



The American Soldier in Jerusalem: How Social Science and Social Scientists Travel

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'The American Soldier' in Jerusalem:

How Social Science and Social Scientists Travel

A dissertation presented

by

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to

The Department of the History of Science

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for the degree of

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The American Soldier in Jerusalem: How Social Science and Social Scientists Travel

Abstract

The dissertation asks how social science and its tools—especially those associated with the precise measurement of attitudes, motivations and preferences—became a pervasive way of knowing about and ordering the world, as well as the ultimate marker of political modernity, in the second half of the twentieth century.

I explore this question by examining in detail the trials and tribulations that accompanied the indigenization of scientific polling in 1950s Israel, focusing on the story of Jewish-American sociologist and statistician Louis Guttman and the early history of the Israel Institute of Applied Social Research, the survey research organization he established and ran for forty years. Along with a wave of scientist-explorers who traveled to the postcolonial areas in the early Cold War, Guttman set out to the Middle East, leaving a secure academic position and settling in Jerusalem on the eve of the 1948 Arab-Israeli War. The inventor of cumulative scaling (known today as “Guttman scaling”)—a method of measurement first developed and used in *The American Soldier*, the classic World War II study of soldiering—Guttman sought to test in Israel the applicability of cutting-edge socio-psychological research techniques to the problems of a new state. With these objectives in mind, he established a small volunteer-based research unit within the Haganah, the largest among the paramilitary Zionist organizations in British Palestine, which then became part of the nascent Israeli Army. By the late 1950s, the military unit had evolved into a successful national research organization—the first of its kind outside the United States—

that employed over two dozen workers and carried out studies on all aspects of social life for government offices, the military, and clients in the private sector.

Joining others who have rejected Basalla's diffusion model, my dissertation shows there was nothing inevitable about the spread of these statistical methods and tools. Rather, they traveled and took root through an active, engaged, and directed process, which required the entrepreneurial initiative and cultural labor of individuals, and depended in turn on the institutional experience and habits of mind they brought with them, their embodied skills, relationships and personal virtues. More concretely, I argue that the eventual institutionalization of this scientific practice and its attendant rationality in Israel was due primarily to Guttman's ability to recreate the conditions of knowing by rendering social science expertise intelligible in the vernacular, and to make an "ecological niche" for scientific claims and methods to feel at home away from home.

Yet, while Guttman was successful in recreating some of the conditions of social scientific knowing, conducting large-scale survey research in a "hostile," or error generating environment – whether shortage of trained workers, resistant subjects and dismissive decision-makers, competing epistemic values, or the strains of war and state building – often engendered local adaptations. Highlighting the "iterability" of science in translation, I also show that behavioral concepts and claims embedded in the 'deliverables' produced by Guttman were often reframed, modified, and infused with local modes of reasoning and understanding as they were vernacularized.

The dissertation thus serves to illuminate both the processes that governed the transnational circulation of scientific ideas and tools in the postwar period and the central role this knowledge migration played in shaping the history of the modern social sciences.

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Israel's rebirth as a nation after two thousand years is a freak phenomenon of history. But in all branches of science the observation of freak phenomena yields important clues to general laws. Dwarf stars and human giants, radioactivity and parthenogenesis, prophets, maniacs and saints are all freaks which carry the conditions of normality to the pointed and profiled extreme. So does this race of eternal victims with its flayed skin and exposed nerves, which demonstrates, with the horrible precision of an anatomic atlas, a condition of man otherwise mercifully hidden from us.

Arthur Koestler, 1949

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Introduction

WITHOUT MUCH TROUBLE AND MUCH SWEAT

Read during an informal Saturday lunch session, the poem reproduced above—a firsthand report from the Israel Institute of Applied Social Research (henceforth: ASRI)—was one of the highlights of the 1953 meeting of the American Association of Public Opinion Researchers (AAPOR).¹ AAPOR’s annual conference, held regularly since 1947, functioned as the ultimate “meeting place” of the profession; an event where academic and commercial survey researchers, who, the saying went, “labored in neighboring plots of the same vineyard” for decades, began cultivating a shared culture; discussed joint problems and opportunities, promoted standards and ethics, and learned about the latest developments in various domains of practice,

Five minutes are no limitations
for telling you what's new in Scaling,
its new developments prevailing,
but - without curves and their equations.

The third and fourth component are
so simple now and popular
that Louis Guttman is enthused
how in all studies they were used:

In Preference for Motor Oil;
in Tension studies for Unesco;
in spoilt relations - and their rescue -
on Merchantships. - Without recoil

did the objective measurement
allow a more precise prediction.
You look just where your curves bend
and know the Content and Conviction,

Decidedness and Prejudice
of your eight groups of population,
and further crosswise tabulation
supplies you with the correlation.

Higher Components, in their nudes,
are attitudes on attitudes!
They do away with motivation
and Question Wording alteration.

Instead, they give you on a scale
in ordered ranks the thorough tale:

How many people may next week
change their opinion, and their habits
on buying, voting, eating rabbits,
as their intensity is weak.

How many have made up their mind
to switch; how many cannot find
a fast decision; (emphasizing
good victims for good advertising).

How many may be still involved
and can be reached, and can repent?
How many can no more be solved
while deaf to any argument?

You see, its no hypothesis.
Instead of Yes or No, you get
without much trouble and much sweat
a kind of, of all depth analysis.

Figure 1 (See footnote 1)

¹ Ruth Ludwig, “Five Minutes on Scaling and the New Developments at the Israel Institute of Applied Social Research,” *Very Applied Social Research (1949-1959)*, The Guttman Family Collection. The poem was included in a compilation of poems on the affairs of ASRI during the first decade of its existence. Ruth Ludwig gave it to the Guttmans as a gift when she left her job as the Executive Secretary of the American Committee for Social Research in Israel, Inc. and the U.S. representative, fundraiser, and PR agent of the Institute. See Chapter 4 for details.

theory and methodology.² Despite the technical nature of the subject, the session, which was devoted to techniques in attitude measurement, attracted a large crowd. According to the conference's proceedings, "the interests of participants ranged all the way from 'wishing to get caught up with a fast developing field' ...to complex and advanced questions of the application of scaling." Many came out of "simple curiosity as to what scale analysis can do."³

A novel approach to the design and analysis of questionnaire surveys, scale analysis was all the rage in those early days of the Cold War. It had been originally developed by social scientists who worked in the Army Research Branch, where it had been useful in a very large number of empirical studies concerned with the morale and functioning of large populations of soldiers, and applied on an unprecedented scale. The final analysis of the resulting massive dataset was published in 1949 and 1950 in a series entitled *Studies in Social Psychology in World War II*, better known as *The American Soldier* (TAS), had an enormous impact. Louis Guttman had been the chief methodologist for the Army Research Branch (ARB), and it was he who, more than anyone else, developed scaling analysis for use on a mass scale.

The American Soldier was, by any measure, a remarkable accomplishment. The fourth volume on measurement and prediction in particular recorded and systematized the

² On the professional organization of survey research in the aftermath of WWII and the interesting story behind the creation of AAPOR, see: Jean Converse, *Survey Research in the United States: Roots and Emergence, 1890-1960* (Berkeley: University of California Press, 1987), 228-236, and 373-376. For first-hand accounts of its founders and leaders over the years, see: Paul B. Sheatsley and Warren J. Mitofsky, eds. *A Meeting Place: The History of the American Association for Public Opinion Research* (AAPOR, 1992).

³ "Proceedings of the American Association for Public Opinion Research at the Eighth Annual Conference on Public Opinion Research, Pocono Manor, Penn.," *The Public Opinion Quarterly*, Vol. 17, No. 4 (Winter, 1953-1954), 545.

new methods and techniques and made them available for the wider social-scientific community. The advance publicity for the book and its subsequent reception heralded and later confirmed *The American Soldier* as the bible of ‘the new social science.’ The early to mid 1950s thus saw an explosion of publications on the theory, procedure, and applications of scale analysis.⁴ This had tremendous implications for both the scientific understanding of human behavior and for the practice of survey research.⁵ It completely transformed the capacities of survey measurement in terms of accuracy, depth, and prediction.

It must have been a big surprise, then, for those in attendance at the 1953 meeting to find out that the most cutting-edge work in both methodology development and application was being done, not in the United States, but by a small, little-known institute in the Middle East, otherwise a social scientific desert. Its director was none other than Louis Guttman. When World War II had ended, he had decided to take the methods he had developed with him to Palestine. The result was the first American-style academic survey research institute to be established outside the United States—and in a new state, called Israel. This dissertation will tell the story of this institute on the global frontier—itsself an experiment in using survey methods in a culture foreign to its magic.

⁴ Plotting the historical frequencies of “scale analysis” in English using Google Ngram Viewer, it appears that up to 1945, the phrase is hardly ever used (nearly 0%). A slight bent of the curve takes place between 1945 and 1950, followed by a very sharp spike between 1950 and 1955, with the highest point occurring in 1956. After some decline, the curve peaks again in 1965 and remains more or less steady through the 1970s. For documentation and analysis of the vast writing on method in American sociology in the 1950s, scaling included, see: Jennifer Platt, *A History of Sociological Research Methods in America, 1920-1960* (Cambridge [England]; New York: Cambridge University Press, 1996).

⁵ In 1950, the SSRC appointed a Committee on Scaling Theory and Methods to review the status of scaling procedures and their relation to basic research in the social sciences.

My argument is that Guttman's Institute, the Israel Institute of Applied Social Research, is best understood as an experiment in the reproduction of survey modernity in the postcolonial frontier, while at the same time an effort to make a whole society into a site of scientific investigation. Indeed, the Israel Institute of Applied Social Research was among the first in the world to integrate scale analysis into commissioned studies it carried out for government offices as well as for private clients. As the poem reports, this came gradually to embroil the Institute in projects that ranged from the study of ethnic tension for UNESCO, to market research for an advertisement campaign spearheaded by Motor Oil, to an analysis of the job satisfaction levels of employees on merchant ships.

More broadly, the dissertation aims to make a contribution to the global history of behavioral science by following the circulation of people, knowledge, data, and tools between Palestine/Israel and the U.S. in the postwar period. The dissertation concentrates on the dynamics of science acculturation in one specific circumscribed locale—Jerusalem—at a particular historical moment: the end of the British Mandate of Palestine, the 1948 Middle East War, and the first decade of Israeli statehood. As new global historians have been demonstrating, studying a particular *locality* is a good way of understanding so-called globalizing processes.⁶ At the same time, I closely examine the efforts undertaken in Israel to introduce and legitimize public opinion polling, and survey-based applied social research more generally, into different domains of practice: public administration and government policy, the private sector (marketing, advertisement, etc.), and personnel management in the army, education, and health.

Throughout the dissertation, I am particularly attentive to the tension between the very

⁶ Ulrike Freitag and Achim von Oppen, eds., *Translocality: The Study of Globalization Processes from a Southern Perspective* (Boston: Brill, 2009)

local conditions that pushed survey research to become a handmaiden to all of these different interests, and its emergence as a perceived universal form of knowledge. How did local experiments in knowledge-making like the ones I am studying facilitate projects to universalize the claims of the social sciences in the postwar era?

Asking questions like these, one discovers something surprising: namely, that the brand-new and tiny state of Israel was in fact part of an emerging transnational scientific network which extended behavioral science to include the postcolonial frontier.

Israel functioned in this capacity in two roles. It offered a culture ripe for modernizing with the tools of American science, and it offered a testing ground or laboratory for the development of this science. Thus, via Guttman, the large-scale survey arrived in Israel/Palestine first to be reproduced, but ultimately to be refitted for future circulation in a larger global system.

None of this was inevitable. Nor was it easy. The authority of the new methods and knowledge would be repeatedly challenged. There would be obstacles and resistance. To prevail required initiative, labor, and negotiation (arguing, convincing, making friends, translating). The labor was performed by specific individuals—scientific explorers, émigré scholars, and other itinerant intellectuals and brokers. It unfolded in specific institutional settings. To carry out the labor, Guttman successfully reproduced some of the institutional conditions for the practice of survey research in his new base in Israel, but conditions in the locale necessitated also that he develop certain novel adaptations: e.g. reliance on the labor generated within family dynamics and on unusually intense bonds of loyalty and friendship that partly replaced professional incentive structures. Later, Guttman would describe his Institute to his colleague back in the United States as a

Woods Hole-like research facility dedicated to methodological development and training of behavioral scientists from all across the world. Certainly, this is how the network of scientists and foundation officials that supported him perceived it. Certainly, also, raw data and facts, as well as theory, did travel back to the U.S. In the mid-1950s, when he came to spend a semester at Harvard, Guttman physically brought with him hundreds of data-filled punch cards. He also brought new and important theoretical formulations that were based on work done in Israel, and eventually became part of the general theory of attitudes.

What his colleagues in the United States did not see is just how contingent, difficult and constructed the entire operation which produced this knowledge had been. By returning to the site of knowledge-making and investigating life on the ground, we come to understand things that Guttman's colleagues did not. We learn about some of the paradoxical ways in which the claims of behavioral science for universality and authority had roots in very local and specific settings. Finally, we learn that survey research was a transnational endeavor, and that the so-called periphery was crucial for the center. The primary terms that provide the analytical backbone for the dissertation are "contact zone," a term first developed by Mary Louis Pratt, "circulation," a term that has gained increasing currency in both global history and the history of science, and "translation." Together they link historical concerns with occurrences at the local level to concerns with the global networks of which these sites are an interactive part. In these linkages, we discover all the in the inadequacy of scholarly tendencies to distinguish between the center and the periphery. It turns out that one's perception of the center changes depending on where you are standing.

Governing the World the American Way

Historians generally consider the decades following World War II to be the golden age of the social sciences, “their highest point of self confidence and of intellectual and popular authority in the United States and around the world.”⁷ It was also inevitably a time of radical changes in the social organization of the disciplines, in epistemic orientation, and in ethos. And yet, until a decade ago, most of the histories written about this period remained constricted within disciplinary boundaries, and drew on a number of fairly well-established punctuation marks to assist in periodization: the shift from interwar institutionalism in economics to postwar neoclassicism, with its physics-like emphasis on mathematical theory-building; the transition from prewar behaviorism to a postwar “cognitive revolution” in psychology; and the move in sociology from the urban ecology of the “Chicago School” and Columbia’s “objectivism” to the methodological preoccupation of “instrumental positivism,” wholesale move into survey methods, and the pursuit of what has sometimes been called “grand theory.”⁸

⁷ Dorothy Ross, “Changing Contours of the Social Science Disciplines,” in *The Modern Social Sciences*, ed. Theodore Porter and Dorothy Ross, Vol. 7 of *The Cambridge History of Science* (Cambridge: Cambridge University Press, 2003), 229.

⁸ For prominent examples of disciplinary histories of the social sciences in the twentieth century, see: Mary S. Morgan and Malcolm Rutherford, eds. *From Interwar Pluralism to Postwar Neoclassicism* (Durham, NC: Duke University Press, 1998); Philip Mirowski, *Machine Dreams: Economics Becomes a Cyborg Science* (New York: Cambridge University Press); E. Roy Weintraub, *How Economics Became a Mathematical Science* (Durham, NC: Duke University Press, 2002); Stephan P. Turner and Jonathan H. Turner, *The Impossible Science: An Institutional Analysis of American Sociology* (Newbury Park, CA: Sage Publications, 1990); Craig Calhoun, ed. *Sociology in America: A History* (Chicago: University of Chicago Press, 2007); Jennifer Platt, “Sociology,” in *The History of the Social Sciences since 1945*, ed. Roger E. Backhouse and Philippe Fontaine (New York: Cambridge University Press), 102-135. The last chapter in Dorothy Ross’ classic book provides an excellent analysis of the advent of scientism in the 1920s and 1930s and its influence on economics, sociology, and political science. Dorothy Ross, *The Origins of American Social Science* (New York: Cambridge University Press, 1991), 390-470.

Offering a politicized alternative to the neutrality of these canonical narratives, historians began to argue over the past decade and a half that developments in the interdisciplinary social sciences of the 1950s and 1960s—now grouped together as the behavioral sciences—and the immense authority they conferred are best understood in the context of the global ideological and military conflict known as the Cold War, rather than in light of intra-disciplinary discussions and trends. “Cold War Social Science,” the descriptor often deployed to mark the proliferating literature that operates within this interpretive framework, therefore suggests that “Cold War” refers not simply to a time period (like ‘interwar’ or ‘postwar’) but to a certain kind of intellectual enterprise which has been thoroughly shaped by ideological interests and national security concerns.⁹ According to Paul Edwards and Sharon Ghamari-Tabrizi—historians of military science and technology who blazed the trail in the mid-1990s—much of what was new and exciting in the social and psychological sciences of that era was in fact an outgrowth of the ‘closed world’ of military computer systems, war games, and simulations.¹⁰ If social scientific research did not always take place within the Army, Air-Force, or think tanks such as the RAND Corporation, it was generally sponsored by the defense establishment, shaped by the demand for “policy relevance,” and performed by scientists who provided expert advice for pay or were otherwise ready and willing to produce the knowledge that

⁹ Mark Solovey and Hamilton Cravens, eds. *Cold War Social Science: Knowledge Production, Liberal Democracy, and Human Nature* (New York: Palgrave Macmillan, 2012).

¹⁰ Paul N. Edwards, *The Closed World: Computers and the Politics of Discourse in Cold War America* (Cambridge, MA: MIT Press, 1996); Sharon Ghamari-Tabrizi, “Simulating the Unthinkable: Gaming Future War in the 1950s and 1960s,” *Social Studies of Science*, Vol. 30, No. 2 (2000), pp. 163-223 and *The Worlds of Herman Kahn: The Intuitive Science of Thermonuclear War* (Cambridge, MA: Harvard University Press, 2005). A recent contribution to this historiographical strain (minus the radicalism) is Paul Erickson, *The World that Game Theorists Made* (Chicago: University of Chicago Press, 2015).

their government supporters desired.¹¹ Even when scientific research programs were allegedly guided by independent intellectual agendas and committed to “basic research” (e.g. behavioral conditioning), other studies have since shown, they often enjoyed DOD or CIA funding, made use of militarized sites and subjects for testing out ideas and techniques (e.g. relocation camps, POWs, soldiers), and/or otherwise contributed to war activities.¹² More recently, historians of science have begun to trace the impact Cold War culture had on how human subjectivity was understood and theorized, and the particular “kind of mind” and model of rationality it gave rise to. An atmosphere of permanent war and persistent fears of dangerous others, aspects of what Catherine Lutz has defined as the “epistemology of the bunker,” shaped the ways psychology construed a new and more vigilant self.¹³ Cold War culture affected the self-image and ethos of the scientists. Indeed rationality itself, it has been argued, was a Cold War object and category.¹⁴

Yet an increasing number of historians have started to point out the ways in which the assumption that all social science during the Cold War amounts to ‘Cold War social science,’ as David Engerman had nicely put it, is limiting, lacking in nuance and is often reductive. In an *Isis* Focus review essay of the field, written several years ago, Engerman

¹¹ A paradigmatic example of an analysis that focuses on the way that social-scientific inquiry and Cold War policy reinforced each other can be found in Ron Robin, *The Making of the Cold War Enemy: Culture and Politics in the Military-Intellectual Complex* (Princeton, NJ: Princeton University Press, 2001).

¹² An exemplary case is that of “brainwashing” science and technology. See: Rebecca Lemov, *World as Laboratory*, chapter 10; “Brainwashing’s Avatar: The Curious Career of Dr. Ewan Cameron,” *Grey Room* 45 (Fall 2011): 60-87.

¹³ Catherine Lutz, “Epistemology of the Bunker: The Brainwashed and Other New Subjects,” in *Inventing the Psychological: Toward a Cultural History of Emotional Life in America*, ed. Joel Pfister and Nancy Schnog (New Haven: Yale University Press, 1997): 245-269.

¹⁴ Jamie Cohen-Cole, *The Open Mind: Cold War Politics and the Sciences of Human Nature* (Chicago: University of Chicago Press, 2014); Paul Erickson, Judy L. Klein, Lorraine Daston, Rebecca Lemov, Thomas Sturm, and Michael D. Gordin, *How Reason Almost Lost Its Mind: The Strange Career of Cold War Rationality* (Chicago: The University of Chicago Press, 2013)

examined this historiographical controversy and identified relevant scholarship that challenged the usefulness of the Cold War as a unifying explanatory framework for social scientific inquiry in the mid-to-late twentieth century.¹⁵ First, it disregards commitment to lines of inquiry and intellectual agendas from before the period in question. Not only were there pre-existing strains of social scientific thinking, but some of the main research programs that were allegedly Cold War products had their roots in the 1930s.

Modernization theory, for example, whose chief theorist was Talcott Parsons, was intimately connected to American foreign policy, but it was an outgrowth of long-running discussions among sociologists.¹⁶ It was WWII, not the Cold War, that marked the significant turning point in the role of social science in policy making by mobilizing a number of social scientists into government work. Using the Cold War as an interpretative framework necessitates, furthermore, a top-down analysis which leaves little agency for historical actors, whether scientists or institutions, and disregards local determinants, on-the-ground dynamics and their disruptive, at times contradictory results.¹⁷ The view from the defense strategist or the policy maker or the Pentagon had sometimes little to do with what went on. Not everything was determined by political agenda, policy, and ideology, not just at the level of practice, i.e. the everyday familiar incongruence between grant application statements and research practice, but also in the sense that things were often shaped on the ground.¹⁸

¹⁵ David C. Engerman, "Social Science in the Cold War," *Isis* 101, no. 2 (June 2010): 393-400. See also: Joel Isaac, "The Human Sciences and Cold War America," *The Historical Journal* 50, 3 (2007), 725-746

¹⁶ Nils Gilman, *Mandarins of the Future: Modernization Theory in Cold War America* (Baltimore: John Hopkins University Press, 2003).

¹⁷ Latham for scarecrow

¹⁸ See Joel Isaac's work on "middle-range contextualizations."

My dissertation substantiates and extends this critique by showing that there were other political and strategic logics at play that did not align strictly with Cold War imperatives. The dominant political logics that governed the circulation of the sample survey and shaped the encounter with American behavioral science were decolonization and Zionism. In the postwar period, Palestine became a site for intense anti-colonial struggle and counter-insurgency measures by the British occupier, as well as the center of Zionism and the object of its claims for political recognition. Growing inter-community animosity, illegal immigration and settlement, politicization of the population and finally a regional war dramatically reshaped the Middle East. In addition, despite the continuous interest that U.S. defense institutions showed in Louis Guttman's work (both the measurement and the theory aspects) and the funding they provided what he did in Palestine was a product of WWII, both in terms of the intellectual impulse and from the engineering standpoint. The particularities and peculiarities that are brought to light in this study by paying attention to what happens locally demonstrate, furthermore, that it was the everyday realities of occupation, war, and nation-building that determined outcomes, far more than ideology, political interests, etc.

Looking at the career of behavioral science in other parts of the world and methodologically focusing on and following what actually travelled around (scientists, methods of research, instruments of measurement, objects, texts) I add a new dimension to this critique. I challenge an assumption that is implicit in most studies cited above, namely, that the development of social sciences during the Cold War was geographically and culturally an American phenomenon. By showing that social science knowledge was generated in various places, I destabilize Americano-centric narratives about post-war

social science. To talk about social sciences in the period is to talk about a global affair; their development was a hybrid phenomenon that emerged out of negotiations with local systems of knowledge.

Survey Modernity

Generally speaking, the development of the survey as a method for the study and analysis of social phenomena has been a neglected topic of study among historians of the social sciences, in the Cold War period and more generally.¹⁹ With a couple of important exceptions, the little existing literature is over twenty years old, devoted mostly to the reformist origins and motivations of conditions-of-life surveys and community studies in the late nineteenth and early twentieth centuries and is largely restricted to the Anglo-American context.²⁰

Until recently, the standard reference for the development and use of modern sample surveys was Jean Converse's history of survey research in the United States.²¹

¹⁹ Mass-Observation, the short-lived British alternative to the American model of public opinion research had fared better, inspiring several studies in the last decade. For the most recent, and first full-scale history of the organization, the movement, and the method, see James Hinton, *The Mass Observers, 1937-1949* (Oxford: Oxford University Press, 2013).

²⁰ Martin Bulmer et al., Eds. *The Social Survey in Historical Perspective, 1880-1940* (Cambridge; New York: Cambridge University Press, 1991) is a paradigmatic example. See also Maurine Weiner Greenwald and Margo J. Anderson, Eds. *Pittsburgh Surveyed: Social Science and Social Reform in the Early Twentieth Century* (Pittsburgh, PA: University of Pittsburgh Press, 1996). English-language historical writing on survey research and opinion polling in Western Europe is surprisingly limited in scope. Noteworthy are Ted Porter's study of Le Play and the governmental (rather than private) French tradition of empirical social inquiry, and Bob Brain's study of the *Verein für Socialpolitik* and German tradition. See: Theodore M. Porter, "Reforming Vision: The Engineer Le Play Learns to Observe Society Sagely," in Lorraine Daston and Elizabeth Lunbeck, eds. *Histories of Scientific Observation* (Chicago: University of Chicago Press, 2011), pp. 281-302; Robert Michael Brain, "The Ontology of the Questionnaire: Max Weber on Measurement and Mass Investigation," *Studies in History and Philosophy of Science* 32, no. 4 (2001), 647-684.

²¹ Jean Converse, *Survey Research in the United States*. The history of political polling, from the straw poll to "scientific" polling can be found in Susan Herbst, *Numbered Voices: How Opinion Polling Has Shaped American Politics* (Chicago: University of Chicago Press, 1995).

The book tells the story of how the systematic collection of data about opinions, attitudes, preferences, and motivations (what she terms “the subjective realm”) by means of interviews and questionnaires, which had its roots in the worlds of political polling and market research, came to be accepted as a legitimate tool of social scientific inquiry, morphing along the way into a technically sophisticated means of observation and measurement. The strength of the book is in its wonderfully rich descriptions of the pioneers of organized survey research—their vision and idiosyncrasies, technical innovations and institutional incarnations—from the late 1930s through World War II, to the postwar period. Relying in the later chapters on personal interviews she held with many of the main actors, and careful to document the interplay of interests as well as the frequent intellectual exchange among the different groups of professionals who were invested in developing survey techniques during that era, Converse provides a lively portrait of the survey research community in mid-century.

Two historiographical themes that organize the book’s narrative have informed—and been substantiated by—the story I’m telling. First, Converse argues that the wartime experience of sociologists, psychologists, and anthropologists who were employed in the survey enterprise in Washington had an enormous impact upon the contemporary social sciences.²² In addition to acquiring a new professional interest in surveys, as well as new technical expertise in putting such surveys together, the postwar transfer of such skilled

²² Peter Buck and Libby Schweber develop and demonstrate this argument in two excellent articles that don’t always get enough attention. See: Peter Buck, “Adjusting to Military Life: The Social Sciences Go to War, 1941-1950,” in *Military Enterprise and Technological Change: Perspectives on the American Experience*, ed. Merritt Roe Smith (Cambridge, MA: MIT Press, 1985), 203-252; Libby Schweber, “Wartime Research and the Quantification of American Sociology: The View from ‘The American Soldier’,” *Revue d’Histoire des Sciences Humaines* 6 (2002), 65-94. Ellen Herman includes a discussion of wartime morale surveys and their significance in her history of psychological expertise in postwar America. See Ellen Herman, *The Romance of American Psychology: Political Culture in the Age of Experts* (Berkeley, CA: University of California Press, 1996).

personnel to universities provided a critical mass for the institutionalization of quantitative empirical research. But WWII impacted the social science disciplines in the U.S. in yet another way. The several hundred researchers at work in a handful of governmental agencies—the Army Research Branch among them—constituted what Converse describes as “a very small world,” whose members “were linked in webs of frequent contact.” As they moved on to occupy key positions in academia, private foundations, and the federal government, these strong personal and collegial ties furnished the transatlantic network that sustained Guttman’s postwar frontier operation and facilitated the circulation of research strategies, hypotheses, and explanations for a wide array of human behavior this dissertation seeks to document.

The second theme that organizes Converse’s book, which has been important for my own account, is that of normative uncertainty. She centers her extended description of the early years of organized academic survey research on the troubles the first major survey centers—the Bureau of Applied Social Research in Columbia, the National Opinion Research Center in Chicago, and the Institute for Social Research in Michigan—had with the universities they became associated with. As she shows, at all three universities there were continual tensions between the large research groups and the faculty of the social science departments, which were in part responsible for the organizational characteristics of these institutes and the emergent normative order. Using this as a backdrop for my analysis of the intercultural encounter between this scientific culture and the German neo-humanist tradition at the Hebrew University, I both build upon and extend Converse’s analysis of leadership and division of labor.

However rich and informative, the book is by and large an internalist account of survey research. Herself a former surveyor, Converse examines the field almost solely from the viewpoint of its academic practitioners, and her main interest lies in the circumstances under which survey research became “an intellectually powerful and influential part of the scientific terrain.” She is attuned to the boundary-work of social scientists as they struggle to differentiate themselves from commercial pollsters, and negotiate their purview and autonomy with government officials, army generals and university administrators; but neglects to explicate these in terms of the wider political and cultural processes that made the careers and problems she considers meaningful.

An important recent corrective, Sarah Igo’s book, *The Averaged American*, considers the history of scientific surveys as it bears on changing notions of society and self in mid-twentieth century America.²³ Turning her gaze away from the professional concerns of social scientists and their elite patrons to the lay subjects of the survey, Igo examines the initial public reception and subsequent uses made of three pioneering survey research projects: Robert and Helen Lynd’s *Middletown*, the polling enterprises of George Gallup and Elmo Roper, and Alfred Kinsey’s reports on sexual behavior. Her main argument is that surveys, which she treats as a social technology rather than a scientific method, constructed the mass public (along with its median representative, the “average American”), whose opinion they claimed to represent; a construction that the masses gradually adopted as their true collective self-representation and a standard to measure themselves against. In survey research and opinion polling, in other words, she

²³ Sarah E. Igo, *The Averaged American: Surveys, Citizens, and the Making of a Mass Public* (Cambridge, MA: Harvard University Press, 2007).

sees the origin of trends in personal and national consciousness that transformed how ordinary Americans understood themselves and their nation.

The insight that surveys are performative, not descriptive, is the other important difference from Converse, for whom the telescope—a popular metaphor among practitioners—most closely captures the scientific function of the survey instrument and the statistical descriptions it generate. Similarly to Igo, I describe how newly baptized survey subjects in Palestine/Israel—the besieged Jewish residents of Jerusalem, the Haganah recruits, immigrant communities—were given a modern identity and came to know themselves through social science.²⁴ And like her, I tie social science to the emergence of a modern notion of citizenry. I conclude that Guttman’s efforts to localize the survey succeeded partly because it asserted a unique ability to measure and express the nation, which in turn helped solidify, at least at the level of representation, a collective identity which was crucial for nation-building; an identity that on the one hand transcended party membership, religious affiliation, ethnic identity, and other divisions (such as the one between old-timers and new-comers and so on), and on the other hand, cut parts of the population—primarily Palestinian Arabs and orthodox Jews—out of the picture without damaging its coherence. Igo shows, however, that the initial response to the survey was a mixture of distrust, misunderstanding and resignation. Individuals and communities resisted these statistical portraits, either because they didn’t trust sampling (they were not asked) or because they didn’t like the facts (as in Kinsey). The values, political and epistemic, which were built into the survey, were made manifest in this process, thus adding an unexplored dimension to the history of mechanical objectivity.

²⁴ The notion of human subjects of science as “interactive kinds” is, of course, Hacking’s. Igo uses it explicitly in a follow-up article to the book.

My study also shows how the tension between local experiential forms of knowledge and the impersonal, quantitative “data” gathered by an outside expert was articulated.

Tong Lam’s *A Passion for Facts* constitutes an important addition to this literature by providing a rich account of survey modernity in another culture.²⁵ Conceptually sophisticated and original, does a great job conveying the importance of social surveys for nation building in a globalizing “culture of fact.” While my work shares some of the book’s analytical sensibilities (e.g. the attention to resistance from below, the emphasis on the role of local elites in the acculturation process), Lam makes the mutual constitution of social epistemology and social order the central problematic of his work. My work builds on these efforts and adds to them by providing a detailed account of the postwar career of the survey, namely, its life as a scientific instrument in the social sciences and the wider world.²⁶

Chapter Outline

The first chapter of the dissertation, “Into the Wild,” introduces Louis Guttman, the knowledge carrier who is the historical protagonist of this mid-century science migration story. Placing him in both scientific-professional and Jewish-Zionist contexts,

²⁵ Tong Lam, *A Passion for Facts: Social Surveys and the Construction of the Chinese Nation-State, 1900-1949* (Berkeley: University of California Press, 2011).

²⁶ I’m relying here on Mary Morgan’s approach to measurement instruments in the social sciences. See Mary S. Morgan, “Making Measuring Instruments,” *History of Political Economy* 33 (2001), 235-251. It is important to note the confusing use of “scientific” in regards to the survey: following the integration of inferential statistics into the polling of public opinion, and the development of sampling techniques, Gallup began referring to his poll as “scientific” in order to differentiate it from earlier polling practice. However, survey techniques were systematically and widely used for scientific research purposes in the social sciences only after World War II.

the chapter discusses on the one hand the rise of objectivism and quantification in 1930s American sociology—how it generated the growing dominance of a narrow and technical conception of social science, while at the same time constituting a fertile framework for methodological innovation, especially in statistical techniques of data analysis—and on the other hand the ideological, cultural and affective factors unique to the Jewish-American experience in the Upper Midwest in that era that contributed to shaping the pioneer scientist identity Guttman had adopted. The chapter culminates with the innovative work carried out under the auspices of the Army Research Branch during WWII and the postwar incentive, created in part by a new funding climate encouraging field research in the social sciences, to extend this wartime research program on the formation of attitudes into the new decolonizing territories.

The mix of postwar polling internationalism, Jewish nationalism, American quest for influence in the early Cold War—which it was hoped would be achieved through a better understanding of attitude change—and the new and immense authority of the social sciences and their aspirations formed a confluence of political conditions under which this wartime research program and model of practice traveled to Mandate Palestine. These conditions were not sufficient in and of themselves to guarantee uptake in their new environment, however, and so “What the (Jewish) Soldier Thinks,” my second chapter, tells the story of Guttman’s pioneering efforts to produce quantitative knowledge about subjective phenomena in this part of the world, focusing on a cross-cultural study of troop morale, which took place among Jewish militia fighters in Jerusalem prior to and during the 1948 Palestine War. Using rare archival materials, journals, memoirs, and reportage on life in the besieged city, the chapter reviews the difficulties involved in

conducting large-scale survey research in a “hostile,” or error generating, environment—whether material and organizational strains, shortage of trained workers, resistant subjects and dismissive decision-makers, or the realities of foreign rule, anti-colonial struggle, and everyday violence—and the kind of adaptations they engendered. I argue that the eventual institutionalization of this scientific practice in the nascent Israeli Army was due primarily to individual entrepreneurial initiative (Guttman’s ability to turn emergency conditions into a research opportunity, to garner the trust of military leaders, to mobilize locally available resources), and to the successful rendering of social science expertise intelligible in the vernacular. Yet, highlighting the “iterability” of science in translation, I also show that embedded in Hebrew guides for soldiers, behavioral concepts and claims were often reframed, modified, and infused with local modes of reasoning and understanding as they were vernacularized. The result was a hybridized military modernity and soldiering experience.

The third chapter, “Who is a Sociologist,” closely examines Guttman’s dramatic encounter with the acerbic local academic community, consisting primarily of Central European émigré intellectuals—trained mostly in the humanities and humanistic social sciences—I refer to collectively as the Hebrew mandarins. The background for the encounter is the search for, debates over, and appointment process of a leading scholar to occupy the central social science position—the Chair in General Sociology—at the Hebrew University, the world’s first and only modern Jewish university, and also the designated national university of the Zionist movement and later the State of Israel. With the HU faculty insisting on their position, and Guttman being supported by donors and the university administration, this unexpected encounter developed into a veritable

Methodenstreit that would be fought for almost a decade over the pages of local newspapers, in professional journals and tenure committees, in the university's negotiations with donors, and across Atlantic Jewish intellectual networks, involving prominent social scientists, and turning Palestine and the seemingly peripheral university into a central stage for a mammoth struggle over the mission of social science, and the character of the ideal social scientist. I argue that what led to the rejection of Guttman and the distinctively American approach to sociology that he represented, and what thus determined the fate of academic sociology in Israel, was not only the politics of method, but the scholarly habitus of the Hebrew or Zionist mandarins (as I call the HU faculty), practical and normative patterns of thinking and doing which were modeled on the unique experience of East-Central European Jews in German universities and the German cultural milieu of the interwar period more generally, shaping their ideal of the "man of science."

Rejected from the university, Guttman found that his projects shifted due to new institutional constraints, different expectations from his patrons, and different problems of public standing due to the complex political circumstances on the ground, primarily the contested legitimacy of the all-powerful Mapai, the Zionist Labor party that took the reins of government after the establishment of the State, with which Guttman and his Institute were associated. The fourth and last chapter, "The Moral Economy of Survey Research in Israel," examines the way Guttman navigated these and other political waters, including being considered a shill (*shofar*) of government policies that made citizens suspicious of his work as justificatory rather than descriptive. He also had to deal with different management issues than he would have had in a university

environment. This combination of powerful legitimation and attendant problems of credibility on the ground, lack of interest and understanding from multiple directions, and difficulty securing substantive (rather than symbolic) resources, necessitated particular strategies to keep the Institute afloat. In particular, the most significant adaptation was in the substitution of family-like dynamics for administrative hierarchies and formalized social relations. The inhospitable conditions in the new state for large-scale, labor-intensive and costly research meant that much of the work had to be done by actual family members. As the institute evolved by the early 1960s into a successful national research organization, familial patterns of interaction and relating persisted, securing knowledge production by operationalizing emotional commitment and intimacy in place of ‘work-discipline’ and a vocational ‘ethic of responsibility.’

Through examining the history, organizational culture, and everyday operation of the first of these institutes to be established in the postcolonial areas—the Israel Institute for Applied Social Research—the questions of how well social science research travelled, as well as what kind of adaptations were made to it in the process of domestication in a foreign locale, can be addressed.

Chapter One

INTO THE WILD

Writing from Mandatory Palestine on Christmas Eve, 1947, Louis Guttman, a 31 year-old American social scientist on an SSRC-funded research trip, reported back to his sponsors in New York on the successful launch of the research project for which they had granted him \$4,500 the previous May. Clumsily—as befits someone who knows his way around numbers, less so around words—yet with much verve, Guttman described the many benefits this faraway land offered the diligent student of human behavior. “There is a vast amount of sociological and psychological material here that is virtually untouched and easily accessible,” he stated excitedly, referring to Palestine’s inhabitants, natives and settlers alike. “And preliminary interviewing has indicated” that though they have never seen a survey questionnaire before, “it is quite possible to make a Gallup-type or Likert-type survey among the Jewish population,” as he had hoped.²⁷ Living among the surveyed—Guttman reported that he and his wife Ruth were sharing quarters with members of *Hamekasher*, the Jerusalem Jewish bus cooperative—made it possible to conduct informal interviews and “experiment with observations of a non-verbal nature.” Even the UN decision on a Jewish State, the most significant event in modern Jewish history, and one which would shake the entire region and displace many of the country’s

²⁷ “Gallup-type” and “Likert-type” refer both to questionnaire-based sample surveys that consist of closed-ended questions, namely, questions to which the answer must be chosen from a list of predetermined responses. In a Gallup-type survey, the respondent has two options to choose from (yes/no, agree/disagree); in a Likert-type survey, the respondent has a set of five responses to choose from (strongly agree/somewhat agree/neither agree nor disagree/somewhat disagree/strongly disagree). The advantage of questionnaires over some other types of survey is that they are cheap and do not require much skill or effort from the questioner.

residents, was framed in similar terms, as “an unusually rich opportunity for learning how to study the dynamic of attitudes.”²⁸

Objectivism Unbound

A big part of the excitement had to do with the novelty of the experience: it was the young scientist’s first encounter with attitudes “in the wild.” Born to Russian immigrant parents and raised in working-class Jewish Minneapolis—an environment that had a profound effect on him—Louis Guttman (1916-1987) had never been to these foreign parts, or anywhere else outside the U.S. for that matter. In fact, until he graduated and took a faculty position at Cornell University several years before, Guttman had never left his hometown.²⁹ Preferring to devise ways of ordering the world at his desk to direct engagement with the mess of the empirical, he had devoted the better part of his twenties to applying mathematical statistics to sociological problems and developing data analysis techniques and tools at the University of Minnesota, where he also earned his three degrees—all in sociology.³⁰

²⁸ Louis Guttman, “Application for Fellowship 1947-48—Renewal,” December 12, 1947. Folder 5693, Box 466, Series 1, Subseries 75, Accession 2, SSRC, RAC.

²⁹ Guttman was born in Brooklyn, New York, but when he was three years old the family moved to Minneapolis, where he completed his formal education. To some extent, it remained the place he considered home for the rest of his life. Incidentally, Guttman also died in Minneapolis, while on a visit to the United States to deliver lectures and receive medical treatment for cancer.

³⁰ Guttman attained his B.A. in 1936, M.A. in 1939, and Ph.D. in 1942. He began with sociology and math, but soon gave up math and took psychology instead. His undergraduate studies included courses in psychology equivalent to a major in the field, and he gradually became interested in psychometrics—a field to which he would make major contributions. During his graduate studies, as he became convinced of the importance of mathematical methods for empirical social research, he went back to study math and both his Masters and his PhD degrees include a minor in mathematics. His earliest publications (the first when he was 22 years old) were in mathematical statistics. This led him to formalize—while still a graduate student—techniques for data analysis, some of which were published in 1941 and constitute the foundation of his later work on scale theory and factor analysis. See: Shlomit Levy, “Guttman, Louis,” in *Encyclopedia of Social Measurement*, Vol. 2 (New York: Elsevier, 2005), 175-188.

When asked in an interview years later what brought a “nice Jewish boy” to pick sociology—a discipline still associated in the Jewish popular imagination with crude, i.e. gentile, practicality—and not the natural sciences, Guttman smilingly admitted:

The truth is that I did not have a strong inclination for anything in particular. There’s propensity for math in our family, and when I had to specify in high school what I would like to study further, I wrote: chemistry. I registered [for studies] at the School of Chemistry, but that year the Great Depression hit and instead of studying, I went to work. I started school the following year but transferred to General Studies in order to hear lectures in all subjects. I didn’t know what to choose. The best lecturer was in zoology. The second best lecturer was in sociology, and the subject appealed to me more. Since I wasn’t sure, I went to the *Vocational Assessment Clinic*, which was then linked to the School of Education—one of the world’s most advanced at that time—and the Director of the Clinic ran my tests. After a battery of tests, he told me: go to sociology, and math won’t hurt you either. So I went to sociology.³¹

A perfect origin myth, this little story about how a psychometric procedure—of the kind he would later devise himself—decided Guttman’s future for him is historically telling. It encapsulates the belief characteristic of the period that social science was able to provide definite answers to very concrete, everyday, practical questions.³² But it also

³¹ Edith Na’aman, “Prof. Louis Guttman, Israel Prize Laureate,” 1978, 8-9 (Hebrew). According to Daniel Boyarin, the qualities ascribed to the nice Jewish boy—an American stereotype of Jewish masculinity—are derived from *Edelkayt* (literally, “nobility,” but in Yiddish “gentleness and delicacy”), the feminized Jewish male ideal of Ashkenazi culture, that embraces the studiousness, gentleness and piety said to distinguish the Talmudic scholar. In this context, it was clearly invoked (by the Israeli interviewer) to suggest the still common association of sociology (and social science more generally) with useful knowledge, which was in turn contrasted with the scientific pursuit of truth and learning for learning’s sake. See: Daniel Boyarin, *Unheroic Conduct: The Rise of Heterosexuality and the Invention of the Jewish Man* (Berkeley: University of California Press, 1997). Boyarin makes the connection between the edelkayt ideal and the present-day American NJB in Rachel E. Gross, “How the Jewish Boy Got So Nice,” *Moment Magazine*, Nov/Dec, 2014, <http://www.momentmag.com/jewish-word-njb/>

³² The kingpin of psychological testing at the University of Minnesota was Donald G. Paterson. Paterson served on the U.S. Army Testing Project during WWI, and in 1921 joined the Department of Psychology. In the years that followed, Paterson became synonymous with the Minnesota tradition of differential psychology, industrial psychology, and vocational guidance, which for many years dominated the field and generated a disproportionate share of its leaders. Paterson’s work formed the basis of many large-scale, multi-aptitude test batteries including the *General Aptitude Test Battery* (GATB). During his Masters, Guttman worked as assistant in education research and was part of this environment. It may be worth mentioning that in 1936, the year Guttman began his Masters, B. F. Skinner joined the psychology faculty.

demonstrates the trust men of Guttman's generation had that these diagnostic and predictive tools could do so objectively and provide a seemingly absolute standard for decision making and social action in place of outworn customs and beliefs (from misogyny to racial prejudice) which were now branded "subjective."³³

And indeed, in the mid-1930s, the Minnesota department of sociology was a place where this promise for knowledge of social life that was free of bias and uncertainty—what Guttman described as "the search for objective approaches to the study of social phenomena" —might be delivered.³⁴ As Bannister explains

objectivity had been a professional norm since the establishment of sociology as an academic discipline in the 1880s, but the new "scientific" sociology of the 1920s and 1930s was to be objective in three quite special senses: first, it had to confine itself to the observable externals of human behavior. Secondly, sociologists had to apply methods equally rigorous to those of the natural sciences in the production of social scientific knowledge. During the twenties, this injunction occasioned debate over the case study, the participant observer, and the comparative methods; by the 1930s, statistical analysis became the method of choice. And finally, sociologists had to observe strict neutrality in matters of ethics and public policy. "Sociology itself passes no moral judgment," the argument went, "and sets up no ethical standards for human conduct." From these premises, certain predilections followed. Among them was the focus on individual

It was during his term at the U of Minnesota that Skinner wrote *The Behavior of Organisms* (1938) and the utopian novel *Walden Two* (1948), designed the famous Air Crib and conditioned missile-guiding pigeons.

