Experts, Social Scientists, and Techniques of Prognosis in Cold War America

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Acknowledgments

I started writing this book on 6 August 2015. On this particular day, the radio station I usually listen to while preparing breakfast played the sound of the Peace Bell in the Peace Memorial Park in Hiroshima. It did so to mark the 70th anniversary of the dropping of the atomic bomb "Little Boy" over this town in South Japan. I had visited Hiroshima the year before. There, despite having been aware of much of the historical facts and circumstances of this attack, and the devastating suffering it brought to the city's inhabitants, the cultural rupture that the new superweapon brought with it became tangible to me. Little Boy and his fellow Fat Man, the bomb that was dropped over Nagasaki some days later, marked the rise of a new culture of insecurity, a culture that was, for the first time in history, truly of global scope.

I would like to start by thanking those who were available for face-to-face interviews. These were Daniel Ellsberg, Joan D. Goldhamer, Theodore J. Gordon, Nicholas Rescher, and Martin Shubik. My ambition and, indeed, hope was that they find resemblance between the events described in the book and their own recollections. I am grateful also for the permissions to use parts of the interviews in the text. I also relied on a series of interviews carried out by Martin J. Collins as part of Smithsonian Institution's RAND Oral History Project as well as on an interview with Olaf Helmer carried out by Kaya Tolon, which is included in the annex of Tolon's PhD thesis "The American Future Studies Movement

(1965–1975): Its Roots, Motivations, and Influences". Ames, Iowa: Iowa State University, 2011. All these sources provide a sense of the RAND culture that cannot be found in research reports and articles.

I visited RAND twice, in 2011 and 2013, and received considerable support at the place as well as via digital channels over the years. I am grateful to Vivian Artebery who, although her primary responsibilities were with other issues, found the time to support me prior to and during my first visit. Vivian also showed me the Olaf Helmer papers that had just been given to RAND upon his death in spring 2011. Roberta Shanman and Ann Horn provided additional support at that time. My second visit to RAND was prepared by Roberta and Susan Scheiberg, and Susan and Cara McCormick were instrumental in clarifying issues that popped up in the final stages of the research and manuscript preparation process. I specifically want to thank Cara for being of such great support in securing the access to historical photographs, some of which are included, with the generous permission of the RAND Corporation Archives, in the following pages.

Mary Osielski and Brian Keough from the M. E. Grenander Department of Special Collections and Archives, State University of New York at Albany, were kind hosts when I inspected the papers from Hans Speier in October 2007. Further, I am grateful to the staff at the archives of the Hoover Institution and at Hoover Library, both located at Stanford University, Stanford (CA); at the Library of Congress, Washington (DC); at the New York Public Library in New York City; and at the Massachusetts Institute of Technology Libraries, Department of Distinctive Collections, Cambridge (MA). These archives are paradises for historians (of social science), and this is in large parts due to the continued effort of their staff. Permission to reproduce materials was granted by the RAND Corporation Archives and the Massachusetts Institute of Technology.

People do not like insecurities, and over the centuries of human civilization, countless numbers of ways have been devised to reduce insecurity. Among these ways, the council of others deemed wise and experienced ranks high. A lot of wise and experienced people offered support over the years, among them Katelin Albert, Peter Baehr, Daniel Bessner, Martin J. Collins, Matthias Duller, Christian Fleck, Peter Gasser-Steiner, Sharon

Ghamari-Tabrizi, Robert Jackall, Philipp Korom, Stefan Laube, Barbara Louis, E. Stina Lyon, Jennifer Platt, Andrea Ploder, Werner Reichmann, Matthias Revers, Erik Schneiderhan, Peter D. Simonson, Mark Solovey, Richard Swedberg, Stephen Turner, Hans Vabitsch, Angelika Wetterer, and Mario Wimmer. Further, the book followed me through times of repeated professional changes, and I was offered support—in one way or the other—from people and institutions at the University of Graz, the University of Klagenfurt, and Graz University of Technology, where I am now based. If I did not accept all offers, this had nothing to do with their inherent qualities, but instead was dictated by the work progress; often, to receive the offer was support enough.

