seek to advance theoretical and practical applications of social science.

The next section lists some of these challenges. I argue that progress and precision in social science is lagging because social science requires that people study themselves; doing so presents specific challenges and requires coordination and tools that have not yet been invented. Making major advances in social science would require a way to observe, record, coalesce, organize, and process the right data. Determining how to make these advances will require serious consideration about what the study of people requires.

After discussing these challenges of social science research, in the next section I present a very high-level summary of the goals and processes of the social science research process as it stands today. This discussion makes explicit how the challenges of studying social phenomena prompted social science researchers to invent a host of novel approaches for observing and recording the social world from many different perspectives. Some of these approaches do not conform to the norms of *scientific inquiry*; however, operating within the familiar structure of the scientific method, these approaches nevertheless produce validated general theories.

Finally, I conclude with recommendations for structuring research and programs to advance methods for studying the social world and identifying the most-essential and most-feasible as-yet unanswered social science questions. These preliminary questions will need to be addressed to enable the eventual answering of more-vexing and more-ambitious questions and better integrate knowledge of the social world into the operations of the National Security Enterprise (NSE).

## Reintroduction—The Story of the Pre-Conflict Anticipation and Shaping Program

The specifics of the science advanced by PCAS are published elsewhere.[3] In this chapter, I discuss only the relevant aspects of the PCAS program design. In the context of under-governed spaces (UGS), the story of PCAS illustrates the types of questions that must be answered about the measurement of the stability and durability of governance structures, as well as the need to turn to the social sciences and social scientists to make these measurements.

At the start of PCAS, research teams were selected to represent the broad scope of social science disciplines; the teams consisted of political scientists, anthropologists, psychologists, economists, and sociologists along with mathematicians, computer scientists, and engineers. Ten interdisciplinary social science teams were given six months to develop causal models to characterize the stability of two Pacific Rim countries.

Each team brought a different approach toward the study of social phenomena to the program. During the first six months, teams shared data and insights in a competitive–cooperative environment. They then proposed theoretical causal "predictions" about the stability of these two countries six months later. The PCAS modelers submitted predicted outcomes and their causal models, which showed how increases in specific variables would precede increases or decreases in the ultimate variables of interest—state stability and political unrest. At the end of the year, the program manager briefed out the effort, comparing the teams' predictions with the state of the real world.

One of the countries had experienced upheaval, which had been predicted by the social science models but otherwise unforeseen by most regional experts. The other country, which many political experts had worried would become more unstable, did not change much—as the models had predicted. Demonstrating that social variables could be observed and measured and that an association between a change in one and a change in a second was possible convinced Tether that social phenomena could be studied using the scientific method and that an investment in a social science program should move forward.

PCAS's objective was not to advance science or technology but to answer a fundamental epistemological question: Is there *science* in social science? In 2005, social science was so alien to DoD that research and development (R&D) leadership had to be convinced that it is possible for people (e.g., social scientists) to observe and measure relationships among *social* variables in much the same way that physicists can observe and measure relationships among *physical* variables.

[3] Robert Popp, Stephen H. Kaisler, David Allen, Claudio Cioffi-Revilla, Kathleen M. Carley, Mohammed Azam, Anne Russell, Nazli Choucri, and Jacek Kugler, "Assessing Nation-State Instability and Failure," in *2006 IEEE Aerospace Conference*, Big Sky, Mont.: IEEE, 2006; Steven C. Bankes, Robert J. Lempert, and Steven W. Popper, *Pre-Conflict Anticipation and Shaping (PCAS): Models-2-Shaping Integration*, Topanga, Calif.: Evolving Logic, September 2005.

I was involved in PCAS from its inception through its conclusion, and I was in the room when the program manager pitched PCAS and briefed the findings. The outcome of PCAS was not a surprise to me: As a mathematical sociologist trained to formalize social science theories and quantify complex and often qualitative constructs, I took for granted that the social world could be measured and modeled. It was only once I became involved in the world of scientific research funding that I discovered two contradictory and dominant opinions about social science that pervaded the research-funding community. It seemed that one half of the program managers I spoke with believed that the social world was beyond scientific exploration—that is, that social phenomena were so special, unique, diverse, and random that any attempt to make sense of them was folly. The other half seemed to think that the phenomena were not only amenable to study but also simple, or *soft*, and that all that was needed to understand the social world was to put really smart scientists—*not* social scientists—on the task.

To the few aware of the program, PCAS showed that neither opinion was true. Rather, PCAS revealed conclusively that social phenomena can be studied scientifically, but it does not mean that it is easy to do. Studying people offers many challenges that, although not unique to social science, are exacerbated by the nature of the phenomena. Therefore, it seems as though the social sciences have advanced at a slower pace than the physical or biological sciences. Even so, the main conclusion drawn from PCAS was that social scientists have identified and can somewhat measure some important variables that indicate or foretell the future state (condition) of a human group (state, region, community, interest group). Social and political outcomes are neither completely random nor so sensitive to initial conditions that any prediction is impossible.

PCAS showed that well-scoped social science questions are answerable using the framework of positivism familiar to physical and life scientists. There are patterns to observe and measure in the social world, similar to patterns in the physical world and biology, and social scientists, using tools available today, can glimpse and describe these patterns. PCAS also revealed that the theoretical relationships among key variables and the tools to measure the variables were far from precise. Science requires precision, and social science has a long way to go to perfect its tools; the laws of human behavior are more variable than the laws of physics. The reason for this lack of precision is not that there are no regularities, patterns, or functional relationships in the phenomena, nor is it that those who study social science, unlike those who study physical or biological science, are bad at modeling and measurement. Yet the comparative lack of precision of social science has somehow been interpreted to mean that hard-science methods are better than social science methods for observing and measuring social science phenomena. This is an absurd conclusion because what requires measurement is so fundamentally different. There is no basis to assert that what works to measure the physical would be appropriate for measuring the social.

Social scientists have made progress in developing some very dependable general models. For instance, social scientists know that economic decline very often precedes civil unrest, even though they cannot provide a single general equation that relates the two with the precision of a chemist's gas laws. A social scientist can say which factors or variables are proportional or inversely proportional, but what is known is not sufficient to formulate trustworthy equations. To discover these *laws of social science* would require the ability to study the relationship among selected elements of the system in a closed or controlled system, such as the laboratory or a petri dish. This level of experimental control was required to make essential breakthroughs in discovering the laws of physical and biological phenomena. However, it is hard to keep exogenous factors from creeping into studies of the social world. Social scientists have not yet determined how to bound their models to ensure that all relevant factors are present in any particular model. Consequently, the existence or value of social science constants, if they exist, is still undiscovered; therefore, the precise formalization of social science principles is not yet possible.

## Separating the *I* from the *Me*: Challenges for Social Science Research

A good first step to tackling challenges in social science research is to enumerate them. What follows is a first attempt at classifying the main challenges to studying social phenomena. Not all of these classes of challenges are unique to the study of people; several have analogues in other disciplines. When this is the case, I highlight the similarity to illustrate examples of how the challenge could be addressed.

### Recognizing That Being Human Does Not Qualify Someone as an Expert in Understanding Human Social Phenomena

One challenge unique to the study of humans is that humans think they know and understand themselves; as a result, the systematic study of humans is undervalued. The most important thing to remember when studying people is that being a person does not provide any insight or benefit toward understanding phenomena involving humans. Quite the contrary—people observe the world only from their own perspectives, and, although they have opinions about how the social world works, those opinions are neither science nor knowledge.

For example, a citizen of India (or of another country) who is a social or political scientist who studies Indian politics is an expert on Indian politics; a person who is solely a citizen of India, however, is not. The Indian citizen is a data point. The social or political scientist gained their expertise by reading the research of others, collecting data, and doing analyses. Whether that expert is an Indian citizen does not matter. The citizen part of the scientist is also just a data point. People do not know any more about people, what they believe, or how they work because they are also people than they understand quantum physics because they are made up of matter. Expertise comes from study.

Yet the atomic physicist has credibility because he or she studies physics, whereas the social scientist often does not have credibility because lay people or hard scientists who attempt to study social phenomena think they have expertise because of their lived human experience. Physical or biological scientists would never generalize from an *n* of 1; similarly, we should not generalize from ourselves.

## Recognizing Our Default Assumptions and Biases

The most unique and glaring challenge of studying people, or of people studying people, is objectivity. We care differently about the outcome or finding when we study people than when we study atoms or plants. Even when we study people, groups, or cultures we do not identify with, it is hard to remain objective and it is hard to recognize our biases. Social scientists must consciously remind themselves of their biases; therefore, social scientists have incorporated elements in their methodological approaches that control for the effect that their presence might have on their ability to objectively observe and record.