³³ In Guttman's case, this was not merely a matter of epistemic fashion or taste. Sociology in the United States had its roots in religiously-driven reform work and many of the first generation of professional sociologists (Albion Small, Charles Henderson, Charles Ellwood and others) were deeply religious and were drawn to sociology, as Calhoun puts it, "through, not instead of, Christianity." And as Hollinger had famously shown, American higher education (especially in the humanities and social sciences) was largely barred for Jews until after World War II, and rife with anti-Semitism. Guttman himself reported having been told, when he began his graduate studies in sociology at the University of Minnesota and sought employment as a teaching assistant: "Jews have no future in sociology." Interestingly, Guttman didn't make much of this incident. When asked on occasion whether his Jewishness had any impact on his career, he would insist that he "never thought about such issues" and "did not feel" his Jewishness (professionally). This assertion goes well with the objectivist ethos he had adopted. Craig Calhoun, "Sociology in America: An Introduction," in Calhoun, ed. *Sociology in America: A History* (Chicago: University of Chicago Press, 2007), 17 and David A. Hollinger, *Science, Jews, and Secular Culture* (Princeton: Princeton University Press, 1996). The incident and quotes of Guttman's appear in Na'aman.

³⁴ On the history of the Minnesota Department of Sociology, see: Don Martindale, *The Romance of a Profession: A Case History in the Sociology of Sociology* (St. Paul: Windflower, 1976).

behavior rather than on the formation and transformation of social structure; an emphasis on method rather than on content; and, in the long run, a bureaucratic vision of team research and social science institutes.³⁵

The man who carried the gospel of objectivism to the upper Midwest and had come to embody its ideal was F. Stuart Chapin, an ambitious upper middle-class quantifier with background in engineering, who, after a short stint at Smith College, was appointed chairman at the University of Minnesota—a position he held for 31 years—and built the sociology department into one of the most distinguished in the country. Having begun, like many others who came of age in the Progressive Era, as a social evolutionist, Chapin steadily drifted toward a more scientific sociology with an ever more technical conception of its mission.³⁶

A programmatic statement on the use of statistics in sociology he issued early on in his career contained already the elements that would form the matrix of his mature thought. “There has been too much deductive philosophic generalization and far too little inductive verification,” Chapin charged. Statistics offered the best means to amass large numbers of observations and also reduce bias and individual error. In the statistical “average,” moreover, sociology had available an objective standard. Generalizations that emerge from such study, he claimed, “will be fairly comparable as regards validity and

³⁵ Bannister, Robert, *Sociology and Scientism: The American Quest for Objectivity, 1880-1940* (Chapel Hill: University of North Carolina Press, 1987), 3-4.

³⁶ Francis Stuart Chapin (1888-1974) was educated at Columbia University, obtaining his PhD in sociology in 1911, with additional training in statistics. He joined the faculty of the University of Minnesota in 1922, and served as professor and chair of the Department of Sociology until his retirement in 1953. In addition to his sociology appointment, Chapin directed the social work program at the University and served as the first director of the newly developed School of Social Work. Throughout his long career, he was a consultant to the League of Nations, to UNESCO, and to the World Health Organization. He was also one of the founders of *Social Science Abstracts*, served on the SSRC, and was the president of the American Sociological Association, among other honors.

accuracy with the generalizations of applied science.” The seeming exactness and certainty of number, its very nakedness of qualitative distinction, promised to “do away with all the uncertainties or doubts that beset the path of social science.”³⁷ This view of the preeminence of the statistical method received another expression in 1917.

Sociologists worked at a disadvantage as compared to natural scientists, he argued, since they could not hold laboratory experiments. In a natural science experiment, “the fundamental rule...is to vary only one condition at a time and maintain all other conditions rigidly constant.” Yet, in so-called social experiments, like the nineteenth century utopian communities or the isolation of Appalachian peoples—on which social scientists were forced to rely—there was always more than one varying condition, and the conditions were often changing. By measuring the correlation between two or more variables, statistical analysis overcame this limitation and was therefore the main business of sociology.³⁸

Chapin’s crowning achievement, the “living room scale,” was an exemplary manifestation of this extreme methodlogocentrism. First built for his graduate classes during the academic year 1927-28, the scale, which won Chapin a reputation as a pioneering technician of the new sociology, was an attempt at developing “a convenient device” to measure socioeconomic status (or social class, for which this was the common euphemism). Chapin started by formulating a working definition of social status that will function as an assumption from which to begin the measurement, and by isolating four

³⁷ F. Stuart Chapin, “The Elements of Scientific Method in Sociology,” *American Journal of Sociology*, 20 (1914): 371, 391.

³⁸ Quoted in Dorothy Ross, *The Origins of American Social Science* (Cambridge University Press, 1991). See her discussion, pp. 370-371 and Bannister, 151.

“measureable elements” in that definition that arguably contributed to deciding the social status of all the families in the sample in question. After demonstrating that the contents of a living room correlated well with these other, objective or independently determined indices of socioeconomic status—primarily income and “participation of group activity of the community” (or group setting index)—Chapin devised an elaborate point system (or rating scale) that would allow the researcher to record, on the basis of home visits and interviews, empirical observations regarding the equipment, condition, and “cultural expression” of different living rooms in urban homes: cleanliness or orderliness of room, kind of flooring and floor covering, type of furniture, and so on, as well as the presence of so-called “cultural possessions” such as books, periodicals, radio, musical instruments, paintings, and the like.³⁹ The final score for each family was the sum total of these weights and stood for the predicted social status of that family.

³⁹ Each household item was assigned a numerical value (an arbitrary weight): an electric light or a fireplace with three utensils was worth eight points, and hardwood floors ten. A kerosene heater in the living room cost two points, as did a sewing machine or an alarm clock. F. Stuart Chapin, “A Quantitative Scale for Rating the Home and Social Environment of Middle Class Families in an Urban Community: A First Approximation to the Measurement of Socio-economic Status,” *Journal of Educational Psychology* 29, no. 2 (1928): 99-111; “Socio-Economic Status: Some Preliminary Results of Measurement,” *American Journal of Sociology* 37, no. 4 (1932): 581-587.

**SCALE OF WEIGHTS FOR RATING THE EQUIPMENT OF THE LIVING ROOM OF AN
URBAN MIDDLE CLASS FAMILY**

Instructions

The following list of items is for the guidance of the recorder only. All of the features listed will not be found in any one home. Entries on the schedules, however, should follow the order and numbering indicated. Weights appear after the name of the respective items.

I. Fixed features.

- 1. Floor. _____
Softwood 1, hardwood 2, composition 3, stone 4.
- 2. Floor covering _____
Composition 1, carpet 2, small rugs 3, large rug 4.
- 3. Wall covering..... _____
Paper 1, kalsomine 2, plain paint 3, decorative paint 4, wooden panels 5.
- 4. Woodwork _____
Painted 1, varnished 2, stained 3, oiled 4.
- 5. Door protection _____
Screen 1, storm door 1.
- 6. Windows _____
1 each.
- 7. Window protection _____
Screen, blind, netting, storm sash, 1 each.
- 8. Window covering..... _____
Shades 1, curtains 2, drapes 3.
- 9. Fireplace..... _____
Imitation 1, gas 2, wood 3, coal 4.
- 10. Fire utensils. _____
Andirons, screen, poker, tongs, shovel, brush, hod, basket, rack, 1 each.
- 11. Heat..... _____
Stove 1, hot air 2, steam 3, hot water 4.
- 12. Artificial light. _____
Kerosene 1, gas 2, electric 3.
- 13. Artificial ventilators 1 _____
- 14. Clothes closets 1. _____
- Total..... _____

II. Built-in features.

- 15. Book containers _____
Shelves 1, cases 2.
- 16. Beds..... _____
In-a-sideboard 1, in-a-ceiling 2, in-a-door 3.
- 17. Desk 1 _____
- 18. Window seats 1..... _____
- 19. Window boxes 1. _____

III. Standard furniture.

- 20. Table.... .. _____
Sewing 1, writing 1, card 1, library 2.
- 21. Chair _____
Straight, rocker, arm-chair, high-chair, 1 each.
- 22. Stool or bench _____
High stool, foot-stool, piano stool, piano bench, 1 each.
- 23. Couch _____
Cot 1, sanitary couch 2, chaise longue 3, day-bed 4, davenport 5, bed-davenport 6.
- 24. Desk _____
Business 1, personal-social 2.
- 25. Book cases 1 _____
- 26. Wardrobe or movable cabinet 1..... _____
- 27. Sewing cabinet 1. _____
- 28. Sewing machine..... _____
Hand power 1, foot power 2, electric 3.
- 29. Rack or stand 1 _____
- 30. Screen 1..... _____
- 31. Chests 1 _____
- 32. Music cabinet 1 _____
- Total..... _____

IV. Furnishings and cultural resources.

- 33. Covers _____
Furniture, table, chair, couch, piano, 1 each.
- 34. Pillows _____
Couch, floor, 1 each
- 35. Lamps _____
Floor (large) floor-bridge, 1 each.
- 36. Candle holders, 1 each _____
- 37. Clock..... _____
Mantel, grandfather, wall, alarm, 1 each.
- 38. Mirror 1 _____
- 39. Pottery..... _____
Factory made 1, hand made 2.
- 40. Baskets..... _____
Factory made, hand made, waste, sewing, sandwich, decorative, 1 each.
- 41. Statues 1..... _____

Figure 2

In a later meditation on measurement in sociology, Chapin contended that the true achievement his living-room scale represented was not the ease and expediency of prediction, but rather the fact it provided a definition for one of the discipline's central concepts "in terms of the method used to measure it and in terms of the result of this measurement process," in the manner done in the physical sciences. The "quantitative description" of social status, as he referred to the score a family would get on this standardized test in bourgeois propriety, was much sharper, he argued, than verbal descriptors such as "poor" or "relief case" which were not only vague and open to interpretation, but also difficult to verify. As he concluded:

The great advantage of measurement as quantitative scientific description is that it is more susceptible of accurate recording, independent verification, and transmission than are other methods...As the sociologist adopts the numerical symbol of description in the definition of his concepts they will become more precise and he will approach the position of the physicist in the precision of his work.⁴⁰

Guttman, who was Chapin's research assistant as an undergraduate and later his doctoral student, and worked with him on further developing such methods and procedures, internalized the "increasing demand for rigorous proof" Chapin's research and writing exhibited and fully embraced his reductive conception of social scientific practice.⁴¹ He grew, however, increasingly dissatisfied with what they were technically able to do: "the statistics was very crude at that time," Guttman recalled years later. "We

⁴⁰ F. Stuart Chapin, "Measurement in Sociology," *American Journal of Sociology* 40, no 4. (1935): 479.

⁴¹ Chapin was notorious for his autocratic ways, which, some have argued, mirrored his rigid scientific practice. On Chapin's scientific persona and the ethos of objectivism, see: Bannister, chapter 10. "Comparable to the increasing demand for rigorous proof which characterized Chapin's writing and research, was an increasing inclination toward formality in his personal relations ... [as he adopted objectivism] Chapin increasingly moved toward a ceremoniously formal style of administrative control which many viewed as "cold," "unfeeling," and even "tyrannical." (Martindale, 82).

did not know how to systematically collect information, how to analyze factors...how to measure...there were many areas of ambiguity and imprecision.”⁴² This, he felt, was especially true about the study of social stratification. “Everybody talked about social class, but from a purely professional point of view, there were neither elegant definitions, nor suitable methods of data analysis.”⁴³

More specifically, he had found Chapin’s measurement procedure for social status wanting in excluding the researchers’ bias and subjective opinions in at least two levels. First, was the lack of justification as to the weights attached to the items of the scale, which “were determined ... only in a rough and somewhat intuitive manner.”⁴⁴ Guttman’s initial solution was to extract those weights in an objective fashion, by using empirical data. Guttman used a specific dataset, obtained in a survey of 67 African-American families in Minneapolis, where he could assess the fit between the specific items in Chapin’s scale, and the other ‘external’ indicators of status (occupation, income, social participation and education), and tweak the weights given to items in Chapin’s scale so that its fit to the data would be maximized. This revision, which, importantly, does not rely on any assumptions made by the researchers, shows “that the presence or absence of a library table does not tell much that is not already told by the other items

⁴² Naomi Gutkind, “The Achievements of Social Research in Israel, A Conversation with Professor Eliyahu Guttman,” *HaTzofe*, 22 February, 1978, 4. (Hebrew).

⁴³ Na’aman. Guttman initially chose to dedicate his dissertation to bettering Chapin’s measurement procedure for social status. The thesis, however, evolved into a formal mathematical work in linear factor analysis. For all I know, it was one of the first productive uses of factor analysis in sociology. See: Louis Guttman, “The Prediction of Quantitative Variates by Factor Analysis,” Ph.D. diss., University of Minnesota, June 1942. I had access to a copy of the dissertation in the Guttman family collection. I thank Sam Schweber for many and patient attempts to explain the thesis to me.

⁴⁴ Louis Guttman, “A Revision of Chapin’s Social Status Scale,” *American Sociological Review* 7, 3 (1942), 365

included in the scale.” “The fireplace item,” on the other hand, “receives the highest weight in this new analysis, and the telephone jumps up in importance.”⁴⁵

The second problem was perhaps more fundamental, and related to a deeper presupposition Chapin made about the nature of ‘status’—assumptions that Guttman wanted to do away with:

A basic problem centers around the definition of status from...income, education, social participation, and occupation...It would be desirable to have a comprehensive treatment of the universe of variates from which these are conceived to have been drawn. In particular, the notion of attitude that seems essential to a definition of social status needs to be... considered.

In essence, Guttman wonders, why these four variates and not others? Specifically, he wonders, what is the role of attitudes in this definition of status? In order to answer this question—to assess to correlation between attitudes and status—one would first have to devise a way to objectively measure attitudes, a task much harder than counting rugs and pianos.

Tricks of the Trade

An opportunity to further his ideas about scaling and gain practical experience in the analysis of “raw data” had come when Guttman received a predoctoral research fellowship to spend the academic year 1940/41 at the University of Chicago. Chicago was then a world center in the development and application of quantitative methods in the study of human behavior, and a place where the eager prodigy from the prairie, who “craved more statistical equipment,” felt he could expand and enrich his limited

⁴⁵ Ibid, 367

methodological arsenal.⁴⁶ Significantly, it was also the only place in the U.S. where factor analysis—Spearman’s psychometric method of data analysis he was looking to adapt for use in sociological research—was taught.⁴⁷

Indeed, a good reason to come to Chicago was L. L. Thurstone, the psychologist and psychometrician whose methods for scaling psychological measures, assessing attitudes, and test theory revolutionized the study of human intelligence, as well as the nascent field of attitude research.⁴⁸ Guttman was especially keen on acquiring solid working knowledge of “multiple factor analysis,” a set of statistical techniques for determining the number and nature of latent constructs within a set of observed variables that Thurstone first published in 1935.⁴⁹ As he had indicated in his application, Guttman was hoping to work alongside the famous professor on a series of experiments the latter was engaged in designing at the time, and thus learn firsthand how proper empirical data

⁴⁶ From its founding until the late 1920s, the Chicago Department of Sociology had maintained unquestioned dominance over the discipline in the United States. It was the first organized department in the country and the first to institute a graduate program in sociology; its founders established the American Sociological Society and later initiated the *American Journal of Sociology*, which became the official organ of the society. Mainly, the Chicago department took the lead in suppressing a pre-occupation with charity and social reform, and was the foremost spokesman during these years for a scientific sociology. Even though Chicago declined as a sociological power in the 1930s—forced to share the lead with worthy competitors such as Minnesota and gradually losing it to Columbia—it remained a center of rigorously quantitative statistical sociology. In the historical literature, interwar Chicago sociology is known primarily for the urban social research associated with Robert Park and William I. Thomas. As Bannister argues—and Guttman’s application demonstrates—Chicago was at the same time a major center for quantifiers. See: Bannister, ch. 12

⁴⁷ For a popular rendering of the history of factor analysis, see Stephen Jay Gould, *The Mismeasure of Man*, Rev. and expanded edition (New York: Norton, 1996), chapter 6.

⁴⁸ The new statistical techniques developed by Thurstone provided the necessary tools for his most enduring contribution to psychology: The Theory of Primary Mental Abilities, a model of human intelligence that challenged Charles Spearman’s then-dominant paradigm of a unitary conception of intelligence. On the origins of factor analysis in intelligence research, see: John Carson, *The Measure of Merit: Talents, Intelligence, and Inequality in the French and American Republics, 1750-1940* (Princeton, NJ: Princeton University Press, 2007), pp. 185-187.

⁴⁹ L. L. Thurstone, *The Vectors of the Mind; Multiple Factor-Analysis for the Isolation of Primary Traits* (Chicago, Ill.: The University of Chicago Press, 1935).

for factor analysis was acquired, and how it was then analyzed in quantitative terms.⁵⁰ His reasoning for wanting to be in close proximity to Thurstone exemplified his tool-oriented and skill-focused approach to social science perhaps better than anything else: “There are ‘tricks of the trade’ with regard to numerical manipulations and computations,” he argued, “which can best be learned at the source, where these techniques are being developed.”⁵¹

For Guttman, then, the practice of science required specialized knowledge, but of a specific kind—that of the tradesman or the craftsman—the kind of knowledge that the traditional modes of transmission in academic sociology were not conducive of and for which the apprentice model was better suited. Reading the fellowship application in light of this interpretation, I suggest that Guttman came to Chicago so that he could “shadow” several of the prominent statistical thinkers among the faculty at the University—in addition to Thurstone, he mentions the Russian mathematical biophysicist Nicolas Rashevsky, who was working then on social interaction, and the sociologist Samuel Stouffer, the “oracle” of large-scale quantitative social research who would become his mentor and friend for many years to come—as they applied themselves, and their analytical tools, to concrete sociological problems. “The objective of the proposed field program,” he stated, was “acquisition of first-hand knowledge of the applications of certain types of mathematical rationale to sociological data.” His experience in statistical reasoning so far, Guttman explained, has been split between two disparate worlds: the very concrete, yet statistically primitive, “ecological research” into social problems with

⁵⁰ Louis Guttman, “Pre-Doctoral Field Fellowship Application 1940-41,” January 26, 1940. Folder 5693, Box 466, Series 1, Subseries 75, SSRC, RAC

⁵¹ Ibid.

which he engaged in his capacity as Acting Director of the University of Minnesota's Sociological Research Laboratory, and the sophisticated, yet wholly abstract and academic realm of mathematical statistics.⁵² He hoped for the kind of craft knowledge that the hands-on learning working with scientists like Stouffer and Thurstone in Chicago could offer, making it possible to bridge the two worlds, to synthesize pure statistical reason with empirical reality. In his words, this was the only way to “to become thoroughly acquainted with the actual numerical techniques involved in carrying an analysis beyond the stage of abstract formulas.”

While in Chicago, Guttman discovered the Chicago school, and as he would often nostalgically tell, there was something extremely appealing in what Herbert Blumer once termed—in critique of Ogburn— “science without concepts.” He liked how down to earth they were, connected to real world problems, extremely adept at empirical investigation, and yet able to provide reliable predictions. “In Chicago they did not study highfalutin theory,” he explained the appeal. “Their work dealt with very practical matters.” Most important, Guttman added, “The results of these prediction studies were applied in practice.”⁵³ One early example of the kind of work done at Chicago he would

⁵² During academic year 1939-1940, while a first-year doctoral student, Guttman served as Acting Director of the University of Minnesota's Sociological Research Laboratory, sociology's state of the art empirical research unit. That year the Laboratory conducted a community survey of the problems and activities of youth agencies in Minneapolis (recreational facilities, juvenile delinquency, unemployment and so on), and prepared a compilation of a “Supplemental Guide to Social Conditions in the Twin Cities” —a companion volume to an annotated bibliography the Laboratory published in 1938. Coming in close contact with the different stages of the applied social research process must have left an impression as well.

⁵³ Na'aman, 9

refer to often was the “Burgess method,” the statistical tool sociologist Ernest W. Burgess developed to predict probation violation.⁵⁴ He was impressed.

Other than a passing mention that Guttman was involved in a large survey research project on marriage and marital happiness [that the sociologist Ernest W. Burgess conducted in collaboration with Paul Wallin], we don’t have any surviving record of his actual training experience working on the analysis of these huge datasets.⁵⁵

Zionism and Scientism

When the ARB was dismantled and its survey warriors were sent back to their civilian lives, Guttman was in a position to find a lucrative job in any American research university. TAS had solidified his reputation as a *wunderkind* of the new social (soon behavioral) science, secured his standing as an academic star and enthroned him as a leading expert on survey research methodology—a fast growing and in-demand field. Back full-time in Ithaca, he was made Associate Professor and university authorities were clearly banking on him to rise up the academic ladder and in time assume a position of leadership in the Department of Sociology and Anthropology.⁵⁶ And yet, once the fog of war cleared up, he began to look for ways to reach Palestine.⁵⁷

⁵⁴ E. W. Burgess, “Factors Determining Success or Failure on Parole,” in *The Working of the Interminate Sentence Law and the Parole System in Illinois*, ed. A. Bruce, A. J. Harno, E. W. Burgess, and J. Landesco. Springfield, IL: Illinois State Board of Parole. (1928).

⁵⁵ The survey ran from 1937 until 1944 and its results were published in *Engagement and Marriage* (1953).

⁵⁶ Guttman later claimed that trying to make arrangements for going away, Cornell “tripped him up” as they did not want him to leave. See: Na’aman, interview with Ruth Guttman. He had been likely designated to replace Leonard S. Cottrell, Jr. (1899-1985), the first Chair of the Department of Sociology and Anthropology (founded in 1939) and the person who was responsible for his hiring. Cottrell, who worked alongside Guttman at the ARB, went back to chair the department at the end of the war, but shortly after was named Dean of Arts and Sciences, and in 1951 left Cornell altogether in favor of the Russell Sage

“Why leave the green pastures of science at Cornell,” an interviewer once asked, “and come to the desert?” Meditating on his motivations in response to the query, Guttman revealed something of the deep ambivalence that colored this decision; otherwise presented as whole-heartedly committed:

I always aspired to come to Israel. When I was in Minneapolis, I studied at Talmud Torah all these years. I spoke fluent Hebrew...I even lectured in Hebrew [when graduating from Talmud Torah] and said, among other things, that it was our duty to make aliyah. The Rabbi remembered my speech for years. He later claimed that I was wrong, but I wanted to go. My wife is a Zionist as well. My older brother was already in Palestine [but] I never thought to be a *halutz* [pioneer]...I received a postdoctoral fellowship and we decided to go for a year or two...⁵⁸

The list of reasons— affective, ideological, familial— tallied in Guttman’s reply suggests that everything about his upbringing and cultural make-up prepared him to make the difficult choice. As family friends and community members would testify, in the Guttman household in Minneapolis, Jewish nationalism was not simply an ideological orientation, but something akin to a religious faith. All encompassing, it functioned as the organizing principle of everyday life and kinship relations: it dictated how Guttman had spent most of his afternoons as a young boy, how he related to his siblings, even how he

Foundation. He and Guttman remained on friendly terms. See: American Sociological Association, “Leonard S. Cottrell, Jr.,” http://www.asanet.org/about/presidents/Leonard_Cottrell.cfm.

⁵⁷ Guttman first sought support from the Jewish Agency and several scientific foundations for a Palestine-based study on Jewish-Arab relations. After this had failed, he switched tactics and came up with a plan for academic exchange: he would be sent to Palestine in exchange for a Hebrew University professor who’d come to teach at Cornell for one year. Much to Guttman’s surprise, the reply from Jerusalem was that they had no suitable candidate to send to the U.S. (see chapter 2). Finally, in spring 1947, a yearlong postdoctoral fellowship for “intensive attitude research in a different culture” was obtained from the Social Science Research Council (SSRC). It was under these auspices that the Guttmans arrived to Palestine. I discuss the fellowship further below.

⁵⁸ Na’aman interview, 1974.

met his wife and probably the wife he had chosen.⁵⁹ It was also the material from which hopes and aspirations were made; as parental love and love for *Eretz Yisrael* [Land of Israel] were one and the same, Guttman's parents had hoped for their son to make the ultimate Zionist act of aliyah and settlement as some hope for their child to become a doctor or lawyer. Guttman's father, a soft-drink jobber who hailed from a small town near Odessa as a young man, was known as a staunch Zionist, and his mother, who, the saying went, was the one wearing the pants in the house, made sure her children attended Jewish school, learned Hebrew, and acquired a strong (secular) Jewish identity and sense of belonging. They were both active in the American labor Zionist movement, and their progeny were expected to follow suit—as they all did. And yet, despite this single-minded agenda, Guttman would emphasize time and again, learning was no less important.

Halutzim were a new breed of Jewish immigrants who arrived to Palestine in the first decades of the 20th century and came to exemplify—in their grueling lifestyle, their asceticism, the totality of their devotion to the national project, but also in their deep and very personal (some claim erotic) connection to the country itself—the Zionist ethos.⁶⁰

⁵⁹ Guttman, like his siblings, attended Talmud Torah, the city's renowned Jewish school, three times a week in the afternoons throughout his school years. He was also a member of the school's Hebrew-speaking club, *Barkai*, and edited *Habarak* (The Lightning), a Hebrew-language magazine the club published every four to six weeks. While in college, Guttman became active in the *Pro-Halutz Club*, a Jewish-Zionist student association on the campus of the University of Minnesota—a political engagement he maintained throughout his academic studies. As a young professor, he served as faculty advisor of *Kadimah*, the Zionist group at Cornell, which was where he met Ruth, his future wife. For a review of these various social institutions and the everyday life of the Jewish community in Minneapolis, See: Rhoda Lewin, *Jewish Community of North Minneapolis* (Chicago: Arcadia, 2001).

⁶⁰ The first halutzim were mostly single men and women in their twenties or thirties from Russia and Poland, who, lacking any defined occupation and motivated by Zionist ideology, came to Palestine with the explicit intention of engaging in manual labor and participating in the country's construction. Collectively, the term refers to the Second and Third Aliyot (immigration waves), roughly between 1903-4 and the mid-1920s. In the case of Zionist immigration from North America, the periodization is slightly different and extends into the 1930s.

Usually translated simply as “pioneer,” the Hebrew word *halutz* has a specific historical and cultural meaning in the Zionist context that goes beyond the frontier settlement connotations the English word may invoke. The term first appeared in its Zionist sense in labor Zionist commentary and educational writing in the late 1910s. Socialist leader Berl Katznelson described *halutziyyut* (pioneering) as a “life project,” insisting that it was not an idea or doctrine, but a “personal vision” and embodied praxis. Joseph Trumpeldor, the founder of Hehalutz movement, emphasized self-sacrifice for the common good and in the service of the nation.⁶¹ As historian Boaz Neumann explained, according to these early proponents of the pioneer movement, the *halutz* was defined by the seminal nature of his or her actions. He did in his life what the nation will do in the future. His role was therefore to lay claim and take possession of the land by working it. Gradually, pioneering was elevated to the status of a hallow ideal, and became associated with *hagshama*—a heavily charged term in Zionist theology best translated as “realization”—namely, the implementation of the Zionist mission which was at the same time the ultimate mode of self-actualization. A state of being (or being-in-the-Land-of Israel, as Nuemann puts it) and an orientation, pioneering was to subordinate one’s personal existence to the general will, to national duty; but it was a subordination that promised a spiritual and moral self-transformation.⁶²

However, to paraphrase Beauvoir, one was not born, but rather became, a *halutz*. This was exactly the role of the Zionist youth movement: to turn “wandering” diaspora

⁶¹ “We need to establish a generation...with no interests or habits. Iron bar! Metal that could be molded into all that is needed for the national machine. Missing a wheel? I’m the wheel!...digging needed? I can dig! To shoot? Be a soldier? I am a soldier! ...I do it all. I have no face, no psychology, no feelings, not even a name. I am the pure idea of service...”

⁶² Boaz Neumann, *Land and Desire in Early Zionism* (Waltham, MA: Brandeis University Press, 2011).

Jews into land-loving laborers. The *Young Poale Zion Alliance* (YPZA), the Labor Zionist youth movement the Guttmans belonged to, was no different.⁶³ In the early 1930s, the YPZA underwent a radical transformation: from a socialist association of Yiddish-speaking East European Jewish workers with a theoretical and rather loose commitment to the goals of Zionism, it became—in the hands of the Americanized second generation—a nationalist organization of native-born youth with a clear focus on *aliyah*, or ideological immigration to Palestine. Propounding the ideas of Labor Zionism to Jewish-American high-school students throughout the country, YPZA made pioneering its central objective: “halutziiyut in the Land of Israel was an end in itself in realization of Socialist Zionist ideology.”⁶⁴

The transition of from political Zionism to “halutzic” or personal Zionism manifested primarily in the decision to incorporate training for pioneering or *hachshara* (lit. preparation) into the educational program of the youth movement. *Hachshara* (pl. *hachsharot*) could be described as a human-in-the-loop simulation of colonial settlement in the Middle East according to the principles or ideal of labor Zionist ideology. The term referred to both the process and the site or center where Jewish European or American

⁶³ The American Labor Zionist movement, originally known as *Poale Zion* (the Workers of Zion), was formally established in 1905. In the early decades of the 20th century, the movement was a cross between an association of *folksmenshen* (a self-help fraternity) and a *farbrente partei*—a political party with strong East European overtones. Unlike the Zionist Organization of America (ZOA) and Hadassah, the two largest indigenous Zionist organizations in the United States, American Poale Zion was an offshoot of the radical socialist Zionist party in Russia and Poland. Opposed to the dominant forces of American Jewish life, it argued that a classless society and national sovereignty are the only means of solving the Jewish problem. Guttman’s father was a member of Poale Zion in Minneapolis, and Guttman, like his older brothers, joined the movement’s youth wing as a teenager. On the evolution of the movement in the first half of the 20th century, see: Mark A. Raider, “From Immigrant Party to American Movement: American Labor Zionism in the Pre-State Period,” *American Jewish History* 82, no. 1 (1994): 159-194.

⁶⁴ Jacob Katzman, “Education, Politics and Pioneering: The Debate Over Habonim (New York, 1985),” in *Builders and Dreamers: Habonim Labor Zionist Youth in North America*, ed. J. J. Goldberg and Elliot King (New York: Herzl Press, 1993), 53-56.

youth—the standing reserve of the settlement project in Palestine—underwent training in agricultural work in preparation for immigration and communal living in a *Kvutza* (lit. group, then a synonym for kibbutz). Hachshara in the United States was organized mainly by *HeHalutz* (The Pioneer) organization and took place mostly in adapted farms. Designed as immersive environments, these sites modeled the physical, social, and psychological conditions in the collective colonies in Palestine, which they emulated, and aimed to mold the bodies and minds of the teenagers that went through them: habituate them to rough conditions, provide them with farming knowledge and skills (plowing, tractor driving), and also teach them the ways of cooperative labor.

One such site was the training farm in Anoka, a Midwest agricultural wasteland some twenty miles north of Minneapolis where the Guttman brothers practiced pioneering. “It was a derelict eighty-acre spread, abandoned by an unfortunate farmer when the depression hit,” Nahum Guttman told. “But in the spring of 1933, it started a new life. A group of inexperienced city-bred young men and women took possession of the rundown farmstead...and transformed it into a *hachshara*, a training farm for would-be Zionist pioneers.”⁶⁵ Nahum, who entered the University of Minnesota as an engineering candidate the year before, switched to agriculture under what he had described as the “spiritual impact” the farm venture had on those who took part in it. But he, like most members of the original group that led the settlement on the farm, was a weekend idealist; he spent weekdays in studies and never lived on the farm for more than a few weeks at a time.⁶⁶ But the senior Guttman brother, the quiet, dedicated, selfless Zvi

⁶⁵ Nahum Guttman, “The Anoka Farm, Minnesota, 1933 (New York 1989),” in *Builders and Dreamers*, 47.

⁶⁶ Nahum, who apparently handled ideas better than he handled the hoe, ended up a spokesman for the movement. He served as the First National Secretary of Habonim, was director of public relations for the

(deer or gazelle in Hebrew), was a different story. He interrupted his academic studies in order to set up the *hachshara*, and “single-handedly held the fort until reinforcements began to arrive from all parts of North America,” Nahum recalled with admiration. “For months, Zvi lived on the Anoka farm alone, with weekend help only from Young Poale Zionists who came down from the Twin Cities.”⁶⁷ Soon thereafter, with the same sense of personal responsibility and level of commitment, Zvi left with the first group of YPZA members for Palestine, trading the farm for the real thing: pioneering life in the Jordan



Figure 3: *Banana harvest at Kibbutz Afikim (1937), copyright Central Zionist Archives*
Courtesy Judaica Division, Widener Library at Harvard University

Valley.

On the website of Kibbutz Afikim, where he remained rooted for half a century and where he is buried, Zvi is remembered as a man of few words, who asked little for himself, and stood steadfast in face of privation and doubt.

“Life was hard. There were many disappointments. But for Zvi, Eretz Yisrael [Land of

National Committee for Labor Israel, and editor of *The Jewish Frontier* magazine. His Zionist work consisted of writing political speeches, arranging pro-Israeli public rallies, and assisting leaders of pre-state Palestine and later Israel when they visited the U.S. See: Steve Lipman, “Labor Zionist Spokesman Nahum Guttman Dies at 97,” *The Jewish Week*, 14 September 2010.

⁶⁷ Nahum Guttman, “The Anoka Farm.”

Israel] was the end of the road; he felt he had come home.”⁶⁸ The union of home and nation, and the condition of willing bondage to that sanctified plot of hard land at the other end of the world, was reproduced as true serfdom in the “generation of the sons.” They, who were born to physical labor, were compelled by forces unknown to them, forces stronger than themselves, to endless duty and personal sacrifice. In the case of Zvi, this socio-cultural truism took on tragic dimensions: both his sons, who had spent their entire lives working in the banana plantations of the kibbutz, died of illness in 1996. Nathan was 60; Nahum was 49. The webpage devoted to the memory of latter, reads as follows:

[Nahi] believed that working the land is an ideal—and an ideal must be held. Out of this belief he persisted in the banana plantations for many years. Even when he turned to other pursuits such as youth education...he did so with all his heart... Always preoccupied by the existential questions of the kibbutz, he was an active member...until the last day of his life.⁶⁹

Unlike his older brother, Louis had found little that was alluring or uplifting about communing with nature, being in the field, sitting with comrades in the open air or engaging in construction, farming, physical training, or the repair of mechanical tools—the prototypical activities of the halutz. Though he was quite involved in the youth movement locally—he became even more so when in 1935 the leadership of YPZ in Minneapolis, his brother Nahum included, moved to New York to assume the national leadership of the organization—the pioneering ideal, which now had a dangerously familiar face, was one that Guttman did not easily identify with. Or practice. A head-on

⁶⁸ “Memorial page for Zvi Guttman (1912-1983),” Kibbutz Afikim website (<http://www.afikim.org.il/>). [Hebrew].

⁶⁹ “Memorial page for Nahum Guttman (1947-1996),” Kibbutz Afikim website (<http://www.afikim.org.il/>). [Hebrew].

collision with the normative expectations and cultural requirements of this already established model took place one summer, when he was asked to be on staff at *Camp Kvutza* in Accord, New York. As he later recalled:

It was the first time I ever went to summer camp. I was faced with a crisis right away. When it came to constructing the site, I had no idea how to go about it. So I just gave the order, ‘put up the tents,’ and the kids did it very well. [Unlike me,] they were all very experienced...[Similarly,] every time Joey [fellow madrich] would point something out, one particular camper would correct him. Both Joey and I learned a great deal about flora and fauna that summer. ...I found myself teaching everything from the geography of Palestine to singing. I can’t sing.⁷⁰

Guttman’s shortcomings in *tzofnut* (scouting) and *yedi’at ha’aretz* (Knowledge of the Land), which were a crucial part of the movement’s educational program—and his general lack of interest in such activities—were not the only expression of his reluctance about the labor Zionist model of pioneering. The more substantial form of resistance, which grew robust during his university years, was the refusal to divorce his Zionism from his scientism. As historian Oz Almog explains, the focus on farming and the idealization of manual labor went hand in hand with anti-intellectualism and “ritualistic affirmation of the practical world.”⁷¹ The denigration of learning for learning’s sake as unproductive, impractical, and even selfish in Labor Zionist discourse of the 1930s and 1940s had its roots in early Zionist writing and scholarship on Jewish degeneration. Thinkers such as Mica Joseph Berdichevsky and Martin Buber seeking to “cure” the Jewish people of its Diaspora malady and return to “the primal instincts of a natural nation,” denounced traditional Jewish studiousness as “excess spirituality” that “crushed

⁷⁰ Louis Guttman, “An Accidental Career in Academia (Jerusalem 1985).” In *Builders and Dreamers*, 322.

⁷¹ Oz Almog, 139.

the stuff of life.”⁷² An expression of vitalism, this anti-intellectual ethos was also a response to the immediate practical and political needs of the young settlements in Palestine. The leadership believed that in order for the movement to have enough laboring hands, higher education must be devalued; that education could stand in the way of building the country and should therefore be discouraged. This attitude became a defining trait of kibbutz education, and found expression in the emphasis schools placed on vocational training and achievement in practical fields, but also in an official stance of disapproval toward academic pursuits.⁷³

Indeed, during his doctoral studies, while he ran the local branch of *Habonim*, Guttman had a direct encounter with this stance:

At one time, I remember, a visiting *shaliach* [emissary or delegate from Palestine] came and spoke to us about Palestine’s urgent need for farmers. He opposed higher education, arguing that agricultural training was the most important type of education. And there I was, an academician, running the movement.⁷⁴

The spirit of defiance, nicely captured in the structure of this last statement—notice how Guttman’s identity serves as the locus for the unity of opposites—would mature into a refusal to lead an unexamined life without understanding or choice and gradually also into an insistence that there’s a place for intellectual pioneering and that he could realize his Zionist beliefs as a scientist. That was Guttman's challenge—to 'show them' (and himself) that his learnedness was 'practical'. That what he had to offer wasn't Talmudic sophistry, but real life, essential guidance.

⁷² Shmuel Almog, “Pioneering as an Alternative Culture,” *Zion* (1993): 329-346. (Hebrew).

⁷³ Oz Almog.

⁷⁴ Habonim (The Builders) in North America was a later iteration of YPZA. Louis Guttman, “An Accidental Career in Academia,” 322.

A War of One's Own

But Palestine was not only a Zionist destination for Guttman; it was also a scientific one. During the years at the ARB, Guttman was fortunate to have been presented with an unprecedented wealth of high-quality empirical data. In the nascent state being established in Palestine, he imagined, he could again be presented with that wealth of data—material that he can sink his methodological teeth into. But Palestine harbored an even larger promise: at the ARB involvement was limited to analysis and methodological development. In Palestine he would run his own lab, controlling all the means of production. That would allow him to conduct scientifically driven research—choosing objects of study to befit his methods, for example—and not be a mere ‘engineer’ solving problems for others.

A close reading of the application, which secured the funding for a yearlong research sojourn in Palestine and allowed Louis and Ruth to leave the safety of their life in Ithaca and go into the wild reveals how Guttman thought of using the unique historical circumstances of postwar Palestine to continue what he had started.

In explaining why he should spend a year away on their dime, and in such an unseemly part of the world, Guttman began his application for the SSRC with a strange argument. “The research experience of the applicant to date,” he stated, “has been primarily concerned with the design and analysis of questionnaire surveys, with emphasis on the statistical problems involved.” Yet said applicant, who invented ‘scale and intensity analysis,’ a groundbreaking *analytical tool* that “affords a solution to many of the questionnaire problems”—another part of the application emphasized, lest we

forget—“had comparatively little opportunity to do intensive fieldwork in the study of attitudes.”⁷⁵ On the face of it, the two parts of the statement seemed unrelated, if not contradictory: what did fieldwork have to do with survey research methodology? And why would an ivory hunter, who had devoted his training and early career—and successfully so—to devising very careful and precise measurement procedures, face the prospect of grim frontier conditions instead of leaving the arduous, dirty, and dangerous task of empirical evidence collection to survey interviewers?⁷⁶

“Fieldwork” had specific connotations in the context of American “scientific” sociology. In Minnesota sociology of the interwar period, for instance, “fieldwork” was largely associated with the kind of mindless and under-theorized collection of observations social workers engaged in. Along with “subjective reporting,” it was considered a practice that could lead uninformed investigators to premature and unlawful generalizations, ones whose universal validity was not overseen and corrected for by the “statistical method of interpretation,” as Chapin often warned his students.⁷⁷ Objectivists had to know how to process and interpret social data, but “going into the field” did not

⁷⁵ Louis Guttman, “Application for Fellowship 1946-47,” May 5, 1947. Folder/Box/Series, SSRC, RAC. On the history of thinking of—and using—theoretical mathematics—proofs, theorems, diagrams—as analytical tools and techniques in normal-scientific practice, see: Andrew Warwick, *Masters of Theory: Cambridge and the Rise of Mathematical Physics* (Chicago: University of Chicago Press, 2003).

⁷⁶ According to Kuklick, fieldwork suffered from a low epistemic status in all field sciences until the latter part of the nineteenth century. On the history of the division of labor between evidence collectors and interpreters and how it changed with the appearance of professional social scientists, see: Henrietta Kuklick, “Personal Equations, Reflection on the History of Fieldwork, with Special Reference to Sociocultural Anthropology,” *Isis* 102, no. 1 (2011): 1-33.

⁷⁷ By later standards, Minnesota sociology was considered still eclectic and “proto-scientific” in the 1920s (before Chapin’s autocratic rule reached its Calvinist apex), largely due to the practical bent and socially oriented cast the unusual fusion with the training school for social workers gave teaching and research in the department. Courses on various aspects of fieldwork is one example that Bannister provides for the blurring of lines between the “scientific” and the “professional” (or vocational) during those years. See: Bannister, 152.

constitute part of the their professional socialization. The postwar SSRC, however, which was not less committed to a natural science model of social science, placed a high premium on fieldwork or “field training,” as the type of fellowship Guttman had applied to was called.⁷⁸

First awarded in 1935, the SSRC field training fellowship intended to encourage and enable promising individuals to separate themselves from their universities for a year of fieldwork in order to gain “first-hand familiarity with the data of social science in the making.”⁷⁹ Originally open only to advanced graduate students at the predoctoral level, the fellowship was designed essentially to fill a gap in the research experience university graduate programs offered. In the aftermath of the war, in response to the steep rise in the public demand for social science expertise, the SSRC decided to revise its fellowship policies and invest funds also in postdoctoral training.⁸⁰ The eligibility criteria of the field training fellowship changed accordingly to include also early career scholars in the first years after receiving the doctorate, and the emphasis shifted from “supplementing lecture

⁷⁸ Interestingly, the language of the application—primarily, the various indications that fieldwork was now positively associated with furthering social science—is testimony of an undergoing change in epistemological values.

⁷⁹ Elbridge Sibley, *The Recruitment, Selection, and Training of Social Scientists*, Bulletin 58 (New York: Social Science Research Council, 1948), 133. Sibley’s study, which was conceived originally as a basis for reconsideration of SSRC fellowship policies in the postwar period, determined that there was indeed need for more liberal support of the social sciences. The main reason was the expectation that the academic scene will see “great numbers of young men and women, awakened by their wartime experiences to the fact that most social problems cannot be well solved by explosives” and who would demand formal training in the social sciences.

⁸⁰ The idea then was that by administering fellowships on a nationwide scale, nonacademic organizations could influence the kind of training social scientists underwent and ensure the availability of well-trained research workers.

room and library” to the development of scientific methodology and tools, for which the field was now considered essential.⁸¹

In Council publications and policy statements from that period, fieldwork was cast as the kind of hands-on bootstrapping training in research methods and techniques every social scientist should undergo—and private benefactors should therefore underwrite.⁸² “The central purpose [of all fellowships] has always been to afford more advanced research training than that which is provided in the usual Ph.D. program,” it was stated in the introduction to the 1951 directory of Council fellows. It was therefore “the training of the individual fellow rather than the production of significant research findings [that] has been the immediate objective” in decisions on appointments.⁸³ Fieldwork therefore did not connote in this context learning about the diversity of human cultures and social life that we have learned to associate with the ethnographic study or the community survey, but something akin to wet lab in biology or chemistry.⁸⁴

For Guttman, who had gained experience in practicing survey research under time pressure and real world constraints during his five-year service at the ARB, such training

⁸¹ The crucial criterion of eligibility for a research training fellowship (the new name of the field training fellowship) was “an interest in the discovery and verification of verifiable and predictable uniformities of social structure and behavior.” *Items*, June 1949, 18.