Christian Fleck and Karen Meehan took the burden to read the whole manuscript and provided valuable suggestions. Also, I am indebted to the series editors and two anonymous reviewers for providing helpful comments and constructive critique. The remaining errors are proud results of my own stubbornness.

I have used parts of the materials presented in this book in earlier articles that approached them with different research interests than the one that guides the argument here. In these cases, I made a reference to the respective article. The cover of this book was made using a poster that the artist Hristo Penev created for the Bulgarian National Peace Committee in 1983. Its title, set in orange capital letters on the top of the poster, was bilingual and read "We допуснем ли?" (Bulgarian, translit. We dopusnem li?) and below, in English: "Should we let it happen?".

Praise for *Experts, Social Scientists, and Techniques of Prognosis in Cold War America*

"Much of modern social science has its origin in the famous RAND Corporation, and this includes studies of the future, or futurology. In this well-documented and exciting study, the author explores this part of the work at RAND with the help of little-known archival material and interviews. Extremely interesting!"

—Richard Swedberg, Professor Emeritus, *Cornell University, USA*

"Paying close attention to the historical context and practical details of social science methods in the Cold War, Dayé's book speaks to questions about truth and democracy, which are both timeless and urgent."

—Monika Krause, Assistant Professor, *LSE, UK*

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Introduction: A Culture of Insecurity and Its Experts

A Culture of Insecurity

Like almost any technology, the atomic bomb had effects that extended far beyond the field of its immediate use into the wider sphere of culture. When media reports of their detonation over Hiroshima and Nagasaki circulated worldwide, "Little Boy" and "Fat Man" became the symbols of the emerging global culture of insecurity. To an extent, unseen before in a weapon, the cultural effect of the weapon became its primary asset. Writing in retrospect, US Secretary of War Henry Stimson made clear that "the atomic bomb was more than a weapon of terrible destruction; it was a psychological weapon" (Stimson 1947, 66). The two bombs had killed hundreds of thousands of people and left many more injured. Yet precisely because of its cruelty, the use of the atomic bomb as a means of deterrence became more effective than its actual detonation. The bomb's primary objective was political and cultural: to create an atmosphere of existential fear and insecurity among those threatening the values of the West. And its outreach was global: while the bombs had been dropped over Japan, the Soviet Union and its potential allies around

the globe became the main addressee of the psychological and cultural effects of the bomb.

Yet, as US strategists soon were to realize, a strategy of deterrence always has repercussions on all parties involved. The culture of insecurity that was emblematized and initiated by the launching of the bombs over Hiroshima and Nagasaki was not restricted to the "East"—quite to the contrary, it pervaded US American culture, especially after the first successful test detonation by the Soviets in 1949. In contrast to its physical radiation, the bomb's psychological radiation could not be restricted in terms of space. Its cultural effects were global. As a "psychological weapon," it paradoxically also affected those who used it.

Two factors fostered the diffusion of the culture of insecurity in the United States. Chief among them was *technical ignorance*: were the Soviets capable of producing a bomb? How well developed was their knowledge of nuclear physics? Could they secure the service of experienced German scientists, as the United States did? Or, being one of the occupying forces in postwar Germany, did they get access to crucial data and results unknown to US scientists? In large parts, the Cold War game of deterrence was played along lines of technical ignorance, with each side attempting to occlude its capabilities and to deceive the other side into assuming the worst. However, a *theoretical* or *philosophical void* accompanied this technical ignorance. While those involved in the game of deterrence tried to apply their means to the most desired outcome, they had to do so without knowing the rules of the game. The atomic bomb profoundly changed how people thought about war. Upon reading about the dropping of the Hiroshima atomic bomb in the newspaper, accomplished strategist and Yale professor Bernard Brodie reportedly "turned to his wife and said, 'Everything that I have written is obsolete'" (Kaplan 1983, 10). To Brodie, and to a majority of his fellow strategists, the bomb had destined the entire body of military knowledge accumulated over the past centuries to be moved to the deep caves of archival oblivion. The world was confronted with a weapon of disastrous force but had not developed theories to understand, let alone handle it. "The whole conception of modern warfare, the nature of international relations, the question of world order, the function of weaponry, had to

be thought through again. Nobody knew the answers; initially, not many had even the right questions" (Kaplan 1983, 10).