In most studies, people are not directly studying themselves; most of the time, they are studying an "other." This introduces a challenge related to perspective and prioritization. When studying this other, one's focus tends toward what is new or interesting about the other. Usually, that moves the focus on differences from one's self as a point of reference. What lay people find interesting and report is novelty, which is influenced by their own experience. This is unavoidable—it is human nature—but only by being explicitly aware of this tendency is it possible to create methods and tools to broaden the aperture.

## Noticing the Similarities and the Differences Among Humans

The mundane, the commonalities, the regularities, and the unexceptional are what really hold together societies, but they go unobserved. Consider the story of a Western visitor to China who witnessed a wedding. Upon the visitor's return, they report with astonishment to their friends that the bride wore a red, not a white, wedding gown. What is really astonishing is that halfway around the globe two young people were matched and united in a ceremony. The ceremony was public. Friends and family attended. It was full of ritual. There was a feast and dancing. The bride wore a special outfit, and it had a special color. All of these instances are identical to customs back home—and all of these instances go unstated. What is reported are the differences: The groom does not smash cake into the face of the bride, and the bride wears red instead of white. The conclusion drawn is that *cultures* are so different, but the story reveals striking similarity.

To measure and quantify regularities across context requires recognizing and formalizing social *regularities*, but those are not interesting. The consequence is that a large portion of reported observation, and a large portion of funding for social science research, is focused on the exceptions. If the goal is to reveal the hidden laws of social science, then it is important to notice the rules and not focus only on the exceptions. However, for people who are studying other people, the rules are often overlooked and not reported.

## Understanding Our Limits and the Timescale of Social Phenomena

Will the grand American experiment ultimately be successful? No single person can answer this question because the experiment is not over and will outlast anyone alive today. This example underscores another challenge for social scientists: They live in the same space and time as their objects of study. However, awareness of bias is not always enough to mediate its effects. This is especially problematic for studying people at a geopolitical scale. The cause-and-effect cycle predates and can outlive the scientist. Another example is our 21st-century perspective on the two world wars. In the 1920s, World War I was considered over. Social scientists studying that war in 1925 would consider it a historical event, but most historians today consider the two world wars as a single protracted conflict.

This problem is not unique to social science. Climate science offers similar challenges about time and scale, and, much like with social science, advances in climate science have suffered because humans are invested in the interpretations of the scientific results. Even so, whatever challenges that climate scientists face are even tougher in the social sciences. Data on human activity are more ephemeral and often do not leave clear physical traces. The archeological record is more prone to interpretation than the geological record (which also does not provide clear-cut answers). Prehistory is a mystery, and history (i.e., "his"—the winner's—"story") is biased in that, at best, it is incomplete. This timescale challenge relates to the perspective challenge. What people think is interesting or salient is often temporally proximal. The result is that a great deal of study is focused on occurrences within a very short time span that may or may not be representative of the phenomena more generally. Attention is focused on small differences between specific examples rather than on the structural similarities that events through the ages share.

This timescale issue is especially true for research enterprises focused on meeting short-term, applied needs of government or defense agencies. Within the NSE, the lines between social science, policy, and intelligence analysis are blurry at best, even though these fields have different objectives. One possible driver of this confusion might be that many intelligence and policy analysts have degrees in social science subjects and use data collection methods and analytic techniques developed for social science purposes. The result of this is the misconception that the product of social science should reveal short-term, specific actionable insights. That is not the case—again, social science is a collection of theories and methods, not factoids. The social scientist seeks to understand the underlying structures and pervasive patterns of behavior, especially differences that stem from location, timing, age, and "culture."[4]

[4] I have deliberately used quotation marks here to emphasize how difficult it has been to define *culture* and how differently it has been defined and measured across the social sciences.

## Recognizing That Administering Interventions and Assigning Control and Treatment Groups Is Rarely Possible

Social change may be too slow (i.e., months, years, decades) to study in the lifetime of one social scientist, but it does occur faster than evolutionary change. Designing an experiment to understand social change is difficult, and perhaps impossible, because the study (or the presence of the observer) can alter the behavior of those being studied. Biological systems often adapt quickly to changes and interventions, and these adaptations are measurable in a condensed time frame. A scientist who is analyzing the adaptation of bacterial colonies, even on the genetic level, can easily acquire data and make a clear comparison with an untreated bacterial colony. However, studying people is more complicated: It is not always possible to find a control group for comparison, and introducing change into social systems requires special care in protecting subjects to ensure that no harm is done. This is especially true for macrolevel studies, in which an intervention can generate second- and third-order unintended consequences. In many cases, introducing interventions or treatments is impossible because even small interventions can have huge impacts on large segments of populations, which leads to restrictions on studies that affect people or can cause harm.

The challenges highlighted here come together when attempting to determine the criteria for evaluating the success of a social science project. It is tempting to observe a situation and attribute an outcome to an intervention or a change in the status quo, but social scientists cannot draw that conclusion. Research is most often inconclusive if it is focused on the results of a geopolitical event because researchers only have a single example to study. Any confirmatory findings might be coincidence—and a negative result may be a false negative. A large sample of examples is necessary for the systematic testing necessary to demonstrate that a certain social science model is valid. However, this sort of testing is difficult to do when a phenomenon rarely occurs.

## Anticipating That Criticism Inhibits Progress

Finally, a challenge that is unique to social science that actively hinders progress is the ease of critiquing social sciences. The underdevelopment of social sciences presents challenges that scientists in other fields in a similar state of development did not have to deal with. Much of the early progress in the physical sciences was made using methods that today would be considered crude or poorly developed; today, all sorts of restrictions exist thanks to lessons learned from the mistakes of this early work. In addition, early physicists and astronomers made measurements, formulated theories, and tested them in their laboratories without worrying that their theories might not generalize. Boyle, Faraday, Kepler, and Newton were not worried about limitations of their work from later developments by Heidegger, Einstein, or Feigenbaum.

However, at this point, "everyone is a critic." Social science tends to be less brave in its assertions and in reporting findings. Social scientists know that a study done on one population might not generalize or that a model might have only appeared to have worked because a key variable was not present in the cases studied. Social scientists are well aware that whatever they find is likely to eventually be improved, so they are very careful to couch their claims conservatively and focus only on their fields of study.[5] This tendency toward modest and bounded claims makes the discovery of general rules less likely.

Although the awareness of people's limitations in studying other people can be a liability that slows progress in social science, that awareness is also valuable. Because social science practitioners know about these liabilities, they are trained in methods to mediate their effects. In the next section, we review some of the methods that social scientists have developed to limit bias and make progress given these challenges.

## Methods That Social Scientists Have Used to Meet These Challenges

Misconceptions about how social scientists go about learning about the social world have led to people untrained in social science research methods to question whether social science is science. Many aspects of social science differ from the way most physical and biological scientists conceive of scientific inquiry. Specifically, from the outside, it appears as if social scientists do not often use the main tool for scientific inquiry—the scientific method. This is not the case. The scientific method is an integral part of the social science research enterprise; it is just that social scientists have adapted the method to meet challenges specific to their field. Engagement in the study of social phenomena has taught social scientists that adopting the physical or biological methodological paradigm to address the complexities of social science is too limiting. Instead, they have adapted and extended the paradigm, seemingly in ways that some scientists from other fields do not recognize as science.

Most concepts fundamental to social science inquiry are not amenable to direct measurement or to laboratory studies, so social scientists have spent over a century developing techniques, rules, and conventions to describe and understand the social world from observation done in settings less controlled than laboratories. But the scientific method is still at the core of the investigation. For social scientists, like physical or life scientists, the scientific method consists of the coupled processes of *induction*—building theory from systematic observation—and *deduction*—testing the theory. For social science, not unlike astronomy and ethology (the study of animal behavior), data used for theory building most often are collected from observations that occur in natural settings outside the controlled environment of a laboratory. A great deal of information that inspired prevalent theories about the social world has been collected in a manner that appears less systematic than methods of collection for other sciences.

Qualitative research is a necessary and important element of the social science endeavor, but it is not all there is. If qualitative or descriptive characterizations of social phenomena

[5] Kieran Healy, "Fuck Nuance," *Sociological Theory*, Vol. 35, No. 2, June 1, 2017.

were all that social science provided, then criticism that social science is not science would be reasonable because the work would only be inductive, and science requires both induction and deduction.

However, induction is not all that social scientists do. Social scientists devise ways to test and retest their theories, just as astronomers and ethologists do. In social science, as with these other sciences, only theories that can withstand repeated empirical scrutiny persist. To be clear, rich qualitative research is necessary to build theory and should not be devalued as an important part of the social science process. Still, testing these theories also is a necessary part of the overall process.