⁸² Interestingly, the training in research practice Sibley recommended for social scientists included also training for objectivity (or subjectivity re(s)training): teaching future researchers to recognize and control “the influence upon observation and upon inference of the mental processes of the observer himself.” See: Sibley, p. 97. On the history of the SSRC, its conception of social science, and its impact on the social sciences in North America, see: Donald Fisher, *Fundamental Development of the Social Sciences: Rockefeller Philanthropy and the United States Social Science Research Council* (Ann Arbor: University of Michigan Press, 1993).

⁸³ Social Science Research Council, *Fellows of the Social Science Research Council 1925-1951* (New York: Social Science Research Council, 1951), v-vi.

⁸⁴ You may recall that the 1940-41 predoctoral fellowship that brought Guttman to Chicago was also framed as a “field program” whose main objective was the “acquisition of firsthand knowledge” of the applicability of existing methods in practice and of techniques for developing new methods.

period meant, it seems, recreating a setting appropriate for tool testing and methodology development on the same scale. The field, namely a circumscribed setting in which observations can be made and data extracted under relatively controlled conditions, was required primarily for further testing and development of the powerful analytical tool that he had invented during World War II, and which had been empirically tested, i.e. modified the design of military questionnaire surveys (and consequently the way the data generated was processed) only toward the end of the war and only in a limited manner.⁸⁵

In addition, Guttman, ever the rigorous Objectivist, saw this as an essential stress test of the tool. The development of sophisticated instruments of measurement and analytic machines paved the road to altering our understanding of the architecture and dynamism of human subjectivity; but for that that to happen, this instrument has to be subjected to further testing. While the war presented a certain set of challenges (time pressure, for example), it also presented the social scientists with a homogenous study population: young American men circumscribed to army service, who not only lead an exceedingly controlled (and limited) lifestyle, but were also captive subjects looking for a respite from military boredom or fighting. In a real world setting, the social scientist would encounter diverse cultural and ethnic groups who lead diverse lifestyles, including people who never encountered this form of investigation before.

Palestine was the perfect site for such an endeavor. First of all, Guttman explained, “Palestine affords cultural contrasts in a relatively small geographical area,” thus allowing for an efficient testing of American techniques of attitude research in a short

⁸⁵ Stouffer on the limited use of Guttman scaling in actuality. Samuel A. Stouffer, *Measurement and Prediction*, Introduction.

period of time. Culturally-disparate groups of Jews—Yemenites and Sephardim, immigrant Europeans and “Sabras”—who all live in great proximity and share a common language (with each other and with the applicant) offered ample opportunity for cross-cultural comparison between different methods of data gathering, such as open-ended interviews and questionnaires. The country was also uniquely positioned, Guttman went on, for “learning how to study an illiterate population like the Arabs.” How would different ethnic groups respond to common methods of data gathering? And what skills may therefore be needed for cross-cultural interviewing? How should opinion polls “as practiced in our culture” be modified in order to generate reliable knowledge about a different culture? Second, and more significantly, due to the special historical circumstances that coalesced with the end of WWII—the diplomatic success of the Zionist cause and attendant preparations for statehood, the approaching end of British rule, and the influx of Jewish refugees—“rapid social change is taking place there that may provide good training materials for developing methodology for studying the dynamics of attitudes.” It was therefore the unique features of the locale rather than its similarity or difference from other places that made it the perfect laboratory for the study of social-psychological processes: the key was not the applicability of these tools and ideas outside the cultural context of their development (although this was certainly a major thrust), but a more ambitious test on a civilizational scale that would in fact establish the absolute modernity of these ways of ordering and understanding the world.⁸⁶

Guttman had anticipated that fieldwork would begin soon after his arrival in summer 1947 and extend over a full year, possibly two. Hoping to take full advantage of

⁸⁶ This rationale was taken to its logical end in later funding requests, the most ambitious and bold among them was the 1952 Ford Foundation proposal.

the opportunity, he came up with an intensive schedule, planning to take measurements throughout the country—in both the urban centers of Palestine and in a sample of rural communities—and also travel across the border to Lebanon, where, with the help of Stuart C. Dodd and his sociology students at the American University of Beirut, he would conduct surveys also among Arabic speakers.⁸⁷ Executing such a plan obviously required more resources than Guttman’s funding could buy, and likely also more than was available in the scientifically peripheral Near East. Following the model of large-scale scientific survey research projects in the U.S.—an emergent phenomena in the social sciences—Guttman intended to solve this problem by setting his base of operations at a local university and making use of its in-house services. The application indicates that contact has been established with the region’s two research universities and they had reportedly offered their assistance. Finally, employing the era’s rhetoric of internationalism, Guttman concluded his statement with a vision of regional collaboration, conveying his enthusiasm about the prospect of training the first generation of indigenous pollsters and bringing systematic research according to the highest standards to this part of the world.

⁸⁷ Stuart C. Dodd was an American mathematical sociologist, pioneer of scientific polling techniques, and author of several college textbooks who during the 1930s and early 1940s conducted the first public opinion field studies in the region as Professor of Sociology at the American University of Beirut. At the end of World War II, Dodd returned to the U.S. and became a vigorous exponent of the expansion of social scientific mechanisms and tools for international governance.

Chapter Two

WHAT THE (JEWISH) SOLDIER THINKS

On a hot day in July 1947, after a two-week sea journey from New York aboard the U.S.S. Marine Carp—a World War II troop carrier that now brought Americans to the Levant—Ruth and Louis Guttman, who had since got married, disembarked at the Port of Haifa.¹



Figure 4: *Louis and Ruth Guttman aboard the Marine Carp*

A black & white photograph taken during the cruise depicts them sailing across the Mediterranean on the waves of victory, so to speak, to an outpost of the newly minted empire.

¹ When the Second World War ended, the Marine Carp first steamed to the Philippines to embark Pacific war veterans for return to the U.S. It then sailed on a troop lift run from the Mediterranean late in January 1946, and again in spring 1946. Between 1947 and 1952 it crossed the Atlantic regularly between New York and the Middle East through the Strait of Gibraltar, and would have layovers in Beirut and Haifa. See the Naval History and Heritage Command website: <http://www.history.navy.mil>.

Young and full of promise, the Guttmans look almost glamorous: he is tall and burly, his strong features and high forehead are well framed by dark slicked back hair; she is very beautiful, smiling confidently like a woman at the beginning of a new life.

They were not the only American experts going overseas in the aftermath of the war. Joining the tens of thousands of refugees, Allied occupation forces, UN diplomats, and businessmen, were travellers of a new kind: economic developers and “technical assistants,” psychiatrists and anthropologists/scientists and doctors—the true architects of the new world order—who traversed this route in the postwar period, bringing the gospel of American modernity to former colonial possessions in the Middle East, among them the new Jewish State that would soon declare its independence.² Tova Eizik, a fellow passenger, provided a vivid description of the array of characters on the boat:

The passengers were colorful: they included a throng of oil men, both American and Arab, headed for Iraq and Iran via Beirut; other Arabs returning home, some of whom were dignitaries (I think there was even a princess among them!); Zionists returning to settlements they'd left in wartime; and first-timers like me. I slept in a section of the ship with the other Jewish kids. They spent their time talking politics and studying Hebrew, singing and dancing... Up on the deck, I came to befriend some of the Arabs on the ship. One man, some kind of big deal in Lebanon, even tried to convince me to disembark with him in Beirut. He said the charms of his cosmopolitan French-influenced city made it an obviously superior alternative to my Palestine: an uncivilized expanse of rocky land.³

² On the role of technical experts in postwar American modernization, see: David Ekbladh, *The Great American Mission: Modernization and the Construction of an American World Order* (Princeton University Press, 2010), especially chapter 5; Mark Mazower, *Governing the World* (Penguin Books, 2012). An exemplary story is that of two medical delegations, one from Palestine and one from the United States, that were sent in 1947 to detainment camps in Cyprus in order to assess the mental health of Jewish immigrants and refugees who were on their way to Palestine. Davidovich and Zalashik show in their fascinating account of this case that the members of these delegations saw the detainee camps primarily as a potential "living laboratory" for scientific exploration. Rakefet Zalashik and Nadav Davidovich, "Measuring Adaptability: Psychological Examinations of Jewish Detainees in Cyprus Internment Camps," *Science in Context* 19, No. 3 (2006), 419-441.

³ Eizik sailed on the second voyage of the Marine Carp, in autumn 1946. Tova Eizik with Andrea Muraskin, *Beginnings, 1948: The Beginning of an Era; The Beginning of a State; The Beginning of a Family* (Bloomington, IN: AuthorHouse, 2012), 2-3.

As presaged by the Lebanese traveller, the Guttmans' initial excitement and sense of adventure were immediately mixed with the harsh reality of the Palestine, as was nicely captured in a rare set of images taken during their first few months in Palestine using a camera the Guttmans brought with them. In one they are sitting by the side of a mountain road, in view of the biblically majestic Judean Hills; in another, standing before the Ottoman-era Montefiore Windmill in Jerusalem. In yet another, we see the lumbering Louis standing proud and smiling, clearly overdressed for the weather, squinting his eyes in the harsh light. He is smiling next to a makeshift basketball court-come-soccer field, a boy in shorts loitering in the background. We see shirtless men from Kibbutz Afikim fishing in the Jordan River and celebrating masses in Jerusalem the morning after the UN decision on partition. But we also see horse carts next to cars in unpaved streets; grazing cows among stone buildings in Western Jerusalem; and Arab men wearing Keffiyeh in Jewish cities—a fact the Zionist authorities were interested in hiding.

While they thought they came prepared for their scientific and Zionist missions—securing funds and visas and studiously packing equipment and consumer goods—they soon found the land (and its folk) to be stubbornly resistant to their transformative agenda. As their son later told, his parents brought along with them when they arrived, in addition to expertise in survey research and the world's most advanced "public mind-reading" technology, as the Palestine Post described it, also the aforementioned camera and a car:

They had a [brand new] 1947 Buick [Super]...in Palestine of those days there were no such things. And as soon as they settled in Jerusalem, people from the Haganah knocked on their door and said: "The District Commander of the Haganah in Jerusalem needs a car. This is his car now." So my parents said, "Okay," and that's it, this was the last time they saw their car.⁴

⁴ Israel Channel 2 Broadcast, "Rare color 16mm home movies of Israel in 1947-1951, filmed by Louis Guttman," Adi Gamon's YouTube channel, <https://youtu.be/YzUMSuvcjFw>, 2008 (Hebrew)

Guard Duty

Meanwhile, the war had begun. On November 29, 1947, the UN officially adopted the conclusions of the United Nations Special Committee on Palestine (UNSCOP) and recommended the partition of Palestine and the establishment of a sovereign Jewish state upon British evacuation in mid-May. Celebrations took place in Jewish settlements and cities all that night, but the next day, spontaneous inter-community violence erupted. In Jerusalem, a mixed city where the lives of the two communities were completely entangled, the violence was particularly intense. A countrywide nightly curfew, which the British authorities had imposed soon thereafter to quell the unrest, made travel to and from the city difficult, as did the ambushes that Palestinian-Arab irregulars had set along the main road. As these became more frequent and more sophisticated, the number of trucks supplying Jewish Jerusalem steadily fell, threatening to leave the city's residents without food and medical supplies. This resulted in distinct signs of mayhem, incidents of fighting in the streets, reports of fresh water supply problems and dysfunctional city services, and disruption of regular business and travel. In response, the Jewish Agency set up the Jerusalem Emergency Committee, which had begun stockpiling food and fuel. By March the siege of Jerusalem had begun in earnest.

The tightening noose around Jerusalem wrecked Guttman's elaborate research plan, and brought the global circulation of the American attitude survey to a screeching halt. Defined geographically, the methodological investigation he had envisioned required travel throughout Mandate Palestine and among its various ethnic groups, which was now practically impossible. With national sentiments growing fervent and inflaming the entire region, Guttman was also prevented from collaborating with Stuart Dodd and his sociology students at the American University in Beirut as planned; a scientific exchange that guaranteed that the gospel of

behavioral measurement extended beyond national boundaries. Stranded in Jewish Jerusalem, Guttman had to thoroughly revise his research agenda: abandon the plan for countrywide and even regional studies, and limit fieldwork to the city's Jewish neighborhoods. His strange proposal to substitute the entirety of Palestine for one small community—the members of the Jerusalem Jewish bus cooperative Hamekasher, among whom he and Ruth lived—included in a request for a second year of funding he had submitted to the SSRC that same month, along with statements that attempt to justify such a redefined and dramatically downsized scope for the project, was clear evidence that Guttman found himself in a pickle.⁵

And yet, a new potential patron came into view. The Haganah, the largest and most established Zionist paramilitary organization, was expanding its ranks, acquiring weapons, and preparing for the decisive battle over Palestine's future. Guttman sought out local leaders to try and lobby the case for the social survey. "I was hoping to be able to put at the disposal of the Haganah the social science know-how accumulated during World War II," he explained.⁶

It was in line with the ideological dictums of instrumentality to proclaim science's subordination to the practical demands and political needs of the day, but Guttman's position was in fact more complicated. In return for putting at the disposal of the nascent national army the most advanced tools for monitoring and regulating the mental state of its armed forces, Guttman expected to get easy access to research subjects and the opportunity to prove the validity of his experimental methods and procedures. This exchange of expertise for support (as well as new territory and scale to take charge of), historian Ellen Herman argues, stood at the

⁵ Louis Guttman, "Application for Fellowship 1947-48—Renewal," December 12, 1947. Folder 5693, Box 466, Series 1, Subseries 75, Accession 2, SSRC, RAC.

⁶ Haya Gratch, ed., *Twenty-Five Years of Social Research in Israel, A review of the Work of the Israel Institute of Applied Social Research, 1947-1971* (Jerusalem: Jerusalem Academic Press, 1973), 12.

heart of the contractual relationship between American behavioral science and the army during the WWII. Unlike the university, too often weary of spending its limited funds on untested ideas, the military environment was particularly conducive at the time for cutting edge research.⁷

In addition, there were circumstances unique to the locale. In the specific context of late 1940s Palestine, operating under the aegis of the popular army was possibly the only viable avenue for a study of the kind Guttman had in mind. Not only was survey research costly and labor intensive, it involved going around and asking people personal questions about their opinions and feelings. This scientific practice being foreign to the culture, it required the patronage of a recognized authority. The Haganah's hegemony—its consensual status as popular symbol of national unity, its reach beyond ideological divides and local cultures or communities, and its widespread influence on and deep involvement in the everyday life of Palestine's settler society—made it perhaps the only institution at the time that could provide both the means of scientific knowledge production and the legitimacy to use them.

The Haganah however, did not see the situation in the same way. In a short autobiographical essay written many years later, Guttman recalled his first miserable encounter with the organization: “I approached the Haganah in Jerusalem with the idea of studying its morale problems in the city. Nobody understood what I was talking about.” It was “difficult to explain to them what social research meant and how it could aid them,” he recounted.⁸

Highlighting the absurdity of the situation and poking fun at Guttman's naiveté, Elihu Katz, Guttman's friend and longtime collaborator, joked that “only an American Zionist could hope

⁷ Ellen Herman, *The Romance of American Psychology: Political Culture in the Age of Experts* (University of California Press, 1996).

⁸ Louis, Guttman, “An Accidental Career in Academia (Jerusalem 1985),” in J. J. Goldberg and Elliot King, eds. *Builders and Dreamers: Habonim Labor Zionist Youth in North America* (New York: Herzl Press, 1993), 323.

that an untested underground army, surrounded by enemies on the verge of attack, would immediately recognize the relevance of attitude research.”⁹

Guttman had come to conduct comparative studies and collect data in an exotic faraway land and found himself stranded in a besieged city, his plans derailed. Yet, exhibiting uncharacteristic flexibility, agility, and ingenuity, Guttman decided to embrace his fate and joined the city’s chaotic emergency management and public organizing efforts. This ‘going native’ proved momentous: while the self-reliant and battle-hardened veteran fighters of the Haganah had little regard for the authority of science (or for any form of expert knowledge for that matter), they held camaraderie in high regard and trusted the intimacy, personal sacrifice, and sense of a shared fate that brotherhood-in-arms inspired.¹⁰ As Ruth Guttman recalled, it was rubbing shoulders with other Haganah members that finally did the trick:

How was the connection with the Haganah made? Very simple, they asked him to serve on guard duty. On his first shift they taught him how to use a gun (he never saw one before...); it was then that he made friends with people in the organization. I don’t remember who had given the green light. [But] during the siege I worked with Asher Hirshberg. We published a newspaper... Hirshberg was the one who convinced them it was important to start already then to study what people were thinking...¹¹

Shortly after, Guttman managed to establish a small research operation, and began to design and carry out surveys on the opinions, preferences, and needs of Jewish Jerusalem.

⁹ Elihu Katz, "In Memoriam: Louis Guttman, 1916-1987," *The Public Opinion Quarterly* 52, No. 2 (1988), 240.

¹⁰ On the military culture and moral economy of the Haganah, see: Uri Ben-Eliezer, *The Making of Israeli Militarism* (Bloomington: Indiana University Press, 1998), especially Chapter 5. I discuss the influence of this culture/economy on the epistemic values and style of reasoning held in esteem by army leaders in the last section of this chapter.

¹¹ Ruth Guttman, video interview. The Guttman Family Archive.

Morale under Siege

Asher Hirshberg, who, according to Ruth, had successfully pleaded the case for public opinion research before the local Haganah authorities on Guttman's behalf and brokered what would develop into a longstanding patronage relationship between the military and behavioral science in Israel, is remembered today primarily as the "voice of austerity."¹² These skills, with which he would later pontificate saving and frugality over the national airwaves, were first honed during the war when he worked as an occasional propagandist for *Mishmar Ha'am*, or, People's Guard, a civilian organization of Jewish city residents that assisted the Haganah in Jerusalem. Established in September 1947, it consisted of volunteers who were over the age of enlistment and those who did not enlist for various other reasons. Engaged in various auxiliary and reserve jobs required by the war, and gradually in charge of duties which usually fall to municipal or local government bodies, *Mishmar Ha'am* was also responsible for preparing the home-front, physically and mentally, for the coming siege. That included trench digging, the cleaning of water cisterns, conducting a census, and so on (and later also managing food rations, distributing bread and water, and building fortifications), but also the maintenance of civilian morale.

Modeled to a large degree after the mandatory government's Public Information Office— itself refigured into a full-fledge propaganda agency a few years before along the vision of the newly founded British Ministry of Information—*Mishmar Ha'am's* "*hasbara*" or Information and Education Division, had targeted, since the early days of the crisis, the mindset of city

¹² In the early 1950s, Hirshberg served as spokesperson for Dov Yosef (Joseph), once the military governor of besieged Jerusalem and after the war the Minister of Supply and Rationing in the newly established Israeli government. In this role, Hirshberg would regularly broadcast messages on the national radio regarding changes in the austerity regime. Along with his wife, the journalist and anchorwoman Thiya Bat-Oren, Hirshberg educated citizens on saving and frugality. On the state-imposed austerity regime in early 1950s Israel and its justification in ideological terms, see Orit Rozin, *The Rise of the Individual in 1950s Israel: A Challenge to Collectivism*, trans. Haim Watzman (Waltham, Mass.: Brandeis University Press, 2011), especially chapter 1.

residents. Its role, in the words of its director, was “to cheer [people] up, provide reliable information, fight melancholy and defeatism, but also cheap optimism that takes failures too lightly.” The Division took various measures to achieve these goals and bolster the morale of the besieged. Furnished with a host of curated facts and figures, news analysis, policy statements, and an arsenal of well-formulated arguments, a cadre of specially appointed hasbara officers went out there every week to channel the official (Zionist, national) interpretation of reality and explain the political, military, and economic “situation” to Jerusalem’s residents. This orchestrated explaining and justifying was part of a decentralized network of grassroots emergency management and community organizing activities that Guttman’s embryonic scientific operation became part of and also took as its object.¹³ Guttman’s team, which by the end of 1947, consisted only of himself, a quantitative social scientist named Uriel Foa who had just completed his PhD at the HU,¹⁴ and a group of volunteer fieldworkers, began carrying out surveys for the hasbara that were designed to examine the public’s morale problems.

The first study Guttman managed to organize was a survey of listening habits and preferences concerning the Haganah clandestine radio in Jerusalem.¹⁵ In the heyday of anti-colonial struggle in Palestine, roughly from the end of WWII to the establishment of the State of

¹³ The form of topical meetings and thematic lectures for the general public held at the Guard’s various offices across town, the staging of “public trials” and “*shiot*” (discussions), and the production and distribution of daily news bulletins. Information flew also in the other direction: Hasbara Officers regularly delivered residents’ complaints, requests, and suggestions for improvement to the relevant authorities, digested into paperwork. Plans were drawn up, memoranda submitted. In addition to the discourse of planning, the Division initiated cultural and educational projects such as the Niv Publishing, and later on, functioned also as a municipal information center for soldiers and their families.

¹⁴ The two would collaborate for the next twenty years, I discuss their relationship in chapter 3.

¹⁵ The following is adapted from these accounts: “Summary of Activities in the Committee by the Information Office to this Date,” 1.29.1948, IDFA; “Report on the Work of the Department, 1-2.48 to 5.20.48,” IDFA (613-4944); “The work of the Research Department by LAHAS Jerusalem, February-June 1948,” 6.11.1948, IDFA; Gratch, *Twenty-Five Years of Social Research in Israel*, 14-15.

Israel, the three paramilitary organizations that constituted the Jewish Resistance Movement—The Haganah, and the two “dissident” organizations, the IZL (acronym for National Military Organization, also known in short as *Irgun*) and the *Lehi* (acronym for Fighters for the Freedom of Israel, also known as the Stern Gang)—operated each an illegal shortwave radio channel.¹⁶ The clandestine radio broadcasts were designed to circumvent the Palestine Broadcasting Service (PBS)—the BBC-supervised state radio of mandatory Palestine—whose programming was subject to censorship and which could not therefore be utilized to broadcast nationalist and oppositional content.¹⁷ Radio was the most powerful medium of the day, and along with the press and formal educational institutions, served as an important nationalizing tool in the hands of Zionist ideologues and social engineers. Beyond the fashioning and fostering of a native Hebrew culture and identity, the Zionist militias made strategic use of the medium’s unique capacity for action at a distance to also reach a wide audience while operating underground in conditions of foreign occupation and tight political control. Trying to sway listeners to their particular points of view (fierce disagreements over the way of struggle often became airborne), stirring up anti-British and at times anti-Arab sentiments among the settlers, and providing an adamantly vernacular and grassroots alternative to imperial ideals and agendas, these covert radio operations were out to win over hearts and minds—among Jews and non-Jews alike, in Palestine and abroad—for the cause of freedom and self-determination.¹⁸

¹⁶ On the history of the Haganah radio, Kol Yisrael (Voice of Israel), see Ben-Zion Dinur, ed. *History of the Haganah*, 3 volumes (Tel Aviv: Ma'arachot, 1954-1972), 1270-71. (Hebrew).

¹⁷ The trilingual (English, Arabic, and Hebrew) PBS, operating as the *Voice of Jerusalem*, began its broadcasts under the British Mandate Authority in 1936. On the history and ideology of the PBS, see: Andrea L. Stanton, *This Is Jerusalem Calling: State Radio in Mandate Palestine* (Austin: University of Texas Press, 2013).

¹⁸ According to Boyd, Jewish clandestine radio operations were among the first successful efforts by revolutionary groups to use the medium to help achieve specific goals, in this case, the departure of an unwelcome occupying force, and eventually the creation of an independent Jewish state. Douglas A. Boyd,

Though it was designated to function primarily as a local war emergency radio service, *Kol HaMagen Ha'Ivri* (Voice of the Jewish Defender), as the Haganah Jerusalem station had been named to distinguish it from the organization's national transmission, had evolved during the months of siege into a prominent weapon in the propaganda war that both sides, Jewish and Arab, were waging over the besieged mind.¹⁹ Much of its airtime, like any army radio, was devoted to the pedestrian sounds of military life: from anecdotes and stories of combat experience, to "live from the battlefield" transmissions, to personal greetings from family and friends to Haganah members in far off bases. But *Kol HaMagen* aspired to be more than an entertainment and information service for the defense forces in the region. Hidden on the sixth floor of an apartment building in the New City, operated by renegade *Voice of Jerusalem* broadcasting professionals, and subject to the British Police's incessant interception attempts, it perceived its mission in heroic terms: to shield the public from despair and anxiety, fight off vicious rumors and official lies, and give voice to defiance, tenacity and perseverance in face of attacks from within and without.

And indeed, during the harsh siege months, when supply of independent newspapers from Tel Aviv had been cut off and official (mandatory) media outlets were no longer trusted, *Kol HaMagen* fulfilled an important role in local hasbara efforts and was considered essential in

"Hebrew-Language Clandestine Radio Broadcasting During the British Palestine Mandate," *Journal of Radio Studies*, 6, No. 1 (1999): 101-115; John D.H. Downing, ed. *Encyclopedia of Social Movement Media* (New York: Sage, 2010), 569-70.

¹⁹ *Kol Yisrael* (Voice of Israel), the Haganah radio channel, was the most extensive and well organized of the Jewish clandestine radio services, having started transmissions first from Tel Aviv in 1940 to protest a British ban on land sale to Jews and press censorship. *Kol HaMagen* began broadcasting on December 2nd, 1947. It operated underground until independence. In July 1948, it became part of the "liberated" *Voice of Jerusalem*. It officially ended its activity in May 1949.

keeping up the “spirit” of city residents.²⁰ Broadcasting daily news updates and the Jerusalem Emergency Committee’s notices (regarding distribution and recommended use of water rations, instructions for food preparation under siege conditions, etc.), it became the city’s information lifeline and also a vital means through which the political and military leadership could communicate with the scattered Jewish neighborhoods and communities.²¹ As it grew in ambition, morale building became the focal point.²² There were curated cultural programs—literary interludes “appropriate to the situation,” Saturday night “readings on current events,” radio plays dramatizing the “life of defense” for youth, and so on—intended to take people’s mind off the harsh reality of hunger, cold, and fear; as well as content selected for its presumed psychological effect, like the broadcasting of “instances of bravery and good conduct.”²³

In a speech delivered at a party celebrating eighteen months of *Kol HaMagen* activity, Avraham Arest, who served as the station’s director throughout this period, relied on dramatic turns of phrase and emphatic tone to highlight the radio’s unique contribution to the survival of Jerusalem during the war:

In those days, the *Voice of Jerusalem* was shackled by the White Paper Government. In contrast to Stubbs’ official announcements and Alex Josey’s news - we gave the Jewish public the truth about the war. We did not just broadcast news and military summaries, but regularly provided opinion and analysis... And when our skies darkened, and the man in the street would wonder and ask: Watchman, what of the night; watchman, what of the day? *Kol HaMagen Ha’Ivri* would come and reply, and explain and illuminate, infusing the population with the steadfastness of the Jewish defender...heartens [their] courage, and lifts the morale of...the besieged city of Zion...as it

²⁰ Sefer Ha’Haganah: 1399; Douglas A. Boyd, "Hebrew-Language Clandestine Radio Broadcasting During the British Palestine Mandate," *Journal of Radio Studies* 6, no. 1 (1999): 106.

²¹ “Re: Report from Hasbara Action in December 1947,” January 12, 1948, IDFA (613-4944).

²² “Standing Orders for the Hasbara Officer,” March 5, 1948, IDFA (584-4944-49).

²³ Dov Joseph, *The Faithful City: The Siege of Jerusalem, 1948* (New York: Simon and Schuster, 1960), 93-94.

defends itself, and conquers, and marches toward its complete and full liberation.²⁴

Its world-historical mission notwithstanding, Guttman believed that the nature of the influence *Kol HaMagen* had on its audience was an empirical question that could and should be tackled scientifically, rather than left at the speculative mercy of its architects and their delusions of grandeur. “The Haganah’s radio station was set up in order to try and better the morale of the Jewish population in the city and environs,” he argued, “but does this broadcast *really* affect public spirit?”²⁵

The Haganah had a gamut of hasbara tools targeting morale, but no procedure or mechanism of quality control. The effectiveness of this and other campaigns is speculated rather than tested. In a morale plan he proposed later, this point was made explicitly, along with the demand to set new standards for decision making based on public opinion research. The social psychology of the crowd should become the foundation for social planning. In other words, they don’t have any systematic, accurate and objective way to know about the actual state of morale and what affects it (and therefore whether their programs are useful and their efforts worthwhile). In this case it was particularly acute to know: “Men were risking their safety to

²⁴ The party took place on May 16th, 1949, on the occasion of Kol HaMagen’s subsumption into Kol Yerushalayim (which had transferred from British to Jewish sovereign control). See: Arest memories, 130. Richard Stubbs was the mandate’s official spokesman. As such, he would meet with Jerusalem’s press corps for daily briefing and deliver the official stance of the British Palestine government on various matters. His statement that the British would maintain military presence in the city after the May 15 departure date became a symbol of official lying. See: Larry Collins and Dominique Lapierre, *O Jerusalem!* (New York: Simon and Schuster, 1972), 360. Alex Josey was a British journalist, political writer, and propaganda expert. During WWII, he was part of the British psychological warfare forces in Cairo, and transferred to Palestine postwar, he served as the PBS’s Controller of News in the last days of the mandate. Josey later went on to have an illustrious career as a propagandist in the Malayan Emergency. Known for his imperialist and anti-Zionist views, he was described by a fellow PBS worker as “plus royaliste que le roi.” See: Edwin Samuel, *A Lifetime in Jerusalem. The Memoirs of the Second Viscount Samuel* (London: Valentine, Mitchell, 1970), 220.

²⁵ June report, emphasis mine.

broadcast over *Kol HaMagen*,” a later account provocatively stated, “but was all this effort worthwhile?”²⁶

A formulation of what he understood to be the problem necessitating a scientific intervention, that appeared in a report on department work from June that year, gives an idea of how such a thing could be objectively and accurately determined:

In order to answer these questions, we need to use the science of public opinion and choose a sample of people from which we could learn what are the right answers. Who is familiar with the aforementioned science knows that it is possible to choose only a small percentage of people, and if the choosing is lawful, we can determine according to this percentage the answers of the entire population, and deviation...will be very small.²⁷

Apparently, in order to know for a fact how people felt, one simply had to ask. As elusive as it may seem, a subjective phenomenon like morale was something that could be measured and represented numerically. Observing actual audience behavior by means of questionnaire, that is, collecting and analyzing empirical data on listening habits and attitudes, was the only way to objectively determine whether the underground radio was a “worthwhile and successful” endeavor, claimed Guttman. Survey research was not only about generating a reliable (objective, accurate) picture of the social world, but also about the prediction and control of the forces that govern it. In this case, knowledge about listening habits and attitudes would allow the Haganah adjust the broadcast and rationalize its emergency management efforts.

²⁶ Gratch, *Twenty-Five Years of Social Research in Israel*, 14. Operating the radio was indeed a risky business that required clever maneuvers to keep the station’s location hidden from the British police. According to legend, power supply was provided by a special cable which was drawn along great distance from one of the hospitals and rigged up to look like a clothesline, on which a few pieces of washing were hung daily to keep up appearances. See: Joseph, 93.

²⁷ “The work of the Research Department by LAHAS Jerusalem, February-June 1948,” 6.11.1948, IDFA.

Observations in a Hostile Environment

Guttman was not interested in finding out about the extent of listening, only in gauging the actual effects of listening. It was therefore decided to make a panel study of people listening to the programs, and get their reactions on the spot. The “panel” was a marketing and mass communication research method Paul Lazarsfeld and his colleagues at the Princeton Office of Radio Research and later at Columbia University developed and refined during the 1930s and the 1940s.²⁸ Working with a significantly smaller sample of people whose ‘members’ are repeatedly interviewed over some interval of time, the “panel” had by that point become an economically favorable alternative to the public opinion poll that went beyond the poll’s descriptive purposes, and also allowed for the study of cumulative changes over a period of time. While Guttman seemed uninterested in a longitudinal study, the level of control the panel afforded compared to other survey methods persuaded him. Conducting before-and-after interviews with “a representative sample of the people the promotional campaign is intended to affect”—the target audience of a certain radio program or readers of a particular magazine rather than the general population—meant that the relationship between particular types of stimuli and changes in behavior could be measured. In other words, instead of reaching conclusions on the basis of data on popularity or audience size, the impact of a media “event”—in this case, the Haganah radio

²⁸ Panel studies appeared in commercial and social science research during the 1930s. Following the end of WWII, the use of panel techniques rapidly multiplied, particularly in the late 1940s and early 1950s: consumer panels were organized to study buying patterns in New York City, evaluate merchandise offerings, gauge changes in magazine readership interest, and follow radio listenership. Panel studies of buying and voting behavior were also conducted around that time. During WWII, the panel was used as a tool for studying the effectiveness of propaganda and promotional campaigns. On the history of the panel method, see: Herbert Hyman, *Taking Society’s Measure: A Personal History of Survey Research* (New York: Russell Sage Foundation, 1991), 195-199; Jean M. Converse, *Survey Research in the United States: Roots and Emergence, 1890-1960*, rev. ed. (New Brunswick, N.J.: Transaction Publishers, 2009); Jefferson D. Pooley, “Mnemonic Multiples: The Case of the Columbia Panel Studies,” *Journal of the History of the Behavioral Sciences* 51, no. 1 (2015): 10-30.

broadcasting—on attitudes could be directly measured. All these characteristics—cheap, probing, and highly reliable—made the panel particularly appropriate for scientific measurement work in field conditions.

Jerusalem in those turbulent last days of the Mandate—as the British mandatory government disintegrated, civil war unfolded and the stranglehold around its Jewish neighborhoods tightened—was a particularly hostile environment that posed unusual metrological challenges. The first set of challenges involved access to high-quality and consistent data. Even though most of the city’s Jewish residents owned a home set, many had only intermittent power supply. Attempting to outsmart this imposition, the Jewish authorities set up two-dozen loudspeakers linked to radios in houses and institutions that were still provided with electricity so that in various parts of the city people could receive the Haganah transmission. However, as Dov Joseph, head of the Jerusalem Emergency Committee and later the city’s Military Governor, recalled in his magisterial memoir, that was only a partial solution: “sometimes when shelling would begin, the listeners in the street would run to safety and the loudspeakers would go on talking into empty space.”²⁹ This sporadic mode of listening constituted a unique obstacle to carrying out an “action panel” study, where the sample is asked to actively do something concerning which they then express opinions, such as listening to radio programs. The methodological problem known as panel “mortality”—subjects dropping out or failing to cooperate after they have promised to do so—was particularly acute in such initiative-dependent studies. According to Lazarsfeld, neither repeated reminders nor payment for active participation seem to guarantee full cooperation, and therefore it was suggested that a panel

²⁹ Joseph, 94.

should be organized so that it contained at the start about twice as many people as are wanted.³⁰

But since besieged Jerusalemites could not always listen to the radio even if they wanted to, even such excessive subject recruitment might not be enough.

In addition, there were difficulties at the level of execution. A large-scale study of this sort required special equipment and office space: sorting and calculating machines, paper, a copier. But there were no available funds. The absence of professional social scientists in the country, as well as technical workers adept in statistical data analysis, posed another serious challenge. Guttman's makeshift team was a far cry from what he was familiar with. Since Guttman had no access to trained observers, he had to rely on the assistance of the Information Officers of Mishmar Ha'am, as well as local teenagers for the collection of data.³¹ As Kohler and Kuklick point out, the involvement of amateurs has been a common feature of field research operations and one that carries with it both advantages and risks.³² The Information officers-turned-fieldworkers, who were Jerusalemites themselves, knew the city and its people well, and were better positioned to defy suspicion and were less prone to communication breakdowns and other forms of resistance. In this sense, they were better "probes." And they came at no cost. However, like other actors in this heterogeneous environment, their actions and conduct were also governed by extra-scientific imperatives and priorities. On one occasion, a volunteer

³⁰ Paul F. Lazarfeld, "'Panel' Studies," *The Public Opinion Quarterly* 4, no. 1 (March, 1940): 122-128.

³¹ Ruth Guttman claimed that they made use also of local children or teenagers in the radio study. I couldn't find any other source to confirm this.

³² Robert E. Kohler, *All Creatures: Naturalists, Collectors, and Biodiversity, 1850-1950* (Princeton: Princeton University Press, 2006); Henrietta Kuklick, "Personal Equations, Reflection on the History of Fieldwork, with Special Reference to Sociocultural Anthropology," *Isis* 102, no. 1 (2011): 1-33. For more on amateur circles, see Etienne Benson, *Wired Wilderness: Technologies of Tracking and the Making of Modern Wildlife* (Baltimore: Johns Hopkins University Press, 2010).

fieldworker left a large stack of questionnaires containing meticulously collected data - never to be seen again - at the Lehi base, as he was sent by the organization on an emergency mission.

In attempt to cope with these local exigencies, Guttman had to improvise. He came up with an ingenious plan: instead of asking city residents to mail their answers, or interviewing them at their homes, as panel studies were customarily done, a carefully put together sample was instructed to come at an appointed time to a certain location in each neighborhood, listen in tandem to the radio broadcast, and fill out a questionnaire designed to capture and record individual experience immediately thereafter.³³ The Information officers of Mishmar Ha'am, who volunteered to assist in carrying out the study and were specially briefed for the occasion, were to be present at each location to administer the questionnaires, make sure they were filled out properly, and then return them to headquarters.

Despite the careful planning, an unfortunate mixture of volatile political climate, subject recalcitrance, and rogue fieldworkers—the “unreliable forces of social interest and natural agency,” as Simon Schaffer has aptly labeled them—disturbed the proper execution of the survey and prevented the epistemological transubstantiation of particular circumstances into universal knowledge.³⁴ On the designated day, there was more tension than usual, and British soldiers rampaged through the streets of Jerusalem, shooting sporadically and putting on display the sheer arbitrariness of sovereign power. Movement around the city in the evening hours had already been severely restricted due to a blanket dusk-to-dawn curfew put into effect in the fall when

³³ Thanks to an improvised census Mishmar Ha'am had made in preparation for eventualities, a complete roster of names and addresses was available for sampling purposes. See: Mekorot.

³⁴ Simon Schaffer, "Modernity and Metrology," in Luca Guzzetti, ed. *Science and Power: The Historical Foundations of Research Policies in Europe*, a conference organised by the Istituto e Museo di Storia della Scienza (Firenze, Italy) (Luxembourg: Office for Official Publications of the European Communities, 2000), 71-91.

disorder began. Even though it was customary to not be kept indoors when duty called, the “the public reluctance - especially housewives’ - to leave their homes in the evening” for science’s sake, Guttman commented bitterly in a post-mortem report, constituted “a factor that seriously interfered” with the successful completion of the radio study.³⁵

Desperate housewives were only one form by which nature in Palestine resisted science’s advances. In the studies that followed, Guttman and his team often met with indifference, incomprehension, and dismissal on the part of both survey subjects and decision makers. Reporting back to his sponsors in New York on the preliminary results of polling different ethnic groups, Guttman noted, “among the Orientals, the interviewer must become accustomed to coffee and cakes at each home in his itinerary.” This did not only slow down the process, it also, and more importantly so, reflected a misunderstanding of the survey situation that required various adjustments such as switching between the Gallup and Likert types of questions and trying out different forms of introduction. And yet, compulsively hospitable, Sephardic Jews seemed to insist on their ways, conflating attitudinal items with personal interest. A description of the first polls made in Israel post-independence clearly demonstrates that survey subjectivity did not travel very well:

The Institute’s field workers are often amazed at the enthusiastic welcome they receive. Some venerable residents make a sheh-hecheyanu blessing, that the State of Israel should be so wonderful that it turns to its ordinary citizens to ask their advice on the conduct of important affairs. Some at first refuse to answer, on the grounds that their own opinion is unimportant; but they know some important person who should be asked instead. Housewives often want to refer the questions to their husbands instead, as being more qualified to answer. Firmness and patience on the part of the fieldworker are required in many cases to convince the person that it is really to him that the questions are addressed,

³⁵ “Report on the work of the Research Unit,” General Report, Brigade Information Office, submitted to Mahtar Haganah, Jerusalem, 1 July, 1948, 15.

and that his personal responses are what counts. On the other hand, there are those who resent being left out of the poll. Some other member of the family insists on being heard also...or there is the kibbutz from which two of the members...were absent the day the poll was taken: the Secretary [of the kibbutz] insisted that our fieldworker make the long trip again [to interview the two]...even though it would have no appreciable effect on the results of the entire poll.³⁶

However, more than anything else, the study's intended clients seemed entirely uninterested in learning "what Jerusalemites think of Kol HaMagen HaIvri's broadcast," as the title of the concluding report promised to reveal. Unfortunately, there is no surviving record of this early report, and existing accounts offer contradictory versions for how things ended.³⁷ In retroactive recounting, the study had been called off due to circumstances over which the researchers had no control grouped under "problems of fieldwork." Yet, contemporary documents suggest that the study had been completed, albeit imperfectly, and report a clear result. According to the quarterly review, it was concluded that listeners felt "great sympathy toward the broadcast," and "unanimously decided the broadcast was worthwhile and successful and positively influenced people's mood."³⁸ Vague and impressionist as it may have been, neither this conclusion nor listeners' suggestions for improvement (which were also recorded and reproduced in the lost report) was echoed in discussions over the content and administration of the radio.

³⁶ The quote appears in Gratch, *Twenty-Five Years of Social Research in Israel*, 21. There is no reference to the original article.

³⁷ In the official history of the Institute: "a perfectly designed sample went to waste because of problems of fieldwork." See also interviews with Gutkind 1978, and Na'aman.

³⁸ It is useful to compare Guttman's work with Robert King Merton, *Mass Persuasion: The Social Psychology of a War Bond Drive* (New York: Harper and Brothers, 1946).

‘Not Volunteers and Not Pioneers’

Things took a surprising turn in spring, when the crisis deepened, preparations for the looming war intensified, and the morale of city inhabitants, both civilians and soldiers, became an urgent military problem and an object of focused political interest and regulation efforts.³⁹ As the stranglehold around the Jewish part of Jerusalem tightened and living conditions severely deteriorated, there was a growing sense that people were losing their composure.⁴⁰ Resident volunteers, as well as Haganah people on the ground, reported daily of violent incidents and petty crimes, skirmishes among people waiting in line for food or fuel, and a pervading depressed mood. One such report—“review of the atmosphere on the home front”—sent to LAHAS in late March, serves well to illustrate the panic around the state of public morale:

From my daily encounters with people of different strata...I watch with great anxiety the dangerous signs of declining morale on the home front. The population is increasingly embittered about the chaos in matters of supply and about the inactivity and administrative impotence of self-governing institutions. The dissenters are running a virulent propaganda campaign, which brings confusion and finds fertile ground among the backward elements...profiteering’s thriving...There are no proper courts [for trying] speculators and criminals. [We see] more and more manifestations of lawlessness, extortion, and so on. This atmosphere and the vocal grievances [aired publicly] pose a grave threat to our fortitude.⁴¹

³⁹ March 1948 brought a series of challenges for the Yishuv, primarily an increase in intensity and number of Palestinian attacks on the main roads and in rural areas, and the failure of food convoys to reach Jerusalem, marking the onset of full-fledge siege. The Haganah’s response represented a strategic change in the use and the organization of force by the Jewish side, which consequently brought about a resounding Palestinian Arab defeat in the initial or “civil” phase of the war. This moment has become known in Israeli historiography as “the March Crisis.”

⁴⁰ For personal narratives of everyday life under siege, see: Harry Levin, *Jerusalem Embattled: A Diary of the City Under Siege, March 25th, 1948 to July 18th, 1948* (London: Cassell, 1997); Amos Oz, *A Tale of Love and Darkness* (Orlando: Harcourt, 2004), Ziporah Porath, *Letters from Jerusalem, 1947-1948* (Jerusalem: Association of Americans and Canadians in Israel, 1987); Nitza Rosovsky, *In the Land of Israel: My Family 1809-1949* (Cambridge, Mass.: Tide Pool Press, 2012). For journalistic commentary, see: Yona Cohen, *Jerusalem Under Siege: Pages from a 1948 Diary* (Los Angeles: Ridgefield, 1982); Amos Elon, *Yerushalayim lo nafla: Matsor 1948* (Tel Aviv: N. Tverski, 1948). (Hebrew).

⁴¹ “Review of the Atmosphere on the Home Front.”