One common human reaction to ignorance and insecurity is to endow large and potentially unjustified amounts of trust in selected social or cultural positions and their proponents. When the world set out to return to a peacetime organization of life in the aftermath of World War II, there was a window of opportunity for a new social figure to climb up the ladder of cultural relevance. The age of the expert in foreign and military politics was about to dawn; and by creating a culture of ignorance and insecurity, the atomic bomb acted as leverage for the expert's success in entering the court of power. The bomb had completely shaken up the structures in this field. Almost every claim to authority had to be newly established and negotiated. Scientists entered the struggle by arguing that a scientific procedure was the most reasonable way to cope with the overwhelming task of restructuring US military defense and foreign policy. They claimed the opportunity to participate in political and military decision processes. In the same breath, they emphasized that earlier experience was no convincing guide in the realignment of the field.

Conceived of as a mediator between knowledge and power, the expert occupied an important position in US Cold War culture. To describe the epistemological characteristics of this position is the objective of this book. It focuses on the capacities ascribed to this social figure and the hopes that were related to it in this culture of insecurity. To anticipate the conclusion, and quite unsurprisingly, both the ascribed capacities and hopes were grand. In the early years of the age of the expert, mass media treated this new figure as a source of the general reason (Brint 1994; Herman 1995). With regard to experts in foreign policy, a widespread hope was that they were able to level out the warmongering impulses from military officials as well as the shortsightedness of political leaders. Since using the atomic bomb was so obviously irrational and inhuman, the expectation toward the civilian experts in foreign policy was that they would ensure a level of reason and rationality in the decision processes.

This expectation was not confined to mass media but was an essential part of the self-image of those scientists who came to be addressed as experts. As such, it influenced their doings. The analytic approach developed in this book makes use of this relation by examining a specific line of

methodological thinking within the social sciences. The main idea of this line of thinking was that the expert could be used as a source of knowledge about the future. This idea was developed at the RAND Corporation, a research organization that emerged from a collaboration between the US Air Force and Douglas Aircraft Company shortly after the end of World War II. Here, scientists created a series of techniques that aimed at producing knowledge about the future by systematically collecting expert opinions and allowing for a certain degree of interaction among these experts.

The book explores a series of studies done in the 1950s and 1960s by two groups of RAND researchers. One group, consisting of members of the Mathematics Division, designed the Delphi technique; the other, consisting of members of RAND's Social Science Division, proposed and developed a technique they called political gaming. Delphi distributed questionnaires to a pool of experts, asking them to estimate when specific future events would take place. These estimations were then averaged and fed back to the participating experts with the intent to have them think about their initial answers again. The expectation behind this repeated procedure was that the estimates would converge over time to a range which could then be called expert consensus. The political games carried out at RAND invited experts to participate in various groups, each of them representing a national government, or a block of national governments (e.g., Western Europe). The groups were then asked to discuss how the government they represented would react toward specific actions of the other governments, thereby simulating a political and military crisis. After each step, game leaders would collect the decision of the groups and use them to synthesize a new game state. Both techniques are still in use today, mainly in the areas of applied policy, market, and trend research.

As the ensuing chapters show, there are considerable differences between the two techniques. Above all, they embody different epistemologies and philosophies of science. They bear the marks of the academic tribes from which their inventors came—the program of logical empiricism in the philosophy of science in the case of Delphi, the sociology of knowledge developed by German sociologist Karl Mannheim in

the case of political gaming (cf. Dayé 2014). On the one hand, the logicians involved in creating Delphi, chief among them Olaf Helmer, Norman Dalkey, and Nicholas Rescher, all studied with important representatives of logical empiricism—Rudolf Carnap, Carl Gustav Hempel, Hans Reichenbach, to name a few (cf. Dayé 2016). On the other hand, the leading social scientists at RAND—Hans Speier, Herbert Goldhamer, and Paul Kecskemeti—had all known Karl Mannheim personally and had been influenced by his understanding of a social determination of ideas. RAND's political gaming incorporated ideas formulated in Mannheim's classic text, *Ideology and Utopia* (Mannheim 1997; orig. 1929; cf. Bessner 2014).