Testing social theory requires carefully measuring social variables, but this is difficult because social science phenomena differ from those of other sciences in two important ways: (1) variables are usually not directly measurable and (2) the phenomena can become unmeasurable, change, or disappear in a controlled setting.

Gaining meaningful insights about the social world and finding generalizable rules about human behavior require the development of innovative techniques to observe and record behavior; some of these techniques are unique to social science (i.e., interviews, survey research, content analysis), others have analogues in other fields (i.e., observational methods, network analysis), and some were developed for social science uses but have been adapted widely for use in other fields of science (i.e., sampling, statistics).

## Social Science as a Collective Effort

Many social scientists are trained in methods that scarcely resemble the positivist tradition that dominates other branches of natural science. For instance, historians and ethnographers can conduct and conclude their study and neither articulate nor test a theory, leading some to think that the work is not *scientific.* These researchers obtain their knowledge by reading and synthesizing the studies done by others and by spending a great deal of time ensconced in the subject matter, observing and learning about the one subject. There is no expectation that these approaches require a positivist frame.

Moving beyond the constraints of positivism expands the potential for high-quality understanding and deep insights. Not only is the accumulation of this type of knowledge useful, sought after, and appreciated in the social sciences, a deep dive into the nuance and detail of a subject may be necessary to recognize new insights and to generate new and useful theories for many subject areas. Here, the issue is that this one study alone is out of context. However, from the perspective of social science writ large, the contribution of one study—a precise description and deep understanding of a specific case—is essential to the larger endeavor. These studies generate insights that inform and refine theory and data that can be used, perhaps by others, to test theory. That the one method used alone does not seem scientific misses the point: Each study is just one part of the collective enterprise that is social science inquiry, which at its core relies on the scientific method, a structure for supporting, rejecting, and refining theoretical assertions.

In contrast, other social scientists fullheartedly embrace the scientific method within their work. These researchers see their objective explicitly as formulating and testing theory, although most recognize that their contribution may not span formulating and testing theory. These social science researchers see the *inductive-deductive loop*—a process of stating, testing, and then refining and retesting theory—as central to the social science research process.[6] Deductive studies more closely resemble studies from other sciences. They require a distilling of the detail of cases, sacrificing depth and nuance to find common or general patterns among many cases. It is these studies that directly and obviously rely on the *scientific method*—standards for retaining a theory that stands up to empirical testing associated with science—but these studies also rely on the production of rich inductive work to provide enough knowledge to formulate theories to test.

The reliance of the social sciences on the scientific method is not obvious just from looking at a single study, and many social scientists will insist that they do not subscribe to the positivist approach. Regardless of intent, however, these researchers contribute to a cumulative process that does conform to the basic tenets of science. The data they collect feed the beast. The qualitative findings help refine theory and broaden the pool of examples to use in analyses. As long as there are also some researchers who test theory, social science as a science can advance. Fortunately, each social science discipline involves, and perhaps is dominated by, researchers specifically trained to measure social science concepts and formalize social theory. These experts in computational and formal modeling and statistics are also experts in social science theory and in knowing how best to study and model the social world.

## Testing Theory in the Real Social World

As mentioned, a great deal of what is interesting to humans about humans is not amenable to traditional scientific approaches, but it would be tragic if the only way to learn anything about humans were through controlled laboratory studies. Some types of research must be done *in situ*. That is the value of rich, inductive qualitative case studies. Just as deductive studies can validate what a qualitative observer reports, inductive studies are needed to validate deductive work that is too distilled and sanitized to be realistic. In addition to benefiting from the collective nature of social science, researchers began developing techniques that allow hypothesis-testing in natural settings. These techniques, which largely fall under the label of *quasi-experiments*, allow researchers to hypothesis-test their assumptions about

6 The pursuit of generalized knowledge of social systems is referred to as *nomothetic* research, while findings that are contingent or applicable to a specific case are referred to as *ideographic*. For discussions, see Douglas V. Porpora, "On the Prospects for a Nomothetic Theory of Social Structure," *Journal for the Theory of Social Behaviour*, Vol. 13, No. 3, 1983; Joseph M. Bryant, "On Sources and Narratives in Historical Social Science: A Realist Critique of Positivist and Postmodernist Epistemologies*," *British Journal of Sociology*, Vol. 51, No. 3, 2000; and Rudra Sil, "The Division of Labor in Social Science Research: Unified Methodology or 'Organic Solidarity'?" *Polity*, Vol. 32, No. 4, June 1, 2000.

causal social science relationships while minimizing interference with the natural behavior of the population under investigation.

The deductive process in social science is reductionist, which has positive and negative aspects. Distilling complex abstract concepts into specific measurable examples (indicators) to devise a test is done at a cost: losing the depth or nuance of the original concept. However, it is important to remember that deductive studies do not really test theory; they only test hypotheses (specific instantiations of the theory). It is only after repeated tests of a theory, in different ways and contexts, that the findings might generalize. This is why it is important to find the right specifics to test or to properly *specify the model*. It is also why it is important to conceive of social research as a cumulative endeavor and not the findings of a single study.

As in the hard sciences, the experiment is considered the gold standard for demonstrating cause and effect. If, however, the only legitimate means to learn about the world is by using laboratory experiments, very little would be known, or knowable, about the social world. The experimental model is so strongly associated with deductive research that there is a tendency for people to think that studies that are not structured as experiments do not lend themselves to theory testing. Unfortunately, as mentioned before, social science phenomena are hard to capture and study in a laboratory. Attempting to study complex societal interactions under experimental conditions would force what is observed into a state of artificiality unlikely to resemble what occurs in the real world. A study of this type would be thought to lack *external validity*. This is a general criticism of almost all laboratory work in social science. On the other hand, any move to relax any feature of an experiment introduces threats to *internal validity,* confidence that the observed or deliberate change in one variable is the cause of an associated change in a second variable. The challenge to social scientists is to design studies that balance internal and external validity.

In their seminal treatment of this topic, Campbell and Stanley (1963) juxtaposed all manner of social science research approaches with the randomized controlled trial (RCT) or *true* experiment to illustrate how and why other approaches to study, including case studies, are vulnerable to threats to internal validity. They also illustrated how specific features of the RCT mitigate these threats.[7] The Campbell and Stanley framework revealed the cost to internal validity of relaxing specific features of an RCT, enabling researchers to properly use quasi-experimental approaches with an awareness of exactly how their design is vulnerable to plausible rival hypotheses. The framework provided a schema to easily identify—and perhaps to find ways to mitigate—the design weaknesses of a study at a glance.

For example, this framework made explicit the threat to internal validity of conducting research to study the effect of a change in status quo in the absence of a control group when it is not possible to find a control group. Rather than not run a study because the conditions are not perfect, the study could proceed—with the researchers cognizant of the limitations. Another instance might present the opportunity for a control group, but with the caveat that

[7] Donald T. Campbell and Julian C. Stanley, *Experimental and Quasi-Experimental Designs for Research*, Boston, Mass.: Houghton Mifflin, 1963.

the assignment to the group was not random—the classic definition of a quasi-experiment. In a third case, there may be two comparable groups—one that experienced an intervention and one that did not—but because there was no measurement of the groups prior to the change, it is impossible to know whether the measured outcome was from the hypothesized cause. Each of these examples has serious flaws, but when the flaw is understood, it provides a guide to recommend complementarity studies. If many studies, all flawed but flawed differently, reveal the same underlying relationship among variables, then social science can begin to make claims about a relationship among those variables. What should not be lost is that the most important contribution here is that this framework provides a guide for capitalizing on, rather than missing, opportunities for testing hypotheses in natural settings.

Campbell and Stanley did not intend to exalt the RCT above other methods. On the contrary, the point of their treatise is to reveal the similarities and complementarities among very different designs by deconstructing social science research into a set of shared elements.[8] This decomposition reveals how different designs relate to one another and offers researchers a scheme within which to classify any particular study. The objective was not to discourage the use of the broad variety of designs but to encourage the informed use of quasi-experimental methods.

In recent years, explicitly multi- or mixed-method approaches have gained favor, but this is a relatively new development. In the past, and for most research today, social science relies on a division of labor. Each researcher carves out both a substantive and methodological niche within which to focus and contribute. As for substance, most researchers only focus on a very narrow topic that interests them, but, even in the investigation of a very narrow topic area, the phenomenon under investigation is so complex and humans are so limited and biased that the only way to begin to understand the topic is by using many methods, in many contexts, over time. Despite this recognition that social science is a collective endeavor, for the most part, the primary mechanism for integration of different studies is ad hoc. One researcher reads the work of another, is inspired, and fills in a gap. Until recently, literature reviews and discussion sections in journal articles were the most common means to sew together a body of research. It is only recently that formal meta-analytical approaches have provided the means to formally compare and aggregate studies. Therefore, it is not surprising that for people outside the social science research community it appears that each study stands alone; the prominence of deduction and hypothesis-testing in social science research is not obvious because most of the social science studies best known to the general public are only inductive.