Low morale bled beyond Jerusalem's civilians into the very troops tasked with defending the city. Following the Jewish National Council's decision from December 1947 to raise an army in preparation for an all-encompassing regional war, the local Haganah command launched a massive mobilization campaign and began recruiting all who were fit to fight. Black-and-white mobilization notices ordering every Jewish male from seventeen to forty-five to register for military service covered the walls of the city, but the public response was less than enthusiastic. Older and poorer than the rest of the Yishuv, and ideologically agnostic or hostile toward the Labor Movement's (hegemonic) version of the Zionist project the organization represented, Jerusalem's population—an illegible and to a large degree impenetrable mosaic of insular ethnic communities—did not march diligently into the Induction Center as had been hoped.⁴² “Many guys who have so far not been given the opportunity to join the ranks of the Haganah will respond to the call on their own initiative,” recalled Jacob Tsur, head recruiter, of his concerns as the campaign began. “But what would we do about the hundreds of draft dodgers? How are we to look for them in the alley maze of Jerusalem's neighborhoods?”⁴³

Indeed, the representatives who engaged in the work of enlistment on behalf of the Public Council for Mobilization during the winter months of 1948 had to deal with manpower problems of a sort Jewish national authorities did not encounter elsewhere in Palestine: the Jerusalem contingents of the Stern Gang and the Irgun, the largest and most influential in the country, who refused to cooperate and competed with the Haganah over the city's youth reserves; the recalcitrant leaders of the orthodox sects who invoked *Torato Omanuto* and would not let

⁴² Ben-Zion Dinur, ed. *History of the Haganah*, 3 volumes (Tel Aviv: Ma'arachot, 1954-1972), 1388. (Hebrew). [in Hebrew]; Larry Collins and Dominique Lapierre, *O Jerusalem!* (New York: Simon and Schuster, 1972). On the idea of state legibility, see: James C. Scott, *Seeing Like A State* (New Haven: Yale University Press, 1998).

⁴³ Jacob Tsur, *The Day is Near* (Jerusalem: Keter, 1979), 237. (Hebrew).

yeshiva students enlist; and the many draft evaders, who kept a small volunteer military police force quite busy. Described variously in condemning officer reports as “lack of volunteering spirit,” “isolationism,” “backwardness,” “defeatism,” or simply, “no desire to fight,” Jerusalem’s failure to appear pugnacious further aggravated the negative image that its masses already had in the eyes of the Haganah’s top brass.⁴⁴ Mishael Shaham, a senior General Staff officer who was sent in January to propel the Jerusalemite forces into action, summed up the position of the organization: “The Jewish public [in Jerusalem] constitutes the weakest link of the Yishuv...the human material is difficult to organize and tends toward ‘*bnei ham,*’ who take every opportunity to undermine the defense forces.”⁴⁵

The fighting force which they did manage to assemble—approximately twelve hundred infantrymen culled from a mixed bag of students, British Army veterans, and new immigrants—by mid-February—had very little weapons, hardly any gear, and suffered from adjustment and motivation problems. In the absence of appropriate administration and logistics, recruits had to take care of clothing themselves and bring with them their own blankets and towels.⁴⁶ They slept in various public buildings (schools, synagogues) or in the houses of refugees, and were sent to eat in nearby restaurants or at home. The very limited weapon arsenal in the district—253 rifles, 440 Sten guns, 4 “Brens” and a small amount of homemade hand grenades—was not enough for even half the men enlisted, and put immediately on guard duty along the city’s many fault lines,

⁴⁴ Moshe Ernwald, "The Military Campaign in Jerusalem during the War of Independence, November 1947-April 1949," in Alon Kadish, ed. *Israel's War of Independence 1948-1949* (Tel Aviv: Ministry of Defense, 2004), 341-388 (Hebrew).

⁴⁵ The expression “*b’nei ham*” (Hamites) refers to the dissent organizations. The biblical allusion is used here in a derogatory manner, to indicate that these were depraved, subversive elements, which despite their weakness, must be treated with suspicion. The expression had likely carried also racial undertones, as the Hamites were associated with the peoples of Africa and the Middle East - in this context, Sephardic (“Arab”) Jews. Quoted in Ernwald, 352.

⁴⁶ The HIM was similar in structure and MO to the WWII British Home Guard.

they had time neither to train nor to develop a sense of solidarity and cohere into meaningful military units.⁴⁷

Cobbled together into battalions in haste, these “instant soldiers” were not very eager to endure the demands that their new military life made of them. Trying to get a read of the situation, LAHAS requested a recent recruit—a private in one of the city’s units—to gather information about the “mood” of fellow soldiers. The testimonials he collected, excerpts of which were quoted verbatim in a letter that circulated mid-February among members of the staff, drew a lurid picture of troop morale in Jerusalem. Soldiering full-time, uncompensated for their labor and unable to work, they walked the streets penniless and frustrated. One soldier, a student, said he was feeling down; another, an immigrant from Salonica, declared he’d rather go back to Greece than serve another day in the army, and a third, the token Mizrahi, was caught threatening a local shopkeeper with his gun: “I don’t have money to even buy a cup of coffee [and] the number of cigarettes you get is not enough. What am I to do?”⁴⁸

Discipline and morale problems of this kind were foreign territory for the commanders and educators of the nascent army. For twenty years—the duration of its existence—the Haganah had enjoyed a consistently motivated and driven fighting force whose members considered warfare an honor and great privilege. Socialized into militarism, the fighters who joined its ranks were strongly identified with the mission and ideals of belligerent Zionism and were willing to kill and be killed on the altar of national self-determination.⁴⁹ However, as Gelber explained, conscription dramatically changed the sociological composition of the Jewish defense forces.

⁴⁷ *History of the Haganah*, 1390-97.

⁴⁸ “Re: Mood in Units.” Savorai to David, 16 February, 1948.

⁴⁹ Ben-Eliezer, *The Making of Israeli Militarism*

Unlike Haganah members of previous decades, who belonged almost exclusively to the secular social elite of Palestine-born Jews of European descent that exercised tremendous political and cultural influence over the Yishuv, the 1948 draftees were a mixed bag of university students, British Army (Jewish Brigade) veterans, Sephardi day laborers, and new immigrants, mostly concentration camp survivors, who entered the country illegally, spoke no Hebrew, and had no military experience.⁵⁰

A sense of alarm started to grow among the senior command in Jerusalem regarding the readiness of the troops for war. Pessimistic assessments of the ability of the citizen army to face its task made their way to the Haganah headquarters. David Shaltiel, the newly appointed commanding officer of the Etzioni Brigade, sent out a series of memos meant to convey with particular urgency the acuteness of the situation. The three thousand men under his command, Shaltiel wrote, were not enough to defend the city. Many of his commanders fell short of their tasks. A third of the recruits were missing clothes and other supplies. Soldiers were not paid on time due to a severe shortage of funds, and their families did not receive the support promised. In a March 28th memo, he called attention to the widespread discipline problems brought about by low morale: the mutiny of HISH (Field Corps) forces in Sodom, the recruit hunger strike in Hartov, and defeatism and lack of battle spirit all around. Without “serious reinforcements in manpower, weapons, and supplies,” he warned, “Jerusalem will fall.” Gradually realizing that they were now tasked with commanding a full-fledge citizen army who knew nothing about fighting and had many morale problems rather than a self-selecting clandestine guerrilla

⁵⁰ Yoav Gelber, *The Emergence of a Jewish Army* (Jerusalem: Yad Yitshak Ben-Tsevi, 1986). (Hebrew). On the role of Holocaust survivors in the 1948 Palestine War, see: Yaacov Markovitzki, *Fighting Ember: Gahal Forces in the War of Independence* (Tel Aviv: Ministry of Defense, 1995). (Hebrew).

organization, the Haganah confronted the fact that they had a serious crisis on their hands. In the face of this crisis, they grudgingly turned to the only collective mood expert they had on file.

The crisis revolved around questions of expertise, knowledge, and style of reasoning. This is why Guttman (the expert) was allowed to bring in statistics and trust in numbers instead of trust in intuition. It had suddenly become clear that available ways of knowing about soldiers-to-be—about their character, dependability, performance history, virtues and faults, values, mentality, are they loyal friends, would they be dedicated to the cause, are they ready to self-sacrifice—were insufficient. The guerrilla epistemology, on which the organization learnt to rely for its survival, had for the first time run into its limits. The new recruits were not in the same social circles and therefore did not have friends who could testify for their character. Many of them were recent immigrants who had grown up in a very different cultural environment. They spoke neither Hebrew nor the native ideological dialect. Some were refugees from DP camps in Europe who were smuggled into the country in the dead of night, carrying with them disturbing memories from “that place.” Some were not Zionist and never intended to immigrate to Palestine, let alone fight a war for Jewish sovereignty in heart of the Middle East.

Various solutions were proposed as for what to do with these non-soldierly soldiers who came from the unsightly social margins of the Yishuv. Ben-Gurion argued that the only way to move forward was to keep them under strict discipline. In an early February party meeting, he pointed out the recruits’ strikingly different values and norms of conduct, arguing that the relaxed and informal military culture of the Haganah could no longer be safely maintained:

This organization [the Haganah] was founded on friendship and *halutziyut* (the pioneering way), not on military regime...there was no room for military regime, people felt it was foreign...but if we fail to institute the new force...on a foundation of iron discipline, like in any proper army, that an order is an order, that he [sic][the soldier] mustn’t ask questions and do exactly as ordered, that he

is dressed up the way he needs to be, gets up on time, goes to where he is told and doesn't go where he shouldn't...it will be a disaster...

Ben-Gurion explained that “now we are recruiting 20 thousand boys, tomorrow maybe 30 thousand...[and those are] not volunteers and not pioneers...” Yigal Alon suggested that the best approach was to ideologically beat these “inner-city boys” into shape. The decision was made to embark upon a massive hasbara campaign.

It was at this point that Guttman became involved. He offered to furnish the hasbara apparatus with accurate data and reliable information on the dimensions of the problem, as well as on the factors affecting the morale of the recruits, in order to guide policy decisions and propaganda campaigns. What he was putting forth was the promise of attitude and opinion measurement: the most sophisticated and accurate method of knowing “what the soldier thinks,” i.e. the best means of getting reliable, objective information on the mental state of the army, and by extension, for calculating potential performance.

The Perfect Scale

It is not clear from the records whether Guttman was indeed asked to assess the morale of the recruits, or whether he had come forward and offered his services on his own initiative, as he had done before.⁵¹ Whatever the case was, that same month, the Research Department—by now a formal part of the army—began conducting large-scale attitude surveys among the Haganah

⁵¹ In several newspaper interviews Guttman had claimed he was asked to assess the morale of the recruits. I was not able to find other evidence to confirm this. However, a document detailing the Standing Orders of the Information Officer mentions Guttman by name. Article 8 in the “Methods” section specifies, “a department for research will check from time to time what is going on among the members [of the Haganah] by means of directed questionnaires. Headed by Prof. Louis.” Notice the use of the proper name and its contrast to the vague language in which the “method” is described.

forces in Jerusalem.⁵² These were the first studies of this kind in this part of the world. Between March and June 1948—starting when the war was still in its “irregular” phase, largely confined to the city, through the intense days of fighting during the Pan-Arab invasion, to the establishment of the Israel Defense Army and the lifting of the siege—detailed reports on the state of morale of the HISH, the HIM (Home Guard), the Jerusalem District Medical Service, the Military Police, and the 205 Women Corps battalion were prepared and distributed among the commanding ranks. “The role of the Department,” concluded a report which was submitted to the Brigade Information Office at the end of that period, “was to test, to the extent that was possible, the factors affecting the morale of the soldiers, to measure the mood of the recruited, discover what bothers them, and come to a conclusion on how to improve their social-psychological military fitness.”⁵³

This preliminary examination of morale-related problems during the early stages of the 1948 Palestine War was a synoptic reenactment of the survey-based troop morale assessment Guttman was part of designing during his WWII consultancy service for the U.S. War Department.⁵⁴ In the context of that project, Guttman—the principal methodologist of the Armed Forces Attitude Research Branch—was tasked with developing a measurement scale for morale.⁵⁵ Measuring morale in Palestine according to the same procedure the Research Branch

⁵² Guttman was drafted into the army on April 15, 1948.

⁵³ June report, 2.

⁵⁴ Guttman served as an expert consultant for the War Department from 1942-1946.

⁵⁵ “My major assignment was to build a morale scale with respect to performance on the battlefield. ... There is no evidence that prediction of behavior on the battlefield can be made on *a priori* grounds, and we shall continue to be boxing with shadows in this regard until we get into a position to observe actual battlefield behavior, and where we can pre-test and repeatedly pre-test sets of items. ... I have thought much about it, and have discussed it especially with Dr. Cottrell. We do have in mind a type of item of interest and are intending to work together on a morale scale—again, not necessarily performance on the battlefield.” Guttman to Stouffer (September 1, 1942):

had used would therefore serve two purposes: first, that of reliability testing. Putting the morale scale Guttman had built during the war to the test of replication would show whether the Guttman scaling method could travel, that is, generate meaningful results on a repeated trial in a different cultural setting.⁵⁶ Second, it was a golden opportunity for Guttman to prove his worth and the restore the credibility of survey measurement (after the radio study had failed to do so). Staging a public demonstration of his instrument would prove its superiority to military reasoning, and tip the scales, so to speak, in favor of his expert authority.

Guttman began inquiring into the logic of psychosocial measurement before WWII, but it was during the war, under the pressure of the practical situation—the demand for broadly applicable and user-friendly methods that could produce quick yet accurate results at a competitive cost—that Guttman scaling, the method of measurement bearing his name, was given its mature form.⁵⁷ Hoping initially to construct a factor analysis for qualitative data, Guttman proceeded to work on a single-factor, unidimensional *scaling* for such data.⁵⁸ “In a great deal of research in the social and psychological sciences,” he explained the purpose of scaling in a widely cited article from 1944, “it is often desired...to be able to summarize data by

Memorandum, “Work of Dr. Guttman for the past Six Weeks.” See also: Cottrell to Stouffer (August 27, 1942): “Review of Selected Literature on Military Morale.” NARA

⁵⁶ For more on traveling methods, see: Rebecca Lemov, *Database of Dreams: The Lost Quest to Catalog Humanity* (New Haven: Yale University Press, 2015), Chapter 2.

⁵⁷ Guttman’s war-work simplified work he had done in the logic of measurement during his doctoral studies, while spending a year at Chicago University under the guidance of Stouffer. See Louis Guttman, “The Quantification of a Class of Attributes: A Theory and Method for Scale Construction,” in *The Prediction of Personal Adjustment*, ed. Paul Horst (New York: Social Science Research Council, 1941), 319-348.

⁵⁸ Louis Guttman, “An Outline of Some New Methodology for Social Research,” *The Public Opinion Quarterly* 18 (1954): 395-404. Scaling is the branch of measurement that involves the construction of an instrument associating qualitative constructs with quantitative metric units. Scaling evolved out of efforts in psychology (and education) to measure “immeasurable” abstract constructs like authoritarianism or self-esteem. In most scaling, the objects are text statements, usually statements of attitude or belief. To scale these statements, they are assigned numbers according to a rule. On the history of attitude scaling, see: Jean M. Converse, *Survey Research in the United States; Roots and Emergence, 1890-1960*, rev. ed. (New Brunswick, NJ: Transaction Publishers, 2009), 62-77.

saying, for example, that one marital couple is better adjusted than another marital couple, or that one person has a better opinion of the British than has another person, or that one student has a greater knowledge of arithmetic than has another student.”⁵⁹ This has so far been quite difficult to achieve. This type of empirical observations—the trials and tribulations of married life, national feelings toward our ‘stiff upper lipped’ political allies, numerical intelligence—did not render themselves easily to classification and ordering, and existing methods of analysis, Guttman went on, were neither rigorous enough nor economical: they required elaborate mathematical manipulations, were extremely laborious and time-consuming, and could not be used with large datasets and therefore outside the college classroom.⁶⁰

More fundamentally, though, available theoretical models of ordered structures of data—or scales—have failed so far to overcome the major source of error in the measurement of social attitudes and opinions, namely, the problem of “response consistency.” “Obviously,” Guttman wrote, “if a [survey] question means different things to different respondents, then there is no way that the respondents can be ranked in order of favorableness.”⁶¹ Meaning indeterminacy was not an occasional difficulty, he emphasized, but rather a structural feature of measurement-by-asking-questions⁶²: “questions may appear to express but a single thought and yet not provide the

⁵⁹ Louis Guttman, “A Basis for Scaling Qualitative Data,” *American Sociological Review* 9, no. 2 (1944): 139.

⁶⁰ Gallup, like other prominent pollsters of the prewar era, remained convinced that scaling was an academic matter, which was rather irrelevant for the practical business of opinion polling. See: Converse, *Survey Research in the United States*, 193.

⁶¹ Louis Guttman, “The Basis for Scalogram Analysis,” in *Measurement and Prediction*, vol. 4 of *Studies in Social Psychology in World War II*, ed. Samuel A. Stouffer et al. (Princeton, NJ: Princeton University Press, 1950), 61.

⁶² As Strack and Schwarz explain, asking questions has been perhaps the most widely used method in the social sciences to measure characteristics of people and their behavior that are not directly observable, such as attitudes or motivations. Standardized questioning in survey situations is one of several models of measurement-by-asking-questions (psychometric testing being another widespread model). Fritz Strack and Norbert Schwarz, “Asking Questions: Measurement in the Social Sciences,” in *Psychology's Territories: Historical and Contemporary*

same kind of stimulus to different people. The responses even to the simplest question can differ in kind as well as degree.” If that is the case, then, “how can one tell if there is enough consistency in the responses of a population to a series of questions to indicate that only a single factor is being measured?”⁶³

What was needed was a safe (aka objective) way of determining the underlying unity and reality of the concept being measured without relying on ordinary language to convey content consistency. Guttman’s ingenious idea for how to do that, Jack Elinson explained, was that of an ordinal scale.

[What Louis did was to devise a test for unidimensionality, whether the thing measured was indeed a single dimension in the same way that any ordinal scale would be.] The typical measures in psychology at the time—psychometrics, attitude scales, aptitude scales, intelligence scores and so on—they just had an idea what content was and they added them up and provided a number to it: how many were right, how many were wrong, favorable, unfavorable, etc. But Louis was very rigorous in his approach; he couldn’t have ‘more or lesses’ unless it came from a single dimension. For example, when you have height: even if you don’t know the exact height (that would make it a cardinal scale), you know that this chair is higher than this table; it’s always higher than this table, which is always higher than this carpet. The relation between these three would be the same under any circumstances if it [height] was a single dimension. That’s the property of an ordinal scale.

Put simply, Guttman’s solution was to treat feelings the same way one treats height or temperature. It is in this sense that scalogram analysis, the scaling method and procedure Guttman developed, constituted an altogether “new approach” to the problem of scaling. In contrast to earlier approaches, it did not focus on the a-priori ranking of survey questions or statements, which inevitably relied on some pre-conceived definition of content to which these

Perspective from Different Disciplines, ed. Mitchell G. Ash and Thomas Sturm (Mahwah, NJ: Lawrence Erlbaum Associates, 2007), 225-250.

⁶³ Guttman, “The basis for Scalogram Analysis,” 60.

questions or statements assumingly expressed (and thus inevitably fell into the black hole of question meaning), but rather on the ranking of individuals, which did not require such a definition. As Stouffer wrote in his overview introduction to *Measurement and Prediction*,

Guttman offered a model, which dispenses with the concept of a latent or underlying continuum to which a response to a particular item is to be related. [Instead] he considered an attitude area “scalable” if responses to a set of items in that area arranged themselves in certain specified ways...[so that] *persons who answer a given question favorably all have higher ranks than persons who answer the same question unfavorably*. From a respondent’s rank or scale score we know exactly what items he endorsed. Thus we can say that the response to any item provides the *definition* if the respondent’s attitude.

The key notion here is that of arrangement “in certain specified ways”: Guttman’s method used graphical reordering of responses to see if a certain pattern or configuration of the data would emerge. The emerging pattern allows the researcher to deduce the rank-order of the questions independently of their content. Hence, questionnaire design did not require a priori assumptions about rank ordering. The organizing of complex data sets was done so that they generated a legible pictorial representation an unskilled clerk could “read” which in turn made Guttman scaling suitable for mass administration in extra-academic contexts, thus revolutionizing socio-psychological measurement:

This approach has been used successfully for the past year or so in investigating morale and other problems in the United States Army... Simple routines have been established which require no knowledge of statistics, which take less time than the various manipulations now used by various investigators...and which give a complete picture of the data not afforded by these other techniques. *The word "picture" might be interpreted here literally*, for the results of the analysis are presented and easily assimilated in the form of a "scalogram," which at a glance gives the configuration of the qualitative data (emphasis mine).⁶⁴

Jack Elinson, who worked with Guttman at the Pentagon, recalled how it was done in practice:

⁶⁴ Louis Guttman, “A Basis for Scaling Qualitative Data,” 139.

We would have a piece of paper, with the scores...and the answers to questions vertically, and this was entry by pencil and paper, with check marks for yesses and so on. Then in order to rearrange the items in the questionnaire, he would cut this into strips and hang those strips on the Pentagon's wall, and then move the strips around. And then when they "order-arrange" from high to low, he would paste these strips of papers back again, with scotch-tape, and cut them the other way, in order to arrange the scores of the men, which men were highest and which men were the lowest. And he'd finally end up with the best fit of the set of responses to the set of questions by this particular sub-sample, and then the question was whether that particular set of items "hang together" as a scale, whether they were "scalable."⁶⁵

While assessing the Guttman scale for a small number of questions and responders can be done by hand, the standard procedure used by the Research Branch involved the use of a 'scalogram board,' a physical wooden board Guttman had invented and designed especially for this purpose. The board was used as a visual aid to facilitate the prodigious labor of computing correlations, for it enabled one to see in a shorter time how each item related to others and to the whole. The board provided holes in 100 rows for persons and 100 columns for answer categories, and each respondent's answers were recorded by distributing metal shots in holes. Because slats in the board could be shifted to change the position of rows and columns (i.e. of items and respondents), the board could be manipulated to reveal a scale pattern if one existed. The board would also make it graphically obvious what questions fell outside the scale, indicating that they did not contribute to the measured entity.⁶⁶

⁶⁵ Nurit Guttman, video interview with Jack Elinson, 1997. The Guttman Family Collection.

⁶⁶ Edward A. Suchman, "The Scalogram Board Technique for Scale Analysis," in *Measurement and Prediction*, 91-121



Figure 5: One of the two original scalogram boards, Jerusalem (photo taken by author, May 2008)

Scalogram analysis, however, did not completely overcome a known conundrum in survey measurements—that percentages of respondents “favorable” and “unfavorable” toward a particular issue often varied with different phrasing of the questions:

In the course of continuing surveys of attitudes and opinions of the men in the US Army during the war, a perplexing problem arose that seems also to occur in civilian surveys. In a large number of cases, it was found that differently worded questions dealing with the same issue produced different percentages of the population as apparently 'favorable'. Slight changes in phrasing, in the order of presentation of answer categories, in the position of the question in the questionnaire...yielded apparently different polling results. Which of the results was the correct one?⁶⁷

Similarly, even after extensive pretest interviewing, “the judgment of what was an ‘unbiased’ question was a rather subjective matter, and that there could be little hope for agreement even among ‘experts’ as long as they could rely only on intuition.” Thus, this so-

⁶⁷ 445, emphasis mine.

called “interviewer effect” was not merely a reflection of lack of skill or experience in questionnaire design:

The analysts of the Research Branch ... which conducted these surveys, had considerable practical experience in the construction and analysis of questionnaire studies. Yet ... they often would not agree as to the best wording of a question. It became very apparent that judgment of what was an 'unbiased' question was a rather *subjective* matter, and that there could be little hope for close agreement even among 'experts' as long as they could only rely on intuition.⁶⁸

In fall 1947, Guttman and Edward Suchman, his wartime collaborator at the Research Branch (the former was still affiliated with Cornell; the latter with the SSRC), published an article declaring they had found a solution to the “perplexing problem” of question bias (also known as the problem of question wording), which has long plagued opinion and attitude analysts. The solution, presented in the article, was described as “an objective method of dividing respondents into pro and con groups which are relatively independent of question wording.”⁶⁹

The underlying reasoning behind this solution is the observation that “intensity of feeling decreases as one moves toward the middle from either end until one reaches a point of least intensity.” In other words, respondents feel strongest (or have the least doubt) about questions or statements that express extreme views, either positive or negative. Conversely, there is a middle point of lowest intensity—and once this point is identified it “serves to divide the population into positive and negative.” Guttman goes on to demonstrate empirically that this point is an “objective zero point”: it “will stay fixed regardless of the particular opinion questions used or of

⁶⁸ 445, emphasis mine.

⁶⁹ Edward A. Suchman and Louis Guttman, “A Solution to the Problem of Question ‘Bias’,” *The Public Opinion Quarterly* 11, no. 3 (1947): 445-455.

the way they are worded.”⁷⁰ Hence, such a number allows the researcher to state that, for example, the American soldiers during WWII had lower morale than the British soldiers even if the comparison is based on two different sets of questionnaires administered by different researchers.

These developments amounted to inventing the ‘perfect scale’: a technique that promised to be a universal data analysis tool, the “adjustable wrench” of survey-based attitude and opinion research. The set of procedures Guttman has devised—involving multi-step graphical representations of survey responses—allowed him to treat attitudes, for the first time, like height or temperature: they can be measured and compared to one another, even between cultures, time points and researchers.

The Ontology of the Questionnaire

This was well reflected in the structure of the “questionnaire for [Haganah] members” which Guttman had designed to measure the morale prevailing in the different units stationed in Jerusalem in the months that preceded the First Truce.⁷¹ Like other self-administered attitude survey questionnaires of that era, it consisted of three parts: an instructions page, a battery of 30 “selected response” or “forced choice” questions (items in survey lingo), and a free comments section at the end. Following a series of instructions, the questionnaire opened with questions designed to obtain demographic data: gender, age, marital status, occupation, education, ethnic origin, and so forth. These questions, known in survey parlance of the time as “questions of personal fact,” referred to social attributes that could be directly perceived or that others could

⁷⁰ Ibid., 455.

⁷¹ “Questionnaire for Members” (internal), 9 pages (undated), print, IDFA 582-4944-1949.

confirm. The function of these questions was therefore to obtain “objective measures” of the individual respondent, which, upon analysis, served as independent variables in the assessment of morale. There were also several questions designed to collect demographic information that might bear specifically on the problem of morale, such as time and branch of army service, current rank, the area in the district one served in (there were tougher and less tough areas) and whether one has any battle experience.

The centerpiece of the questionnaire was a series of questions designed to assess attitudes, which, the Research Branch had determined, were associated with troop morale. Several items inquired directly about the respondent’s state of mind: “Are you usually in high spirits?” or “Do you ever get worried or embarrassed?” But most items measured attitudes towards various aspects of military life, from the quality of army food, to accommodations (or sleeping arrangements) and hygienic conditions at the army base, to satisfaction with assignment and training, and finally also regarding relationships with unit members and commanders.⁷² In terms of their content, these questions were designed to probe what the US Army’s Research Branch had referred to as “specific attitudes” of morale. These attitudes were related to—a scale of them yielded an index of—four different “general attitudes” or factors, that is, underlying unobservable variables empirically determined to be highly correlated with “good mental

⁷² Since there was no one single questionnaire Guttman had translated, but the items that were used to build the Haganah instrument were borrowed from various survey schedules, it is not entirely clear to me whether the “mood” questions were part of the original measurement of troop morale. These particular items may have been part of the Neuropsychiatric Screening Adjunct, a survey instrument developed by Robin Williams, Shirley Star, and Guttman in late 1944 in order to boost up the efforts of the Surgeon General’s office to weed out recruits most likely to suffer neuropsychiatric symptoms if they were allowed into the army. See: Joseph W. Ryan, *Samuel Stouffer and the GI Survey: Sociologists and Soldiers during the Second World War* (Knoxville: The University of Tennessee Press, 2013), 76-77.

conditioning of troops.” The four ‘general attitudes’ were zeal, self-confidence, discipline, and satisfaction.⁷³

This elaborate architecture of attitudes was the culmination of intensive efforts at the Research Branch, and was originally presented in the opening of the first issue of *What The Soldier Thinks*, a monthly digest of war department studies on the attitudes of American troops:

A commanding officer knows that if his men are zealous, disciplined, self-confident, and free from basic personal dissatisfaction, he is leading troops with high morale. Such mental armor combines with technological proficiency and physical fitness to produce an efficient soldier who can be counted on to train, to endure boredom, to fight.⁷⁴

Hence, for instance items 20 and 22 in the Haganah questionnaire—“What is the quality of food that you get in the army?” and “Are there good sleeping arrangements in your unit?” respectively—inquired both about the degree to which the respondent believed the army cared about and looked after him or her as an individual. Since the degree of “belief in the army’s concern for individual welfare”—one of the seven specific moral attitudes—affected the zeal, discipline, self-confidence, and satisfaction of the troops, responses to these two questions generated data on the basis of which morale could be calculated.

⁷³ The seven ‘specific attitudes’ were: “faith in the cause and in the future,” “pride and confidence in outfit,” “belief in the mission,” “confidence in training and equipment,” “realistic appraisal of the job ahead,” “satisfaction with job assignment,” and “belief in the Army’s concern for individual welfare.” I discuss the translation of these categories of measurement below.

⁷⁴ *What the Soldier Thinks, A Digest of War Department Studies on the Attitudes of American Troops* (Periodical Publication of the Research Branch, Information and Education Division, War Department) 1, no. 1 (December 1943): 1.

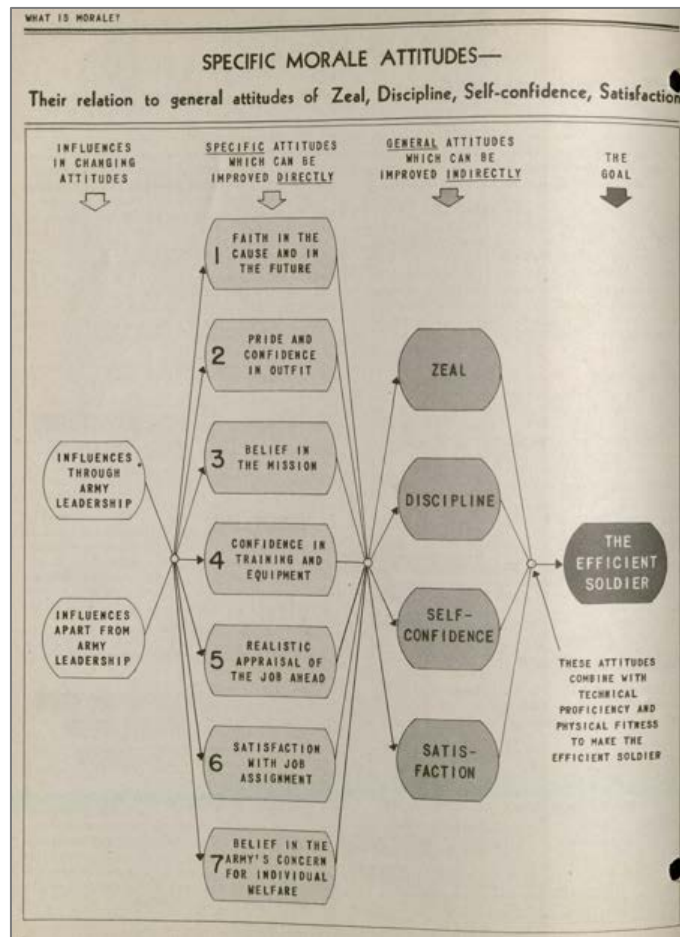


Figure 6: Specific morale attitudes (TAS)

The true novelty of this measurement instrument, however, laid not so much in the categories of analysis, but in the design. Since scalogram analysis required a set or “sample” of items for every “universe of content,” the questionnaire consisted of several similarly phrased questions for each of these factors or attitudes rather than the single-question-per-topic typical of the public opinion poll. In a survey of morale among members of a Women Corps unit stationed in Jerusalem, which took place several months later, Guttman added a methodological comment to the report, explaining the advantage of this multi-item structure in lay language:

The final results of this survey...are a general summary of the questions in their entirety. And justifiably so, since from an analysis of the responses provided for each question separately it is impossible to get an accurate picture of the attitudes of the respondents. In many cases, the response is influenced by how the question was

worded or by other factors. It is therefore necessary to view each topic and all its responses as one whole in order to know the percentage of those with a favorable or unfavorable attitude.⁷⁵

This multi-item structure is perhaps easier to demonstrate with a later questionnaire:

<p>Your opinion about the food being served</p> <p>8a) Are you being served enough food, given your condition?</p> <p>1. <input type="checkbox"/> Yes</p> <p>2. <input type="checkbox"/> Maybe yes</p> <p>3. <input type="checkbox"/> Maybe no</p> <p>4. <input type="checkbox"/> No</p> <p>8b) Are the food products used in the hospital of good quality?</p> <p>1. <input type="checkbox"/> Yes</p> <p>2. <input type="checkbox"/> Maybe yes</p> <p>3. <input type="checkbox"/> Maybe no</p> <p>4. <input type="checkbox"/> No</p> <p>9a) Is the food being prepared in the hospital tasty?</p> <p>1. <input type="checkbox"/> Yes</p> <p>2. <input type="checkbox"/> Maybe yes</p> <p>3. <input type="checkbox"/> Maybe no</p> <p>4. <input type="checkbox"/> No</p> <p>9b) Is the food usually served hot and fresh?</p> <p>1. <input type="checkbox"/> Yes</p> <p>2. <input type="checkbox"/> Maybe yes</p> <p>3. <input type="checkbox"/> Maybe no</p> <p>4. <input type="checkbox"/> No</p> <p>10) Is the menu varied enough?</p> <p>1. <input type="checkbox"/> Yes</p> <p>2. <input type="checkbox"/> Maybe yes</p> <p>3. <input type="checkbox"/> Maybe no</p> <p>4. <input type="checkbox"/> No</p>

Figure 7: Example questionnaire

As the example above shows, each attitude—marked in this case by a sub-heading—is probed five times. This seemingly superfluous construction would allow Guttman and his team to divide the soldiers into five groups of ascending food satisfaction in a way that is quantifiable

⁷⁵ “What Do the Soldiers in HEN (the Women Corps) Think? Results of a Survey Conducted in September 1948 in HEN Battalion 205” (Classified), The Institute of Public Opinion Research, Psychological Research Unit, General Staff of the Israel Defense Forces, The Human Resources Directorate, 24 pages and appendix (undated), IDFA 283-852-1951, 4.

and reliable—which cannot be said for a competing questionnaire method of asking the soldiers to rate their satisfaction from “1” to “5.”⁷⁶

How was that achieved? The questions were sorted according to the number of positive responses they received, pointing to the question with the least positive responses as the ‘strongest’ one. The next step in the analysis relied on the observation that if indeed this set of questions formed a scale, the respondents could also be rank-ordered: those who answered positively to the strongest question almost invariably answered positively to the weaker ones, but not vice versa. Guttman demonstrated this principle with the knowledge of arithmetic: a student that correctly answered a long division question will almost invariably be able to answer a simple addition question, but the reverse is not true.⁷⁷ A conceptual demonstration of such ordering is shown below:

⁷⁶ “Questionnaire for Soldiers in Hospitals” (Classified), The Institute of Public Opinion Research, Psychological Research Unit, General Staff of the Israel Defense Forces, The Human Resources Directorate, 19 pages (undated), IDFA 283-852-1951. For the results, see: Major L. Guttman to Deputy Head, The Human Resources Directorate, “Re: Hospitals Study,” August 16, 1949. IDFA 11-6722-1949.

⁷⁷ More elaborate analysis methodology, incorporating exclusion of outliers and more informed grouping of possible responses, was omitted here for the sake of simplicity.

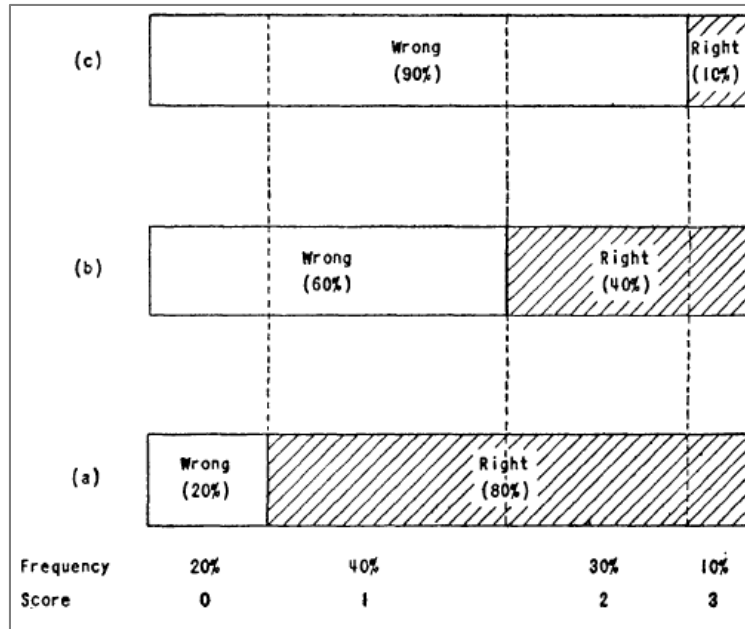


Figure 8: Three questions labeled a-c divide the responders to four groups, labeled 0-3

This analysis utilized relatively simple (“clerical”) methods to dramatically simplify the data. Graphical representation also made assessing the frequency of ‘outliers’ simpler, as respondents who answered positively on the strongest question but negatively on a weaker one could be easily identified. A small frequency, which will always be present, gives an objective and independent measure of the reliability of the questionnaire. Large frequency—essentially an inability to form a scale - is an indication that the questions do not in fact refer to the same underlying object of interest, which forces the researcher to either exclude some of them, or to conclude that this set does not examine a single underlying attitude.

The final yardstick in objectivity—the ability to compare attitudes between surveys—was achieved in the Haganah questionnaires using intensity functions. As the army food item below demonstrates, each item in the Haganah morale questionnaire included a pair of questions. The first question collected data on what is known in survey lingo as the ‘direction’ of the attitude, i.e. whether it was favorable or unfavorable. The second question measured the so-called ‘intensity of feeling,’ that is, how strongly was that particular opinion or attitude held.

20) What is the quality of food you get in the army?
1 _____ Very good quality
2 _____ Fairly good quality
3 _____ The quality is not that good
4 _____ The food is very bad
21) How sure are you of your above opinion?

Figure 9: Example questionnaire

As we see above, the first question (number 20) resembles in structure the ones described above. The second question in the pair - question number 21—then inquires about the extent to which the respondent was certain about the response he or she gave to the previous question. Again, a numbered series of four sequential options followed, going from a high level of conviction (“[I’m] totally convinced”) to low (“I am not sure [about my above opinion]”). Such ‘intensity questions’ followed each and every ‘content’ question in the questionnaire.

Again, since the final report for the very first morale study could not be found, an example from the detailed Women Corps report would help clarify how these intensity questions were utilized during the analysis:

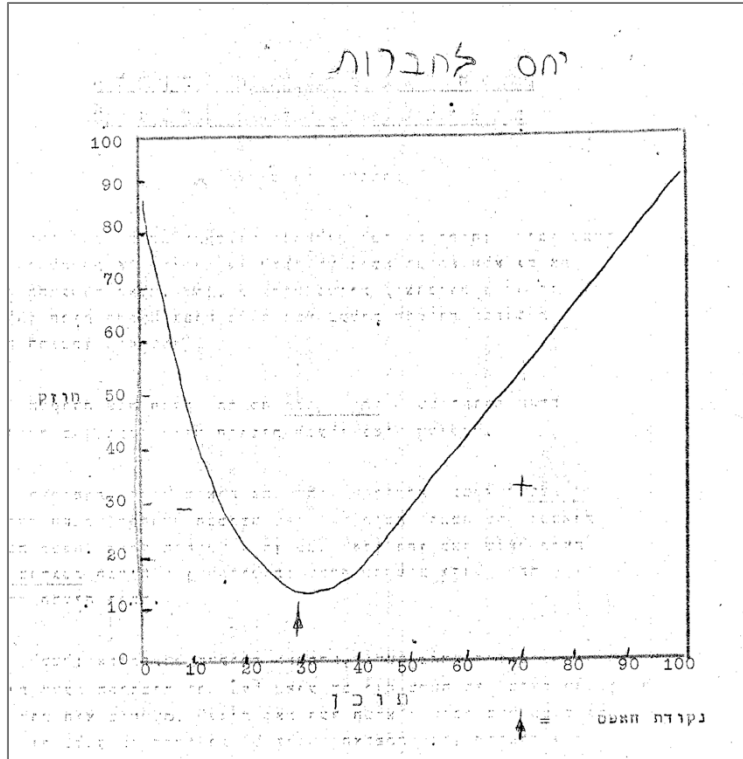


Figure 10: "Relationship to your Comrades"

In the plot above, titled “Relationship to Your Comrades”, the horizontal axis is titled content, and is scaled from 0-100, reflecting a scale of questions regarding attitude towards one’s comrades ranked from the weakest to strongest (one that yielded the most positive responses are on the right). The vertical axis is labeled “intensity.” The plot, drawn by hand, connects the intensity values for each of the questions to form a lopsided U shape. The very bottom of the curve - the “zero point” marked by an arrowhead - points to an exact number on the horizontal axis: 28%. That value is the Holy Grail that Guttman sought to achieve when trying to tame the ‘immeasurable’: an objective representation of the appreciation the female soldiers have of their comrades. That number is independent of the choice of questions or their number, the language they are written in or their wording, the administrator of the questionnaire or the statistician analyzing the responses.

Guttman was aware of the power this number held. It can be compared to similarly obtained values for other measured attributes: in the same study, similar values were obtained for “opinion on the commanders” and “mood”, but only a 50% satisfaction from the “conditions at the base.” Crucially, similar comparisons are also made to other studies: after concluding that “58% of women soldiers are not satisfied with their jobs,” the report goes on to state, “In the American army it was found that 50% of soldiers are not satisfied with their job.” This discrepancy opens up a room for improvement: “This situation can be rectified by careful selection of appropriate people for the job... Widespread propaganda work and personal and attentive treatment can yield vast improvement in this field.”⁷⁸

Throughout March 1948, an unknown number of HISH recruits, 649 HIM recruits, and 121 privates of the Haganah Medical Service in Jerusalem (more than half of them women) between the ages of 18 and 50, answered the morale questionnaire for Haganah members and surrendered the thrills and woes of their soldiering experience to the strictures of Guttman’s attitude categories. These newly ordained ‘respondents’ came from every corner of the Jerusalemite public. Among them were students and tradesmen, clerks and shop-owners, professionals and day laborers. Some had less than six years of schooling and some were university graduates; about two thirds were Ashkenazi and half were married. The vast majority immigrated to Palestine in the 1920s and 1930s or very recently (as part of organized illegal immigration of Jewish refugees and survivors from postwar Europe), and only a small number were native born. What they most clearly had,—that is, lacked—in common was military experience. More than half were rather new to the Haganah (less than a year), and even though a

⁷⁸ “What Do the Soldiers in HEN (the Women Corps) Think?,” 9-11.

small minority had served in a regular army before in various capacities, almost none of them had been in battle conditions before.⁷⁹

The reports of the results reuse the template Guttman and his colleague in the research branch used during WWII. Short sentences, interspersed with percentages and quotes from the ‘free form’ section of the questionnaires, serve to paint a detailed picture of the daily lives and experiences of the soldiers: The American soldiers “like Army food. Almost all report they get enough to eat. But they want *more* milk. ... Boys from the North want less sweet potatoes and hominy grits.”⁸⁰ “Most of the [HISH soldiers] find the room for improvements in the sleeping arrangements. On the other hand, most of the soldiers are satisfied with the food.”⁸¹ “50% of the women soldiers serve in bases lacking books.”⁸²

Despite that, and in contrast to the expectations of all parties involved, including those of the Research Department, the study concluded that the recruits of the Haganah in Jerusalem enjoyed an extraordinarily high morale. Whilst acknowledging that there were definitely “factors that reduced” morale—“almost a third of the privates believe they did not receive sufficient training,” was stated in the early report on the HISH, and “over a third...are of the opinion that their current job assignment is not right for them”—Guttman felt that the official pessimistic

⁷⁹ “What the HISH Men Think, Summary of Results,” Report 02-1 (Confidential), Research Department, The Information Office of the Haganah in the Jerusalem District, Adar alef, Tashah (March, 1948), 3 pages, IDFA 582-4944-1949; “The State of Morale among the HIM in Jerusalem,” Report 02-3 (Confidential), Research Department, The Information Office of the Haganah in the Jerusalem District, Adar beit, Tashah (April 1948), 4 pages, IDFA 584-4944-1949; “What the Medical Service Personnel Think” (Confidential), undated, 3 pages, IDFA 514-4944-1949.

⁸⁰ *Studies in Social Psychology in World War II: The American Soldier*, 4 vols., ed. Samuel A. Stouffer et al. (Princeton, NJ: Princeton University Press, 1949).

⁸¹ “What the HISH Men Think,” 1.