Thus, while there are interesting differences between the two techniques, some of which I explored in earlier articles, the main interest of this book is with the similarities of the two techniques of prognosis. Since both techniques rely on expert opinions or expert knowledge to produce statements about the future, these techniques can be understood as manifestations of the expectations and hopes related to the alleged capacities of the expert. Thus, an analysis of these expectations and hopes might help us understand how in a culture of insecurity, trust in a social figure was created, justified, maintained, and corroborated.

Techniques of Prognosis

Many in the military saw the advent of the expert as an attempt to oust military officers and other proponents of the armed forces from their positions of authority. However, this was more than just a struggle over organizational power. It concerned the question of whom to entrust with decision-making in the new culture of insecurity. The stakes were unprecedentedly high and nobody knew the rules of the game. This corroborated the experts' claim that what the world required in order to confront the challenges of the new culture was the production of new knowledge by the sciences, not the outdated wisdom passed on by one generation of military artisans to the next (Connelly et al. 2012). As a matter of fact, many military officials acknowledged, if somewhat grudgingly and hesitantly, that times had changed. As US Air Force General Curtis E. LeMay,

who had been involved in planning and executing the strategic bombing campaign against Japan during World War II and after the war became deputy chief of Air Staff for Research & Development at the Pentagon, claimed in 1946: "Warfare is no longer a military problem" (cited in Jardini 2000, 314).

In the attempt to cope with the culture of insecurity, the newly appointed experts on foreign policy and military strategy perceived it as their task to develop techniques of prognosis, instruments, and procedures informed by (social) scientific methodology with the objective of "envisioning an unknown future" (Mallard and Lakoff 2011, 339). Bestowed with the expectation to deliver to the nuclear age what the augurs delivered to the people of ancient Rome, they searched for innovative ways of social scientific prognosis. The most established form of a scientific prognosis, statistical extrapolation, was deemed inadequate both with regard to scientific-technological advances and to social and cultural processes. In both cases, data were rare. Yet, more importantly, non-schematic actions on the micro level could lead to leap-like changes or revolutions that completely transformed the social, cultural, and societal scenery on the macro level. Statistical data of the past might help, the experts were convinced, but it would not suffice to allow for solid prognoses. "Thoughtful observers had recognized that the existential fact of the bomb altered time significantly and permanently" (Ghamari-Tabrizi 2012, 269). This had a huge impact on all those involved in decision-making in the nuclear age; for them, "the present, future, and conditional worlds ran together" (Ghamari-Tabrizi 2012, 269; see also Byrne 2010).

One solution to this methodological problem of social prognosis that the foreign policy experts explored is in the focus of this book. This solution was to conceive of experts as persons with a privileged amount of—explicit as well as tacit—knowledge and to devise techniques and tools that would make systematic use of this knowledge in producing prognoses. The methodological solution pursued by the RAND researchers, however, implies a telling irony. In search of ways to cope with the culture of insecurity, decision-makers asked experts to deliver prognoses. They trusted them to find methods and ways to deliver stable knowledge of the future of the social, technological, and political sphere. And the solution proposed by the experts was: ask scientific experts. The circular character

of this argument astonishes, especially when one realizes that it went virtually unnoticed by the experts themselves. From the historical distance, however, we can use this circularity. To explore how experts feature in the two techniques informs us very broadly of the expert's role in US American Cold War culture. The epistemic role attributed to the expert within the relatively narrow frames of the techniques can be interpreted as a manifestation of the hopes and expectations attached very generally to the social figure of the expert during this period. And these expectations and hopes, in turn, formed the basis of trust.

Two concepts, thus, are at the core of this study: epistemic roles and epistemic hopes. Building on the traditional sociological concept of the social role as a bundle of expectations attributed to a specific social position, the epistemic role of the expert consists of the expectations related to the participating experts in the methodologies of the various techniques of prognosis. What knowledge can we expect from experts? What, and how, does she/he know? And what is the epistemic character of expert statements? Epistemic hopes, on the other hand, refer more generally to the cultural functions of expert knowledge. In a culture of insecurity, experts were trusted to bring clarity, certainty, and guidance into an increasingly Byzantine world. That these hopes were sometimes largely exaggerated, has been repeatedly observed (recently for instance by Collins 2014, 1–11); that coeval experts nonetheless thought them plausible is evinced by the abovementioned circularity. However, both the epistemic roles assigned to experts in the techniques of prognosis and the epistemic hopes attributed to them in contemporary culture are key to understanding the rise of the age of experts. It is their framing as sources for understanding a social figure so crucial for Cold War culture, the scientific expert, that motivates and at the same time justifies the in-depth study of the two techniques.