To discover regularities and general principles shared across human engagements is a mammoth task. Social research explicitly recommends repeated grinding through the inductive-deductive loop. No single inductive, deductive, or mixed-method study is sufficient for generating sustainable generalizable insights because the social world is full of variance. In social science research, to draw conclusions requires more than the type of experi-

[8] Campbell and Stanley, 1963.

mentation and replication used in science. Just as clinical trials with human subjects, not just laboratory experiments, are needed to prescribe a drug, multiple complementary methods are needed to understand how the cause-and-effect relationship observed in a laboratory translates to the more complex system.

For the social sciences, even more repetition and reproduction are required to draw a generalizable conclusion. It is not enough to rerun the same study under identical conditions; for social science phenomena, this is usually impossible, but, even if it were possible, the findings would be too idiosyncratic. Unlike gas law experiments, which can be replicated anywhere at any time to give fairly identical results with each experimental run, social science research requires replicating the main effect regardless of context; if that is not possible, it requires identifying what factors are the differentiators to make more precise the scope conditions (boundaries on the studies, caveats, and controls), which, if met, do produce the expected result. The goal is to replicate, as much as possible, what the scientists can control and, rather than expecting identical results, noting which factors differed. In this way, the approach is similar to the gas law example insofar as the results of an experiment performed in Nepal versus Death Valley might differ and noting what factors differed was informative to theory development. To find general principles in social science that transcend a single case replication implies looking for similar findings using different approaches to measurement of the same concepts in a multitude of contexts.

## The Seduction of Induction

Social science—like other fields of science—is not a set of facts about the way things work. Instead, it is a method for finding the facts. The method requires the feedback loop between induction and deduction. The method also requires replication under many conditions. It is only through that iterative process that social science regularities will be revealed.

It is important to remember not to overgeneralize or overemphasize the findings of a single study about an interesting topic or case. The social science research that least resembles science—interesting writeups about historical events or exotic people—is most likely to capture the imagination.[9] Given that this research might generate theory, it contributes to social science, but, if the theory does not stand up to empirical scrutiny, it is imprudent to generalize from that single case. Inductive work alone—whether traditional qualitative social science research or today's computational social science models—is only suggestive. Interesting cases or correlations do contribute to social science, but they do not stand alone. Science searches for regularities, not anomalies, but the most interesting preliminary findings in social science are interesting because they are not typical. Like idiopathic cases in medical science, single cases are interesting because they raise interesting questions, but to contribute to science and be useful the cases must be considered within the context of what has been learned using the sci-

[9] Many studies attributed as social science research are done by non-social scientists. Some of what non-social scientists consider social science is really journalism or political commentary.

entific method. When an idiopathic case presents to a doctor, it must be treated, but successful treatments are not derived from that one case; treatment options are developed from the understanding of general medical science principles learned from studies of more common ailments and from an understanding of physiology, biology, and biochemistry. Doctors do not use what is learned from the exceptions to the rule, or the outliers, to recommend general treatments. Hundreds of laboratory studies are needed to prepare doctors to treat these atypical idiopathic cases when they present. As Stephen Marrin and Jonathan D. Clemente put it,

> In the medical field, one of the most often repeated pearls of wisdom for diagnosing patients is that "uncommon manifestations of common diseases are more common than uncommon manifestations of uncommon diseases," or "when you hear hoofbeats, look for horses and not zebras."[10]

In a similar vein, social science studies that are focused on, or conducted in, environments that are outliers may be socially interesting but unlikely to advance social scientific knowledge independently.

Much that falls under the rubric of social science research scarcely resembles what scientists and the general public think of as science, but that is only because of the qualities of social phenomena. Social science appears different from science, especially in those aspects most non-social scientists are exposed to. However, laypeople are not exposed to those less familiar, "more scientific" aspects of social science: the division of labor and the development of methods for measuring ephemeral complex constructs that cannot be observed using more conventional scientific approaches. One key point is to emphasize how essential it is to consider social science inquiry as an ongoing, iterative collective endeavor. Complementarity of studies is essential if social science is to advance. Complementarity implies that the strengths of one study can compensate for the weaknesses of a study on a similar subject. Replication and even seeming redundancy is necessary for both inductive and deductive work, as well as the necessary complementarity of inductive and deductive efforts. It is only by addressing social science questions from multiple perspectives and from multiple angles using multiple methods that the fields of social science can accumulate a collective understanding of a phenomenon. Given the challenges of studying social science, this cumulative and collective approach is required.

## What Is Needed to Accelerate the Social Science Enterprise in National Security Work

Opportunities exist for research sponsors to create programs to accelerate the social science enterprise. This will require changing how social scientists, the sponsors of social sci-

[10] Stephen Marrin and Jonathan D. Clemente, "Improving Intelligence Analysis by Looking to the Medical Profession," *International Journal of Intelligence and Counterintelligence*, Vol. 18, No. 4, December 1, 2005.

ence research initiatives, and the users of social science research conceive of social science research. This section is focused on how recent government initiatives to advance social science conceive of the social sciences, although the general arguments apply to all sponsors and users that seek to employ social science in their strategies, plans, and operations. The need to advance conventional social science is driven by the recognition that theory and methods do not yet exist that can be reliably used to anticipate future social states using information gathered in real time and that a capability of this type would be advantageous.

For example, within DoD, there is a hope for social science products that are based on the creation of new *sensors* and computational models. The warfighter would use these sensors and models to predict the direct effects and anticipate the second- and third-order effects of their actions or inactions on the people in the environments in which they operate. To realize this vision will require programs centered around basic nomothetic and deductive social science research. To develop this social science capability will require revolutionary advances in data collection and measurement capabilities, so there are reliable and valid data available to test and validate formal social science causal models. Investment in the studies that use those data to test and refine foundational and generalizable social theories also is needed.

The obvious limitation of social science is that it seems immature or underdeveloped when compared with the hard sciences. Specifically, theory formulations seem to be too immature to be the basis for reliable tools. There are no validated formal models, such as $F = ma$ and $PV = nRT$ in physics, that can be confidently applied in real-world settings. It is not that social science researchers using conventional social science research methods do not regularly learn a great deal about the phenomena they are studying; they do regularly achieve this, even to the point of developing usable formal models (even if these models are not generalizable). However, although some general principles about behavior and the social world are known, what is known is not precise, and what is precise is not generalizable. Relational patterns are directional, but, unlike formal gas laws, the plug-and-play functional forms of equations have not yet been discovered.

In that sense, social science today is a bit like planetary physics in 1600. There were models in 1600 that worked well enough, but these models needed to be updated occasionally because they got out of sync. Luckily, thanks to Tycho Brahe's meticulously collected and recorded celestial data and the serendipity that brought Johannes Kepler to Prague to work with these data, old geocentric models were retired and more-accurate heliocentric elliptical models replaced them. These corrected theoretical models—Kepler's three laws of motion—were necessary for accurate predictions. Once *discovered*, these three laws could be inserted with confidence into a computational model to produce reliable and believable predictions about where a planet will end up at just about any point in the future.

The Brahe-Kepler metaphor perfectly illustrates the three major objectives that should be at the core of programs seeking to advance social science. The first objective is represented by Brahe's work: creating tools needed to obtain precise measurements of social science variables. The second is creating standards and tools for coalescing data and making these data available and usable to the theorists. The third is finding those theorists with the skills and creativity required to properly use and to draw the proper conclusions from the data. I discuss each one of these objectives here.

## The Need for New Tools for More-Precise Measurement

The precision with which Brahe measured celestial objects required the invention of special tools and careful recording. From the perspective of the social scientist, it was easy for him to identify what he wanted to track: The phenomenon under study consisted of discrete physical objects that most would agree would exist in the absence of human consciousness. Social scientists have the unique challenges of measuring objects, or subjects, that are not as discrete and that in some ways exist only because of human development. Tracking democracy is more nuanced than tracking a comet. That is not to say that all key social variables are purely social constructs. Some variables present only the same types of challenges that Brahe faced. Most demographic variables (i.e., fertility, mortality, morbidity, migration) require that social scientists make the same types of decisions for measurement as the astronomer: how we define units of distance and time for measurement. For example, counting or tracking the number of births in a given geospatial segment for the time it takes for the earth to rotate the sun one time defines fertility according to the tracking of a celestial object for a year. Measurement of variables of this type are amenable to error, but there are few discrepancies between studies. Measurement of variables that are purely social constructs, such as happiness, group cohesion, identity, and democracy, are more challenging to define; therefore, the measurement of these constructs is less consistent and precise. There is no way to measure these variables directly, and so social science hypotheses are tested using latent, not direct, measures of most key variables. What is measured, how, and what it means are closely tied to theory, but different scholars invent unique ways to measure identical concepts.