⁸² “What Do the Soldiers in HEN (the Women Corps) Think?,” 17.

prognosis regarding the fighting capacity of these men and women was unjustified.⁸³ “The state of morale—as far as it is possible to discuss it [sic] in this early report—is not so bad,” he remarked.⁸⁴ The men liked their food; they felt rather useful (“they didn’t feel like they were wasting their time”) and most of them were on friendly terms with their fellow unit members. In fact, as the Research Department had reported a couple of months later, the study had found that “the approach of the Haganah recruits in Jerusalem was one of self-confidence, dedication, and willingness to suffer.”⁸⁵

The scores on ‘personal morale’ were surprisingly high as well.⁸⁶ Nearly 85 percent of HISH recruits were in a “positive personal mood,” and only about 15 percent exhibited different levels of “negative mood.” This surprising distribution repeated itself to even a greater degree in the results of the HIM survey (90 percent positive mood) and the Medical Service survey (almost 100 percent positive mood).⁸⁷ Attempting to convey the radical nature of these results yet shy away from hyperbole and empty rhetoric, the June report provided a summary conclusion of these surprising results: “In all the units, spirits were higher than in that part of the American

⁸³ “What the HISH Men Think,” 1.

⁸⁴ “What the Medical Service Personnel Think,” 2.

⁸⁵ Quarterly report, 3; a similar but less dramatic formulation appears in “The State of Morale among the HIM in Jerusalem,” 1.

⁸⁶ It seems that ‘personal morale’ or ‘mood’ was a separate measure from morale with a capital M. Since the final report—if such a document existed—did not survive, it is difficult to know what was the relationship between these amazingly high scores and overall morale. Quoted in a news item a decade later, when the fact such studies took place during the 1948 War became public knowledge (they were kept secret up until then), Guttman said that in final analysis, two thirds of the HISH recruits had positive morale, and 70 percent of the HIM recruits. These are clearly lower scores. See: “Research on ‘Morale’ in the IDF,” *Ha’artz*, January 28, 1957.

⁸⁷ Quarterly report, 3.

Army which hadn't yet left to fight overseas in 1944, out of which only about thirty to forty percent had a positive degree [sic] in this direction.”⁸⁸

It turned out that the solicited research into the Haganah's foreseen manpower problems was “not so crucial” after all, wrote the Institute chronicler. “No matter how bad the situation was, there was no manpower problem. Everyone felt they could cope with whatever might happen and the spirits were the highest ever recorded in any survey which we had experienced.”⁸⁹

The Warrior's Spirit and the Soldier's Mood

As demonstrated above, Guttman's specific innovations in survey measurement promised to serve as a Babel fish - to be capable of generating an instrument that can be rendered in a different languages and contexts without affecting the measurement. This technique promised to overcome some of the known problems of translation: literal translation of specific questions and specific instrument (the scaled attitude questionnaire built by the Research Branch), and perhaps as importantly, translation of the cultural model (the standardized questionnaire itself or the practice of asking questions).

Guttman was preoccupied with the problem of translation already during his stint with the Research Branch, formerly the Morale Division of the U.S. Army. The mission of the social scientists employed by the Army was to study and improve the morale of soldiers. Various attitudes that were found to be in correlation with morale were studied, from complaints about

⁸⁸ Ibid.

⁸⁹ Gratch, *Twenty-Five Years of Social Research in Israel*, 15. This is repeated with emphasis in the interview with Gutkind from 1978, and in Guttman's short autobiographical essay, where he writes, in his matter-of-fact style: “During the siege of Jerusalem, morale was very high. No problems there.” (323)

food to willingness to fight the enemy. “While it was tacitly agreed that such varieties of behavior were kinds of morale, no formal definition for ‘morale’ emerged, even in the postwar volumes published on this work.” Guttman argued that a scientific definition of morale that transverse the fuzzy, largely impressionistic, one could only comes out of measurements like the ones he has been conducting. However, he also felt it was first necessary to introduce the concept of morale and provide an outline of the psychology of the soldier into a (military) culture he believed was unfamiliar with the concept. The choice that was made by the Research Branch at the time was a reflection of this conundrum: to measure ‘adjustment’, which was a better defined and, perhaps more importantly, measurable entity correlated with morale, but to refer to the result of the measurements as if they are measuring morale itself.

Instead of solving the problem of measurement of group morale, the Research Branch, in large part, bypassed it. Faced with the necessity of giving the Army command, quickly and reliably, information that would be useful in policy-making, the Research Branch concentrated primarily not on evaluation of the cooperative zeal of groups toward Army goals, but rather on [the] study of personal adjustment. . . . Even though the concept of personal adjustment is an individual and not a group concept, it is nevertheless useful for group comparisons. If most of the soldiers in one outfit show evidence of adjustment, from the Army’s point of view, and if most of the soldiers in another outfit show little evidence of adjustment, it is not unlikely that the former outfit would be found to have, if it could be measured, higher morale than the latter. Hence, since morale is presumed to be an important element in performance, the groups with the higher average individual adjustment scores should, *all other factors in the situation held constant*, show the better performances.⁹⁰

However, beyond the fact that individual personal adjustment was substituted for group morale, “adjustment” had a history and carried cultural baggage, which the questionnaire carries with it. Similarly, I argue that there were adaptations of the measured world (the experiences of

⁹⁰ Samuel A. Stouffer and Leland C. DeVinney, “How Personal Adjustment Varied in the Army—Preliminary Considerations,” in *The American Soldier: Adjustment During Army Life*, vol. 1 of *Studies in Social Psychology in World War II*, ed. Samuel A. Stouffer et al. (Princeton, NJ: Princeton University Press, 1949), 85.

the Haganah recruits) to the instrument of measurement. Most importantly, there had to be a translation, both literal and conceptual, of the notion of morale, naturalizing and democratizing it by transforming it from an ideological and spiritual achievement to a psychological notion and an attribute possessed by everyone. Hence, the concept of morale went through a two-step translation under the auspices of Guttman: first, as explained above, it was transformed from the fuzzy, impossible-to-measure ‘morale’ to ‘adjustment.’ In Palestine, this again had to be translated, this time from ‘morale’ to ‘spirit’.

The dilemmas involved in the importation of a behavioral scientific vocabulary into the Jewish army in the making manifested already at the preliminary stage of study, when Guttman and his team had to find an appropriate and meaningful lexical equivalent to the notion of morale, which did not exist in Hebrew. Even though the records indicate that local actors were familiar with the term, and it appeared from time to time in documents related to the plight of civilians in besieged Jerusalem, or as we have seen, in commentary about the mental readiness of the recruits, ‘morale’ was not in common usage in Jewish Palestine of the 1940s. A quick search through the Historical Jewish Press database reveals that the word first appeared in print—in Hebrew transliteration (לאַרוּמ) —in 1948 in the context of the war, but usage was at first sporadic.⁹¹ Interestingly, in early usage ‘morale’ would often appear with quotation marks placed around it, to signal that this was a borrowed term. Like other Western nationalist terms imported into the language in the process of Jewish political modernization and the making of Israeli militarism, its status was ambivalent.

⁹¹ Popularity had risen steadily through the mid 1950s and eventually it had been entirely vernacularized. See: Historical Jewish Press, accessed January 6, 2016, <http://web.nli.org.il/sites/JPress/>.

In Zionist Palestine of that time, speaking Hebrew was a matter of utmost importance, conceived as a cultural imperative and a national duty. The chosen lingua franca of the new society, a lot hinged on successfully vernacularizing what has been just a generation before a liturgical written language, used primarily (and many times exclusively) for religious and literary ends. And as leading agents of Zionist socialization, the underground military organizations - and the Haganah in their lead - made the use of foreign words an anathema. When an illustrated brochure surveying the problems of morale—with suggestions on how to cope with them—was prepared by the Research Unit and submitted to the Chief of Staff, General Yaakov Dori, he approved it for publication, and ordered it to be distributed under his own signature to the six thousand officers of all ranks. But with one proviso: the Hebrew title had to be changed from “On Morale Victory Depends” to “On the Spirit of the Army Victory Depends.”

‘Spirit’ (*ru’ach*) was the closest lexical equivalent of ‘morale’ in vernacular Hebrew of the time was. Spirit appears either on its own or in the construct state, such as in *helech-ru’ach* (the tendency or trend of public mood, feeling, or atmosphere) or *ru’ach-khav* (battle spirit)—it could also signify mental and moral stamina, vitality, a condition or state associated with moral virtues such as courage, and so on. And indeed, propaganda documents from that period referring to the crisis of morale in Jerusalem, expressed concerns over “*helech ha-ru’ach* in the public” and spoke about the need to “strengthen the *spirit* of the home front and the frontline,” “encourage the *spirits*,” and “bring interesting, on-the-mark material that lifts people’s *spirit*.”⁹²

But it was not simply a linguistic matter. As Shalem explains, “battle spirit” was extremely important in the Haganah. “The spirit is means, but should be viewed as an end in its

⁹² Mishmar mission statement, 2.2.48, Mishmar docs, special squad report. In the letter Zvi sends to Patkin re the morale plan (the initiative to establish a committee of experts), there an actual translation. The expression “public morale” appears in English followed by “*helech-ru’ach* in the public” in parenthesis

own right.” Battle spirit was a feature or quality of the unit as a group, and the fellowship of warriors was its social basis. Spirit could be developed, the belief went, through intensive educational and social activity, which was built into the unique training regime and agricultural labor. A tangible manifestation of camaraderie was the willingness to self-sacrifice for the group - the sacrosanct ideal of never leaving a wounded, or even dead, comrade behind in the battlefield.

This ethos of soldiering received its clearest expression in a foundational text by the Palmach’s commander Yigal Allon, arguing that what made a soldier was not undergoing military training. Soldiering was not a job, and war was not a form of labor: skills, maneuvers and doctrine are secondary and at the service of this higher aim - spiritual transformation, the fostering of “the warrior’s spirit”:

Technical military training cannot by itself create perfect soldiers and units, unless they received also their spiritual ‘training’, that is, their education...while the purpose of training is professional knowledge, [as well as] the forging of the body, the senses and instincts of a warrior, education comes to deepen the Palmachnik’s feeling and constitute his ideological consciousness as a Hebrew pioneer and man of defense, and foster [his] spiritual resilience, what’s called ‘the warrior’s spirit.’⁹³

This was achieved via propaganda and education as well as through physical labor in nature. A mental training regime was developed to generate this inner state. “The Palmach consistently sought supreme discipline that makes coercion unnecessary...discipline whose content is responsibility and clear consciousness [hakara]...where formal rules are only the outer shell.”⁹⁴

⁹³ Shapira describes Allon himself as the ultimate embodiment of the Zionist warrior figure he had illustrated and prescribed in “Megamot u-Ma’as”: manly, self-confident, and native born, “a sabra personified”: “All the girls at Ginossar were attracted to Yigal: apart from his reputation as a farmer and warrior, he was handsome, nice, cheerful, and good-hearted.”

⁹⁴ 211. While this notion carried particular importance during the times of the Haganah, it still remained crucial once conscription was introduced. In the messianic vision of these generals-ideologues, military service was perceived as

‘Spirit’ therefore, while not an exact literal translation to begin with, was also laden with a rich cultural history and revealed commitments to particular metaphysical views concerning behavior. These notions contrasted sharply with the Research Department’s working definition of morale: “a general psychological state, a tendency toward pessimism or optimism, depression or elevation, *a feeling of being able to overcome obstacles or the absence of such a feeling.*”⁹⁵ Guttman’s used this definition, explicitly or implicitly, when he refers to ‘spirit,’⁹⁶ treating it as a simple lexical equivalent: “the morale of the Women Corps and the extent of its contribution to the war effort depends on these psychological factors in the heart of each and every soldier.”⁹⁷ Same goes for what is measured in the nationwide bases study: “70% of the soldiers are in good personal mood”; and the emphasis is on factors of personal adjustment (as was the case in *TAS*): “the soldier’s satisfaction with the arrangements at his base determine to a great extent his satisfaction with army life and thus impacts military moral in general.”⁹⁸

Fear in Battle

Shortly after the morale study results had been distributed among relevant Haganah officers and commanders, the Research Department initiated the preparation of another

a necessary “corridor” for the utopian society, a transformative phase and “breeding ground for the kind of man” required by the needs of the national project: military service is where the Zionist subject-citizen, the member of this new society, is being created.

⁹⁵ My emphasis, Development of Mood booklet, 1949.

⁹⁶ Not only that, Guttman chose the Hebrew construct “*matzav-ru’ach*” which means “mood.” Rather than talking about ‘battle spirit’ (esprit de corps), he used *matzav-ru’ach*, which, treated apart, is literally the state or disposition of spirit, but the construct in colloquial Hebrew means “mood.”

⁹⁷ “What Do the Soldiers in HEN (the Women Corps) Think?”

⁹⁸ Bases report, 7.

handbook; this time a personal guide for soldiers on how to handle fear.⁹⁹ Little is known about the specific reasons behind the decision to issue such a document at that time. According to chronicle of the Institute, the guide was thought of and designed as a mental health prophylaxis; one of several such measures that were introduced in the context of a campaign to systematize preventive psychiatric care in the nascent army. Even though morale had proven to be high, there was a growing worry that “battle fatigue”—combat stress reaction in WWII military parlance—would eventually catch up with those among the Haganah forces who were continuously engaged in fighting along the city fault lines.¹⁰⁰

The major piece of the campaign was a counseling service in the spirit of American mental hygiene clinics.¹⁰¹ A collaboration of the Information Office, Welfare Branch, and Medical Service of the Haganah (with Hadassah Medical Organization providing the psychiatrist), the ‘Personal Consultation Station’—the first of its kind in the country—began operating in early June 1948, offering individual psychotherapy to Jewish soldiers in need.¹⁰² However, based on studies of American combat soldiers, Guttman felt it was important to put

⁹⁹ “Report on the work of the [Research] Department,” Research Department, Information Office, Jerusalem Brigade, May 20, 1948. IDFA 613-4944.

¹⁰⁰ Gratch, *Twenty-Five Years of Social Research in Israel*, 15.

¹⁰¹ The mental hygiene movement first arrived to Palestine with the Austrian psychoanalyst and psychiatrist Martin Pappenheim in the mid-1930s. American-style mental hygiene clinics or “stations,” along with a greater emphasis on preventive treatment were a development of the early 1950s, with the growing influence of American conceptions and models on local psychiatric practice. See: Rakefet Zalashik, *History of Psychiatry in Palestine and Israel, 1882-1960* (Tel Aviv: ha-Kibbutz ha-Meuhad, 2008), 119-125. (Hebrew).

¹⁰² The station’s official task was to handle all the cases of “mental disorder and nervousness,” but every soldier who needed counseling could contact the service. See the official announcement of the Brigade Welfare Officer, June 15, 1948, IDFA 18-6722-1949. Guttman, the spirit behind the initiative, was in contact around that time with Dr. Gerald Caplan (1917-2008), the world-renowned child and community psychiatrist (and later Harvard professor), who was a great proponent of preventive mental health. Working in Israel from 1949 to 1952 as a special advisor to the Israeli Ministry of Health, Caplan was in charge of preparing a national program of mental hygiene for the State of Israel. It is not clear whether Caplan was directly involved in the counseling service initiative, but the two had similar ideas regarding military morale and its discontents. See: Gerald Caplan to Louis Guttman, January 30th, 1949. IDFA 13-6722-1949.

preventive information in the hands (or pockets) of *all* the men and women in service in Jerusalem so as to reassure them that “almost every soldier, whether rookie or veteran, is somewhat afraid before battle.”¹⁰³ The US Army made use of such handbooks and pamphlets during the Second World War to build up a permissive attitude toward fear and anxiety symptoms among the troops.¹⁰⁴ It was hoped that teaching Haganah recruits that feeling fearful in face of danger was perfectly normal would similarly contribute toward minimizing “maladaptive fear reactions.” The initiative could therefore be seen as complementing the measurement of morale with more interventionist strategies for modernizing and normalizing Jewish soldiering according to an American model.¹⁰⁵

The direct inspiration for Guttman’s army pocket guide and the source of the facts it cited and the techniques it recommended was John Dollard’s 1944 *Fear in Battle*.¹⁰⁶ Dollard, an American sociologist trained in psychoanalysis who was known for his provocative studies on race relations in the deep South, began working on the problem of battle fear in the context of his tenure as expert consultant to the Research Branch of the War Department’s Information and Education Division, which is also where he and Guttman crossed paths.¹⁰⁷ Though the specific

¹⁰³ *Fear in Battle, Handbook No. 2*, published by the Research Department, the Information Office of the Haganah in the Jerusalem District (undated), 16 pages (stencil), the Jewish National and University Library, Jerusalem, Introduction, 3. (Hebrew).

¹⁰⁴ For the US Army’s ‘fear is normal’ policy, see: Irving L. Janis, “Problems Related to the Control of Fear in Combat,” in *The American Soldier: Combat and Its Aftermath*, vol. 2 of *Studies in Social Psychology in World War II*, ed. Samuel A. Stouffer et al. (Princeton, NJ: Princeton University Press, 1949), 196-207.

¹⁰⁵ In regards to a later survey on soldiers’ feelings in combat (“lessons learned under fire”), which was conducted in August-September 1948 in Jerusalem and asked also about battle-related fears, it was noted as reassuring that response patterns were similar to those of American soldiers during WWII, and that Israeli soldiers’ feelings and attitudes were “normal.” See: Russell A. Stone and S. Ilan Troen, “Early Social Survey Research in and on Israel,” in *Israel: The First Decade of Independence*, ed. S. Ilan Troen and Noah Lucas (Albany: State University of New York Press, 1995), 378.

¹⁰⁶ John Dollard, *Fear in Battle* (Washington, D.C.: The Infantry Journal, 1944).

¹⁰⁷ Dollard served as consultant to the Department of War from 1942 to 1945. The preliminary work for the fear-and-courage study began in fall 1941. For Dollard’s ethnographic work in the South and his intellectual sensibilities,

needs of the army were inevitably a consideration—like Guttman, Dollard was employed in investigating morale factors—studying the workings of fear in combat soldiers was very much a continuation of his prewar research agenda by other means.¹⁰⁸

In the late 1930s, Dollard was a member of a distinguished group of young researchers at the Yale Institute of Human Relations that, inspired initially by neo-behaviorist Clark C. Hull, sought to integrate classical learning theory and Freudian psychoanalysis in hope of constructing a unified “science of human behavior.”¹⁰⁹ The group’s first major publication, the 1939 *Frustration and Aggression*, outlined a new social learning theory. According to the frustration-aggression hypothesis (otherwise known as the frustration-aggression displacement theory) the book had famously put forward, the many diverse forms of aggressive behavior—“strikes and suicides, race prejudice and reformism, sibling jealousy and lynching, satirical humor and criminality...wife-beating and war”—could be explained by reference to a previous individual experience of frustration. No matter what form it took, Dollard and his group argued, aggression was always the consequence of frustration, namely, the inability to satisfy some want or wish (anything from sex to wealth to winning the appreciation and respect of your boss). When the original source of frustration could not be confronted, often due to fear of punishment or social inhibition, aggression got displaced onto an innocent yet legitimate target—from “in-groups” to

see: Rebecca Lemov, *World as Laboratory: Experiments with Mice, Mazes, and Men* (New York: Hill and Wang, 2005), Chapter 6. For the genesis of *Fear in Battle*, see: Rebecca Lemov, “The Laboratory Imagination: Experiments in Human and Social Engineering, 1929-1956,” Ph.D. diss., (University of California, Berkeley, 2000).

¹⁰⁸ The following refers to an explicit research agenda that guided the work of a specific group of Yale scientists, and had a major influence on the techniques psychological experts used both to boost and destroy morale during the war. The study of fear has a longer history. It had been a central object of scientific inquiry in behaviorism from its early days, notoriously in Watson’s ‘Little Albert’ experiment.

¹⁰⁹ Jill Morawski, “Organizing Knowledge and Human Behavior at Yale’s Institute of Human Relations,” *Isis* 77 (1986): 219-242.

“out-groups,” from the other to the self—through a culturally-coded “substitute response.” This explanation, which located the root cause of aggression in the sociocultural environment, challenged the widely accepted notion that aggression was an innate human tendency and therefore constituted a “basic instinct.” Furthermore, it suggested that aggression, itself an acquired behavior (one of various responses to frustration one learned in the process of socialization), played a role in social learning in childhood and adolescence.¹¹⁰

This last understanding, regarding the relationship between aggression and behavior adjustment, carried over to a series of experiments on “avoidance learning” that took place at Yale around the same time. Hooking human subjects up to electrodes, O. Hobart Mowrer—one of Dollard’s collaborators on *Frustration and Aggression*, who had been studying aversive conditioning since the mid-1930s—discovered that it was the subject’s anxiety—her “state of expectancy,” as he called the nerve-wrecking (learned) anticipation of a painful stimulus (such as electric shock)—rather than the painful stimulus itself, that conditioned her responses and made her change her behavior.¹¹¹ Experimenting on rats, Neal Miller, another researcher in the Yale group and Dollard’s partner in the early stages of the fear-and-courage study, had demonstrated that acquired fear indeed motivated the learning of new responses.¹¹² This experimental work,

¹¹⁰ John Dollard et al., *Frustration and Aggression* (New Haven: Yale University press, 1939). For this last point, see particularly chapter 4, “Socialization in America” and chapter 5, “Adolescence.” This idea would become a central tenet of Dollard & Miller’s mature social learning theory.

¹¹¹ O. H. Mowrer, “Preparatory Set (Expectancy)—A Determinant in Motivation and Learning,” *Psychological Review* 45 (1938): 271-299; “A Stimulus-response Analysis of Anxiety and its Role as a Reinforcing Agent,” *Psychological Review* 46 (1939): 553-565; Lemov, *World as Laboratory*, Chapter 5.

¹¹² Miller placed a group of Albino rats in a contraption consisting of two compartments separated by a door (‘avoidance chamber’). One compartment was white with a grid as a floor; the other was black without a grid. Before training, the rats showed no marked preference for either compartment. They were then placed in the white compartment, received an electric shock from the grid, and escaped to the black compartment through the open door. After a number of such trials, the rats would run out of the white compartment even if the grid had not been charged. Fear of the white compartment had been learnt. To demonstrate that the general pattern of the fear response was that of an “acquired drive”—the term Miller and others have chosen for strong stimuli-producing responses which were learned through socialization but could motivate new learning—the rats were taught a new habit without

which continued through 1941, when the United States entered the Second World War, led to the conclusion that fear could be learned as a response to previously neutral cues and motivate the learning and performance of new and better adaptive responses. In other words, moving away from the classic view of fear as a disruptive emotional reaction, Mowrer, Miller and their colleagues emphasized the role fear may play in learning processes and suggested it could be controlled and purposefully manipulated.

Like other behavioral scientists who found the war a proper and productive extension of the laboratory, Dollard and Miller were looking to show that this behavioral model of fear applied also in the real world, in battle. In pre-Pearl Harbor United States, however—Dollard explained in an introductory note to the study report—“battle-wise riflemen were hard to find.”¹¹³ The only veterans of recent combat were the American men and women who had volunteered for service in the Spanish Civil War—the veterans of the Abraham Lincoln Brigade.¹¹⁴ Although the Lincoln Brigade was, for the most part, made up of ideologically driven and highly motivated volunteers who differed markedly from the conscripts of the Second World War, Dollard had hoped that their experience would be adopted as representative of “the

further shock: the door between the compartments (previously always open) had been closed and the only way for it to reopen was by rotating a little wheel. Under these conditions, the rats exhibited trial-and-error behavior and gradually learned to escape from the white compartment as before. Neal E. Miller, “Studies of Fear as an Acquirable Drive: I. Fear as Motivation and Fear-reduction as Reinforcement in the Learning of New Responses,” *Journal of Experimental Psychology* 38 (1948): 89-101.

¹¹³ Dollard, *Fear in Battle*, 1. Mechanized warfare had changed the conditions of battle to such an extent that the experiences of WWI American Army veterans were largely irrelevant for a study intended to illuminate infantry reaction to theaters of war swarming with tanks and dive-bombers. Since American troops had not yet been sent overseas, and simulating such a complex “coercive environment” as war was beyond their abilities, the two needed to find subjects who already had their baptism by fire.

¹¹⁴ An estimated 2800 American citizens traveled to Spain between early 1937 and mid-1939 to fight with the Spanish Republic—the progressive government of the Popular Front—against General Franco’s Nationalist rebel forces and fascist agenda. Most of them joined the International Brigades, organized in 1936 by the Communist International. The U.S. volunteers, who formed several battalions and served in various units, came to be known collectively as the Abraham Lincoln Brigade (ALB).

conditions that affect men's behavior in battle" in general and "results would prove to have military value" during the contemporary war.¹¹⁵ In mid-1942, after funding had been secured, Dollard and his Yale research staff (Miller had been assigned soon after to the Army Air Corps' Psychological Research Unit in Nashville, Tennessee and had to leave the project) conducted depth interviews with twenty "Lincolns," on the basis of which a lengthy survey questionnaire was drafted and mailed out to the rest of them.¹¹⁶ The data obtained from a sample of three hundred questionnaires was quickly processed and findings—primarily a statistical analysis of veterans' replies—were summarized in a report and published in November 1943 as *Fear in Battle*.¹¹⁷

Dollard had found that a fear response under battle conditions was nearly universal among the Lincolns, and there was nothing distinctive about those who had lost their resolve. Dispelling a centuries-old belief that associated war-related fear with cowardice, the study determined that a pervasive sense of terror was psychologically inevitable under modern battle conditions for both "green troops" and seasoned soldiers, and did not constitute a character flaw deserving of moral scorn. "Fear is normal," the report concluded; "experienced men admit it and are not ashamed." Indeed, nearly two-thirds of survey respondents confessed that they had

¹¹⁵ Generalization was not so easy to achieve. The ALB veterans were controversial subjects, and beyond the question of the representativeness of their experience, there were other obstacles, including an FBI investigation of the project early on, because the group of veterans that participated in the study contained a number of communists. I would not be able to elaborate on this in the framework of this chapter.

¹¹⁶ The project took place under the auspices of the Yale Institute of Human Relations and was funded with a grant of over eight million dollars from the Rockefeller Foundation. For the detailed account of the study and its subjects, see: José M. Gondra and Manuel S. de Miguel, "Yale University's Institute of Human Relations and the Spanish Civil War: Dollard and Miller's Study of Fear and Courage under Battle Conditions," *The Spanish Journal of Psychology*, 12, no. 2 (2009): 393-404.

¹¹⁷ *Fear in Battle* was first published by the Institute of Human Relations Press, and then reprinted in June 1944 for mass distribution by the Infantry Journal. Dollard appeared as single author in both editions. Unless otherwise mentioned, all the quotes are taken from the 1944 edition.

suffered moments of panic when they “lost their heads...couldn’t control themselves and were useless as soldiers.” Such moments were marked by palpable physical symptoms. “Many men think that ‘fear is in the mind,’” Dollard explained, but “this is an error. Fear begins with strong bodily responses and is then registered in the mind.” This was important, as early detection of these bodily signals was crucial for preventing the development of a full-blown panic attack. According to the replies the Lincolns had provided, and later, data that was collected from U.S. Army combat veterans in four infantry divisions, the most common symptoms were a “violent pounding of the heart” and “a sinking feeling in the stomach.” A second group of lesser frequency symptoms included trembling, “cold sweat,” and “feeling faint.” Involuntary defecation or urination, “the legendary signs of battle fear in the novice,” were comparatively rare.¹¹⁸

Fear was not only a normal, healthy, and reasonable reaction to danger; it was useful. After the initial shock—fear was reportedly greatest just before action—the veterans, much like Miller’s albino rats, had learned to control their visceral reaction to the terrifying sounds of artillery shells and dropping bombs (and other danger cues in their environment), and were able to function effectively. In fact, on many occasions, fear seemed to improve their performance: it made them alert, resourceful, and “cautious under fire,” discouraging recklessness and the taking

¹¹⁸ It was Guttman who developed a scale for the physiological manifestations of fear in battle on the basis of surveys conducted in April 1944 among U.S. Army troops in combat divisions in Pacific areas. Applying scale analysis to infantrymen’s reports proved that all symptoms but one fit into a scale pattern: soldiers who vomited, for example, also reported trembling, and so on down the scale; while those who reported trembling did not necessarily report vomiting too. This was important, as it permitted a rank ordering of respondents along a single continuum according to the symptoms they’ve reported, which mattered for both prediction and control of troop behavior. See: Stouffer, Samuel A., Edward A. Suchman, Leland C. DeVinney, Shirley A. Star, and Robin M. Williams, Jr., eds., *Combat and Its Aftermath*, vol. 2 of *Studies in Social Psychology in World War II: The American Soldier* (Princeton, NJ: Princeton University Press, 1949), 201 (Table 3); and *Measurement and Prediction*, vol. 4 of *Ibid.*, 140-142 (Scalogram no. 7). Guttman’s analysis largely confirmed Dollard’s findings, but also adjusted them. For reasons that are not clear to me, the information about fear symptoms in the Hebrew translation of *Fear in Battle* followed Dollard’s original rather than the adjusted results.

of unnecessary risks. Having learned how to defend against the dangers of the battlefield in turn lessened the veterans' fear. As Miller summed it up years later, the study had demonstrated "the fear-reducing power of learning and performing coping responses to...danger. Everyone was afraid—the crucial factor was whether fear motivated maladaptive or adaptive behavior."¹¹⁹

The last part of the report, then, consisted of "techniques of fear control" the Lincoln Brigade veterans engaged in and found helpful. Emphasis was placed on anticipating fear, discussing it openly, and concentrating on the task.¹²⁰ A proponent of behavioral engineering, Dollard was interested in formalizing and systematizing these local practices and introducing fear-conditioning procedures into training routines. "Fear may be aroused in training so as to serve as useful purpose. It can motivate men to learn those habits that reduce danger in battle." The hope was that by drilling recruits in specific, appropriate reactions to battle conditions they would circumvent mental breakdown and become better soldiers.¹²¹ The handbook format of *Fear in Battle*, its reliance on lay language, and its reprinting in 1944 for mass distribution, however, suggest that Dollard thought of this behavior modification primarily as an exercise in self-study and mastery.¹²² This was well exemplified by the DIY "Fear Policy for the Soldier"

¹¹⁹ Neal E. Miller, "Obituary: John Dollard (1900-1980)," *American Psychologist* 37, no. 5 (1982): 588. Fear could of course result in panic, paralysis, or flight, as it often did. Dollard mentioned these "maladaptive" responses in the report, but focused on the military usefulness of fear, which also implied that phenomena such as desertion and "cracking up" could be controlled for, perhaps even avoided altogether.

¹²⁰ The techniques included, among other things, learning to recognize the first bodily signals of fear, mental preparation (e.g. planning in advance to meet the possible dangers of battle), and adopting regular habits of emotional communication. Mantras, selective attention, and guided visualization were also recommended. See: Dollard, *Fear in Battle*, 24-36

¹²¹ Military psychologists were reluctant to recommend "the elicitation of fear in a training situation" on the basis of Dollard's study results, in the absence of clear parameters for adjudicating adaptive and maladaptive fear responses. See: Janis, "Problems Related to the Control of Fear in Combat," 194-195. Actual training in fear control in the British Army during WWII is mentioned in Ben Shephard, *A War of Nerves* (Cambridge, MA: Harvard University Press, 2001), 234-236.

¹²² The idea that one could "use fear to end fear" first appeared in a popular "home therapy" book Dollard published the year before on common fears and how they can be overcome. See: John Dollard, *Victory Over Fear* (New York: Reynal & Hitcock, 1942). As one reviewer funnily wrote: "insofar as Dr. Dollard identifies these anxieties, and

section added at the end of the study report, and the “Twelve Rules for Meeting Battle Fear,” a short self-study promo guide published in the *Infantry Journal* shortly before the reprint came out.¹²³

But Dollard did not mean for his study report to be just a handy reference for the nervous G.I. or even an inspiration for an American-style ‘Battle School’ which would be guided by the latest insights of behavioral science.¹²⁴ *Fear in Battle*, despite its packaging, also propagated a new world-picture or understanding—intelligibility, in Peter Dear’s terms—of human psychology at war, which constituted a rational and morally neutral alternative to common conceptions of battle fear.¹²⁵ Discussing norms and expectations toward British rank-and-file during WWI, historian Ben Shephard explained, “men were either sick, well, wounded, or mad; anyone neither sick, wounded, nor mad but nonetheless unwilling to or incapable of fighting was necessarily a coward, to be shot if necessary.”¹²⁶ This “rough and ready model of human psychology,” as Shepard dubbed it, which guided military discipline & punishment for generations, was now challenged. According to Dollard and Miller’s learning theory, the battlefield was no longer the ultimate test of character, a theater stage on which humanity appeared in all its greatness and depravity, but a claustrophobic “coercive environment” in which

implies that one can be free of many of them, he does the average man...a great service. But in asking this same man to master them by regimen of self-observation and discipline, the author shoots above this man’s aptitude for the task...” A. Kardiner, “Victory Over Fear (Book Review),” *American Anthropologist* 45, no. 2 (1943): 290.

¹²³ Dollard, *Fear in Battle*, 56-57; John Dollard, “Twelve Rules for Meeting Battle Fear,” *Infantry Journal* 54 (May 1944): 36-38.

¹²⁴ The ‘Battle School’ was a British invention introduced into infantry training in 1941. It included training in fear control. See: Shephard, *A War of Nerves*, 232-238.

¹²⁵ According to Ryan, Dollard’s report was part of a concerted effort on the part of the Research Branch to change the terms in which war-related fear was discussed in the army, and “do away with notions of cowardice and malingering.” See: Ryan, *Samuel Stouffer and the GI Survey*, 76-77.

¹²⁶ Shephard, *A War of Nerves*, 25.

fear was inevitable, and rat-like hyper-stimulated and stressed-out soldiers ran around, gradually forced to adapt their behavior. Divorcing cowardice from its moral connotations, the behavioral understanding of fear projected a model soldier who was fearful, yet psychologically aware, self-conscious and mentally prepared, a soldier who efficiently managed his emotions and controlled his fears.¹²⁷

Fear in Hebrew

Pocketsize and simply written, *Pahad ba-Krav* (lit. fear in battle)—the abbreviated Hebrew translation of *Fear in Battle*—was printed in spring 1948, just a month and change before the onset of the First Arab-Israeli War, and distributed to all the soldiers in the Jerusalem district.¹²⁸ Though it was much shorter, and adapted here and there to fit the local circumstances, the Haganah fear guide remained true to the original. It faithfully cited the main findings of the fear-and-courage study: described the psychological dynamics of battle-induced dread, emphasized the role it played in aversive conditioning (the effect it had on soldiers' behavior in combat), and listed “the signs of fear”—the common physiological symptoms of fear response that required the soldier's immediate attention. A prescriptive section—“overcoming fear”—recommended the same techniques of fear control along with the neo-behaviorist reassurance that “learning to overcome fear [was] exactly like learning to use weapons.”¹²⁹ The second half

¹²⁷ For a fascinating discussion of wartime self-help literature (including *Fear in Battle*) and the military Subject it projected, see Herman, *The Romance of American Psychology*, 100-112.

¹²⁸ Jerusalem was still under siege at this point and the guide had likely been distributed without the Army headquarters' knowledge, as was the case for many of Guttman's initiatives.

¹²⁹ *Al ha-Pahad ba-Krav*, 6.

of the booklet—“what veteran soldiers think about fear”—was devoted to a selection of paraphrased Lincoln Brigade attitudes and statistics.

In the introduction, after paying tribute to the ingenuity of militarized American human science, Guttman encouraged the soldier to read the booklet carefully, and promised him it would be worth his while. “The main point is: he who knows the nature of fear can for the most part overcome it.”¹³⁰

The keyword here was “nature.” If Dollard’s guide labored to convert traditional military beliefs regarding the meaning of fear response, and struggled to do away with notions of cowardice and malingering, Guttman’s translation had successfully dissociated fear from its historical bearers.¹³¹

Perhaps the clearest example for the complete de-personification of fear in the Hebrew version was a slight interpretive adaptation Guttman introduced when translating the momentous opening sentence of *Fear in Battle*: “fear is a normal experience in battle.” Instead of “normal experience,” Guttman translated “*tguva tiv’it*” (lit. natural reaction), narrowing the semantic field and thus shifting the connotation of the original. Dollard had intended to reassure the soldier that fear was no cause for shame or embarrassment since it was perfectly ordinary amid the extraordinary circumstances soldiers faced. The qualifier “normal” came to suggest a common or usual experience that conformed to a standard of conduct common to all soldiers in the American Army. Guttman’s choice to translate “normal” as “natural” could be read as narrowing the

¹³⁰ *Al ha-Pahad ba-Krav*, 3.

¹³¹ Dollard made use of the notions of cowardice and bravery, but tried to charge them with new meaning. For instance, brave was now “any soldier with the guts to face the ordinary fact that everyone gets scared in battle”; or, “the fear of being known as a coward was a useful fear. Pitted against fear of battle, it can help a green man go through his first actions.”

meaning or shifting it from the colloquial to the scientific: it's not simply ordinary and expected in this context, but constituted by nature.¹³²

While Dollard's pamphlet entered the new marketplace of psychological advice for normal soldiers, and had to compete against other "self-help" diets (specifically Boring's) over a territory previously governed by the moral logic of duty and shame, Guttman's translation was an official Haganah guide, not scientifically authored and authorized educational material (Dollard still outlined all the details in a systematic manner), and therefore leveraged the institutional authority of the organization. By distributing an abbreviated Hebrew translation of *Fear in Battle* as an official Haganah guide on battle fear, the behavioral model of fear completed its scientific transubstantiation process and became universal knowledge.

In summer 1948, after the siege of Jerusalem was lifted, Guttman and his team learned that *Mahtar*, the Culture Department of the General Staff of what was by then already the Israeli Defense Army, had plagiarized their pocket guide without acknowledging the source, and distributed it as its own official publication throughout the entire army.¹³³ The fact there are two Hebrew-language wartime military fear guides, not one, doesn't appear in the record. The two iterations of the translated guide were also archived as one, with a copy of the RD's version safely stored at the Jewish National and University Library in Jerusalem, and Mahtar's version available through the Haganah Archive in Tel Aviv. The only indication that *Fear in Battle* may

¹³² One could argue that the American original began at an eye-level, language wise, with the soldier as its addressee (direct speech). The sentences are short and the tone friendly and reassuring. The very choice of words seems to reflect an acknowledgment of the normative register to which soldierly fear belonged. Also the statistics stand for something like "everybody think," or the majority does. In contrast, Guttman's language is sanitized, technical. The authority is that of science, not that of public opinion.

¹³³ In the archive of the Israeli defense forces, where only this later plagiarized version is archived, the date stamped on the cover of the handbook is August 10, 1948. See: *Fear in Battle*, published by the Culture Department of the Israel Defense Army, August 10, 1948. IDFA 102-6127-1949

have led a double life in Palestine/Israel can be found in the Institute chronicle, where this strange incident is mentioned in passing.¹³⁴ However, nowhere is it said (imagine the surprise!) that the Army's guide was not a true copy of Guttman's handbook.

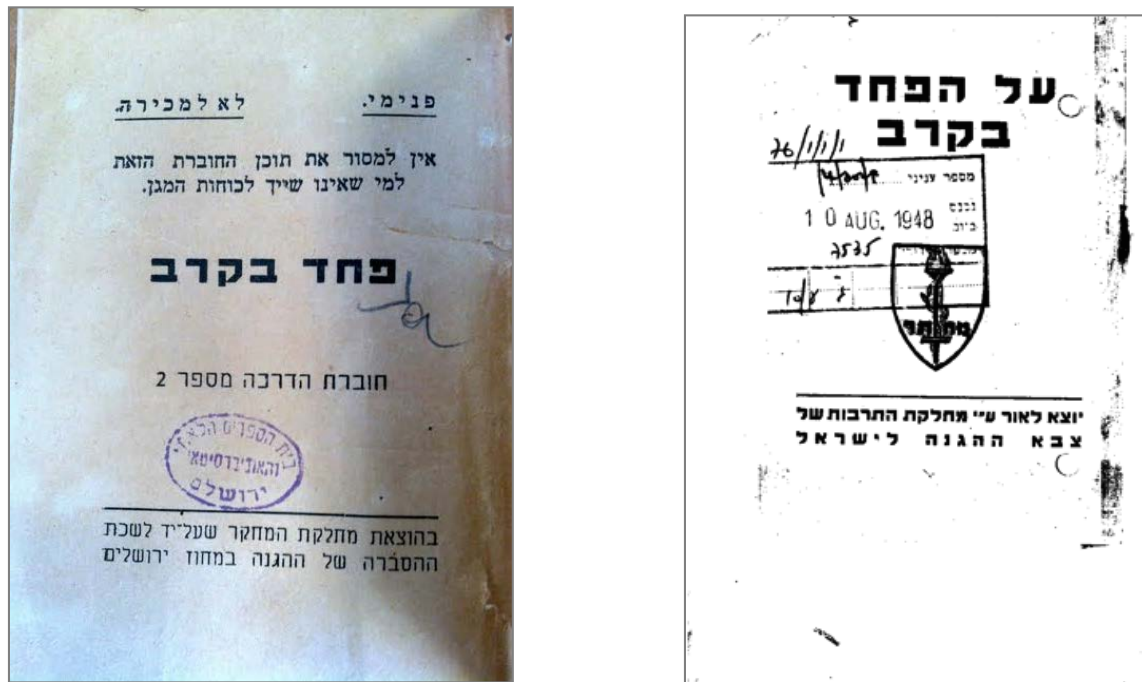


Figure 11: left, the cover of the Haganah bookley; right, the cover of Mahatar version

Nearly identical in shape and size, bearing (almost) the same title, and meant to serve the same purpose, *Al ha-Pahad ba-Krav* (lit. Of Fear in Battle) was a totally different animal. The bare bone, image-and-ornament-free design, which was typical of Zionist military culture of that era, was a common feature. They also carried the markers of their identity and function in a similar fashion, even though ‘Handbook No. 2’ was omitted, a sword-turned-torch insignia—the Culture Department’s symbol—was now triumphantly imprinted on the cover page, and a

¹³⁴ Gratch, *Twenty-Five Years of Social Research in Israel*, 16.

conspicuous preposition, “of,” along with the definite article, was added to the title.¹³⁵ Yet, once attention is shifted to the text, it becomes readily apparent that the two documents were in fact very different.

It was not a matter of content alteration. *Of Fear in Battle* quoted the same set of facts about fear Dollard first highlighted in his study report, recommended the same techniques for overcoming fear once it arose, and literally repeated the same selection of Lincoln Brigade statistics—under the rubric of “what do veterans think of fear”—that Guttman had originally believed to be relevant. The key difference was in the idiom and rhetoric.

In Guttman’s version, the words “coward” and “cowardice” did not appear at all. Fear was described as something that “happened” to the soldier involuntarily and therefore had nothing to do with his or her will, sense of duty, reason, or other moral faculties. It was described as a “natural reaction” or as “*regesh*” (feeling or emotion) associated with certain bodily sensations or signs (symptoms). Even though it could potentially encompass the soldier and overwhelm her entire being and sense of self, it was also importantly distinct from her. She might experience fear and act fearfully in certain circumstances, but that did not define her as a particular kind of person.

The *Mahtar*’s version opens in a vein similar to that of its predecessor: “Fear is a natural emotion. Every creature is afraid, and there is no man in the world that is not likely to be afraid.” It even outdoes its predecessor in providing a familiar everyday example to illustrate what an

¹³⁵ My only explanation for the slight title change is that the plagiarizers, whoever they were, imitated the heading format of early modern European philosophy treatises, which were being translated then into Hebrew. For instance, Rousseau’s *Of the Social Contract, or Principles of Political Right* first appeared in Hebrew in 1932. The translation was edited by Leon Roth, a distinguished philosophy professor at the Hebrew University, and published by the Magnes Press, the HU’s academic press. Even those who never heard of Rousseau knew about the Hebrew University, and this title format became culturally associated with epistemic authority. It is therefore possible that he preposition was added to signal this was the ultimate account on fear in battle.

involuntary response might feel like: “[it’s] like the closing of your eyelids when a foreign object suddenly moves up.” The comparison to pain in the knee extends the eyelid metaphor, but is used here for introducing the idea that fear might not necessarily be negative: fear is not simply a menace, a hindrance to action, but a valuable check on action in the face of danger. This was an idea that Guttman had included in the list of facts about “the reality of fear.”

Interestingly, this claim about the value of fear, which is a side issue in the first Hebrew version, was brought to the fore here. Fear was a blessing in disguise; it was “*the coward [who] is a slave to fear*, and does not want to learn how to overcome the fear instinct” that was problematic. This figure of the “slave” displaces scientific knowledge and it is deployed in the service of instituting a crucial distinction between fear and cowardice, or between legitimate and illegitimate manifestations of fear in battle. Raising the question of this difference subverted the modern scientific ideal of warfare or soldiering in more than one way.