In focusing on techniques, this book takes a path only rarely followed in the historiography of the (social) sciences. Most publications in this field are concerned with either scholars or theories (Fleck 2015; Fleck and Dayé 2015). However, in concurrence with other scholars sharing this focus (e.g., Platt 1996), I argue that there is a lot to learn from analyzing in some depth the history of methods or techniques. While analyzing techniques sheds light on the actual practices of social researchers, it

also allows for exploring the more intrinsic sediments of coeval ideas, conceivabilities, imaginations, and worldviews. To focus on these sediments means to write the history of the social sciences in a way that recognizes the core tenet of historical epistemology, as proposed by Gaston Bachelard (1984, 2002): scientific reasoning shows implicit and nonrational traces of human existence: fears, myths, beliefs, dreams, and so on; and sometimes, these traces can explain both the success of a scientific idea and its demise (on Bachelard, see Lecourt 1975; Tiles 1984; Chimisso 2013; Dayé 2019).

The history of social science—or, for that matter, of any science—can be written in various ways and for various reasons (Dayé and Moebius 2015). Yet, chief among the reasons is the wish to provide something of value to current problems tackled in the discipline. In order to do so, histories can attempt to strengthen the disciplinary identity, for example, by the construction of a disciplinary canon and the continued critical assessment of its value. They can serve as resources in the teaching of students about the core ideas of the discipline. They can reflect on the current shape of the discipline and its position in the social, cultural, and societal whole. And finally, they can attempt to inform current research and theorizing, for example, by historically contextualizing the development of important notions in order to ensure a more sophisticated, history-conscious handling of the notions, or by understanding the history of a science as a strategic research material (Merton 1987) that allows for addressing contemporary scientific problems (cf. Dayé 2018b).

This last position motivated the writing of this book, and it is the one that comes the closest to Bachelardian historical epistemology. The history of the two techniques of prognosis is of interest because it delivers knowledge that might lead to more sophisticated use of the two techniques. By systematically addressing the epistemic role of the expert, we question the tacit assumptions on which the two techniques—and those many that are similar in this regard—rely. Yet beyond that, the history of the two techniques also allows us to address contemporary (or perhaps eternal) questions regarding the role of hope in science—how it informed the invention, development, and reception of scientific techniques.

There is another aspect of Bachelardian historical epistemology that I had to confront in writing this book. To seek for the sediments of implicit

ideas requires one to be very attentive to details. Apart from the risk that the reader might perceive an overwhelming precision with regard to out-moded and seemingly peripheral ideas to be exaggerated at best, unjusti-fied at worst, there is also the fact that one is tempted to enter a methodological discourse about the method itself and its (current or optimal) use. While I attempted to keep reflections in this direction to a minimum in order to not endanger the readability of the text, such reflec-tions certainly relate to what the word "epistemology" meant in this lin-guistic compound to Bachelard: investigate the history of a line of thinking in order to improve its current use by sensitizing current practi-tioners for the historical, psychological, cultural—brief: nonscientific—ideas it entails as sediments.

Some final words on the use of specific terms. To begin with the easier one, I use *technique* as a very generic term, comprising both procedures deemed to produce *true* knowledge (as implied, e.g., in the term scientific method) and procedures deemed to produce *useful* knowledge (as implied, e.g., in the term tool). As anyone with a basic understanding of the his-tory of science will realize, the debate on whether there is a fundamental link between these two realms of knowledge is centuries-long and ongo-ing, and it is not the task of this book to explore the underlying under-standings of science and its counterparts (cf. Dayé 2018a). The term technique is used in this book to cover all sorts of reasoned and explicit procedures in the realm of prognosis.