Another feature of social science research is the sensitivity of results to sampling bias. If two studies have identical approaches for measuring a concept but their approaches for collecting samples differ, then it is not possible to be certain that the difference in the relationship between variables that they observe is the result of differences in the population. It is worth noting that other sciences also rely on latent variables. Astronomers today construct latent variables when studying distant objects: What they observe is not a direct measurement of a phenomenon, such as a black hole or a quasar, but a construction that combines observables and theory.

That said, astronomers mostly build consensus around the meanings of these measurements, because at least their objects of study are agreed upon. That social science studies are conducted by so many different researchers, from different disciplines and training, in different contexts and on different populations, makes calibrating measurement and forming consensus impossible. As a phenomenon becomes harder to directly measure, it becomes less likely that a unified approach to measurement will emerge, and as multiple measures of the same phenomenon compete, it becomes impossible to formulate precise models of how that variable behaves relative to others.

## The Need for Coalescing and Standardizing Data

The second related objective is to coordinate efforts to consolidate results. Kepler needed Brahe's measurements. Without those numbers, Kepler would not have made his critical breakthroughs. That the two were colocated was an amazing accident. That the collaboration teams needed to integrate and advance social science research will occur by chance is unlikely. Today, each independent researcher with a question uses whatever tools are available or invents new idiosyncratic tools to answer the specific question that interests them. Each study is a separate capsule linked to others only through citation networks and literature reviews. Disciplinary or topical separations partition the collection of findings so that researchers doing theoretically similar work—often with similar findings but in different domains—are typically not aware of the advances of others and the potential significance of their own results if applied to other contexts. A great deal of important and excellent work has been and is being done, but there is little effort to coalesce and make sense of the body of work.

The goal of today's social science enterprise is to "put the findings out there," but where is "there"? And once the information is "out there," what happens to it? Unfortunately, there is no magic method or incentive to organize all that is out there to make sense of the body of social science produced. Without intentional coordination, the discovery of "general theories of social science" will remain out of reach. It will also require the coordination and consolidation of data and of research efforts. This is where investment is needed. A deliberate integrative research program is required to move toward that goal. A useful frame for prioritizing investment in social science would be to focus on two activities: integration and standardization of data and theoretic synthesis.

## The Need to Build Strong Social Science–Led Teams

The third objective is to find the right people to do the science. Among the obstacles to advancing social science is that, except for a few initiatives and programs, there is a government-wide institutional resistance to investing in social scientists. Expenditures on social research are miniscule compared with other sciences.[11] Tether's recognition that social science insights can benefit the warfighter did spur additional investments, but the progress made has been minimal. Some of the reasons for the limited success align with the general challenges to progress in social science research discussed in the section "Separating the *I* from the *Me*: Challenges for Social Science Research." The focus of most R&D efforts has not been on building programs to discover the "rules of social science" and on developing the needed

[11] Funding for social science research at NSF has consistently been dwarfed by research funding for other scientific fields, such as the biological sciences, computer and information science and engineering, engineering, geosciences, and mathematical and physical sciences. From the 1960s to the 2000s, on average, funding for social science research as a percentage of total NSF research funding was approximately 5 percent. See Larsen, 1992; and National Science Foundation, 2020.

technology to achieve that;[12] instead, the focus has been on solving short-term DoD needs and on repurposing technology developed to answer very different questions and meet different needs.

This ad hoc and short-term focus has produced some useful insights and tools, but it cannot advance the *science,* because the focus is almost entirely inductive. To create a social science capability that can provide useful benefits will require serious attention to refining social theory so that there are valid and reliable formal models (i.e., equations to drive computational models). This in turn will require serious investment into new tools specifically designed to gather and organize data for social science analysis. Finally, it will require investing in social scientists to manage and lead the scientific development.

## Institutional Obstacles to Advancing the Social Science Enterprise in National Security Work

DoD has led the drive to invest in social science with several investment initiatives, but three main institutional obstacles have deflected social science's research focus from basic to applied research and distracted attention from validating theory and developing useful social science tools.

### Funding for Social Science Priorities

The first obstacle is that DoD R&D program managers generally do not consider it their job to advance tools to improve the productivity of social science researchers: Their customer is not the social science community but the warfighter.[13] However, if it is true that warfighters require tools built on trustworthy, valid social science theories, and that the social scientist requires better tools to refine theory so that it is trustworthy, then developing tools to accelerate the production of good social science should be a priority. The most efficient way to get

[12] Important developments in this direction, however, include recent efforts by DARPA that have made social science processes, models, and outputs the objects of study. Notable programs in this regard are Next-Generation Social Science, Ground Truth, and Systematizing Confidence in Open Research and Evidence (SCORE). See DARPA, "DARPA Next Generation Social Science," webpage, undated-a; DARPA, "Putting Social Science Modeling Through Its Paces," April 7, 2017; and DARPA, "Systematizing Confidence in Open Research and Evidence," webpage, undated-b.

[13] Again, DARPA's SCORE program is a notable exception with its efforts to develop tools to assist in determining whether research itself can be reproduced or replicated and whether scientific findings might be regarded as reliable. See Adam Rogers, "Darpa Wants to Build a BS Detector for Science," *Wired*, July 30, 2017; Rajesh Uppal, "DARPA SCORE Program Aims to Develop Automated Tools to Score Social and Behavioral Research Important for National Security," *International Defense Security & Technology*, August 20, 2019; and Yang Yang, Wu Youyou, and Brian Uzzi, "Estimating the Deep Replicability of Scientific Findings Using Human and Artificial Intelligence," *Proceedings of the National Academy of Sciences*, Vol. 117, No. 20, May 19, 2020.

into the hands of warfighters what they requires is to get into the hands of the social scientists what they need to test and refine theories and build social science tools; this approach should follow the priorities set by the social science community, not what others imagine the social science community would like to have. The fact is that most of the social science work that has been funded in the past ten years was led by people other than trained social scientists.[14] It is not that program managers do not encourage teams to use social science expertise—it is often an explicit requirement. In practice, however, teams usually are led by engineers, computer scientists, or other physical scientists and have a token subject-matter expert with a social science Ph.D. Very few project leads (either program managers or principal investigators) have been quantitative, formal, or mathematical social scientists—the people explicitly trained to develop methods to measure and model social phenomena.

## Differentiating Social Science from Policy and Intelligence Analysis

The second obstacle is that many in the NSE research community who are focused on meeting the needs of policymakers and "boots on the ground" still confuse social science with intelligence and policy analysis. The misconception that social science should reveal actionable insights in real time is a liability. Making a short-term prediction about a specific case is not an appropriate ask of a social scientist and should not be the goal of a social science research effort. Again, the benefit of investing in social science *research* is to refine social science theories, not to produce intelligence or factoids. By providing valid, reliable, and generalizable models of underlying structures and pervasive patterns of behavior that include parameters to adapt the general model to accommodate exceptions to the rule based on location, timing, age, or "cultural" factors, social scientists can create a capability that will allow policy and intelligence analysts to produce reliable actionable insights when needed.[15]

The relationship between social *science* and policy or intelligence analysis is like the relationship between theoretical physics and engineering or between biology and medicine. The focus of social science is on defining and refining the model or characterizing the "signal" or the pervasive pattern so that it can be easily recognized. The focus of the social science–trained analyst is on detecting or preventing noise or an anomaly. Ironically, if the social scientists do their job well and develop good models of the signal, it will be possible to create tools to make it easier for analysts to detect, characterize, and recommend actions to recog-

[14] Even NSF funding for social science favors non-social scientists. The NSF direct social science budget is small, but NSF invests in research conducted on social science–related outcomes proposed by non-social scientists. For instance, research on social media data mining or natural language processing focused on understanding trends and behavior.

[15] For example, Roger Hilsman noted that social science theories and models are distinct from intelligence analysis but that, often, developments in the social sciences can be applied to meet the needs of intelligence analysts, as in the case of the bureaucratic model of political decisionmaking (Roger Hilsman, "International Environment, the State, and Intelligence," in Alfred C. Maurer, Marion D. Tunstall, and James M. Keagle, eds., *Intelligence: Policy and Process*, Boulder, Colo.: Westview Press, 1985).

nize and eliminate noise so that social scientists can detect and help mitigate crises in real time. If investments continue to focus on the imminent problem, then the tools needed to mitigate imminent crises will still not exist five, ten, or 20 years from now. The consequence of confusing social science with policy or intelligence analysis is that most of the research focus has been on the *crisis du jour* instead of on the less interesting, status quo behaviors that dominate the planet.