First, the emphasis shifted away from the scientific analysis of fear and toward the ethical. Questions about what constituted cowardice were brought back. The text was repopulated with characters, signaling something of the return of the repressed. Whereas in Guttman’s account fear was an abstract force of nature, here it was personified as the coward. The coward was not the same coward, however, but a culturally specific one. On the opposite side of the first page, an epigraph from Shakespeare’s *Julius Caesar* announced, “Cowards die many times before their deaths. The valiant never taste of death but once.” The invocation of Caesar’s words set the tone. If Guttman’s translation displaced and decontextualized war-related fear, divorcing it from its military siblings, shame and moral denunciation, the army’s version brought it back, while contextualizing it anew in a discourse about soldiering and military

conduct. That discourse suggested a different moral and libidinal economy, in which we find a hybrid of the natural and supernatural, or the scientific and mythological.

In the abbreviated translation, the Hebrew pamphlet prepared by Guttman's Research Unit followed the original pretty closely, offering a shorthand version of Dollard's somatic fear theory and a list of the most common bodily symptoms. One of the major findings of the study was that fear is first and foremost a somatic experience: "many men think that 'fear is in the mind.' This is an error. Fear begins with strong bodily responses and is then registered in the mind." According to the 1943 report, the vast majority of men exhibited an array of physiological phenomena—pounding heart and rapid pulse, tenseness of muscles, trembling, dryness of mouth and throat and so on— either just before or during battle, which heralded the conscious feeling or psychological condition colloquially associated with fear. Subjecting the survey responses of the Lincoln Brigade veterans to statistical analysis, Dollard and his team had concluded that the variety of these fear "symptoms" (in the Dollard report) or "signs" (in the two Hebrew versions) is rather limited. The frequency of their occurrence followed a certain pattern (i.e., the same set of symptoms repeated in most men). It was therefore possible to determine which were the most (and least) common symptoms, and what percentage of men were likely to exhibit them. Learning how to read this body language, Dollard et al. argued, was key to learning how eventually to control fear.

In the *Mehtar* version too the symptoms of the fear were somatic, "Fear begins in the body, you know its signs: the heart is beating [fast], you're sweaty, breathless, your knees are trembling... the stomach is hard as rock—your body is giving you trouble." This description, however, gave way to a quite different characterization. "You want to be brave, and this body doesn't let you!" continues the pamphlet further. Describing the body as "your enemy" it

suggested to the soldier that it can be “deceived. How? If you know its tricks in advance, if you know in advance how it will behave. Wisdom is needed here, cunning! The devil must be tricked!”¹³⁶ Fear was the devil, as ancient and devious as the devil himself, and had to be outsmarted.

Such theological undertones infuse the descriptions of fear elsewhere in the pamphlet. The fearful soldier who, in gossiping, talks about “awful cases,” is compared to a child telling ghost stories till they scare themselves awake and can’t fall asleep: “by much talking [about horrible cases] they created an atmosphere of demons, and the demons creeping all over the room. The story creates demons where there aren’t any!” The language here is gothic—Hebrew gothic, itself a pastiche. Ghosts and demons populate the pamphlet, in close proximity to natural forces such as “instinct.” The psychological categories of “instinct,” “natural response,” and “fear” have a similar ontological status to these supernatural or mythical entities. The boundaries between the ethical and the theological become blurred.

Personifying fear as cowardice reintroduced it into a moral economy of virtue. But more significantly, suggesting that cowardice was fear’s horizon meant that fearfulness, its logical conclusion unless averted, was not a passing state of mind or a quality of behavior but rather, a kind of person; a way of being and soldiering that encompassed the entire character of the soldier and dictated his or her judgment and action. To the characters of the coward and the reckless man, the pamphlet contrasted the wise man. His wisdom strategic and prudent, the wise man could be true and cunning when needed. Where there was instinct, now there is moral sentiment. The language is normative rather than naturalistic. If in the scientific discourse the important distinction was between normal and pathological or abnormal, here it was between something

¹³⁶ *Al ha-Pahad ba-Krav*, 6-7.

akin to good and evil. Perhaps ghosts and demons did not exist, but the devil apparently did. The new aim was to overcome it by reason: if the soldier could see fear objectively, the spirit would reign.

Chapter Three

WHO IS A SOCIOLOGIST

Any poodle under ten inches high is a toy.
Almost always a toy is an imitation
of something grown-ups use.
Popes with unclipped hair are called "corded popes."
If a Pope's hair is allowed to grow unchecked,
it becomes extremely long and twists
into long strands that look like ropes.
When it is shorter it is tightly curled.
Popes are very intelligent.
There are three different sizes.
The largest are called standard Popes.
The medium-sized ones are called miniature Popes.
... After a poodle dies
all the cardinals flock to the nearest 7-Eleven.
They drink Slurpies until one of them throws up
and then he's the new Pope.
He is then fully armed and rides through the wilderness alone,
day and night in all kinds of weather.
...
All the time their bodies are becoming bigger and stranger,
but sometimes things happen to make them unhappy.
They have to go to the bathroom by themselves,
and they spend almost all of their time sleeping.
Parents seem incapable of helping their little popes grow up.
Fathers tell them over and over again not to lean out of windows,
but the sky is full of them.
It looks as if they are just taking it easy,
but they are learning something else.
What, we don't know, because we are not like them.
We can't even dress like them.
We are like red bugs or mites compared to them.
...
Some of the full-bodied popes are a million times bigger than us.
They open their mouths at regular intervals.
They are continually grinding up pieces of the cross
and spitting them out. Black flies cling to their lips.
Once they are elected they are given a bowl of cream
and a puppy clip. Eyebrows are a protection
when the Pope must plunge through dense underbrush
in search of a sheep.

(James Tate, "How the Pope is Chosen")¹

¹ James Tate, *A Worshipful Company of Fletchers* (Hopewell, N.J.: Ecco Press, 1994), 28.

A Job Interview Gone Wrong

In January 1949, as the two-year term of his SSRC “field training” fellowship drew to a close, Guttman, still formally on academic leave from Cornell University but better settled and looking for a way to stay in Israel, approached the Hebrew University of Jerusalem (henceforth: HU) to try and develop a relationship that would support his work on a more permanent basis.² His previous experience with the small Jewish university was rather discouraging. Guttman had initially sought institutional support for a research project in Palestine in the form of exchange professorship with the HU. Cornell made an offer on his behalf, but the reply from Jerusalem was that they had nobody to exchange for him and the plan fell through. Once external funding had been obtained and he was preparing to leave, Guttman contacted the university again, expressing his interest in collaboration and asking for assistance with finding student volunteers to help with the research. He never heard back.

Over the next two years Guttman was absorbed into the Zionist state apparatus. Put in charge of human engineering in the Israeli army, he built the Psychological Research Unit whose “public opinion” department was the first in the country to conduct social surveys on a national scale. As shown in the previous chapter, the military environment was conducive to large-scale research: it provided access to workers, captive subjects, data and material resources, and also the authority to compel cooperation. Guttman was given extensive freedom in terms of topics of research, and because the army itself was in the making, he got to take part in designing it by developing a personnel selection system and defining military professions. On a more personal

² Louis Guttman to David W. Senator, January 21, 1949. Guttman Personal File, HUA. Guttman sent his application materials to the University a couple of weeks later. See Louis Guttman to David W. Senator, February 4, 1949; Edward I. J. Poznański to Louis Guttman, February 8, 1949 and February 15, 1949, Guttman Personal File, HUA.

level, the job offered recognition, respect, and other miscellaneous perks. For example, Guttman had a dedicated army jeep and a driver to take him every day from his apartment in Tel Aviv to the Unit's building in Jaffa.

Armed with new status and authority, Guttman felt that he was in a better position to negotiate the terms of his inclusion in the local academic community. While the military setting held many advantages for Guttman, his success brought with it new worries and concerns. In a letter to the university, in which he offered his candidacy for a position in sociology, Guttman wrote: "we have been and are doing a good fact-finding job—perhaps no one could do any better—but this could hardly be construed as sociology. It is rather a service job, hurriedly done, under emergency conditions." Elaborating on the perils of applied research, Guttman complained that under the auspices of army and government, scientists were routinely exposed to meddling outsiders, "laymen who happen to have a high position [and] can dictate how a study should be done." Like his wartime American colleagues, who, at the end of the war, chose to transfer from the federal agencies in Washington into academic institutional environments and tie their social research institutes to leading research universities, Guttman concluded that "the scientific integrity of social research cannot be guaranteed except when the full responsibility for its execution is in the hands of a university."³

The HU did not have a designated academic unit for social science at the time, but after years of aborted attempts, a concrete plan for introducing sociology, economics, and political science into the academic curriculum was approved. A worldwide search for a prominent scholar who would lead a new department was at its height. A special university-wide committee, the Committee of Social Sciences and Economics (henceforth: the Committee), was given the task of

³ Guttman to Senator, March 5, 1949, Guttman Personal File, HUA.

introducing social sciences into the program of instruction and research at the university, and locating appropriate candidates for the academic staff of an expanded, new, and cross-disciplinary department.⁴ Consisting of a small group of distinguished Humanities faculty, all German-educated Central European émigré scholars, and headed by Martin Buber, the renowned Viennese-born Jewish intellectual, the Committee had been casting its net widely, on both sides of the Atlantic.⁵ Thus far, however, they had failed to find someone of the right caliber who would be willing to come to the poor, peripheral university. The search for “the one” had been going on unsuccessfully for five years at that point, and pressures to get social science on its feet were mounting. Guttman’s timing could not have been better.

In response to his unsolicited application, Guttman was invited to a meeting with David Werner Senator, the Executive Vice-President of the HU and the administration’s representative on the Committee. Senator was also a leading member of the bi-nationalist *Brit Shalom* and *Ihud* organizations and a longtime political ally of Buber’s.⁶ The two met in the “Terra Santa,” the

⁴ Initially, the plan was to establish a “Department of Social Sciences” within the Faculty of Humanities—the equivalent of division in American academic parlance—that would include economics, political science, sociology, and statistics. In 1949, the Committee concentrated on the consolidation of two discipline-specific “Hugim”—“Hug” being the equivalent of department. Once consolidated, the “Hugim” of sociology and economics would serve as the base upon which the Division would eventually develop. Since the Chair of Economics had already been appointed at that point, the search focused on finding a sociologist.

⁵ The original committee, elected in 1944, included the historian Richard Koebner, philosopher of religion Julius Guttmann, philologist and classics professor Moshe Schwabe, Talmudic literature professor Simcha Assaf (who was also Dean of the Humanities), David Werner Senator (see below), and Martin Buber. In 1949 its composition was adapted and lecturers in relevant subjects, who have since joined the academic staff (as adjunct and part-time faculty), replaced some of the original members. Buber remained the Chair. See: “Committee Regarding the Institute of Social Sciences and Economics,” First Meeting Protocol, July 27, 1944; Standing Committee Meeting Protocol, February 18, 1949, “Narrowing down the Committee of Social Sciences and Economics,” File 2276, HUA.

⁶ David Werner Senator (1896-1953) was a Berlin-born and educated Zionist activist and functionary. After completing a doctorate in political economy, he was active during the 1920s in various Jewish welfare organizations in Germany. Settling in Palestine in 1930, he served on the Jerusalem Executive of the Jewish Agency - first as treasurer and later as head of the Immigration Department and the Department of Settlement of German Jews. Resigning in 1935 for ideological reasons, he became a university administrator, and under Magnes’ leadership—the controversial first president of the HU whose political views he shared—came to play an important role in formulating the university’s ideology of science in general and its policy regarding the social sciences in particular, especially during the crucial war years and postwar period. After Magnes’ death in late 1948, Senator was appointed

temporary residence of the HU.⁷ Reporting to the logician Poznański, the Academic Secretary of the Committee and a friend of both Buber and Senator, Senator described Guttman as “a young man from America who did not make a great impression on me.” Even though he admitted not having understood much of what Guttman told him about his work, Senator added, as if compelled to reassure his addressee, “of course I told him [Guttman] that his main area of specialty seems rather narrow to me.”⁸ In a separate memo that he had sent to Buber, along with Guttman’s application materials, Senator was more resolute in his judgment: “I heard that professor Guttman gave an unsuccessful lecture...I find his sociology extremely one-sided [sic]. I think he is out of the question as *the* sociologist.”⁹

Guttman met Buber the following month. The meeting did not go well. Shortly thereafter, Guttman received an official letter of rejection from the university. In his mind, the letter

Executive Vice-President, and for a while, until the university’s Board of Governors had elected a new president, was the one who oversaw its everyday functioning. This was the case during the “Guttman affair.”

⁷ Terra Santa (*Italian*: sacred land) was a beautiful neo-classical building complex in Jerusalem’s center. Designed by renowned Italian architect and Franciscan monk Antonio Barluzzi and owned by the Franciscan Church, it served throughout the 1930s and 1940s as a prestigious interreligious high school where the city’s Christian, Muslim, and Jewish youth could receive quality European education. An exemplary Mandate era institution—emblematic of the cultural syncretism that typified Middle-Eastern, and specifically Palestinian colonial culture in that period—the Terra Santa College, as it was known, closed down in 1947, when the area was included in ‘Bauvingrad,’ the British fortified and fenced off government compound. When the State of Israel came into being and the Israeli Army took control of the west part of the city, Terra Santa was made into the university’s temporary residence, after access to its buildings was cut off (Mount Scopus came under Jordanian rule during the war). The radical shift in the identity of the building— in less than two years, Terra Santa transformed from a relatively insulated Jerusalemite institution, which was a sanctuary of religious and cultural pluralism, to a center of national culture and ideological conviction—was in a sense metonymic to what happened at that historical moment to Palestine as a whole.

⁸ Senator to Poznański, January 26, 1949, Guttman Personal File, HUA. Edward I. J. Poznański (1901-1974) was a Warsaw-born and raised logician and philosopher of mathematics and member of the Lvov–Warsaw School—the Polish counterpart of the Vienna Circle—who served as the Academic Secretary of the HU during the crucial postwar years, 1946-1964. Like the other Hebrew mandarins, he was a wide-ranging intellectual of great humanist convictions, rather than a professional academic. Versed in many European languages, Poznański was known for his ability to recite entire volumes of the Russian and Polish poetry and for his love of photography. For his conception of the social role of university, see Edward I. J. Poznański, “The Crisis in Higher Education,” in *Essays, Reviews, Reminiscences* (Jerusalem: Magnes Press, 1980), 309-315 (Hebrew).

⁹ Senator to Buber, January 27, 1949, Guttman Personal File, HUA. Emphasis original.

reflected a lack of good judgment, provincialism, and disregard for professional standards.

Guttman recounted the event in a long, exasperated letter to Senator:

The conversation I found to be a rather strange one. I had rather imagined that its purpose was to be to enable Professor Buber to learn more about me. Perhaps he wanted more details about my academic training and teaching experience. Perhaps he wanted to know my views on various phases of sociology. Perhaps he wanted to ascertain my ideas on the problem of a sociological curriculum. Perhaps he wanted to know if I preferred different teaching methods for undergraduate students than for candidates for advanced degrees. Perhaps he wanted to know how I thought a research program fitted in with the teaching of sociology. But none of this seemed to be the case. It seems that Professor Buber had his mind made up that he knew all there was to know before he ever saw me, and had asked to see me only out of courtesy to your committee.¹⁰

Guttman found this unexplained rejection difficult to fathom. The idiosyncratic and elusive language of the letter he was sent offered little consolation in this regard, except for a vague admission that the matter was neither financial nor related to “the high scientific level of professor Guttman, which the committee most definitely recognizes,” but rather pertained to some mysterious incongruity between “the direction that you represent” and what was required of the single head sociologist of the HU.¹¹

With the HU faculty insisting on their position, and Guttman being supported by donors and the university administration, this unexpected encounter developed into a veritable *Methodenstreit* that would be fought for almost a decade over the pages of local newspapers, in professional journals and tenure committees, in the university’s negotiations with donors, and across Atlantic Jewish intellectual networks, involving prominent social scientists, among them Columbia sociologist Paul Lazarsfeld and the South-African LSE economist Herbert Frankel. The latter were consulted as experts on what came to be a burning question: who will be (or what

¹⁰ Guttman to Senator, March 5, 1949, Guttman Personal File, HUA.

¹¹ Senator to Guttman, March 27, 1949 Guttman Personal File, HUA. See also the Committee’s meeting protocol, March 22, 1949, File 2276, HUA.

was required of) the first Hebrew social scientist? From the flurry of correspondence between Guttman and the leading personalities of the university, as well as a host of internal memos, committee minutes, and reference solicitation concerning what became known as “the Guttman affair,” it is clear that the question of his employment was not as simple as Guttman had hoped. Rather, it opened the door for a mammoth struggle over the ethos and identity of the social sciences and the university as a whole.

The story of the development of social science scholarship at the HU has traditionally been told as part of a national independence and state building story, subordinating the course of its development to the logic of political history. For example, Uri Ram explains the emergence and hegemony of the Jerusalem School, as well as its allegiance to theory and rejection of empirical and quantitative style, as a function of an ideological commitment to modernization theory.¹² Against this common historiographical view, this chapter argues that Guttman’s rejection was not simply a matter of preferred methodology or a clash between styles of reasoning about the social world. Rather, the bitter feud between the “defenders of the text” at the HU and the civil servant of mechanical objectivity encompassed a complicated story about the politics of knowledge in colonial Palestine. Following Chambers and Gillespie, I treat Palestine/Israel as a scientific (rather than geopolitical) locale, making sense of what happened there by reconstructing a “polycentric” picture of mid-century changes and travels.¹³ As I will show, the crucial questions concerning the value-orientation of social science that it brought to the fore gained particular urgency and political meaning in the Zionist and Jewish context.

¹² Uri Ram, *The Changing Agenda of Israeli Sociology* (Albany, NY: State University of New York Press, 1995).

¹³ David Wade Chambers and Richard Gillespie, “Locality in the History of Science: Colonial Science, Technoscience, and Indigenous Knowledge,” *Osiris* 15, no. 1 (2000): 221-240.

The Last Prussian University

Established in 1925, the HU was, as commentators described it in years to come, the last Prussian university. Modeled after the nineteenth century German research university and populated predominantly by émigré German and German-educated scholars—many of them refugees who fled Europe after 1933 in the context of the large intellectual migration of the Nazi era—the small Jewish university was, at least during the first decades of its existence, a quintessential if historically anomalous manifestation of the dying mandarin culture and ideology described in Fritz Ringer’s historical sociology of the German academic community.¹⁴ What was unique about the few dozen scholars who occupied key positions at the HU and were responsible for its design and curriculum is that they largely succeeded in building an insulated institutional and cultural environment that mimicked their alma mater to a surprising and at times comic extent.¹⁵

As various historians have shown, the founding of a modern Jewish university was a top priority on the agenda of Zionist ideologues and activists since the turn of the century. Accordingly, it was habitually discussed in national terms. Advocates felt that a Jewish academic center in Palestine was necessary for demonstrating the vitality of the Zionist vision and for showcasing the possibility of its realization. It also represented an urgently needed solution for the numerous East-European Jewish students who were gradually being barred from entering

¹⁴ Fritz Ringer, *The Decline of the German Mandarins: The German Academic Community, 1890-1933* (Cambridge, Mass.: Harvard University Press, 1969).

¹⁵ On the intellectual migration of German-speaking scientists and scholars in the 1930s and 1940s and its significance for the global history of science in the twentieth century, see: Mitchell G. Ash and Alfons Söllner, eds. *Forced Migration and Scientific Change* (Washington D.C.: Cambridge University Press, 1996).

institutions of higher education in their lands of residence.¹⁶ In its capacity as the first Jewish university, it also had a revolutionary historical role that went beyond its academic functions. Its founders envisioned a university that would merge “Judaism and humanity,” religious tradition with enlightenment, and would serve as a spiritual center for world Jewry.¹⁷

Its “idea” notwithstanding, the HU ended up being structured first-and-foremost as an elite research institution. In March 1920, the Executive Council of the World Zionist Organization met in London. A memorandum in both Hebrew and English called “Report [on] Preparations for the University in Jerusalem (to be submitted to the Annual Conference of the Zionist Organization)” was tendered at this meeting. Chaim Weizmann, a well-known Russian-born organic chemist and Zionist leader from Manchester, initiated this memorandum in order to provide the vision of the Zionist movement for a Jewish university with a concrete, operational form. Even though the ultimate purpose was the establishment of a “national university,” it was agreed that the academic structure of the university in its initial phase should be that of an umbrella organization for several semi-autonomous research institutes for natural science, medical research, and Jewish studies; rather than an institution focused on undergraduate teaching. Systematic instruction of students would begin only after the research capacities of the university were well established. The memorandum determined that only the development of basic research could establish the reputation of the HU and create the necessary scientific atmosphere in Palestine, “thus ensuring it against the danger of low scientific standard and a correspondingly low reputation like that from which new universities in backward countries,

¹⁶ Israel Kolatt, “The Idea of the Hebrew University in the Jewish National Movement”; Israel Bartal, “Jews in East - European Higher Education,” in *The History of the Hebrew University in Jerusalem: Origins and Beginnings*, eds. Shaul Katz and Michael Heyd (Jerusalem: Magnes Press, 1997), 3-74; 75-89. (Hebrew).

¹⁷ Yair Paz, “The Hebrew University on Mount Scopus as a Secular Temple,” in *Ibid*, 281-308. (Hebrew).

especially in the East, generally suffer.” The memorandum further stressed that these research institutes should avoid parochialism, and that the entire university, despite its limited dimensions, should be a world-class institution of academic distinction. In the same spirit, five years later, during the official inauguration ceremony of the HU on the first of April 1925, Weizmann declared, “we have made up our minds that it is for our university to win its spurs and build up its reputation by the distinctive value of its contributions to the common stock of knowledge.”¹⁸

Thus, the HU’s leaders treated teaching as a relatively low-priority concern, granting only graduate degrees in the first twenty-five years of its existence and making room for neither professional schools nor applied disciplines.¹⁹ The classical Humanities disciplines were at the core of the institution, joined by the departments of the natural and life sciences, and an institute of mathematics. In terms of its administrative structure, the university consisted of semi-independent research institutes. According to the German paternalistic model, each was headed by a single male Herr professor who was an established authority in his field and a “man of personal integrity,” capable of leadership. This professor was tasked with cultivating junior faculty members and advanced students, as well as setting a research agenda.

Supported by Jewish communities around the world and enjoying relative financial independence, the leaders of the university and its core faculty managed to set these priorities and aims despite the fact that they were hardly in line with the needs of the Yishuv. Although

¹⁸ Chaim Weizmann, “Weizmann’s Speech [at the HU’s inauguration ceremony, April 1, 1925],” In *The History of the Hebrew University in Jerusalem: Origins and Beginnings*, eds. Shaul Katz and Michael Heyd (Jerusalem: Magnes Press, 1997), 320-322 (Hebrew).

¹⁹ S. Ilan Troen, “Higher Education in Israel: A Historical Perspective,” *Higher Education* 23.1 (1992): 47.

centrists, socialists and revisionists alike wanted a university that would answer “the needs of everyday life,” their requests, often formulated as an unequivocal demand, were largely ignored.

Despite the university’s status as the only institution of higher learning in the Mandate, the HU mandarins consistently refused to offer training in the so-called “free professions,” and were concerned primarily with the educational diet of the elite.²⁰ This posture was a constant source of animosity between the university and the political leadership, which promoted competing visions of Jewish nationalism. This tension only intensified in the mid-to-late 1930s with the inclusion of refugees from Germany who brought new interests to a table already full of disagreement, both among the Jews (the Diaspora, the Yishuv, and the Zionist movement) and between the Jewish and Arab communities and the British government.

While the 1920s saw much social experimentation and institutional innovation in the two centers of activity of the Yishuv, namely, in the “valley”—the center of communal organizations in social and work life—and in the “city”—the center of political, commercial, and cultural organization—the HU, nicknamed “the mountain,” both for its location on Mount Scopus and for its snobbism, the way it “looked down” on the settler society, was viewed by many as a conservative attempt at transplanting a European model into a new habitat. The HU appeared to be a foreign element, disconnected from the life and the needs of the Yishuv. It betrayed the Zionist vision of a national university. As Cohen explains, its residence on top of Mount Scopus, located far and above the centers of civic and commercial life, became a symbol of its isolation and elitist posture.²¹ With its own mandarin elite espousing a clear, loud and uncompromising

²⁰ Naomi Shepherd, *Ploughing Sand: British Rule in Palestine, 1917-1948* (New Brunswick, NJ: Rutgers University Press, 1999)

²¹ Uri Cohen, *The Mountain and the Hill: The Hebrew University of Jerusalem During Pre-Independence Period and Early Years of the State of Israel* (Tel Aviv: Am Oved, 2006). (Hebrew).

vanguard and anti-establishment stance, the university constituted an alternative and autonomous cultural center in the Yishuv in the second half of the 1930s.

Against Utility

One area of contention between the HU and the Yishuv was the question of vocational training for civil servants in the “sciences of the state.” At the turn of the century, social scientific research was practiced in Palestine in the context of the Zionist social engineering and colonization project, with most social knowledge produced in the form of Jewish statistics by Zionist organizations. Shaul Katz’s history of the “Zionist research multi-system” during Ottoman and British mandatory Palestine shows that in addition to the research university (the HU) and to technological institutions (such as the Technion), there were active “extra-scientific organizations” engaged in the production of “para-canonic certified knowledge.”²² National institutions such as the Jewish Agency and the Histadrut labor movement commissioned social and ethnographic studies that were oriented toward settlement engineering and, though prevalent, held relatively low epistemic status.²³

²² Shaul Katz and Michael Hed, eds., *History of the Hebrew University of Jerusalem: Roots and Beginnings* (Jerusalem: Magnes Press, 1997).

²³ Established in December 1920 during the British Mandate for Palestine, the Histadrut became one of the most powerful institutions in the state of Israel, a mainstay of the Labor Zionist movement and, aside from being a trade union, its state-building role made it the owner of a number of businesses and factories and, for a time, the largest employer in the country. At the head of the Histadrut stood Berl Katznelson, one the intellectual founders of Labor Zionism. He was considered an intellectual authority in the Zionist Labor movement and contributed to the strengthening of nationalist trends over class orientation. According to Katznelson, social science – mainly the instruction of socialist thought – had a crucial educational and intellectual role in promoting critical thinking among the workers. He wanted to fight what he saw as the “intellectualism” and pro-Soviet orientation of left factions in the movement. Katznelson, through establishing a publishing house— “Workers’ Library” —as well as anthologies of socialist theory—a popular university of sorts—was instrumental in the development of these extra-scientific social sciences in the Jewish Yishuv and in the wide spread of sociological research.

This “para-canonic certified knowledge” consisted primarily of problem-specific and often quantitative data collection and analysis that served to furnish and guide decision-making and policy design (e.g. economic policy, settlement plans), the engineering of solutions for various social and economic problems, and the day-to-day political management of life at the colony, offering little by way of abstract generalizations. In the context of Zionist colonization efforts, practical reasoning of this modern variety acquired an additional political function that further shadowed the epistemic status of its truth claims: it was extensively deployed by Zionist organizations as an ideological weapon in their struggle against the immigration quotas set by British colonial authorities. As Ilan Troen shows, social scientific calculations helped to naturalize and render technical the question of mass Jewish immigration to Palestine, producing conclusions that could respond on even footing to the rationale behind British quotas and engage directly in the debate over Palestine’s demographic future.²⁴

According to Gross, attempts to canonize these governmental sciences as true sciences—in the form of a separate institute or department of social sciences (to be included in the Faculty of Humanities)—date back to the first days of the university.²⁵ The future possibility, pending appropriate funding, of a permanent position in sociology or economics was on the administration’s agenda throughout the 1930s, yet the social sciences did not become part of the HU curriculum until after the Second World War.²⁶ Adopting the university’s own version of the

²⁴ Ilan Troen, “Calculating the ‘Economic Absorptive Capacity’ of Palestine: A Study of the Political Uses of Scientific Research,” *Contemporary Jewry* 10, no. 2 (1989): 19-38.

²⁵ Nachum Gross, “Social Sciences until 1948/49 – Plans and Beginnings,” in *The History of the Hebrew University of Jerusalem: A Period of Consolidation and Growth*, ed. Hagit Lavsky (Jerusalem: Magnes Press, 2005), 503-542. (Hebrew).

²⁶ In addition to more concrete plans of introducing individual subjects, such as political economy, into the regular program of instruction.

events, Gross argues that the problem was primarily financial, and secondarily that of appropriate candidates—as several glorious failures to attract “central personalities” in the social, economic and psychological sciences to the university at that period could testify.²⁷ However, both contemporary commentators and critical historians argue that, despite many memos, schemes, plans, and initiatives, the failure to institutionalize academic research and instruction in the social sciences at the university was more a matter of posture than means.

Following Cohen’s suggestion to understand the HU as an “alternative cultural center,” which enjoyed an unprecedented level of institutional, economic, and ideological autonomy with respect to the political and social centers of power, I argue that the status of the social sciences had much to do with the emerging incompatibility of cultural values and ideology between the HU’s leading intellectuals and a changing population.²⁸ These culture wars came to a head in the mid-1930s, with pressing demand from both the growing private-commercial urban sector and the political leadership for professional training. The question of social science congealed into a delineated problem and a focus of contention and struggle around 1935, when the mandarin intellectuals were at the height of their power but pressures from the outside grew, especially in relation to recent immigration from Germany in the *Aliyah Hamishit*. It was only around these conflicts that the HU began to implement a distinction between truth and utility-oriented approaches. In other words, the cultural and political struggle was translated into academic terms around the question of vocational education and training for administrators in the “sciences of the state.”

²⁷ Kurt Lewin was a paradigmatic case. See: David Bargal, “Kurt Lewin and the First Attempts to Establish a Department of Psychology at the Hebrew University,” *Minerva* 36 (1998), 49-68.

²⁸ Moshe Lissak and Uri Cohen, “The Hebrew University as an Alternative Cultural Center of the Jewish Community in Palestine,” in *The History of the Hebrew University of Jerusalem: Academic Progression in a Period of National Struggle*, ed. Hagit Lavsky (Jerusalem: Magnes Press, 2009), 87-133.

The important point here is that, until the mid-1930s, there was no real animosity around social science between the HU and the outsiders. Rather, social science was very much a decentralized scientific field, practiced in multiple institutional settings (including the University), serving various purposes and interests, and consisting of a syncretic mix of practices and approaches. It was not governed by one hegemonic epistemological-methodological regime. This was arguably a case of an informal intellectual division of labor between the university and the Zionist organizations.

Explicit challenges to the university's indifference towards social science first appeared in 1934, in the form of the Survey Committee of the Hebrew University of Jerusalem (better known as "the Hartog Committee"). Its concluding report, which addressed, among other things, the absence of technical training in the applied sciences and the professions at the university, included a detailed section on "law, political science, and economics." The very inclusion of the social sciences, next to agriculture, medicine, and education, under "new departments proposed to be instituted at once or in the future," itself attested to their changing status at that period. Furthermore, endorsing the "indecent proposal" of Moshe Zmora, then the Chairman of the Jewish Bar Association of Palestine, and Berl Katznelson, the chief ideologue of the Jewish Labor Organization and a long-time nemesis of the HU mandarins, to establish at the university "a definitely vocational course for the training of men for administrative posts in connection with Jewish and other organizations, by whom such officials are now needed," the Committee recommended—to the university leadership's great dismay—the institution of a "modest and

economical” Department of Social Administration and Economics, that would offer courses in law, political science, regional economics, and public finance.²⁹

In order to understand why the social sciences were anathema to the Hebrew mandarins, we need to understand the particular history of the social sciences within or in relation to Zionist politics—mainly the struggle between political and cultural Zionists—and the Palestine settlement project. Social research was mostly made-to-order, in the service of sectarian interests. It was conducted in a charged political context, and used as an instrument in political and ideological struggles. It was therefore often considered partial and tainted, guided by local concerns and limited to the realm of practice, i.e., administrative knowledge that could not enjoy the epistemological status of scientific knowledge (generality, universality, objectivity).

Knowing a World in Crisis

The chief formulator of this epistemic divide between truth and utility in the culture wars between the HU mandarins and the Yishuv over academic instruction in the social sciences during the late 1930s and throughout the war years was Martin Buber. Buber left Nazi Germany shortly before the war began and immigrated to Palestine in 1938. After a decade of negotiation, he joined the HU as a member of the faculty. Appointed Professor of Social Philosophy (a title he himself chose, which was later changed to “sociology of culture”), Buber thus became the only sociologist in Palestine with a formal academic designation.³⁰

²⁹ Hartog Report, *Report of the Survey Committee of the Hebrew University of Jerusalem* (London: Waterloo & Sons, 1934), 88-89. For a discussion of the academic politics around the establishment of the Committee and its report, see Cohen, *The Mountain and the Hill*, 76-80.

³⁰ Individual courses in Jewish statistics and political economy (e.g. “Sociology of the Jews,” “Land of Israel Economics”) were already part of the HU curriculum at the time. However, these were treated as something akin to “area studies”—geographical and cultural knowledge defined ethnically or regionally—rather than as social science

If the creation of a permanent position in sociology seemed at first glance to be a much anticipated change in attitude on the HU's part, the filling of this spot with an eccentric Jewish philosopher and mystic who had little formal training and absolutely no research or teaching experience in modern social science could hardly be considered as a gesture of goodwill toward the business and political circles with which the university had been fighting a bitter turf war. It reflected, at least in the eyes of contemporary commentators, a further entrenchment of the university's position against the Yishuv's demands for "useful knowledge" and professional training. In fact, as Buber's biographer makes clear, the appointment was a custom-made compromise, designed to resolve an internal dispute. It was tailored to accommodate Buber's wide-ranging interests, while keeping him at a safe distance from religious subjects and thus calming the fears of "the narrowly clerical clique" at the university, who were not too keen on his unconventional conception of Judaism.³¹

Buber himself harbored doubts about what he described in a letter to Hugo Bergmann, his friend and then the Provost, as "switching over to another discipline at this relatively late hour of my life."³² He was concerned that such a position would demand too much of his time, caging him in a sterile academic existence and leaving little time for the "real work" awaiting him in Palestine.³³ Once at the university, however, Buber quickly ascended to a position of leadership.

subjects. In contrast, Buber's position was in the Institute of General Humanities, a fact that granted sociology a disciplinary status. See *Hebrew University of Jerusalem, Its History and Development* (Jerusalem: 1939).

³¹Although Buber had the support of prominent figures in the university, the Board of Governors rejected the faculty recommendation to appoint him as professor of the science of religion (*Religionwissenschaft*). "The narrowly clerical clique did not find him Jewish enough," Magnes explained, "and the specialists did not find him scholarly enough." On Buber's decade long negotiations with the Hebrew University, see: Maurice Friedman, *Martin Buber's Life and Work, The Middle Years, 1923-1945* (New York: E. P. Dutton, 1983), 251-258.

³² Ibid.

³³ The limited scope of this chapter does not allow me to elaborate on Buber's existential conception of intellectual or spiritual work and its significance for understanding his critique of positivist epistemology.

Becoming something of a chief ideologue, he articulated his vision, set up a model for practice, and, most importantly, set the terms of discussion and thus intelligibility for the social sciences at the university. He determined who and what could make sense epistemologically and morally.

His inaugural lecture, delivered to a large audience on Mount Scopus in April 1938, was a biblical tour de force of boundary setting vis-à-vis the political establishment, executed through a radical critique of the ideology of modern science.³⁴ He outlined the rules of engagement with the state, undermined the moral status of positivist epistemology, and concluded with a redefinition of social science as an ethical project.

Buber began his lecture by introducing a meta-historical distinction between social science and social critique, which underwrote, he argued, the history of sociology as a modern discipline since the mid-nineteenth century. Sociology, he argued, did not emerge from a concern with abstract forms and structures but grew instead out of “the meeting of the spirit with the crisis of human society that occurred at the beginning of the nineteenth century.”³⁵ The historical and experiential—and thus deeply personal—embeddedness of sociological reasoning, according to Buber, was born out of the human experience of crisis, and it remained dialectically bound to it. A state of crisis is a condition for social scientific knowledge (and its primary object), but the act of knowing is transformative for both. In his words:

Modern sociology originates... in the meeting of the spirit with the crisis of human society, which the spirit accepts as its own crisis and which it undertakes to overcome through a spiritual turning and transformation. Sociology is just this

³⁴ Peter Dear, “What Is the History of Science the History Of? Early Modern Roots of the Ideology of Modern Science,” *Isis* 96, no. 3 (2005): 390-406.

³⁵ The inaugural lecture was delivered in Hebrew. Martin Buber, “Tvi’at haRu’ah ve-haMezi’ut haHistorit,” *Moznayim Literary Monthly Journal of the Hebrew Writers Association*, vol. 7, ed. Ya’acov Fichman (Tel Aviv: Hebrew Writers Association, 1939), 218-227. For the English translation, see: Martin Buber, “The Demand of the Spirit and Historical Reality (Inaugural Lecture, the Hebrew University, Jerusalem, 1938),” in *Pointing the Way: Collected Essays*, ed. and trans. Maurice Friedman (London: Routledge and Paul, 1957), 177-191. All the quotes below are taken from the English version of the lecture.

insight into the nature of the crisis, its causes, and the problems set by it, the beginning of this turning and transformation.

Sociological knowledge is thus rooted in and contingent upon direct experience: only by experiencing social life from the inside can the sociologist understand it. It is also an inherently ethical posture towards that experience: the sociologist stands within the crisis and actively participates in the search for a solution. Experience is the seat of both knowledge and action. Being in the world—in the midst of social crisis—is not only a necessary condition for understanding, but also an ethical imperative. The epistemological disposition of the sociologist is inseparable from his ethical disposition. Buber defined social science as a moral relation to the object of inquiry. Experience—not methodology—is the source and site of reflection and knowledge. It is no wonder that Guttman’s methodological positivism appeared anathema to Buber’s approach. Buber’s intervention is against the imminent danger of sociology becoming—or rather, coming to understand its own position as—a “voice” of reality:

Where the spirit becomes the mere voice of reality, it forgets that that altered state of its expectations... becomes false in becoming exclusive. Only when it remains the partner of reality does it remain aware of its office of working for the transformation of the spirit, for its own transformation without which the altered institutions must decline to emptiness, to unfruitfulness, to ruin.

While Buber’s language tends to the abstract and Hegelian, when read in the local context the content of his speech becomes expressly political. When he asserts that, “if the new house that man hopes to erect is not to become his burial chamber, the essence of living together must undergo a change at the same time as the organization of living,” he is speaking of the vicissitudes of the colonial project in Palestine. However, beyond objecting to the specific administrative uses of social knowledge, Buber makes a more fundamental claim that positivism, scientism, and objectivism are inherently linked to imperialism. Buber’s sociology is a field of

judgment and action, the ultimate purpose of which is “to know in order to change.” Aspirations to value neutrality could only obfuscate underlying relations of power and instrumentality:

The demand for the ‘value-freedom’ of sociology has resulted in a resignation that may be formulated in these words: The spirit is still effective indeed, but only in so far as it places itself under the sway of powerful groups, under the dictates of what rules in history, that is, of power—we wish, therefore, to define its limits as a sphere where spirit is not to act but only to know, and within this sphere to guarantee still its independence.

For Buber, value freedom is an illusion of freedom from bias. By giving up on inhabiting an ethical standpoint—selling its right to judge for cordoned-off independence—sociology resigns itself to knowing, which is divorced from doing. Action becomes the privilege of the powerful, who use knowledge as they please (deterministically, not truthfully, to promote their interests, that is, to justify and reproduce power). To Buber, this was exactly the kind of policy orientation and service mentality into which “scientific” social science appeared to have evolved. In naturalizing the existing state of affairs, in forgetting that things could be different, value-neutral social inquiry, to his mind, “becomes false by becoming exclusive.” In contradistinction to Weber and the classical tradition, Buber thought it was impossible to separate “value relevance” (*Wertbezogenheit*)—that which is important and worth knowing and makes the subject matter of social scientific inquiry—from “value judgment.”³⁶

In Buber’s view, social philosophy entailed valuation, criticism, and demand. In contrast to the value-free orientation of sociology, Buber envisioned a critical investigation of problems to which the sociologist was existentially committed. Concerned with far more than description,

³⁶ For an illuminating discussion of the history of value-neutrality in German social theory and its inadvertent contribution to the liberal conception of the place of science in society in twentieth-century Europe and America, see Robert Proctor, *Value-Free Science? Purity and Power in Modern Knowledge* (Cambridge, Mass.: Harvard University Press, 1991).

the sociologist's responsibility was to inquire into the human meaning of social life in order to help reshape it.

In this inaugural speech, then, Buber expresses the oppositional and critical standpoint of the mandarin intellectuals vis-à-vis the Yishuv and the Zionist establishment. Clearly signaling his political alliances, he establishes his authority as a “preacher at the gate”: a social philosopher who remains loyal to the ideals of Zionism as Hebrew humanism and would refuse to be the “voice” of the establishment. The interesting thing here is that he's doing it through a discussion of sociology, or rather, a meta-sociological critique of the epistemological commitments of the science of society and their moral implications.³⁷ Ironically, therefore, Buber's inauguration speech as the new (and only) chair in social science is an anti-scientific manifesto. These culture wars regarding the role and meaning of sociology provide the context needed to understand the Guttman affair.

Looking for an Heir

The official reason Guttman was given for his rejection was the “narrowness” of his scientific training and the “one-sidedness” of his conception of sociology. Buber wanted someone who would be an inspiring social theorist and “continue the European tradition of

³⁷ Anne Harrington provides a masterful explanation of the interwar cultural milieu that gave rise to this wholesale rejection of science as instrumental knowledge and a demand for an ethically and existentially meaningful picture of human existence. That Buber was the prime carrier of this rage-against-the-machine epistemology to Palestine, validates her conclusion that holism - and concerns about the mechanization of science and society - held a promise for Jewish liberals and socialists during that historical moment. Yet, seen in the context of the Zionist colonization of Palestine, which Buber supported, it also complicates the accepted understanding of Buber's position as truly humanistic. See Anne Harrington, *Reenchanted Science: Holism in German Culture from Wilhelm II to Hitler* (Princeton, NJ: Princeton University Press, 1996).

teaching all branches of sociology,” while Guttman, citing contemporary practices and norms of American sociology, insisted that being a specialist was the only valid professional identity in the social sciences.

The debate over “general sociology” encapsulated much more than a professional quarrel. Rather, it was a generational and cultural struggle between the old man of Europe and American modernity. Guttman’s clash with the university was a clash between two differing and, to a large extent, morally incommensurable views of the political and social role of the social scientist, pitting the individual intellectual philosopher and hermeneutist against the expert bound to service.

In a barely civil letter to Werner Senator that Guttman wrote after his meeting with Buber, he effectively laid down a manifesto for his vision of sociology, presenting himself as the bearer of news from cutting-edge America who needed to explain to some backward ex-Europeans that a big change was coming. Guttman brought to Jerusalem the news that the pyramid of knowledge that old Europe was clinging to was being turned on its head: what used to be the foundation of social science was now an unnecessary derivative, both in teaching and in research.

This crisis coincided with the beginning of a process of social science discipline formation in Palestine/Israel. “Disciplinization” was a concerted effort on the part of the university to secure the right to define what counted as intellectually established and culturally legitimate in the realm of social knowledge. In practice, this effort consisted primarily of purifying the syncretistic mixture of approaches, settings, and practitioners that characterized social and economic research in Palestine at the time, and unifying what was until then a rather decentralized intellectual field where various players—interest groups, organizations,

individuals—competed over the meaning and political uses or ends of its products under the authority of one epistemic, normative, and institutional regime. This, I suggest, is how we should understand the drama around appointing a new Chair of Sociology.

The subordination of the disciplinary project to the logic of the crisis explains, I argue, the intensity, investment, and the length of the search (which officially took place from 1944 to 1951, but unofficially went on for nearly a decade), as well as its strange negative criteria. When the hiring committee said that it needed a “general sociologist,” it was not in its traditional substantive sense of object-specificity, nor even the positive idea of “fundamentals” or “methods.” A “general sociologist” would be able to lead a reorganization and epistemic elevation, while remaining morally “superior to history.” In Buber’s words, they were looking for the sociologist who could be the “representative of the spirit”—who would “oppose[] power with criticism and demand” and “spea[k] his word to a generation of the spirit that must be educated.” “He speaks,” Buber noted, “to a world that must be changed.”

The stakes were high because success in this regard would ideally result in the concentration of moral authority in the hands of the faculty vis-à-vis the ideologues of the state. Furthermore, it would provide a new impetus to the bi-nationalist critique of the Zionist state project, instituting it as a rational discourse and valid knowledge, defying political determinism (“sociologism”) and saving “he who opposes it with criticism and demand” from the charge of the “impudent madness of believing there is something that is superior to history.”³⁸ Hence, although Guttman was the most able to fill that position of authority, indeed the only qualified academic social scientist in the country, his scholarly habitus, shaped by rigorous training in the

³⁸ Buber, "The Demand of the Spirit."

discipline of objectivity, professional ideals and career trajectory and identity defined by technical expertise made him wholly inadequate for the job.

The transformation of social inquiry from a mode of public reasoning and intellectual equipment of the liberally educated, into a field of academic learning (university science) and occupation for specialists—an international pattern of intellectual organization in the human sciences after WWII—obeyed in Palestine a different historical logic, that of competing visions of Jewish modernity and the national project.