Further, there is considerable confusion in the literatures involved with regard to the use and the precise meanings of terms like prognosis, prog-nostication, prediction, prospection, forecast, and foresight. While I have not tried to iron out this confusion, the following definitions in my view still match the understanding currently shared by the majority of com-mentators. In this book, *prognosis* is used as a generic term, encompassing a variety of types of foreknowledge production, that is, knowledge about the future. If all the techniques discussed in this book are concerned, I will thus speak of techniques of prognosis.

Three specific types of foreknowledge are treated in this book: predic-tion, forecast, and prospection. Each of these types results in statements of different epistemic status. A *prediction* is defined as a statement made by a person (or an organization) without reference to evidence

corroborating the statement. The credibility of the statement fully depends on the authority of the person (or organization) uttering the statement.[1] In contrast, a *forecast* always relies on a set of evidence (or data). Further, this evidence is assessed "using tools not easily employed by the general public" (Friedman 2014, x), but requiring some specialist (scientific) training. The relation, however, between evidence and forecast is not strictly logical or mathematical; a forecast is not simply a statistical projection but involves judgment. Finally, a *prospection* singles out factors that are relevant in shaping the future and explores their interdependence. A prospection refrains from making firm statements about the future, but instead indicates potentialities and tries to determine how current developments might play out in the future. Thus, as Mallard and Lakoff (2011, 339–340) note, techniques of prospection are sometimes used to understand the present rather than the future. For instance, in national security, the use of these techniques helps to understand whether or not ambiguous events in the present can pose security threats in the future.

None of these types of foreknowledge had its origin at RAND; and as the current historical literature on prognosis shows, scientists and entrepreneurs of various stripes developed techniques across the three types (Andersson 2018; Andersson and Rindzevičiūtė 2015; Friedman 2014; Harper 2012; McCray 2013; Pietruska 2018). Also, many of these earlier techniques had involved expert opinions. What the RAND researchers added to this already extant body of knowledge, however, was interaction. The techniques developed at RAND were diverse, but all provided for some form of (controlled) interaction between the experts. The idea that emerged at RAND was that interaction would lead the experts contributing to these prognoses to produce results of higher stability and, thus, credibility.

Each of the subsequent chapters introduces a specific phase of RAND prognostic studies. Since the focus is on the development of the core idea—systematically using expert opinions to produce prognoses—the chapters are ordered chronologically. After Chap. 2 introduces the place of origin of the various techniques of prognosis, the RAND corporation, Chap. 3 describes the first attempts at RAND to produce predictions based on expert knowledge. In 1951, these attempts led to the first Delphi

study, yet there had been an earlier RAND study from 1948 that the Delphi developers conceived of as "precursor." The analysis shows that the two studies confronted the expert with different epistemic expectations. While in the precursor study, the expert had the epistemic role to produce a prediction, the first Delphi study expected the expert to deliver a forecast. Chapter 4 then turns to a series of political games conducted by RAND's Social Science Division between 1954 and 1959. This technique, in contrast, attributed to the expert the epistemic role of prospection. Chapter 5 describes an attempt to develop a philosophical foundation for the various techniques of prognosis developed at RAND. In line with the first Delphi study described in Chap. 3, the proposed "epistemology of the inexact sciences" was built around the idea that under certain conditions experts were able to deliver forecasts. The ensuing Chap. 6 assesses the long-range Delphi study carried out by members of the Mathematics Division in 1963. This study became the paradigmatic example for the use of the technique. The curious finding is that in spite of the earlier effort spent on philosophically corroborating the use of experts to produce *forecasts* (described in Chap. 5), the long-range Delphi study was again based on the idea that experts could come up with *predictions*. This applied also to a parallel study that addressed methodological issues of Delphi. Prior to a summary of the book's main argument, Chap. 7 sketches the further trajectories of techniques of prospection.

Note

1. Although not treated in this book, a *prophecy* thus is a subtype of a prediction where the credibility fully depends on the ascribed transcendental abilities of the person making the prediction.

References

Andersson, Jenny. 2018. *The Future of the World: Futurology, Futurists, and the Struggle for the Post Cold War Imagination*. Oxford and New York: Oxford University Press.