## Testing in Permissive and Conflict Conditions

The third obstacle is the idea that social science research should be carried out and tested in a war zone to prove that social science is a "real" science. R&D investments that seek to advance other *sciences* do not require this. Social science is intrinsically difficult. It is unnecessary to create artificial challenges to make real advances less likely. Just as other research in physics, chemistry, and biology is conducted in laboratories and early prototypes are tested in controlled environments before they are battle tested, social science must be conducted in calm, data-rich environments. It is hard to imagine a physics-based research project that would require that basic research be conducted only in a hurricane. Basic research is conducted in a laboratory. Prototypes are tested in wind tunnels.

The motivation that leads some to think it is important that social science research applicable to a specific environment of interest must be conducted in that environment is likely inspired by work showing that some findings from psychology and social psychology—largely conducted using exclusively *WEIRD* (Western, educated, industrialized, rich, and democratic) subjects—do not generalize to some non-WEIRD populations.[16] These are important findings, but they mean that social research must be replicated in multiple contexts before a claim can be made about generalization; they do not mean that social research methods can be developed and calibrated on the fly in non-WEIRD settings. The opposite is true. The reason social scientists were able to discover these differences in WEIRD and non-WEIRD populations are because of the existence of reliable and valid measurement tools and instruments developed and tested in WEIRD laboratories. It was these instruments, when implemented in new environments, that revealed differences. It is not necessary to develop different thermometer technology to measure temperatures in different locations. Indeed, it is only because we use a common measurement tool that it is possible to conclude that temperatures differ in different locations. The development of new instruments exclusive to specific environments will not advance and may hinder progress in social science because the findings from dissimilar instruments cannot be interpreted or compared. It is also important to realize that just because some research conducted in WEIRD countries does not generalize does not imply that none generalizes or that research done in one non-WEIRD country would

[16] Mostafa Salari Rad, Alison Jane Martingano, and Jeremy Ginges, "Toward a Psychology of Homo Sapiens: Making Psychological Science More Representative of the Human Population," *Proceedings of the National Academy of Sciences*, Vol. 115, No. 45, November 6, 2018.

generalize to other non-WEIRD countries. What generalizes—or does not—in each instance is an empirical question that requires the development of validated and reliable instruments created in data-rich controlled places where validation is possible that can be applied in many different environments to detect and measure differences by location, region, or "culture."

If a new social science concept or measurement tool is to be adopted, then testing would eventually need to move to more-realistic and more-extreme conditions using subjects other than college freshmen, but the initial development and testing should be done in the most-controlled conditions possible. If the science and theory is sound, then the results found in these basic research studies will generalize to a broad variety of contexts; therefore, the models will allow recognition of the conditions that extend beyond the model and the prescription of modifications. Social science phenomena are no different from physical or biological phenomena in that most behavior lies within normal parameters. Basic social science research should not be treated differently than other types of basic research and be expected to find structure when restricted to studying only outliers and anomalies.

## Overcoming the Obstacles to Investing in Research for Social Science

A long view for social science and investment in research is necessary—one that is not directly focused on the most pressing issues of the day. The objective of social science research should not be to provide actionable information about a particular idiosyncratic event; instead, it should be creating trustworthy tools to allow decisionmakers to make sense of chaos in real time. Science should mature in advance of a crisis.

The downsides of shortsighted science planning are not unique to social science. Myriad examples emerged of the neglect of unfashionable science slowing the world's response to the coronavirus disease 2019 (COVID-19) pandemic, alongside stories of forgotten and undervalued science allowing a quick ramp-up to the development of vaccines.[17] Social science (and all science) is slow. For it to be useful when needed, the investment must come well in advance of the need. Social science is not emergency room diagnostics; it is theoretical knowledge production that allows practitioners to know the effects of their actions to mitigate a crisis with confidence. To do this requires the study of many cases in many situations over time, eventually leading to trusted practices.

This is not to say that past social science programs have not advanced the field and produced tools that can be employed to advance it further. There are many examples of social science–funded research that have produced data collection, analysis, and modeling tools that could, if used properly, accelerate the accumulation of scientific knowledge. But

[17] Fedor Kossakovski, "Why Some People Are Superspreaders and How the Body Emits Coronavirus," *Science*, October 27, 2020; Leah Asmelash and A. J. Willingham, "She Was Demoted, Doubted and Rejected. Now, Her Work Is the Basis of the Covid-19 Vaccine," CNN, December 16, 2020.

advances have been slow because the focus has not been to advance science. For most programs, the goal was much narrower: to demonstrate the usefulness of a theory or approach for a particular use case or to improve a tool for a specific application. These are useful endeavors, but engaging in these activities will not revolutionize the basic science. To make the type of advances needed will require programmatic and scientific reboots.

## Programmatic Reboot: Changing How Social Science Research Is Handled

Tether's vision for advancing social science was to discover and validate social theory so that it could be used in computational models. As discussed at the beginning, PCAS was conceived to demonstrate the *potential* to create tools based on social science formulas. PCAS was not expected to discover or validate these formulations; the goal was only to provide evidence that there were observable and measurable regularities inherent in the phenomenon of interest that could be formalized. In retrospect, PCAS revealed—both in what it did well and where it fell short—features important in a program to advance social science theory. Specifically, the strength of the program was in the right framing of the objectives of the program and research question and in the diversity of performers. The weaknesses were the result of a lack of access to good data and the short time line for completion (six months).

To move social science forward expediently will require programmatic coordination. Advances in social science will be slow if each study is independent in design and scope. Social scientists are largely excited by incremental advances—small problems and simple designs. Thoughts of grand programs linking together or coordinating their work with the simultaneous work of others do not occur to them. Aggregating efforts into bigger projects is rare. One exception is the coalescence of survey research questions into large, regularly administered and publicly available questionnaires. Otherwise, most research agendas are independent or involve small collections of frequent collaborators. What is most exciting about the prospect of new, large, sponsored investments in social science is that, with an ambitious programmatic perspective, many different studies using different methods or focused on different populations can be launched simultaneously to address the same question. Changing the culture of social science research so that the community begins thinking explicitly in terms of active integrative collaboration would create an environment that reduces redundancy and increases the recognition of similarity among diverse research areas.

### Social Scientists Should Lead Social Science Research

The first step toward building large and productive basic research programs would be attracting the right set of researchers to participate in each program. The goal would be to create a diverse set of social scientists from different fields engaged in different types of methods and focused on different populations but with a common theoretical focus. Over time, working together, this group can identify common features and determine the sources of the differences.

To do this for social science today would require a concerted effort to seek out and attract social science researchers. As mentioned before, most social science funding, except for NSF funding, is awarded to non-social scientists. If this continues to be the case, the right people will not in the room to advance social science. Institutional inertia is powerful, and the moratorium on social research imposed with the cancellation of Project Camelot still imposes barriers to productive social research. This has changed: Today, there are several social scientists working to fund social science across DoD, but their senior management and most of their peers that review proposals are non-social scientists. Therefore, social science programs are often structured to be counterproductive to producing good social science findings. The programs inherit the lessons learned and conventions about research from other fields even if they are not relevant.

Because there is no demand for social science researchers to engage with funding agencies, the relationships between these agencies and social scientists have dissolved. Except for a small number of regulars, social scientists are not recruited to or informed about upcoming funding opportunities. Even if they do become aware of opportunities, their proposals are often framed differently than expected by review panels made up mainly of non-social scientists. The result is the most innovative, methodologically sound, and theoretically compelling proposals are rejected in preference to what appeals to, and can be understood by, reviewers with no training in social science. The converse never occurs: Nobody ever puts a panel of social scientists in charge of the committee to select proposals about material science or polymer physics.

## Well-Scoped Challenges Are Needed for Well-Specified Models

A second vital feature for a social science program is correct framing of the research question. The most-productive projects should be narrowly focused on a general phenomenon. The specificity is in the type of behavior or interaction that is of interest, not the context or the specifics within the context. As an example, if the interest is in understanding the recruitment of terrorists or insurgents to a specific cause, the objective of the science is to focus not on the group of interest but on the recruitment processes more generally. Data about this group are appropriate for intelligence analysis but insufficient to inform science. The science question is more generally about recruitment, and the study of all sorts of other groups (i.e., religious, political, recreational, and other groups that recruit new members) is equally relevant.