Vienna in Jerusalem

In an interview conducted many years later, Ruth Guttman, Louis' wife, recalled how unwelcome they felt upon arriving to Jerusalem. They were both surprised—and deeply disappointed—by the hostile attitude and seeming insularity of the Jerusalemite academic community:

When we immigrated in 1947, Louis informed the relevant departments that he was arriving, but we didn't hear back from anyone; they didn't even invite him...the only [academics] who met with him were [Professor Michael] Fekete, the famous mathematician, who was familiar with Louis' contribution to matrix algebra. He invited Louis to have coffee with him at Café Vienna. The second was a physics professor, who invited us to his house. But neither of them said, "actually, the place of Louis Guttman is at the HU," [even though] the HU was a small university, and it needed such [prominent] names [among its faculty]. But Guttman's story is not unique [in this regard], the University did that with many other important people, who are today in other places."³⁹

³⁹ Ruth Guttman, video recording, the Guttman family collection. Michael (Mihály) Fekete (1886-1957) was a Jewish-Hungarian mathematician, known today primarily as John von Neumann's early collaborator. Following his immigration to Palestine in 1928, Fekete was among the first instructors in the HU Einstein Institute of Mathematics, an Institute he eventually headed. He later became the Dean of Natural Sciences, and between the years 1946-1948, served as the university's Provost. Even though Ruth singles him out as an exception to an overwhelmingly unwelcoming community, the fact that Fekete was at the time, due to his administrative position and overall status, in a capacity to offer institutional recognition and yet he chose not to is evidence that Guttman was considered an outsider regardless of disciplinary divisions.

Ironically, the establishment of the National Jewish Home, for which the university was meant to be a center, had marked the end of a golden era for the HU that had lasted for nearly two decades. Eroding institutional autonomy and gradual enforced nationalization meant that ideological ordering, and hence some measure of exclusion, was the order of the day in the university. True, the university was guaranteed the right to manage its own academic affairs, but the ultimate supervision and control of higher education, together with its financial support, was now in the hands of the state. The question was how immediately that control was to be exercised and how directly the teaching program of the university was to be adjusted to the practical needs of the government. State institutions (including the *Histadrut*) were pressing to expand the study of economic and social sciences, which hitherto had been a minor section of the General Humanities. A separate Faculty of Social Sciences (the Kaplan School of Economic and Social Sciences) would come into being only in 1953, but as a first step, the University built up a Department of Social Sciences and Economics around the several subjects in this field which had been taught since before 1948. The composition, mission, and curriculum of this new department became an arena of a mighty struggle, in which the question of the ethos and political role of social science was tightly intertwined with the momentous question of the future of Jewish modernity

The postwar moment was also crucial for the Jewish world as a whole, against the backdrop of the destruction of European Jewry and mass displacement. The question of the future of Jewish culture and learning enhanced the symbolic dimensions of the university's existence as a new center in a refigured Jewish world. The role of the American Jewry in maintaining the university shaped this question. In this context, Senator's dramatic declaration can be understood as an appeal to the Diaspora, to both benefactors and intellectual supporters,

to make the future of the HU their concern. The Jewish (European) postwar ‘Republic of Scholars’ played the role of extended, transnational (in fact, supra-national or extra-territorial) Jewish intellectual “republic of letters.”⁴⁰

The Committee on Social Science was a designated academic committee appointed by the senate to plan and populate the projected school. Except for Assaf, the original committee members were German-born or educated Central European Jews who received secular academic education in the classical humanities in the finest universities.⁴¹

In the first official meeting of the Committee, held in July 1944, Senator gave an overview of developments in recent years, highlighting the different problems the university had encountered in its attempts to institutionalize academic instruction and research in the social sciences. “The main question to be settled,” Senator wrote in a letter to a colleague around that time, “is that of the central post,” that is, of the scholar who would administer the founding of the School or Department and eventually occupy the head position. At that first meeting the

⁴⁰ Hacoen argues that the dilemmas of national integration Central European Jewish intellectuals faced inspired their utopian visions of a Republic of Letters. “An entire generation of émigrés imagined a Central European culture that survived the collapse of Central Europe and continued to exist in exile as a Republic of Scholars.” Malachi H. Hacoen, “Dilemmas of Cosmopolitanism: Karl Popper, Jewish Identity, and ‘Central European Culture,’” *The Journal of Modern History* 71, no. 1 (1999): 105-149.

⁴¹ The Committee members: were Assaf (Chair, Talmud and Rabbinical Literature), Guttman J. (Rabbi, Jewish theologian and philosopher of religion, Jewish Philosophy), Koebner (History), Tur-Sinai (formerly Torczyner, Semitic Languages), Schwabe (philologist, Classics dept.), Senator and Buber. In 1941, a committee was appointed to “re-discuss the general question of developing the social sciences in the university.” This committee produced another report, which was discussed by the Standing Committee on December 8 that year. In the report the committee suggested the creation of two “minors” (secondary fields), “the economic sciences” and “social and political sciences.” They made no concrete suggestions regarding personnel. Later on, Buber replaced Assaf as Chair; Poznański was appointed Secretary and the Committee, which by mid-1948 consisted of no less than thirteen faculty members, had been joined by the adjunct lecturers appointed to teach an assortment of courses in the social sciences offered by the university and formed the nucleus of the planned School: Bonne (Economics of Palestine and the ME), Bacchi (Statistics and Demography), Tartakower (Sociology and Demography of the Jews), and Feinberg (IR). The jurist Moshe Smoira, President of the Supreme Court and member of the HU’s Executive Committee, joined as representative of the interests of the newly founded State. The Committee became more “professional” in early 1949, when its membership was narrowed down and limited to faculty with social science background.

Committee decided to launch an international search and cast their net widely, tapping into a reconfigured Jewish intellectual network, and mobilizing contacts on both sides of the Atlantic.

At that point, Senator had already been in touch with two benefactors of the university. In March, he had written at length to Norman Bentwich, the Jewish-British diplomat and Zionist enthusiast who had served on the faculty of the HU and stood at the helm of the British “Friends” organization, asking for his help in looking for suitable candidates in England.⁴² In May he approached Salo Baron, who in reply offered to form an academic committee to help with the search in the United States:

Our Committee could appoint a subcommittee of social scientists, especially the older men who would not have any personal interest at stake, and ask them to review the whole field and to ascertain both the qualifications and the availability of the prospective candidates. Such procedure would have the advantage of greater objectivity and wider range of both specialized and personal knowledge...Otherwise I might institute informal, personal inquiries and let you know what I hear.⁴³

The “problem” itself was seen as a general Jewish or Zionist matter—in the sense that the HU was a general Jewish matter, and in that the future of the social sciences was of general Jewish concern for those in the diaspora as much as in Jerusalem.

The Americans Are Coming

Things shifted when at the conclusion of the 1949 meeting of the HU’s Board of Governors (the first meeting ever to be held in Jerusalem), Dr. George S. Wise, a wealthy

⁴² Senator to Bentwich, March 2, 1944. File 2276, HUA.

⁴³ Salo W. Baron to Senator, July 18, 1944. File 2276, HUA. Salo Wittmayer Baron (1895–1989) was at the time the Nathan L. Miller Professor of Jewish History, Literature and Institutions at Columbia University and was recognized as *the* outstanding living Jewish historian.

industrialist and business executive from New York, announced an annual gift of \$5000 for the next five years for the University's Department of Social Sciences, "particularly for research in sociology."⁴⁴ Although the donation was not directly related to the sociology search, and even though it involved a rather modest sum, Wise's "gift" proved to be a game changer. It allowed a powerful new player into what was, until then, an internal affair, and through the instrument of the board, it gave him the power to influence decisions regarding the hiring of academic staff.



Figure 12: George Wise (photographer unknown, courtesy of Tel Aviv University Archives)

Wise's involvement turned Guttman from a

nuisance into a real problem and gradually also a threat at the institutional level.

Wise's investment in the advancement of sociological research in Israel was motivated by both personal and ideological reasons. Markedly different from the previous generation of American university benefactors in his cultural identity, and consequently in how he conceived of his role, Wise was a new kind of donor, who evidently also wanted to "manage." Support for the HU during the first decade of its existence came primarily from non-Zionist Jewish groups in the United States. Fundraising was made possible through the personal ties that Judah Magnes,

⁴⁴ "Gift to Hebrew University's Department of Social Sciences," 31 May 1949, File 720, HUA. Since the establishment of the university and until the State came into existence, the board never met in Jerusalem. All its meetings, except for one, were held in Europe. After 1948, all the meetings were held in Jerusalem. This alone was a clear indication for changing tides in the status of the HU.

the University Chancellor (and later president), had with the old money of American Jewry: bankers Felix Warburg and Jacob Schiff, lawyers Louis Marshall and Samuel Untermyer, investor Maurice Wertheim, and other extremely wealthy Jewish leaders and magnates from New York collectively known as the ‘North Manhattan Jews.’ These philanthropists gave Magnes, whom they trusted to represent their interests, full authority to decide on how to use their and their friends’ immense contributions, and remained wholly uninvolved in the ongoing management of academic affairs.⁴⁵ By contrast, Wise, a boy from the *shtetl* who made a fortune in state modernization and economic development projects in post-revolutionary Mexico and later in Israel wanted in on the plans and programs the funds he had donated helped finance.

Seeking active involvement in academic decision-making, he was soon able to propel himself into a position of real influence. In 1951, shortly after he made the donation, Wise was elected to head the ‘American Friends,’ the largest and wealthiest among the postwar Societies of Friends of the HU around the world. Two years later, with the support of former U.S. Secretary of the Treasury, Henry Morgenthau Jr. (who was in charge of the United Jewish Appeal for the young State of Israel during the 1948 Palestine War and one of Wise’s many prominent connections) was appointed chairman of the HU’s board of governors.⁴⁶ Serving in

⁴⁵ In the years 1925-1929, the ‘Magnes group’ was able to channel to Jerusalem enough funds to finance the University’s current expenditures and its entire construction plan. On its relations with the HU and the changing structure of U.S. private financial support, see: Eliyahu Honig, “The Societies of Friends of the Hebrew University, 1925-1948,” in *The History of the Hebrew University of Jerusalem*, vol. 2, ed. Hagit Lavsky (Jerusalem: Magnes Press, 2005), 113-142. (Hebrew).

⁴⁶ The Board of Governors was the supreme governing institution of the HU, and the university’s official owners. It functioned as both the legislature and the executive, deciding on the budget and overseeing its execution. Before the establishment of the State, it consisted of 34 members: representatives of Zionist and non-Zionist organizations from both Palestine and the Diaspora, private donors, mainly from the U.S., and senior Jewish scientists and scholars from around the world. Wise’s election for chairman symbolized the de-Europeanization of the Board and the beginning of a new era: while in 1927, the representatives from Europe constituted 50% of board members, by 1948, European representation was cut down by half, and none of the European board members was from the continent. U.S. representation remained stable throughout the 1940s (40+%), and the representation of the settler society in Palestine doubled: from 14% in 1927 to nearly 30% in 1948. Uri Cohen, “The Management Institutions of the

this capacity for nearly a decade—a period that saw the building of the HU’s new Givat Ram campus, an ambitious urban and architectural project which was financed primarily through donations—Wise carried clout and became known as a forceful, strong-willed, and daring executive, who liked things done his way. “Dr. George Wise was not a man for tears,” said those who worked with him.⁴⁷ Once he had proven himself, fundraising, which had been his main responsibility as chairman, was no longer satisfying enough. His passion became managing the projects that the fundraising had made possible. Having forged close ties with Israeli state officials and politicians, Wise was offered to preside over the HU. “*’Ich darf haben a balebos’* (Yiddish, lit. I need someone in charge), [prime minister] Levi Eshkol told me, and...I thought it wasn’t such a bad idea.”⁴⁸

Wise did not consider himself an academic *per se*, yet he had a social science Ph.D., some applied research experience, and a clear view regarding the trajectory the University should take, and he felt more than qualified to lead the way. As he explained in various newspaper interviews, for him, the presidency was a way to promote a new understanding of the research university and its relationship to society, an understanding he shared with other postwar American academic leaders and extra-university elites. According to this new understanding, the exemplary social function of the university no longer lay in its embodiment of an ideal life form, that of the academic community (itself bearing an exemplary character according to Habermas), but in its being a prime instrument of national purpose for social engineering aiming at

Hebrew University, 1925-1948,” in *The History of the Hebrew University of Jerusalem*, vol. 2, ed. Hagit Lavsky (Jerusalem: Magnes Press, 2005), 3-70. (Hebrew).

⁴⁷ Haim Ben-Shahar, ed. *Festschrift in Honor of Dr. George S. Wise* (Tel Aviv University, 1981), vii.

⁴⁸ Raphael Bashan, “At the Beginning There Were Twenty Dollars,” *Ma’ariv*, April 3, 1970. The Yiddish expression literally translates to “I need someone in charge.” Yet, the English equivalent misses the cultural mark, as the word “balebos” means proprietor, owner, or master of the house.

establishing an American style modernity—an instrument devoid, or rather, free, of a normative self-image and a representational role.⁴⁹

Much to the chagrin of the mandarins, who considered the university an end in itself (which also justified and secured their communal existence), Wise believed in a service-oriented university suited to meet pressing social needs. In the case of Israel, these were reflected in the national agenda the government had set. Since the University was, in his view, an essential means for achieving economic growth and social development, Wise rejected the elitism of the HU and wanted to make higher education widely accessible.⁵⁰ He envisioned a popular and inclusive institution that would cater to the masses, providing basic scientific literacy to the citizenry, training professionals in various required fields, while at the same time advancing cutting edge technoscientific research and development.⁵¹ “This presidency united the two great convictions of my life,” Wise reflected in retrospect, “my faith in the State of Israel, and my

⁴⁹ For a critical discussion of the Humboldtian ideal and an attempt to save something of its spirit, see Jürgen Habermas and John R. Blazek, “The Idea of the University: Learning Processes,” *New German Critique*, no. 41 (1987), 3-22. The notion of the “instrumental university” is taken from Ethan Schrum, “Administering American Modernity: The Instrumental University in the Postwar United States,” (Ph.D. diss., University of Pennsylvania, 2009). Interestingly, Schrum argues that for Clark Kerr, the “inventor” of the instrumental university conception, social sciences were at the core of this new kind of American university (replacing agriculture) because they were the key to the administrative knowledge required to fulfill the university’s main goal – to “administer the present” – particularly by fostering economic development.

⁵⁰ Uri Cohen, *Academia in Tel Aviv The Making of A University* (Jerusalem: Magnes Press, 2014), 226 (Hebrew).

⁵¹ Wise’s vision was articulated and implemented through Tel Aviv University (TAU), the frivolous younger sister of the HU that Wise developed and led during the 1960s. Due to fierce resistance from the faculty, Wise was voted out of the presidency, and left the HU in anger. He then became the first—and most glorious (as well as notorious) president of Tel Aviv University. In eight years, Wise turned it from a small crippled institution under the long shadow (and actual control) of the HU, into a full-fledge, forward-looking, modern university known for its technoscientific orientation and strong professional schools. For the history of TAU and Wise’s role in its making, see: Uri Cohen, *Academia in Tel Aviv*.

belief that the fate of a people is determined by education. Education is more crucial than economics and even politics.”⁵²

The conviction that technoscientific knowledge—which was what Wise meant by “education”—paved the road to prosperity and self-realization, both collective and individual, was partly rooted in his personal biography. Arriving in the United States as a poor Jewish student from Russia-occupied Poland where anti-Semitism was rife, Wise discovered that his ethnic identity no longer mattered; academic merit was the ticket to social acceptance and “success in life.”⁵³ Wise began to study medicine at Columbia University after graduating *summa cum laude* as a bachelor of science from Furman University, a small southern Baptist college in South Carolina. However, due to illness and financial difficulties, Wise had to abandon his dream of becoming a doctor after two years and enrolled in sociology instead. Earning a Master’s degree in 1930 and continuing on to a Ph.D., the skills he had acquired conducting empirical social research on immigrant communities in New York led him to Cuba and then to Mexico, where he found both a dissertation topic and a once-in-a-lifetime business opportunity. Forging close ties with movers and shakers in Mexican politics in the late 1930s, Wise gradually turned himself into the largest individual supplier of newsprint to the country. He made a fortune, while at the same time becoming involved in various state modernization projects. After supporting a national campaign for the eradication of illiteracy, he was invited to sit on the government's Committee on the Elimination of Illiteracy. Similarly, after building the first newsprint mill in the country as part of efforts to encourage manufacturing via forced

⁵² Reuma Ziskind, “You Can Accumulate Money, But You Can Never Accumulate Enough Science,” *Ma’ariv*, July 6, 1987.

⁵³ Bashan, “At the Beginning.”

industrialization and import substitution programs, he was appointed a member of a governmental delegation for facilitating Latin American-wide economic cooperation.⁵⁴

Throughout this entire episode Wise became very rich. He moved to Mexico and spent a few years there to ensure that his business ran well. He would do the same in Israel. Mexico served a double function in Wise's career. It proved to be a bountiful place for doing business at a time when the U.S. economy was reeling from the depression, and Mexico also turned out to be a uniquely American "object of development."⁵⁵

Although Wise often declared his love of nation, his link to Israel should be viewed in this same light. Wise brought to Israel the successful model of parallel, rapid development in both industry and education under state sponsorship. His experience and success in Mexico nurtured his ambitions regarding Israel.⁵⁶ Israel was another "object of development," with TAU the crown jewel. Recruiting top scientists, primarily from the U.S., who would bring cutting-edge ideas and techniques with them, as well as the right kind of ethos, became a central strategy. But it should be noted that his was a market approach to knowledge and implied the commodification of scholarship, which went hand-in-hand with a subordination to national

⁵⁴ Wise's Mexico connection is made intelligible if placed within the context of the formation of a uniquely American style of development. As Ekbladh argues, the ideas and practices that would comprise modernization were extant well before the term earned its common currency in the years following World War II. New approaches were emerging from the social sciences...that would transform how reformers conceptualized the needs of societies the saw demanding of their intervention. See: David Ekbladh, *The Great American Mission: Modernization and the Construction of an American World Order* (Princeton, NJ: Princeton University Press, 2011), chapter 1.

⁵⁵ Timothy Mitchell, "The Object of Development," *Rule of Experts: Egypt, Techno-politics, Modernity* (Berkeley: University of California Press, 2002), 209-241.

⁵⁶ Wise became the President of American Israeli Paper Mills Ltd. in Hadera—a paper plant established by a group of Jewish paper industrialists in 1950 to provide employment opportunities to hundreds of immigrants. He was also among the founders of *Clal*, an industrial and commercial development corporation. *Clal* became an instrument for channeling the support of Latin American Jewry, which had not been involved in the Zionist state project until then. Through *Clal* he fundraised significant sums for the development of the Israeli economy, and his ties with Sapir and Eshkol tightened. Wise served as the company's president in its early years. He was also on the Board of Directors of the Industrial Development Bank, chairman of the American-Israeli Society and President of Israel-America Chamber of Commerce.

aims.⁵⁷ And coupled with this was a belief in and commitment to an aggressive managerial style.⁵⁸

In accordance with this credo, it took Wise only five months to intervene and begin actively advocating for Guttman's hiring. For Wise, the hiring of Guttman was a means to implement his vision of what the university should become, and the way in which he endorsed him foretokened that brave, new future. The HU appointment process commonly relied on personal contacts and good faith, rather than on professional metrics. The leading faculty would enquire with their confidants overseas, ask around about the candidate's reputation, and then look for someone who knew the candidate personally, or knew someone who (allegedly) knew the candidate personally (and so on) and could testify for the latter's character strengths and flaws as well as for his scholarly profile. Certainly a longstanding and carefully cultivated cultural preference, this academic procedure was also a matter of necessity. Since the mandarins sought a nominee of a certain kind—an intellectual of Central European decent, a man of letters, with whom they shared upbringing, values and worldview, in other words, a nominee who was a “Jewish gentleman,” just like them; and since they feared state intervention, decisions on appointing a new member of the faculty had to be concluded, as Buber often said, among friends. Wise's unsolicited nomination—especially as it came from an outsider who was not part of the HU professoriate, nor part of the Jerusalemite mandarins' transatlantic Jewish intellectual

⁵⁷As President of Tel Aviv University part of his overall conception was the construction of new academic frameworks like those already in existence at the Hebrew University, and but also others that would be innovative, based on recent academic developments in the United States. See Cohen, *Academia in Tel Aviv*, 243. The case of the business school, the school of journalism, and the Fashion Institute of Technology, (Ibid., 258-59) are illustrative of Wise's approach. Furthermore, Wise became very involved in academic appointments in the manner of American university presidents before World War II. This interventionist style put him on a collision course with the faculty, who felt he was undermining their authority and status.

⁵⁸ At HU, following the Hartog Committee report, the chancellor position was cancelled, and the position of the chairman of the board was separated from the presidency. A rector was elected in 1935 and was responsible for all academic matters. Wise wanted to reunite the functions at TAU, and aimed to expand his job description and become the sole authority in both administrative and academic matters.

network—therefore violated both custom and manners and challenged the authority of the faculty over academic matters, intellectual value, and scientific credibility. But far more significantly, it threatened to bring in a mindless professional they could never trust.

In addition to leveraging his new position of power and influence as a member, and soon-to-be chairman, of the Board of Governors of the HU, Wise mobilized his scientific ties at Columbia University in order to promote the appointment. That fall, when Senator was on a study tour of university administration in the U.S., Wise invited him to a dinner-party at his house in New York.⁵⁹ He also invited two of the most eminent sociologists of the period: the Austrian-Jewish survey research tycoon Paul Lazarsfeld, who had founded Columbia University's Bureau of Applied Social Research and had just been appointed Chair of the university's famous Department of Sociology, and the neo-Malthusian sociologist and demographer Kingsley Davis, who had recently completed his first and key work, *Human Society* (1949), and had left his Princeton professorship to become the Bureau's acting director. Both men were at the helm of the discipline, exerting great influence on its professional associations and shaping the epistemological taste and values of an entire generation of American social scientists.⁶⁰

⁵⁹ According to correspondence with Salo Baron, Senator initiated this study tour in preparation for a massive expansion and reorganization at the Hebrew University that was about to take place with the establishment of the State of Israel. The original plan included visiting universities in various countries, among them Soviet Russia. In the end, Senator traveled only to the United States. This time of change – questioning existing arrangements, searching for relevant institutional models of knowledge production and transmission – made the HU more vulnerable to meddling. It should be considered as part of the wider context for the Guttman affair, as it had likely contributed to the question of his hiring becoming an arena for a mammoth struggle over the cultural identity of the university, and by extension, of Jewish intellectual modernity. See: Letter from Salo W. Baron to Werner Senator, May 11, 1949, and David Werner Senator, Circular Letter to the HU Staff Members, August 31, 1949, Senator Personal File, HUA.

⁶⁰ Craig Calhoun & Jonathan Van Antwerpen, "Orthodoxy, Heterodoxy, and Hierarchy: "Mainstream" Sociology and its Challengers," in *Sociology in America: A History*, ed. Craig Calhoun (Chicago: University of Chicago Press, 2007), 367–410. Jennifer Platt, "Sociology," in

The unstated goal of this unlikely get-together, as Senator well understood, was to make the case for Guttman's inclusion in the HU's nascent Department of Social Sciences. Indeed, as Senator noted in his travel journal, Guttman was the main topic of conversation post-dinner.⁶¹ The two sociologists commended his unique abilities and applauded his brilliance. This was not empty praise. Lazarsfeld, who met Guttman when the two collaborated, along with many others, on turning the results of wartime research into *Studies in Social Psychology in World War II* (better known as *The American Soldier, TAS*), thought the work of the young Minnesotan was truly revolutionary.⁶² "Guttman's first publication was quite tantalizing to the research fraternity," he later recalled.⁶³ Both Lazarsfeld and Guttman were trying then to figure out how to quantify what has since been termed subjective phenomena—social attitudes, political beliefs, personality traits—and how to do so systematically.⁶⁴ Both were deeply committed to developing new methodologies, techniques, and tools which were to be uniquely appropriate for the observational study of human behavior and social relations, and both had doubts regarding the applicability of existing statistical methods, such as factor analysis, to data collated by means

The History of The Social Sciences Since 1945, ed. Roger E. Backhouse and Philippe Fontaine (Cambridge; New York: Cambridge University Press, 2010), 102-135.

⁶¹ David W. Senator, "Addendum to Diary of October 30," Copy M3 to Permanent Committee, Mr. Poznański, Prof. Bonne, Prof. Buber, November 16, 1949, Senator Personal File, HUA.

⁶² Samuel A. Stouffer, et al., *Adjustment During Army Life*, vol. 1 of *Studies in Social Psychology in World War II: The American Soldier* (Princeton, NJ: Princeton University Press, 1949); *Combat and Its Aftermath*, vol. 2 of *Studies in Social Psychology in World War II: The American Soldier* (Princeton, NJ: Princeton University Press, 1949); *Measurement and Prediction*, vol. 4 of *Studies in Social Psychology in World War II: The American Soldier* (Princeton, NJ: Princeton University Press, 1949).

⁶³ Paul Lazarsfeld, ed. *Mathematical Thinking in the Social Sciences*, rev. 2nd. ed. (Glencoe, Ill.: Free Press, 1955), 11. Lazarsfeld (as did several others later on) had crowned Guttman as the true heir of L. L. Thurstone, and said he was the first psychometrical theorist in twenty years to renew factor analysis.

⁶⁴ For the most comprehensive review of the field and its development, see Charles F. Turner and Elizabeth Martin, eds. *Surveying Subjective Phenomena*, 2 vols. (New York: Russell Sage Foundation, 1984).

of survey questionnaires. Though they came up with competing theoretical models — scalogram analysis (or cumulative scaling) in the case of Guttman and latent-structure analysis in the case of Lazarsfeld — and though Guttman was unabashedly critical of his senior colleague, the two corresponded intensively as they prepared their respective contributions to the fourth methodology volume, and each held the other in great esteem.⁶⁵ A clear expression of that appreciation appeared in print a few years later, in an edited volume on mathematical thinking in the social sciences. Introducing the different authors, Lazarsfeld attempted to give the reader a sense of Guttman’s novel approach to measurement:

The early efforts of social and psychological measurement took very much for granted that there would be no logical differences between the variables to be used in the social sciences and those which the natural sciences dealt with. The population expert was interested in age or number of children; the economist dealt with quantity of goods or amount of dollars; the psychologist interested in intelligence tests seemed to deal with the number of questions accurately answered. Guttman was one of the first writers who saw how much of the social scientist’s work deals with what has come to be called qualitative characteristics or attributes. Such distinctions as male or female, white or negro, owning or not owning a house, agreeing or disagreeing with a certain position—are the kinds of raw data from which we build up behavioral indexes and scales. A clear analysis of the operations of combining these basic elements into more complex measures had been badly neglected... Guttman’s scalogram analysis and...the cumulative scale which has since become well known under Guttman’s name [offered the first systematic approach to analyzing such data].⁶⁶

Once *TAS* was out, these new sophisticated methods of survey instrument design and data analysis became of interest also to individuals and organizations outside the Research Branch orbit. Guttman quickly gained a reputation as a rigorous and imaginative problem solver and the top echelon of American behavioral science wanted him on various projects (and was willing to

⁶⁵ The correspondence continued after Guttman had left for Palestine/Israel. See, for example: Louis Guttman to Paul F. Lazarsfeld, 18 June 1949, HUG (FP) 31.6, Box 4, Samuel Stouffer Papers. The work on the series began in summer 1945. The fourth and final methodology volume was published in 1950. For the two models, see: Stouffer et al., *Measurement and Prediction*.

⁶⁶ Lazarsfeld, *Mathematical Thinking*, 11.

pay for it).⁶⁷ In fact, mentors and colleagues felt that his talents might go to waste in the scientific wasteland that was Israel and tried to convince him to reconsider his plans: “some of us think you can contribute more fundamentally to the promotion of social science by continuing to serve full time in some capacity in this country,” said a heartfelt letter from F. Stuart Chapin, Guttman’s advisor at the University of Minnesota and a founding father of scientific or “objectivist” sociology.⁶⁸ “While the experiment in which you are participating is of great interest throughout the world, it must necessarily be at a stage of development that could be best served by people with less specialized skills and knowledge than you have.”⁶⁹

The HU’s executive vice-president, however, remained unimpressed. Moreover, he managed to twist things around so that the distinguished guests ended up agreeing with him, despite themselves. As his report of the evening concluded, all present “spoke very highly of Guttman.” And yet, he added—not without a hint of satisfaction—even “a great admirer of professor Guttman” as Lazarsfeld “admitted that the special qualities of Guttman lie in the field

⁶⁷ “[I’d like us] to come to some arrangement or other where we could trade some of our funds for some of your ideas,” Lazarsfeld wrote shortly after the dinner at Wise’s, asking Guttman to join his Columbia team as consultant on a new Rand-funded project. Paul F. Lazarsfeld to Louis Guttman, November 16, 1949, Guttman Personal File, HUA. Samuel Stouffer wrote to Guttman earlier that year inviting him to collaborate on an extensive team project on scaling. Like Lazarsfeld, he promised to pay “a really juicy honorarium” in exchange for Guttman’s time. In the same letter, Stouffer also mentioned Talcott Parsons’ enthusiastic response to Guttman’s work. The two would later collaborate. Samuel Stouffer to Louis Guttman, January 10, 1949, HUG (FP) 31.6, Box 1.

⁶⁸ Francis Stuart Chapin (1888-1974), a sociologist, university administrator, and academic entrepreneur, who was one of the first American sociologists to apply the research procedures of the physical sciences and the techniques of statistics to studies of social behavior, particularly in the areas of social and cultural change and social status. He played a key role in creation of a quantitative, statistical sociology in the United States in the interwar period. For his role in the rise of “objectivism,” see: Robert Bannister, *Sociology and Scientism: The American Quest for Objectivity, 1880-1940* (Chapel Hill: University of North Carolina Press, 1987), chapter 10.

⁶⁹ F. Stuart Chapin to Louis Guttman, November 10, 1949, Guttman Personal File, HUA.

of sociological statistical research,” namely, in what Senator and his German-educated brethren considered a mere considered a merely descriptive endeavor with little scientific value.⁷⁰

Shifting the emphasis from the question of Guttman’s suitability to that of his endorsers’ credibility—which in turn depended entirely on their embodied in-person performance, as the Jerusalemite Vice-President was unfamiliar with the work of either sociologist—Senator reported that Lazarsfeld “makes a good but not overwhelming impression” and may not be “objective enough to judge the case of Goodman [sic] with regard to his possible appointment at the University.” Kingsley Davis, on the other hand, “who made by far the impression of a more measured and scientific man,” showed “a discriminating understanding for Guttman’s special qualities and our special and general needs.”⁷¹ Elusive as this talk about special qualities and general needs may have been, the message was clear: the fact that Guttman was good for science did not necessarily make him good for the Jews. It did not matter how qualified he was by American professional standards; the HU had its own ways of assessing academic merit, and considerations other than technical prowess and specialized knowledge had to be taken into account.

Reportedly “taken aback” by the results of the evening, Wise decided to take a more straightforward approach.⁷² A few weeks later, he telegraphed the HU: “[I] would appreciate greatly your contacting Dr. Louis Guttman and cabling me [the] results [of] you [sic] conference with him. Stop. Matter very urgent.”⁷³ The urgency was due to the fact Guttman was still

⁷⁰ On the German origins of statistics as a descriptive science of the state, see Theodore M. Porter, *The Rise of Statistical Thinking, 1820-1900*(Princeton: Princeton University Press, 1986), chapter 1.

⁷¹ Senator, “Addendum to Diary of October 30,” November 16, 1949.

⁷² Ibid.

⁷³ George S. Wise to Dr. Senator, November 22, 1949 (telegram), Guttman Personal File, HUA.

debating whether to stay in Israel, where his heart was, or to return to the United States. But with his research funding running out, Cornell pressing him for an answer, and both Columbia and Harvard awaiting his call, he was nearing the point of decision.⁷⁴ Wise was concerned that unless the university authorities acted fast, another disillusioned Zionist scientist would choose his career over his ideological commitments and sense of mission, both political and scientific, and trade the uncertainty of a scientific life on the postcolonial frontier for the material comforts and job security of the American research university. The exodus of technical intelligence out of Israel during the 1950s, Wise would argue later, was to a large extent the fault of the HU's exclusionary policies, and it came at great expense to the young Israeli state and its rapidly modernizing society.

Others felt that letting go a "high-level scientific researcher" such as Guttman would be a mistake regardless of the national agenda. As proponents of his appointment argued over the next two years, securing the services of "a young scholar who made a name for himself in the scientific world" was an opportunity a university chronically short on the late-modern kind of academic charisma could not afford to miss.⁷⁵ Furthermore, Guttman was potentially profitable to the university also in dollars and cents. Even though the vast majority of the faculty could not make sense of his work, various indicators suggested that his academic market value was quite high. Whereas the *Verstehen*-focused, long-winded proposals of the HU humanistic social

⁷⁴ Louis Guttman to Werner Senator, March 5, 1949, Guttman Personal File, HUA.

⁷⁵ See, for example, The Standing Committee of the Senate, Minutes of Meeting *vav* (Sixth), November 17, 1950, File 2276; The Senate, Minutes of Meeting *heh* (Fifth), February 7, 1951, Guttman Personal File, HUA. On the rationalization of academic charisma, see: William Clark, *Academic Charisma and the Origins of the Research University* (Chicago: Chicago University Press, 2006). I elaborate on this point below.

scientists had brought in very little grant money,⁷⁶ Guttman's methodologically rigorous projects, which promised practical results, fared extremely well in the U.S.-dominated research economy that emerged after WWII.⁷⁷ Clearly, a solution that would result in affiliating the young social science star with the University had to be found, and promptly so. But there were major obstacles to be overcome in order for this to happen.

As a devout Zionist and a friend of the ruling party,⁷⁸ Wise supported the subordination of the needs of the university to that of the state. The mandarins knew this and were concerned. Senator, for instance, described Wise as a Zionist who was "close to the Labor Movement."⁷⁹ His strong ties with the government raised the serious threat of over-involving the ruling elites—donors, officials, businessmen, generals—in academic affairs. Furthermore, Wise represented an American modern-instrumentalist vision for the university, which also involved the rationalization of the academic appointment process and criteria, that is, making academic

⁷⁶ See, for example, the collective application from May that year that Bonne submitted to the Rockefeller Foundation on behalf of the social science faculty (which was denied). Alfred A. Bonne to Joseph H. Willits, "Economic and Social Research in Jerusalem," May 3, 1949, File 2276, HUA.

⁷⁷ In the first four years of its existence, the Israel Institute of Applied Social Research carried out studies with the financial support of the Marshall Field, the Littauer, the Henrietta Szold, and the Ford foundations, as well as the U.S. Office of Naval Research and the U.S. Air Force (in addition to commissioned studies sponsored by Israeli government and public agencies, the Israeli Army, and commercial entities) in a sum totaling hundreds of thousands of dollars. See: Uriel G. Foa, "Four Years of the Institute of Applied Social Research," *International Social Science Bulletin* 6 (1954): 71-75; "U.S. Navy grants funds for surveys in Israel," 1951 press release (source unknown); and The Israel Institute of Applied Social Research, *Institute Projects for the Period 1951 - 53* (New York: The Institute, 1953). On the postwar patronage system for the social and behavioral sciences, see: Roger L. Geiger, *Research and Relevant Knowledge: American Research Universities Since World War II* (New York: Oxford University Press, 1993), 92-110; Hunter Crowther-Heyck, "Patrons of the Revolution," *Isis* 97 (2006): 420-446; and Mark Solovey, *Shaky Foundations: The Politics - Patronage - Social Science Nexus In Cold War America* (New Brunswick, N.J.: Rutgers University Press, 2013).

⁷⁸ Wise had close ties with Sapir, Eshkol, and Ben-Gurion.

⁷⁹ Letter from D. W. Senator to Ernest Jackh, October 29, 1949, File 2276, HUA. Ernst Jäckh (1875-1959) was a German-Jewish author and orientalist. During the First World War, Jackh was one of the main propagandists of the German-Turkish alliance and worked for Eugen Mittwoch and his Intelligence Bureau for the East (German: *Nachrichtenstelle für den Orient*). He immigrated to Britain in the 1930s and held the position of international director of the New Commonwealth Society until 1940 when he became Professor of Public Law and Government at Columbia University. He founded the Columbia Middle East Institute in 1948.

appointments according to professional expertise and regardless of virtue. This new vision also involved a new ideal of service, and an overall aspiration that the university would become a prominent site for social engineering. This dual assault on the autonomy of the mandarins made them nervous.

Yet despite his resistance to Wise's initiative, Senator agreed to bring the matter before the Committee of Social Sciences and to meet Guttman. One month later, Senator sent Wise a letter in which he launched the mandarin counterattack against Wise's meddling and the attempt to usurp the faculty's autonomy.

"It was most difficult to contact Professor Guttman," Senator opened the long letter, dramatizing the special efforts required on his part to satisfy Wise's request, while at the same time preparing the ground for the rebuttal he was about to deliver. "For three weeks I tried by phone and mail, only to hear that the phone had been disconnected and that he was not at that address. Finally, we ascertained that his [research] department had been moved and was listed under the Security Department (!) and I had him for a talk while I was ill in bed. I regret that although our talk was cordial, it was inconclusive."⁸⁰

Senator's account of his frustrating foray into the barely civilized state apparatus in search of Guttman (notice how filled with fear, scorn and loathing is the exclamation mark!) painted a vivid tableau of postcolonial uncertainty. Against the backdrop of ramshackle empty offices, a disconnected phone, letters that fail to reach their destination, and a whole research unit gone missing, the University stood firm. Despite the vast transformations that went with the founding of the new State, the Hebrew mandarins remained guided by a clear *Weltanschauung*, and refused to surrender their reasoning to fleeting political pressures, to national agendas, or to

⁸⁰ David W. Senator to George S. Wise, December 28, 1949, Guttman Personal File, HUA.

the logic of the market. Although Guttman was available and willing, as well as supported by strong institutional players, *they* sought intellectual virtue, not a functionary who could do the job. The image of the sick European university administrator bravely at war with the circumstances from his bed completed the picture and gave it an aura of heroism.

“Cordial but inconclusive” were decorous terms that barely hid Senator’s annoyance. With the trouble of contacting Guttman serving as a metonymy for the troubled hiring negotiation and, perhaps, for Guttman’s overall disposition, this framing story about communication breakdown meant to indicate to Wise at the outset that the matter could have been resolved if Guttman had not caused difficulties, in more ways than one. As Senator went on to explain, the Committee, to which he had reported on his New York conversations, considered finding a distinguished ‘general sociologist’ to take Buber’s chair its top priority. Once Buber’s successor is appointed, the creation of an additional post “for someone like Guttman” could be contemplated. Yet, in light of the unfortunate situation the young American found himself in, the committee had decided to graciously extend him an invitation to temporarily join the social science ranks at the HU as an “external teacher” and offer courses “in the special direction” that he represented.⁸¹ Guttman, however, failed to appreciate the gesture. He was deeply dissatisfied with the offer and felt he deserved more. While the Committee believed that adjunct lectureship—“a position held by our Professor Bonne for many years,” Senator added pointedly—was a perfectly sensible starting point and a good arrangement, Guttman refused to be put on hold and wanted them to consider his candidacy for a permanent, tenure-track position solely on its merits, that is, independently of the question of the Department headship.⁸²

⁸¹ Ibid. On the External Teacher status and its meaning, see below.

⁸² See also: Louis Guttman to Martin Buber, October 22, 1950, Guttman Personal File, HUA.

Impudently, Senator ranted on, Guttman kept comparing the limited possibilities at the HU with the promising offers he had from leading American universities and did not seem disposed to make the personal sacrifice the faculty in power deemed necessary in order for him “to find his way to and in the university.”⁸³ Insisting that the intra-disciplinary nomenclature the Committee employed in its decision-making made little sense, he repeatedly argued that the public opinion surveys he was running for the Israeli Army, and more recently, for the government, were not the center of his *scientific* work but rather a source of raw data and a testing ground for the socio-psychological and methodological research he was primarily engaged in.⁸⁴ In fact, Guttman added in a separate letter to Buber, he had received training “in all the areas of sociology, the philosophical, the empirical, and the practical,” and was mostly known for innovations in theory and methodology. In that sense he was as ‘general’ a sociologist as there was, and should not be relegated to “sociometry.”⁸⁵

Finally, there was “the uncoordinated work of his institute.” “I know well that many enterprises in Palestine were created by the determination of an individual,” Senator admitted. But now, with the establishment of the state, “we have reached a stage where individual enthusiasm, initiative, and creative ability must be coordinated within the national framework, with due consideration of the means available.” In studying the adjustment of new immigrants as

⁸³ Senator to Wise, December 28, 1949.

⁸⁴ See also: “Memorandum of Understanding for a Meeting held in January 19th, 1950, among Dr. Senator, Prof. Bonne, and Prof. Guttman,” January 23, 1950, Guttman Personal File, HUA.

⁸⁵ Martin Buber to Louis Guttman, July 19, 1950 and Louis Guttman to Martin Buber, July 25, 1950, Guttman Personal File, HUA. In a private letter to Bonne, Senator admitted that the attempt of the faculty to “limit Guttman’s orbit” to such an extent that his teaching and research are confined to the study of public opinion might make his position “too uncomfortable, perhaps untenable.” Senator to Bonne, October 22, 1950.

part of a large-scale study in which the Institute had been engaged, Guttman had conducted himself in complete disregard of other scholars' efforts.⁸⁶

In an exchange between Bonne and Senator regarding Guttman's appointment that took place in early January, 1950, Bonne, whose institute had become the social sciences' empirical research organization of the university, stressed that the problem was not teaching needs, there were enough students to justify employing more than one person, but rather the issue was the demarcation and delineation of different areas in the field of research. Bonne stressed: "The country is small and the few existing research organizations must collaborate and coordinate...It doesn't make sense that...some individuals and their institutes could work at the same time and without planning on the same research topics,"⁸⁷ implying that such free-market mentality may be appropriate in America, but not in Israel. The three great C's of collegiality—'coordination,' 'cooperation,' and 'collaboration'—were key terms Guttman had to memorize if he wanted to join the faculty. If Professor Guttman was willing to be considerate, "taking into consideration the basic [more foundational] status of general sociology, an agreement satisfying all parties involved could be reached."⁸⁸

The main issue with appointing the argumentative and unruly young American, then, was neither institutional needs, nor area of expertise. It was, as Senator indicated to Wise, Guttman's shortcomings of character:

A man of Guttman's caliber is inclined to be biased and think that there are no better methods of research than his own. This is natural, if the scientist has won recognition in his country. It is also clear that such a person is apt to become conceited, arrogant, and

⁸⁶ Senator to Wise, December 28, 1949.

⁸⁷ "Memorandum of Understanding," January 23, 1950.

⁸⁸ Ibid.

somewhat contemptuous in his behavior towards others. Unfortunately, Professor Guttman has developed these undesirable attributes, which does not make things easier.⁸⁹

The Jewish Man of Science

Opposing an imposed appointment by means of dirt throwing and character-slaying was a well-established tradition in modern German academic culture that, like other aspects of this culture, had been imported to Jerusalem and acclimated well to the local weather.⁹⁰ A close review of the search record — official and personal correspondence, memos, meeting minutes and so on that span the entire tenure of the Committee— reveals that the Hebrew mandarins spoke in this manner about other candidates as well. Passing references to an individual’s personality, conduct, and disposition were a constitutive part of how the humanities faculty discussed and evaluated potential candidates among themselves, as well as in communication with confidants in their transatlantic network of trust. Sometimes it alluded to how the scholar in question acted on a particular occasion, or to how their virtues or vices manifested in their style of teaching. Even seemingly “professional” remarks on the quality of academic work and/or the abilities of said candidate were often moral assessments in disguise (e.g. “they think his scholarship unreliable, though pretentious”). Merit and morals were inseparable in these discussions and mattered equally in faculty reasoning over appointments. Or, put differently, personal virtues constituted an important criterion for judgments of academic worthiness and were almost always calculated into determining the best-qualified candidate.

⁸⁹ Senator to Wise, December 1949.

⁹⁰ Clark, *Academic Charisma*, 239-240, 418-419.

As Steven Shapin has convincingly demonstrated, personal virtues always matter in modern science.⁹¹ This was especially true for the academic culture at the HU, which was a small, closed off, and isolated institution that was in conflict with the Yishuv, and hence, on the defensive. In the case of Guttman, his personal vices — being arrogant, intolerant, argumentative, hardnosed, and competitive or not collegial — made him into someone they could not trust. They could not envision him in Buber’s place, especially in the political climate that had been created immediately after the establishment of the state.

Rather than simply a vicious ad hominem attack (though it certainly was that as well), I suggest that we read Senator’s letter, along with similar remarks and allusions in the archive of the Guttman affair, as an honest, if strategic, critical assessment of Guttman’s adequacy and suitability for the leading position in sociology at the HU. The critique was not directed simply at Guttman’s psychological disposition, what people would later refer to as his “difficult personality.” It was directed at the cultural ideal and values that Guttman embodied. The unflattering portrayal of Guttman’s behavior in the letter provides a particularly lucid window into the values and ideals at work informing their conception of the Jewish man of science in general and the Jewish sociologist in particular, namely, he had to be a certain kind of person and a certain kind of intellectual to be a truth-speaker.