Social scientists, if they are doing their job properly, can only draw conclusions long after the fact. Data collection, cleaning, and analysis take years. This may be not a useful timescale for getting ahead of a crisis, but it is what is required to ensure studies that are representative, that can account for biases, and that can measure indicators in a reasonably valid and reliable manner to generalize the results appropriately. Until long-term science is complete, no short-term answer can be relied on.

## Incentivize Diversity, Integration, and Synthesis

PCAS was unique in that it was a coalescence of many different types of quantitative, formal, or computational researchers all focused on a unifying theoretical question. Their challenge was to create models that would predict or anticipate state failure in two specified countries, but the objective was to come up with a generalizable model of state failure. All the social science teams selected had experience modeling complex social systems. Each team brought a different approach to PCAS. Some were focused on state-level variables and used classic political science models of state failure. Others looked at the question from a social-movements perspective. Still others took a more anthropological perspective and focused on substate social organizations. Methods used also varied: Some of the teams instantiated formal theory into models directly. Other teams used empirical data to drive models.

Normally, each team would work in a silo, study the problem, generate results, and publish its papers in journals that none of the people on the other teams would ever read. What was innovative about PCAS was that the program provided a unifying umbrella, and this feature was critical. The benefit and key advantage of a programmatic approach is in gathering diverse groups of researchers that represent multiple complementary positions in the field of social science production. At a minimum, representation should span the continuum from inductive through deductive, micro to macro, observational to experimental. Once teams are created, the tasking focus on a common problem should set each on its mission to find answers, but the program will ensure that no performer goes it alone.

The point of this diversity is to generate communication and collaboration about both theory and design. This element is necessary to generate a transformative advance. It will work only if at every stage of the program each study informs and is informed by all others about their progress and challenges, so that, when the studies are completed, the results can be easily compared, contrasted, integrated, and meaningfully interpreted by all others. The compulsory communication is what facilitates the recognition and identification of what model elements are shared. The task provides a common focus for each approach to meaningfully demonstrate its value and reveal the similarities and differences between approaches. In an ideal world, this sharing and convergence would occur naturally, but it does not; if revolutionary advances are to manifest, the connections among research groups must be manufactured.

Gathering research groups to work on a common problem is important but not sufficient. Structuring the program and the evaluation of the program so that performers do not feel like competitors is also important. This goes beyond a common task framework. The objective is to produce a new model that more precisely and parsimoniously describes the general phenomena than could be achieved by any one team. To do this, teams must be willing to work together and be willing to cooperate—performers need incentives to work as a team toward a common goal. A standard model that frames initial phases as competition forces performers to try to outdo one another and does not motivate sharing. To encourage learning and growth, the program must reward collaboration. Milestones and deliverables should be structured with collaboration in mind.

### There Is a Need for Well-Scoped Questions and Innovative Solutions

Once engaged, performers should not be limited in what data they use to inform theory. All methods, all context, and all previously studied examples should be encouraged. Programs should be focused on answering a specific theoretical question. The social scientist selected to study this question is the best person to decide how to design a study. Any restrictions placed on the case or data used, beyond obvious fiscal and ethical restrictions, are counterproductive to the goal of synthesizing theory.

This point dovetails nicely with recommendations for evaluation. The goal is not to pick the best approach but to assemble a new approach that brings together the best of the collective. For example, programs structured to down-select performers who do not come "closest" to predicting an outcome constrain their performers and restrict the potential for revolutionary science. Furthermore, this type of evaluation criterion is not sufficient to truly evaluate the models. Future predictive model evaluation, especially when the topic is macro-level social science, is not appropriate. To evaluate causal models—to test hypotheses—requires many cases because the outcomes are not expected to be probabilistic. By definition, any "point prediction" (a single value at a single moment) is inconclusive.

There are two main messages. The first is that the effort to turbocharge social science should be led by trained social scientists. The second is that although social science in many ways is similar to other sciences, in some ways it is not. Thus, some of the structures that have proven beneficial for advancing other sciences should be implemented in social science research programs and some should not. What should be replicated is program management of the funding of researchers with expertise in the field and support for basic research conducted in data-rich and controlled environments. What should not be replicated, because the main objective of the investment is synthesis, are evaluation criteria designed to eliminate perspectives prematurely.

## Scientific Reboot: Focus on Integration and Synthesis

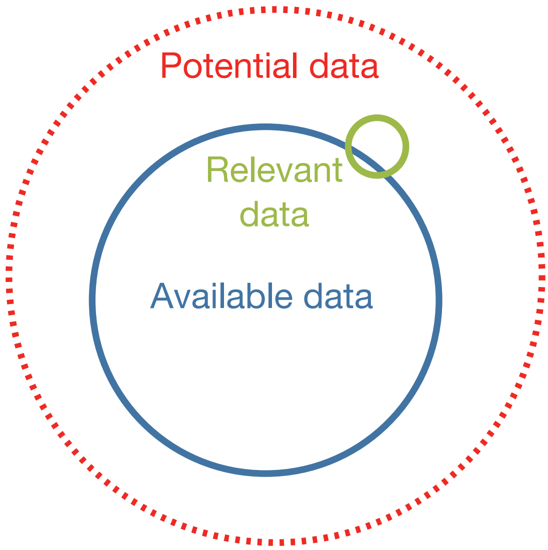
Ultimately, the objective of advancing social science research is to have valid social science algorithms at the ready. To do this requires advances in data collection methods and innovative designs to test or validate the theory. One of the biggest challenges for social researchers is to find good data. The second challenge is to develop approaches to test the types of hypotheses that will matter to sponsors and users, such as DoD decisionmakers. These issues are not independent. For example, Kepler needed good data to test his theory, but Kepler's design was a one-shot design for the most part. In addition, the data collected were unchanging, at least at the time. Furthermore, the theory that Kepler was testing was descriptive, not causal. Kepler was not required to introduce or wait for an intervention to remeasure the environment. The social science phenomena that are most compelling to sponsors are primarily, but not exclusively, macro and definitely causal. The objective is not just to predict outcomes but to understand the mechanisms of change so that decisionmakers can take an action to bring about desired changes, or at least avoid detrimental repercussions.

Realistically, it will be a while before social science models are as dependable as physical models, but commencing research now is essential; even if we do not discover the universal laws of social science by the end of a research program, progress toward that goal is bound to produce many useful by-products. The recommendation here is to do the science right, with the goal of perfecting theory and generating useful by-products along the way.

Social science is slow because practitioners are passionate about sampling, data integrity, and methodical and repeated theory testing. To make social science useful, it is important to make social science better, as well as cheaper and faster. This does not mean that technology invented for other purposes cannot be repurposed and used for social science; rather, it means that technology will likely have to be adjusted to fit into the social science process. As an example, from the social science perspective, data mining is a form of convenience sampling, which is generally problematic given that the data have known and unknown biases that make the data difficult to generalize. Considerable investments have been made into developing tools to capture, code, and use information available online to study social science. However, most of this development is done without the input of social scientists. Many articles are published where the subject is social science–related, but the research team is entirely trained in computer science and engineering. Some of these papers provide interesting insights, but, because of design flaws, such as reliance on nonrepresentative samples and a completely inductive nature, few make lasting contributions to our understanding of people or behavior. That should not be surprising. To computer scientists, data are data, and what the data represent is secondary. The explosion of data generated by people as they use the internet should be exploited by computer scientists to demonstrate their prowess in churning through and organizing data. What is important to remember is that their results concern computer science metrics, not social science outcomes. From the perspective of social scientists, the exploitation of data because they are available is likely insufficient to represent social constructs and to produce valid results.

Figure 9.1 illustrates data availability from the perspective of social scientists. Large amounts of data exist in digital form, but most data are irrelevant to answer any specific social science research question. Unfortunately, the data available may not be appropriate to operationalize the concepts of interest. Other data, represented as the red circle, might exist, but those data may exist outside the digitally available data (blue circle). To operationalize social concepts and properly specify a model may require the intentional collection of new data that are not available in a format that can easily be mined. That is not to say that some of the digital data available on the internet or through Application Program Interfaces are not useful as proxies for some social constructs, only that it is unlikely that the data already conveniently available are optimal or representative—and whether these data are useful is itself an empirical question. The small green circle, which may intersect with the blue circle, represents the data that are needed to empirically address a question. Assuming or pretending that the data available are a good enough measure of a concept can lead to invalid conclusions. Even more pernicious, sampling biases inherent in this type of data acquisition would make it unreasonable to assume that the available data, without vetting, would be representa-

FIGURE 9.1
**Limitations of Digitally Available Data as a Source for Social Science Research**




tive enough to draw conclusions. It is not just that only a small amount of the data available might be useful to answer social science questions, but it is also that the data used to answer the question were found in another part of the available data circle entirely.

It is not that the social scientist cannot benefit from these technological advances; it is that additional technology is needed to make this technology useful. Social scientists need tools to identify biases and perhaps to leverage different biases in data collection to introduce correction factors so that the data would be representative. Moving beyond just the data, these approaches are mostly entirely inductive, drawing conclusions from mined data based on correlations. Few studies are structured to support theory testing. For social scientists to advance technology, they will have to also support theoretically motivated data collection and the standardization of structure data, so that quantitative deductive analysis is feasible.

Social scientists are not enthusiastic about many computational models popular today because most of the work, especially black-box machine-learning and predictive tools, is at best inductive. However, many of these models are not even inductive, because they fall short of revealing the causal theory to support their conclusions. With this in mind, and given the vast amount of data and studies that need analysis, some of the technology developed for prediction may be useful, but not if investment is made in place of developing computational models focused on revealing causal theory and theory testing. Social scientists have the deepest understanding of what is required to formulate, test, and refine social theory and are required to be at the heart and at the head of the effort to advance social science.

The social science frameworks discussed are useful heuristics for thinking about which short-term by-products to focus on and, more importantly, how to produce short-term benefits that also move us toward a long-term goal. Social science insights can benefit decisionmakers, and social science research can provide real-time evaluation of the social and political changes in an area of operation during an engagement. What is important to keep in mind is that if research being done is informative for one subject, then the research should be done, but the data should be collected in a manner that makes them more generally useful. Data collected to research specific problems or questions are rarely designed to be combined with or inform larger research programs and therefore cannot contribute to broader research needs and applications. Careful research designs can allow case-specific research to proceed, but in a context that allows data to be reused to contribute to future, possibly larger, studies, allowing knowledge to accumulate. This is not standard practice today.

The emphasis here is that studying real-world events can be done from an idiographic perspective and focused on only one case but must be seen as an opportunity to contribute to the more general nomothetic, or scientific, approach and must be done so that it can facilitate theory testing. If each engagement, mission, or incident is treated as a collection opportunity and if there are some established criteria for data collection that allow consistent comparison across cases, then each becomes a case and a source of data that feeds deductive studies to validate the underlying science. These studies, along with the development of the commensurate tools, could allow the understanding and interpretation of social dynamics in real time.

Ongoing military, diplomatic, and humanitarian engagements can be opportunities to feed the social science data collection and experimentation machine to enable the development of a reliable set of social science laws and principles, as can studies of the activities of other U.S. agencies or nongovernmental organizations. The engagements can be decomposed into thousands of data points and blended with similar data from other cases to feed nomothetic work. Considering a bigger event as the accumulation or aggregation of many events at a lower level expands the analytical possibilities and value of scientific research. Focusing at too high a level reduces an assessment to dichotomous categorization, while measuring at lower levels allows us to measure variance and see the distribution or impacts. It also provides opportunities for experimentation.

Clearly, a one-shop macrolevel intervention can be risky and problematic. Thinking of an engagement as a collection of many micro-interactions and capturing the implementation and then measuring the effect is a means of transforming policy actions into running quasi-experiments. This breaking up of the major event into components, rather than one case, changes the one-shot design to one with repeated trials. A creative researcher may even be able to introduce random assignment in some cases. It also provides decisionmakers with a means to pretest the impact and repercussions of interventions under consideration in real time.

Take, for instance, any humanitarian engagement. Studying the impact on a national scale is ideographic and generates an *n* of 1 for study; in contrast, collecting data on the impact at the individual or village level generates data that can be placed in a collective database and used to inform multiple analyses with a sufficient number of cases to test a variety of hypotheses. The data at the national scale that are used to evaluate whether the policy had an impact might be gross domestic product or infant mortality before and after humanitarian aid is distributed. A more nomothetic design—an experiment or a quasi-experiment—could focus on data at the village level. If some villages receive aid and others do not, the comparison is a quasi-experiment. If decisionmakers were able to determine which villages received or did not receive aid using random assignment, then the humanitarian activity is also an RCT. If this type of data eventually were collected in many places over time, researchers would be provided with rich data to test hypotheses about the impacts of humanitarian relief efforts and explain why some interventions produce the desired outcomes while others do not (controlling for nationality, region, religion, cultural factors, and a variety of other factors). However, doing so will require coordination and standardization, especially if data from many events are to eventually be used in the same analysis. The benefit of this type of coordinated data effort is huge, as would be the effort required to build the infrastructure to organize, store, and make available the data so that they could be used.

Similarly, baselining before an engagement is essential. Waiting until a crisis to begin measurement makes measurement less useful because if the crisis is in progress, the system is not in equilibrium. From Campbell and Stanley's perspective,[18] if posttreatment measurements are all that is available, the opportunity is a one-shot case study, rife with threats to internal validity. Getting data to baseline in advance allows for pretest and posttest comparison, turning a one-shot design into what is referred to as a static group design experiment and allowing the researcher to at least make the claim that the event had an impact on the group. If measurements or baselining of another comparable group is also available, then the situation presents a common quasi-experimental design opportunity: a nonequivalent group (difference-in-difference) design. With each step, threats to internal validity disappear. The only feature of an RCT missing from the difference-in-difference design is the random assignment. Of course, if only two comparable instances are available to observe, there are only two trials and not a great deal of statistical power or ability to generalize. Baselining broadly across the globe extends this farther, introducing the potential to run natural quasi-experiments comparing the changes that occur on measured variables before and after they experience similar crises. Over time, if there are standards for data collection and measures across locations, the design can become closer and closer to an experimental design.

Baselining widely across the globe and at the lowest level of analysis possible is a prerequisite for being able to apply models in real social situations when they are needed. It is not just that beginning study at the start of a crisis or even at the first signs of an imminent crisis is too late. One needs to take multiple measurements of multiple variables at many time points. Most social systems remain in equilibrium for a long time, but this does not mean that they are static. What is needed is surveillance of the impacts that multiple variables have on each other over time and space. Surveillance before a crisis and careful recording of dynamics are

[18] Campbell and Stanley, 1963.

necessary to support the development of models that can reliably reveal a system's tolerance to shocks of different types.

Finally, and most importantly, if we are ever to have valid and useful formulas and algorithms, there will have to be a concerted effort to work toward theory testing and standardizing the way data are stored, organized, described, and saved. Convergence and integration will require work. Study design, including data collection, is only one aspect. Data analysis, including meta-analysis to find general patterns from among many studies and synthesis, is needed. Automating some aspects of these analyses will be important because the scale of accumulation of studies will exceed the capabilities of any researcher or research team. What is important is that automated processes, including data collection and analysis approaches, are inspired by social science. Methods and models used today may become obsolete as technology expands what is possible. That said, the inspiration for all aspects of the social science endeavor should come from the tried-and-true theoretical and methodological foundations from the social sciences.

## Concluding Thoughts

The main theme of this chapter is that social science research is not easy, nor is creating programs to accelerate the production of valid social science. However, the problems posed by UGS, whether similar to those studied in the PCAS program or other problems related to emerging forms and domains of competition, all require insights from the social sciences and the contributions of social scientists to meet the nation's needs. This chapter is meant to highlight my point of view of the past 20 years—from the social science research reboot with PCAS until now. Revolutionizing social science research will require cultural and institutional changes from all parties. Social scientists will have to start thinking more programmatically, and government agencies will need to become more comfortable with social science and social scientists.

Beyond that, I recommend focusing on using and gathering existing data as much as on creating new science. That is not to say that new data collection and new tools are not needed. However, as in other fields of scientific research, what is needed are legacy research projects that lead to definitive and substantial progress and tools that can be universally used by all who engage in social science research, such as tools to integrate what are now different data and data types into a standard form available and usable by social scientists of all types. Other efforts might involve investment into theory development, such as computational models to test theoretical assumptions or make formal theoretical models more accessible and useful. There is a huge need for new technology to capture, code, coalesce, and analyze data. However, the most critical ingredient needed to advance social science is the active engagement of the leaders driving the research and the research agenda.

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## Abbreviations

| | |
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| DARPA | Defense Advanced Research Projects Agency |
| DoD | U.S. Department of Defense |
| NSE | National Security Enterprise |
| NSF | National Science Foundation |
| PCAS | Pre-Conflict Anticipation and Shaping |
| R&D | research and development |
| RCT | randomized controlled trial |
| SCORE | Systematizing Confidence in Open Research and Evidence |
| UGS | undergoverned spaces |
| WEIRD | Western, educated, industrialized, rich, and democratic |

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