The sense that for Guttman the HU was primarily a potential place of employment (rather than the secular temple it was in the eyes of its members and the general public), and the sociology professorship first and foremost a job (and one that carried no special spiritual or moral meaning), did not only offend the mandarins' cultural sensibilities, it also offended their much-cultivated self-and-public image as clergy serving at a secular temple. It put Guttman at

⁹¹ *The Scientific Life: A Moral History of a Late Modern Vocation* (Chicago: University of Chicago Press, 2008).

odds with the mandarin ideal of the man of science. The them, he appeared to be preoccupied with worldly (not to say petty) matters like academic rank, salary, and benefits. Senator's unflattering portrayal of an undecided Guttman who was toying with offers from other universities, carefully calculating his moves, and bent on maximizing both his economic gains and academic territory, meant to suggest, and critically so, that the American sociologist had his own best interests in mind, not the best interests of social science.⁹²

The faculty's moral fury over Guttman's reluctance to enter the HU as an external teacher manifested this dilemma well. It is not entirely clear whether "external teacher" was the equivalent of the German *Lehrbeauftragter* (lecturer), a paid part-time non-tenured teaching position for scientists who hold a Ph.D. (comparable with an adjunct assistant or associate professor in the U.S.).⁹³ Understood in this context, the seemingly offhand mention of Bonne's long-term adjunct position in the letter was not naïve, as punctuation may have suggested, but rather exemplary. "The poor and patient Bonne," as Max Warburg once referred to him, represented the selfless devotion of these struggling academic nomads.⁹⁴ A pioneer in the study of the economy of Palestine and of the modern Middle East, Bonne had been appointed in 1936 as a part-time External Teacher (adjunct lecturer) in 'Economics and Sociology of the Near East at This Time' within the Department of Islamic Culture at the Institute of Oriental Studies. Over

⁹² Throughout the negotiation, Guttman expressed concerns about the transfer of his pension, the procedure and timeline of appointment and whether it corresponded to his Cornell departure schedule. He wanted to know whether the time he would serve as a guest or adjunct professor – in case he agreed to this condition – would go into his seniority calculation and credited toward sabbatical. And he insisted that once tenured, the university "will arrange for a suitable apartment for my family as it is customary with professors invited from abroad." See: Letter from Louis Guttman to Marin Buber, August 20, 1950 and August 30, 1950, Guttman Personal File, HUA. The university representatives he corresponded with mostly ignored these concerns in their replies.

⁹³ Walter Rüegg, *A History of the University in Europe* (Cambridge: Cambridge University Press, 2011), 185.

⁹⁴ Quoted in Nachum Gross, "Social Sciences until 1948/49 – Plans and Beginnings," 512. (Hebrew)

time, minor changes in the scope of his teaching and the definition of his courses took place, but his status remained essentially the same. Bonne was the paradigm of self-denying and self-sacrificing devotion to social science. He waited for many years to be included among the faculty, teaching at the university without pay. Though he was fully employed at the Jewish Agency and was the director of a research agency, Bonne dreamt of the day that he would not have had to split his time between work and worship, the sacred domain of study and the profane world of work. Endlessly frustrated by the obstacles to the long-awaited transition of the Institute of Economic Research to university sponsorship, Bonne, in an outburst of emotion, lamented to Senator about his “wasted years.”⁹⁵

The preference for academic asceticism reflected the mandarins’ German-infected sensibilities. William Clark explains that in the German setting, the scholar was expected to reject material comforts. Such itinerant academics often lived in poverty, depending on their wives (if they were lucky enough to have one), and student fees. Clark argues that even famous professors needed to perform and demonstrate asceticism.⁹⁶ Guttman’s reaction to the offer, and the way he negotiated, made him seem a coldly calculating and wholly instrumental “player,” catering to his own needs. As such, he was thought to be defective with respect to collegiality. He wanted more than the share he was offered. Last, and most significantly, he was narrow-minded. The implication was that a high-degree of specialization and excessive preoccupation with a particular technique or procedure (and by extension, with method more generally) led to a tunnel vision and to scholarly parochialism and narrow-mindedness. In the world of the mandarins, narrow intellectual focus was always already parochial. Parochialism in turn was

⁹⁵ Alfred Bonne to David W. Senator, October 27, 1949. File 2276, HUA.

⁹⁶ William Clark, *Academic Charisma*, 255-256.

thought to breed vanity, stubbornness and disrespect toward others. In other words, Guttman's sophisticated methods of attitude measurement may have won him recognition, but they also made him a lesser man. Moreover, his "undesirable attributes" were invoked not simply because they were universally recognized moral defects; but because they were—to borrow from Steven Shapin—scientific vices: personal traits or manners that mattered for the practice of science in the world of the Jerusalemite mandarins.⁹⁷

Being wide-ranging rather than a specialist was both an intellectual and personal virtue in mid-twenty-century Jerusalem. The candidate for the lead position they were secretly considering—in fact, courting—during this phase of the negotiation with Guttman, and the qualities which made him attractive, serve well to illustrate the cultural logic and sociological dynamics that underwrote academic hiring at the HU.

The name of Leo Strauss (1899-1973), the renowned German-Jewish political philosopher, classics scholar, expert and radical interpreter of medieval Jewish and Islamic thought, first came up immediately after the war in the context of efforts to professionalize social inquiry in Palestine. The university was preparing for a large-scale expansion and the longstanding plan to build a social sciences unit finally moved into action. In line with these developments, the Committee of Social Sciences conducted an intensive search in political science during academic year 1945/46, looking for a widely respected scholar who was fluent in Hebrew and willing to relocate to Jerusalem. A committed Zionist, who had had Jewish religious schooling, Strauss had already made one attempt to join the HU faculty in 1934, when, at the urging of his lifelong friend Gershom Scholem (who emigrated to Palestine a decade earlier and had recently been made a professor of Jewish mysticism), he applied for a lectureship in

⁹⁷ Shapin, *The Scientific Life*.

medieval Jewish thought.⁹⁸ Strauss, who in the meantime left Germany, first for Paris and England and then for the United States, had spent the war years teaching at the New School for Social Research in New York, where he was when the HU approached him.⁹⁹ The Committee reviewed his list of publications and examined the syllabi for the courses he taught, but for various reasons, the university hesitated, Strauss doubted his own suitability, and the appointment fell through.¹⁰⁰ In late 1949, shortly after he became Professor of Political Philosophy at the University of Chicago, Strauss received another call from Jerusalem, this time about the chair in sociology.¹⁰¹

Strauss had neither formal training nor research interest in sociology, but the Jerusalemite mandarins felt that as a scholar of commanding intellectual stature, he was the most adequate man to fill the post. As in the case of Buber's position, specialized knowledge was considered neither necessary nor sufficient for the ability to carry out the tasks associated with particular

⁹⁸ According to Sheppard, Scholem pushed Strauss to publish work in medieval Jewish philosophy after the latter had lost his job at the Academy for the Science of Judaism in Berlin, and in an effort to obtain an appointment for him at the HU. The result was *Philosophie und Gesetz* (1935), Strauss' groundbreaking study of the political philosophy of Maimonides and his Islamic predecessors and a tour de force of maverick scholarship, which, Scholem believed, ruined the prospects for Strauss' appointment. The position eventually went to the target of the book's polemic: Strauss' former research director in Berlin, Julius Guttmann. Eugene R. Sheppard, *Leo Strauss and the Politics of Exile: The Making of a Political Philosopher* (Waltham, Mass.: Brandeis University Press, 2006), 73-74.

⁹⁹ In 1932, Straus was awarded a Rockefeller Fellowship in the social sciences, with the aid of which he studied medieval Jewish and Islamic philosophy in Paris and Thomas Hobbes in England. Unable to return to pre-war Germany, he continued his work on Hobbes at Sidney Sussex College, Cambridge. Like many other refugee intellectuals, he was unable to find permanent academic employment in England, and so in 1937, he emigrated to the United States, first taking an appointment as Research Fellow in the Department of History at Columbia University, then a professorship at the New School for Social Research, where he remained until 1948. During these years, Strauss served as associate editor of Social Research.

¹⁰⁰ According to Gross, the Committee was concerned that Strauss' philosophical orientation made him unsuitable to lead a discipline that should also include a description and analysis of reality. Strauss, in turn, doubted his own suitability and recommended another candidate. For details, see: Gross, "Social Sciences until 1948/49," 531-32.

¹⁰¹ During their November meeting, Buber informed the members of the Committee that Strauss "might agree to accept an invitation with us." It is not clear from the records what was the basis for this announcement. The Committee of Social Sciences and Economics, Minutes, November 28, 1949, Guttman Personal File, HUA.

academic positions. On the contrary, a high degree of specialization, itself a requirement of modern science, was associated with anti-intellectualism. What mattered was moral stature. This is well illustrated by a private letter Buber sent Strauss in January 1950. Buber opened with a mention of a conversation he had prior to writing with “our friend Scholem.” Establishing a pretext of commonality that rested on a shared history and intellectual friendship at the outset, Buber stirred away from teaching requirements and other professional considerations, focusing instead on Strauss’ scholarly virtues and how suitable they made him for the position. “I realize that the subject of Sociology, even broadly defined, does not exactly meet with your own research interests... but a man of your versatility and caliber can certainly be expected to accommodate himself to an institution as well as to modify the content of a subject to meet their own interests, as I myself did several years ago.”¹⁰² ‘*Vielseitigkeit*,’ the German word Buber invoked to describe Strauss’ central scientific virtue in the letter, means both versatility, in the sense of being able to adapt to many different functions, activities, or positions, and broadness (literally: many-sidedness), in the sense of covering a large number and wide scope of subjects or areas which are different from each other. As Buber indicated, versatility works both ways: the versatile scholar or scientist was so broad and varied in his knowledge and interests that he could adapt to any given institutional situation, which in turn he could transform. The mind of the versatile scholar or scientist was agile and limber enough to take the shape of a given position if so was required, but once inside, it redefined the position. This stems from intellectual agility. The versatile scholar was not a cookie-cutter product of the disciplinary division of labor; he was not sold on one way of doing things.

¹⁰² Martin Buber to Leo Strauss, January 16, 1950, Box 1, Folder 3. The Leo Strauss Papers, The University of Chicago Archives.

Versatility, as Buber kept explaining, was not only a testament of true intellectual greatness, but also an asset in rationalized modern academia, where job descriptions were used as a political tool. Strauss was everything that Guttman was not: a wide-ranging and versatile intellectual, cultured, a writer, and politically, a believer in the traditional view of the academic as priest. It was thought that he could hold the fort and protect against the danger to the normal development of the department posed by quantification and the specter of instrumentalism that was haunting Zion.

Guttman, by contrast with Strauss, swore by his methods. In fact, getting rid of the subjective dimensions of scientific judgment was at the heart of his project. The sovereignty of method ran counter to a venerable tradition in German academic culture: the absolute sovereignty of the Professor Ordinarius to legislate about people and texts, which in this case was impacted with religiosity and world-historical significance because of the aura that became attached to the sociology chair due to Buber's papal-like moral authority. According to Clark, this was a remnant of the early modern paternalistic power that the steamroller of rationalization had failed to eliminate. It stood in contrast to the disciplinary logic of organization, and to the institution of the academic department: a modern organizational structure based on division of labor and collective self-governance. And this was a culture Guttman knew little about and did not understand. When Strauss finally declined and the mandarins back-peddled on their promise—invoking again the senior appointment as impassable obstacle, Guttman lost it.¹⁰³

¹⁰³ In mid-March Strauss said he was principally leaning toward accepting the offer, but delaying his final reply for personal reasons. A month later he declined. See: Minutes of the Committee of Social Sciences and Economics, Minutes, Meeting Zayin (7th), March 13, 1950, and Minutes, Meeting Heit (8th), April 25, 1950, Guttman Personal File, HUA.

A letter he wrote to Buber in the summer is a case in point. In the letter, as if talking to a deaf man, he emphasized every word when outlining the common procedure for populating a department, as well as the academic etiquette of appointments. The rhetoric is that of rationalization: legally binding agreements and their violation, academic division of labor, etc. He speaks the language of expertise in areas of study, while they kept talking the language of persons (“important candidates”, “difficult person”) and collegiality (“we were friendly till now”).

The issue was really the person (group affiliation, cultural identity, pedigree, belonging in the ‘aristocracy of intellect’) and not the type of expertise or even style of reasoning.¹⁰⁴ The narrative of German-Jewish cultural history—such scholarship took as its point of departure the search for models of personal meaning or spirituality. It assumed a Jewish identity conceived as part of a mission to moralize the world or pass critical judgment on its imperfections. The letter is interesting not so much for how it captures the mandarin style or for the difficult coordination of needs and expectations, which was par for the course in the academic hiring process, even in mid-century Jerusalem, but for how it outlines the cultural values and ideals that governed and informed hiring judgment. Reading closely through the documents of the long and tedious hiring negotiation that followed, which took over a year, it becomes clear that it was not about a clash between traditions in sociology, but rather about the moral economy of social science. In the end, the mandarins would not accept technical expertise unaccompanied by personal virtue.

¹⁰⁴ Enthusiastic interest of committee members in Lazarsfeld was already mentioned in the long letter Senator sent to Wise. However, when the hiring negotiation went into crisis, Buber and Bonne were explicit about whom they saw as suitable. Lazarsfeld was one of the two sociologists they named. See: “Memoranda of Meetings with Prof. Guttman [held] at the Home of Prof. Buber in July 30th and August 1st, with the Participation of Poznański (in both meetings) and Dr. Senator (in the first meeting),” August 4, 1950.

Chapter Four

THE MORAL ECONOMY OF SURVEY RESEARCH IN ISRAEL

This dissertation as a whole adds to existing accounts of how social science changed at mid-century by arguing that crucial transformations happened in frontier as it travelled. In other words, I aim to show that social science became what it is only through the negotiation of its character outside the United States. This chapter shows the *organizational* aspects of a transition from humanist interpretive speculative social inquiry to scientific social science: a story of local variation on a model of organization, experimentation at a stage where different solutions for “the problems of organized research” (or large-scale, collective research projects) were being locally tried out or devised. I contend that the case of Guttman’s institute in Palestine is crucial to understanding changes in social science more broadly because the divorce from the university—a disadvantage in terms of credibility, funding, administrative apparatus, and personnel—led to specific, compensatory organizational innovations that would later come to be adopted in the design and adaptation of other such institutes.

The contrast here will be with the American survey research institute, a postwar institutional formation and form of scientific life, which was in many senses the equivalent of the industrial lab in accounts of “big science.”¹ American survey research had its origins in market

¹ Jean M. Converse, *Survey Research in the United States: Roots and Emergence, 1890-1960* (New Brunswick, N.J.: Transaction Publishers, 2009); Susan Herbst, “Surveying and Influencing the Public: Polling in Politics and Industry,” in *The Modern Social Sciences*, vol. 7 of *The Cambridge History of Science*, ed. Ted Porter and Dorothy Ross. (Cambridge, UK: Cambridge University Press, 2003); Herbert Hyman, *Taking Society’s Measure: A Personal History of Survey Research* (New York: Russell Sage Foundation, 1991); Theodore M. Porter, “Reforming Vision: The Engineer Le Play Learns to Observe Society Sagely,” in Lorraine Daston and Elizabeth Lunbeck, eds. *Histories of Scientific Observation* (Chicago: University of Chicago Press, 2011), 281-302; Joel Isaac, “The Human Sciences and Cold War America,” *The Historical Journal* 50, no. 3 (2007), 725-746.

research and political polling, but the institute organization was a direct result of the disbandment of governmental agencies for social research at the end of WWII. Survey organizations, like the Bureau of Applied Social Research at Columbia, the National Opinion Research Center at the University of Denver and later at the University of Chicago, and the Survey Research Center at the University of Michigan, were funded by the government and staffed by social scientists who had worked for the government in wartime. Before WWII, social science was generally a solitary scholarly activity, with projects and data pools small enough to be handled by a single researcher. But in the postwar period with the emergence of new survey organizations, projects grew in scope and complexity.²

Indeed, many social scientists started leading a very different kind of professional life in the postwar era, including career trajectory, daily routines, workspace, institutional affiliation, and source and form of compensation, but also in identity, ethos, and norms. Huddled in independent research centers and institutes, working in teams rather than alone, on commissioned projects funded by government and industry, in an interdisciplinary environment where labor was divided not according to academic specialty but according to project, its products (reports and findings rather than scholarly articles) were very different and, quite crucially, there were no teaching responsibilities.³

These American institutes were experimental spaces, where new arrangements of scientific organization were posed as alternatives to existing disciplinary arrangements and career patterns.⁴ Nevertheless, and in spite of some suspicion of their unconventional nature, all

² Converse, *Survey Research in the United States*, 244-245.

³ Other knowledge producers—mainly commercial pollsters—are beyond the purview of this article.

⁴ Despite popular representations that portrayed these institutes as bureaucratic machines, historical sources—particularly actors' accounts—draw a different picture. These institutes were—at least during that period of change—described as dynamic and experimental spaces (both intellectually and interpersonally), lively and youthful

the major American survey research institutes managed to settle in university territory. This academic status was central to their success and the institutionalization of the new social science at this early stage before government money started pouring in.⁵

Crucially, it was exactly that status—and all which went with it—that was lacking in Guttman’s case. This chapter will demonstrate how, for this reason, Guttman’s scientific survival and his ability to produce new knowledge (and reproduce the means for producing knowledge) depended instead on how successful he was in manipulating contacts both in Israel and in the U.S.: forging alliances with the military and the state, making friends in high places, utilizing his personal and professional ties in American academia and industry, and so on—so as to guarantee a more or less constant flow of economic, social and symbolic capital into his projects.

The development of impersonal, procedural methods in the social sciences is often cited as a precondition for the spread and democratization of credibility. I argue instead that the ability of the institute to claim scientific authority, and its immense success in doing so, was primarily due to social factors rather than sophisticated measurement techniques. An ambiguous kind of institution, everyday life in the institute was characterized by what Charles Thorpe and Steven Shapin have termed “normative uncertainty,” where “structures of authority, communication and

spaces of camaraderie, intense interactions, and even fun, where social scientists experimented with the organization of intellectual activity, questioning—and in some cases successfully redefining—the traditional social relations of science: the gendered and ethno-racial, as well as academic criteria for membership in the scientific community, modes of collaboration and mentorship, the cycle of credibility, and the very form of professional life that was until then fully dictated by the norms and structures of the university.

⁵ For first-hand actors’ accounts of organizational experience, see Herbert H. Hyman, *Taking Society’s Measure: A Personal History of Survey Research* (New York: Russell Sage Foundation, 1991); Burkart Holzner and Jiri Nehnevajsa, eds., *Organizing for Social Research* (Cambridge, Mass.: Schenkman Pub. Co., 1982); James S. House et al., eds. *A Telescope on Society: Survey Research and Social Science at the University of Michigan and Beyond* (Ann Arbor: University of Michigan Press, 2004).

the division of labour were contested and unclear.”⁶ In the absence of institutional infrastructure and academic backing, Guttman was pressed to artificially generate the qualities of “organized social science” by relying on the one hand on his own reputation, connections, and charisma, and on the other hand, on certain interpersonal dynamics and the moral economy he imposed on the people around him. This meant that in reality, the source of authority of this case of “organized science” was ultimately vested in his person, depending to a large extent on personal relations and trust, the dynamics of an emotional-moral economy. The institute was not about the institution, but about Guttman.

I argue that the work in the institute was organized not toward efficient production of data and facts, but toward epistemic elevation of Guttman’s reputation. First, I will describe the self-positioning and the setting of infrastructure—the literal making of the institute. Secondly, I will review the special status of Guttman’s persona and elaborate on the organizational, social, and affective mechanisms that generated it. I will then describe some of the organizational adaptations to hierarchy and control and how it manifested in the division of labor—specifically, the splitting of the functions of the director into mutually exclusive administrative and scientific halves. Finally, I will touch on the role of family in the human composition and daily lives of the institute, and the kind of emotional and moral economy that came along with it.

“Statesmen’s Laboratory”

With the signing of the 1949 Armistice Agreements that ended the 1948 Palestine War, and the gradual transition of the Israel Defense Forces from an ad-hoc umbrella organization of

⁶ Charles Thorpe and Steven Shapin, “Who Was J. Robert Oppenheimer? Charisma and Complex Organization,” *Social Studies of Science* 30, no. 4 (August 2000): 545-90.

Jewish militias to a standing army, the Psychological Research Unit that Guttman had created and commanded during wartime was disbanded and its personnel demobilized. A civilian Institute of Public Opinion Research (*Ha-machon le-heker da'at ha-kahal*) came in its stead with Guttman—signing “Professor” again rather than “Army Major”—as its scientific director. Operating now under the auspices of the Ministry of Defense and conducting periodic opinion surveys throughout Israel’s population on issues of the day, for the guidance of government and civic bodies, the *Machon*⁷ soon relocated from its Jaffa premises to government quarters in Jerusalem and was formally established as a permanent research organization.⁸

In an interview he gave to *The Palestine Post* for the occasion, Guttman was intent on distinguishing between the *Machon*, which



Figure 13: from left to right: Judith T. Shuval, Uzi Peled, Louis Guttman at President Zalman Shazar’s quarters in Jerusalem (mid 1960’s). Image from the Guttman family collection.

was a public research organization, and commercial polling firms with which the lay reader may have been familiar, especially after the much publicized polling fiasco of the 1948 U.S.

⁷ In order to prevent confusion, I will henceforth use the Hebrew word for institute—“*machon*”—whenever I refer to the Israeli Institute, and the English “institute” whenever I refer to the generic form. In doing so, I am following my historical actors, who often used the word “*machon*” as proper noun.

⁸ Haya Gratch, *Twenty-Five Years of Social Research in Israel: A Review of the Work of the Institute of Applied Social Research 1947-1971* (Jerusalem: Jerusalem Academic Press, 1973).

presidential elections.⁹ The designation “public” or “nonprofit” in this context, though, did not refer merely to knowledge produced and disseminated in the public—in contrast to private—interest. Rather, it was meant to invoke the epistemic values, calculative abilities, technical prowess, and organizational qualities that came to be associated in the late 1940s with Big (state) Science. Guttman thus set the work of measurement done at the *Machon* categorically apart from the simple counting of individual opinions, preferences, and attitudes.¹⁰ He explained that while the famous “Gallup poll” was based on interviewing the random “man-in-the-street,” the *Machon* sent rigorously trained field workers to visit the homes of a systematically drawn national sample. Furthermore, it did not rely on the response to a single question for determining a person’s attitude. Series of cross-checking questions which were analyzed for scalability and other properties using innovative methods Guttman had developed during his World War II stint at the U.S. Army Research Branch helped ensure unbiased and objective results, on a higher standard than that obtained elsewhere.¹¹ In drafting the *Machon*’s charter, Guttman insisted on the expansion of its mandate to include long-term empirical research in the broader fields of social psychology and sociology as well as work on methodological problems in attitude measurement and survey technique (“basic research both on content and on method”).

⁹ C. Z. Kloetzel, “Our Public Mind Readers,” *The Palestine Post*, November 17, 1949. Most polls predicted that Dewey would win, but Truman did. Even though the result was within the acceptable margin of error (5%), having picked the wrong candidate to win devalued polls in the eyes of many administrators.

¹⁰ On the Cold War development of “big science,” see the various articles included in Peter Galison and Bruce Hevly, eds. *Big Science: The Growth of Large-Scale Research* (Stanford, CA: Stanford University Press, 1992).

¹¹ On the history of commercial polling in the U.S., see: Susan Herbst, *Numbered Voices* (Chicago: University of Chicago Press, 1993) and “Polling in Government and Industry,” in *The Modern Social Sciences*, ed. Theodore M. Porter and Dorothy Ross, vol. 7 of *The Cambridge History of Science*, ed. David C. Lindberg and Ronald L. Numbers (Cambridge; New York: Cambridge University Press, 2003), 577-590. I discuss the rise of scientific survey research and the efforts of empirical social scientists to differentiate from professional pollsters in the first chapter of the dissertation.

In a short report on the organization and work process of the *Machon*, published in one of the first issues of the *International Journal of Opinion and Attitude Research*—a Mexico-based English-language journal that Guttman helped to establish—Uriel Foa, the *Machon*'s co-founder and executive director, explained the emerging “purpose” of the small Israeli institute:

Both from the theoretical and practical point of view, there is in Israel an unusual opportunity for the study of social behavior in all its ramifications. Before the founding of the Institute, there was...no sociological laboratory which could undertake research in this wide area...[Since] Israel offers an incomparable field of inquiry to the social scientist, the Institute and its field staff will collaborate with foreign scientists who wish to undertake research projects in Israel. It is well known that one of the main difficulties in conducting social research in foreign countries lies in the enlistment and training of local field staff. The permanent field staff of the Institute provides a solution this problem.¹²

Uriel G. Foa was a tall, dark and handsome Italian-Jew his same age, who was at the time in the final stages of his doctoral studies at the HU and perhaps the only other person in the country who had both formal training and actual experience in conducting social research. Born to a Jewish family in Parma, Foa first studied medicine, which he did not enjoy, and then law, graduating from the University of Parma. In 1939, when Mussolini signed the “Pact of Steel” with Nazi Germany, Foa and his wife Milena decided to leave, arriving to Palestine on the last boat that left Italy before the war. They settled in Jerusalem and Foa soon found a job as a university clerk, concurrently pursuing post-graduate studies in sociology and statistics under the guidance of Professor Roberto Bachi, a statistician of Italian descent, who was also the one who introduced him to Guttman.¹³ Interested in social psychology and the new techniques of data

¹² Uriel G. Foa, “The Israel Institute of Applied Social Research: Its Organization and Purposes,” *International Journal of Opinion and Attitude Research* 3, no. 4 (Winter 1949-50), 597. See also the first issue of the Institute’s quarterly bulletin in English: *What Israel Thinks* 1, no. 1 (Summer 1950).

¹³ According to his personal file, Foa was employed as an academic secretary and librarian at the HU’s Department of Pathological Anatomy and Histology from 1944 until his mobilization in mid 1948. Prof. Franco to Mr.

analysis then being developed, Foa had to look beyond the small university for instruction. He carried out most of his studies on his own with materials available at the University library, and through correspondence with Stuart C. Dodd, an American sociologist and pioneer of scientific polling who, due to curious personal circumstances, resided throughout the 1930s and up until after WWII at the American University of Beirut in Lebanon.¹⁴ Deeply impressed by Dodd's 1942 magnum opus, the 944 pages long *Dimensions of Society*,¹⁵ Foa's doctoral dissertation was an attempt to provide a quantitative description of social change through a study of attitudes in a well-defined sample population.¹⁶

Such a statement by Foa informed the worldwide readers of the journal that the *Machon* was primarily a scientific entity—and an internationally oriented one, not merely a local service-provider. This change in scope and ambition was reflected in a festive and widely broadcasted renaming act—"social research" replaced "public opinion" and the name of the state was added—that accompanied the official christening of the *Machon* as a national science institution, and in many senses completed, or at least so it seemed at the time, this carefully orchestrated

Schneersohn, Financial Secretary, March 9, 1944; Y. M. Briskman to Uriel Foa, September 6, 1944, Uriel Foa Personal File, HUA.

¹⁴ Born in Turkey, the son and grandson of medical missionaries, Dodd (1900-1975) was trained in psychology at Princeton, and then in London, where he worked with Karl Pearson and Carl Spearman. Settling in Lebanon in 1927, he founded the American University of Beirut's department of sociology and served as its chairman until 1947. He then moved back to the U.S., where he founded and directed one of the first centers for applied social research—the Public Opinion Laboratory at the University of Washington, Seattle. Dodd is known primarily for his international professional organizing and for his efforts to promote standards of competence in the field.

¹⁵ Stuart C. Dodd, *Dimensions of Society: A Quantitative Systematics for the Social Sciences* (New York: Macmillan, 1942).

¹⁶ The study examined change in the cultural practices of Yemenite Jews following their encounter in Palestine with "the complex culture of Western groups." See Uriel G. Foa, "Social Change among Yemenite Jews Settled in Jerusalem," *Sociometry* 11, no. 1-2 (1948): 75-99. In December 1946, Foa informed Senator of an open invitation he received to visit the American University of Beirut, and boastfully added that Dodd described his research as "an excellent piece of work, for which you ought to be congratulated." Uriel G. Foa to D. W. Senator, December 21, 1946. Foa Personal File, HUA.

work of differentiation and uplift: what was now the Israel Institute for Applied Social Research became the only state-recognized social science research organization in the country other than the HU.¹⁷

An enthusiastically received lecture given a few months later by a representative of the *Machon* at the annual conference of the American Association for Public Opinion Research (AAPOR) at Lake Forest, Illinois, brought the small yet ambitious enterprise to the attention of the international scientific community and of policy-makers. According to a reporter who covered the event, the lecture was followed by “a lively discussion”: a representative of the State Department stated that he was truly “impressed by the efforts to use the most modern methods of research in guiding the policies of a democratic government,” while the chairman of the session, clearly moved by what he described as “a most interesting and heartening report,” concluded that this “may well become a model for others to follow.”¹⁸ Such comments had a particular political significance. The 1950 conference was the first to be held jointly with the World Association for Public Opinion Research (WAPOR)—the international counterpart of AAPOR, and included delegates from various West European countries (including Germany), and interestingly also from Allied-occupied Japan and pre-revolutionary Cuba. The recently established U.N. was also represented, along with the U.S. State Department, the U.S. Armed Forces and other government departments. Established two years earlier in light of the proliferation of survey research activity outside the United States, WAPOR was the first international professional association of

¹⁷ “New Sociological Institute to Investigate Immigrants’ Life Here,” *The Palestine Post*, December 25, 1949. On distinction as a social strategy for establishing a “difference out of the undifferentiated” and its relationship to cultural or symbolic capital, see Pierre Bourdieu, *Distinction: A Social Critique of the Judgment of Taste*, trans. Richard Nice (Cambridge, Mass.: Harvard University Press, 1984).

¹⁸ Ernest Aschner, “Opinion Experts Abroad Show Interest in Israel Research.” For the proceedings of the conference, see *The Public Opinion Quarterly*, 14, no. 4 (Winter 1950-51): 832-33.

psychosocial metrologists. According to its mission statement, it was dedicated to promoting collaboration among practitioners working in presumed isolation—separated by geographical distance from each other and from authoritative “centers of calculation.” Unofficially, though, its main task was in regulating and standardizing practice on the frontiers of the measured world: keeping tabs on renegades and charlatans and making sure that no one scientist created his own kingdom in uncharted territories.

That spring, the IIASR also made national news in the United States. A *New York Times* article, intriguingly titled “Statesmen’s Laboratory,” described in detail a first-of-its-kind experimental project under progress in “one of the newest nations in the world.”¹⁹ The article spoke about “a staff of about forty which is soon to be expanded,” consisting of “field workers, sociologists, statisticians, and office personnel” who were led by a team of “highly trained scientists” supervising a host of studies into immigrant adjustment, worker productivity, the social aspects of housing, juvenile delinquency, as well as consumer habits and preferences. Like the postwar American research organizations it was modeled after, the *Machon* put forward its division of labor—separating questionnaire design and pretesting functions from sampling and statistical analysis—as its defining features. The report seemed to demonstrate that a remotely located operation could function like a top-notch “sociological laboratory” according to the highest standards (“using the full range of techniques of modern social science”) to the benefit of both the local government and the scientific community: “For the first time, perhaps, a nation in the process of birth is being scientifically appraised and analyzed.” The work of the *Machon* was hailed as a successful attempt at turning a nation into a laboratory for recording social processes;

¹⁹ Ruth Ludwig, “Statesmen’s Laboratory: social research helps in governing Israel,” *New York Times*, March 26, 1950.

a feat that had never been achieved before but would soon be replicated in the postcolonial areas, based on the *Machon*'s model.

Laboratory Life

Its claim for novelty notwithstanding, in reality, laboratory life on the global frontier was not so glamorous. The phantasy of modern social science heroically guiding the new state through troubled waters to the safe shores of liberal democracy and rational decision-making met the harsh realities of a nonprofit and “independent” public research institute subsisting on a humble government grant-in-aid in a peripheral and poor new state. Housed in a decrepit government building, just above the offices of the Ministry of Agriculture and next door to the National Headquarters of the Israel Police, the *Machon* looked more like a necessary fixture of the modern state than a true “place of knowledge.”²⁰ It did not only look this way. Absorbed into a rapidly evolving bureaucracy with many urgent problems to solve, the *Machon* was considered by its main “employer” as yet another agency offering advice, and an insignificant one at that: in most cases, their recommendations were simply disregarded.²¹ From commanding a large and successful research unit akin to the Research Branch, that enjoyed a high degree of autonomy,

²⁰ The Institute's first residence was in the first floor of the Sergei Building's left wing, formerly home to the Mandatory health offices. An impressive yet badly maintained 19th century courtyard structure (along with a Renaissance style imperial tower!) the Sergei or “Russian Building” was part of Jerusalem's Russian Compound. Intended to serve as a luxury hostel for pilgrims from the Russian aristocracy, the building was nationalized under the British Mandate and made part of a fortified and heavily secured British government center (nicknamed “Bevingrad”). After the establishment of the State of Israel, it was taken over by the Israeli government and for many years housed the offices of the Israeli Ministry of Agriculture. In 1952 the Institute moved to a small apartment in the Shell Building on King David Street.

²¹ Gratch, *Twenty-Five Years of Social Research in Israel*, 29. Gratch reviews two early studies, both commissioned by the government, whose conclusions were entirely ignored: a study on social aspects of public housing projects, which was inspired by the social research-based rebuilding of the city of Coventry, England; and a job classification/evaluation scheme for public servants meant to guide policy on wages.

and towards the end also respect, Guttman found himself without his officer rank in a decrepit government building, for all intents and purposes a civil servant in the lowest echelons of a rapidly growing state apparatus.²²

A visit report of an American official from that early period captures the gloomy atmosphere, rundown conditions, and uncertainty under which the *Machon* had been operating:

EFD took a taxi to go to the quarters of the Institute. The directions he had been given, however, were very vague and the taxi driver had some difficulty in locating the building. Fortunately, a member of the Institute staff came out and approached EFD while he was wandering around in the vicinity and took him inside. The building where the Institute is located is known as the Old Russian Building and is in a bad state of disrepair...Guttman took EFD around to visit his establishment. He has a staff of approximately 15 working in a few small rooms...They have a great deal of modern equipment...the buildings, however, are most unsuitable for their purposes...the light is poor, there is no heat, and the plaster is falling from the ceiling...²³

Clearly, government sponsorship in the host state and the “nationalization” of the *Machon* had disadvantages as well as advantages—most notably, with respect to the latter, “a great deal of modern equipment.” More importantly, endorsement by the government conferred legitimacy. It gave the *Machon* unhindered access to subjects and the authoritative backing of the state (in enterprises such as the interviewing of almost 2000 new immigrants). Yet it also put it at the risk of meddling: the government could dictate the topics the institute studied and the manner in which they were carried out. The first commissioned studies consisted of a dozen nationwide polls that were carried out in 1949.

²² The army research branch had been housed in a beautiful confiscated Arab building in Jaffa—which was separated from the Manpower Headquarters both physically and organizationally. The perks of being situated in the army included the power to impose a country-wide curfew in order to distribute a questionnaire, and the power to transfer people between units, including his wife.

²³ Excerpt from an interview with EFD (identity unknown, but possibly Edward Day, a Rockefeller officer) dated December 8, 1950, 2-GC, Rockefeller Archives, 825, 526, 3348.

In the early 1950, the *Machon* started studying people's reactions to the austerity regime, including its demand of rationing. The results were surprising, and officials did not always like it. Government sponsorship also created various credibility issues: since the *Machon* was perceived as a government agency, it was associated and identified with the ruling party (*Mapai*). Predictably, allegations of partisanship appeared in the newspaper. The *Machon* was portrayed by some as serving to legitimate government policy, while dressing that policy in the cloak of objective science. Suspicions arose that the *Machon* was laundering the party agenda in the context of Labor Party Israel and the political culture of *Mapai*. The *Machon* was thus embroiled, at least by association, with questions of public image.

Yet while Guttman was worried that in the absence of academic backing, the institute would suffer chronically from meddling (and as a consequence, the burying of reports), in reality, the most serious problem was lack of interest and lack of understanding on the part of the public. Under-budgeted and under-staffed government institutions were preoccupied with the immense task of state building in an inhospitable climate. The bureaucratic culture was neither liberal nor trusting in numbers. It seemed that the government was more interested in having the symbols of modern administration and democratic governing than in following scientific directives. Thus, in its early days, the *Machon* didn't get enough requests, and often advice was ignored and expertise dismissed. Eventually, Guttman and Foa had to campaign among government offices, and (rather ironically) "market" survey research to their main client.

In July of 1950, Guttman wrote to Chief of Staff, Yaacov Drori, in the Prime Minister Office. In the correspondence, preserved by the State Archives, he remarked:

In Israel, like in every young state, social [scientific] research is of no lesser importance than research in the natural sciences. After all, many of the State's fundamental problems are social. Being the only institution in the country that has devoted itself to the investigation of social problems, and the only one in

possession of an experienced [research] apparatus in this area, maybe it would be desirable to familiarize yourself with it. As you may recall, the Institute was once part of a military unit.²⁴

As support for his point, Guttman attached to the letter an article that summarized institute reports, as well as “a news item from the U.S. received by local newspapers, which shows the respectable position of the Institute in science among professionals all over the world.” A few months later, in November of 1950, Foa wrote to Drori, concerning the issue of human relations and the prospects of appointing Gutmann as a special advisor to the Prime Minister on psycho-social problems.²⁵

In October 1951, a list of the studies the *Machon* had conducted from April 1950 to September 1951, along with a list of suggested studies for the government, was distributed among government officials. In a chiding tone, Foa remarked, “I hope that you’d find interest in this material...you’d be able to see for yourself that the work of the Institute could greatly contribute for the solution of the country’s basic problems if the responsible institutions would get accustomed to making use of research [results] and would commission required studies in time.”²⁶ His chiding apparently went unheeded, and later in October 1951, Foa wrote to the head of the Urban Planning Department (in the Prime Minister’s Office) and the head of the Absorption Department in the JA. “You may recall,” he remarked, “that two years ago we approached you and suggested to make use of [social] research in order to plan the distribution of population in the country in a manner that will accommodate state needs but won’t raise the

²⁴ Guttman to Chief of Staff, Yaacov Drori, the Prime Minister Office, July 1950, State Archives.

²⁵ Prof. Eliyahu L. Guttman to Lieutenant General Yaacov Dori, July 7, 1950, and Dr. Uriel Foa to Lieutenant General Yaacov Dori, November 22, 1950. 3027/11, ISA.

²⁶ Uriel Foa, Cover letter, “The Institute of Applied Social Research, Research projects proposed to Government offices for the next fiscal year,” October 11, 1951. S41/363, CZA.

objection of the settlers. Regrettably, we did not get your cooperation in the matter.”²⁷ To his mind, the consequences of this ignorance were already demonstrable: “The desertion of distant *Ma’abarot* and [undirected] migration to urban center, a phenomenon we are now witnessing, shows that it is not worthwhile to make plans without the needed social and psychological data. We’d be happy to meet and discuss if you’re interested in collaborating on the necessary studies for establishing policy in the areas of your responsibility on a scientific basis.”

In the end, the institutional status of the *Machon* was ambiguous and uncertain, even in the eyes of its main client. Its staff members were not academic workers, but they were not government workers either. And while the institute was receiving a grant from the government, it still had no formal status. In an interview with the *Jerusalem Post*, Guttman complained: “the Institute has neither become a government department nor have those working for it the status and privileges of government officials; the government should decide: efficient instrument or showy gadget?”²⁸

After two years of working under the auspices of the Prime Minister’s Office, the *Machon* became independent and started operating as non-profit organization whose main client was still the government. Research projects were now commissioned by various government departments, but also by public and academic institutions in Israel and abroad, and by private commercial and industrial organizations.²⁹

²⁷ Uriel Foa to Head, Urban Planning Department, the Prime Minister’s Office and Head, Absorption Department, the Jewish Agency, October 3, 1951. S41/363, CZA.

²⁸ C. Z. Kloetzel, “Our Public Mind Readers,” *The Jerusalem Post*, November 17, 1949.

²⁹ By 1963, the Institute had carried out 300 research projects in a wide variety of topics both in theoretical and applied fields of social science, from economic behavior and market research, through occupational recruitment and choice, industrial sociology, and social and psychological problems of medicine and health, to mass communications and opinion polling. Gratch, *Twenty-Five Years of Social Research in Israel*, 29.

The *Machon* was now free of some of the constraints and legitimacy issues that plagued its work in the first four years of its operation (and necessitated efforts to resist meddling, and prove its worth to uninterested state officials). Yet the need for assured and continuous support, central for the *Machon* to carry on its scientific function properly, had increased significantly.

This dependency on commissioned project-based work was a problem that all postwar SRIs had to deal with, but one that, for the others, was mitigated by their university affiliation. The university usually paid overhead, as well as the salaries of the senior staff, who were all, without exception, academics. The university also funded many of the grad students who served as the lower ranking research workers. Since such support did not exist for the *Machon*, it depended on commissioned work for its very existence. This meant no time or funds for pure research, which in turn led to a problem of authority and credibility. The *Machon* operated on a project basis, without the ability for long-term planning and more integrative work. In addition to the fact that the country suffered from a dearth of professionally trained scientific workers, for the *Machon*, operating outside the HU, it was even harder to recruit qualified workers, especially research students.

Eventually an ugly public fight with the HU developed. It was waged in the press over the authority to speak scientifically about the new immigrants. All of these issues, taken together, led to the problem of maintaining the identity and reputation of the *Machon* as scientific research organization. After the HU decided not to appoint Guttman, and refused to incorporate the *Machon*, it was relegated to academic homelessness.

Beyond the difficulties posed by material conditions, Guttman and the institute had bigger problems: extra-academic research, even if done according to academic standards, automatically had a lower epistemic status. In Israel of the late 1940s and early 1950s, the HU

held an unchallenged national—even regional—monopoly over intellectual production, setting the standards for what was considered proper and worthy knowledge and knowledge endeavors—and aggressively so. Research institutes, which depended on funding from government and private clients, and worked largely on commissioned projects with clear policy implications were especially vulnerable to the credibility problems from which all big science suffered: suspicion of bias in results and research driven by a political agenda, economic interests, and pressures from clients. As Guttman himself confessed on several occasions, conducting research outside the university meant being constantly exposed to “the elements”: changing circumstances, pressures and demands typical of the world of practice, and the occasional hard-headed general or official who tried to shut down your business.

Even though it would later become prevalent in the U.S., a non-academic research institute that also had a claim to scientific authority was a new concept for the social sciences.³⁰ This model existed in the natural sciences in the form of the state or national laboratory—appearing in the 1940s and widely known as places like Los Alamos and Oak Ridge.³¹ It was with reference to this model that Guttman chose to position the institute. Despite some advantages—mainly the ability to differentiate his work from that of the “pollsters” and invoke the national lab image—Guttman found himself having to deal with the strains, limitations, and problems of the government environment that posed a threat to scientific authority. In the end, the *Machon*’s existence depended on a tiny grant-in-aid from the government, commissioned studies from various sources, and Guttman’s international scientific reputation.

³⁰ Both NORC and Michigan were semi-academic in terms of teaching, and many of their staff members were not faculty, but being part of the university was a crucial component of their institutional identity and credibility.

³¹ Galison and Hevly, *Big Science*, 135-37.

The negotiation with IBM over a discounted purchase of electronic data processing equipment for the *Machon* may serve as a case in point for how its ambiguous academic status translated into difficulty in securing recognition as a center of scientific endeavor. In the early 1960s, IBM had a program for educational allowances that offered significant discounts on the purchase or rental of computers for universities.³² Sidney Diamond, a New York attorney who represented the interests of the *Machon* in the U.S., made contact in fall 1962 with IBM World Trade Corporation, the subsidiary that dealt with all markets outside the U.S., in regards to such an allowance for the *Machon*.³³ Allowances were granted to research organizations that performed “a basic educational function,” evidenced primarily by having a direct connection, financial and/or administrative, to an institution of higher education. The *Machon*, however, was not under the direct control of either a university or the Israeli Ministry of Education. Worse, the majority of the research it executed fell under the “applied” rubric, that is, it consisted of commissioned work for the Israeli Government or private clients, designed to address specific practical problems. Guttman, who was spending the fall semester as a visiting professor at Michigan State University, instantly wrote to Jerusalem and instructed Foa to carefully prepare a portfolio that emphasized “the role that ‘pure’ research plays in our operation.”³⁴ His concern that they would not qualify for the allowance unless they polished their academic appearance was based on an unsuccessful appeal made a few months before to the local IBM office in Israel.

³² The IBM educational allowance program began in October 1955, with 60% reductions in lease rates to universities. In May 1960, IBM changed the allowance to 20% for administrative use and 60% for academic use, targeting university research in particular. David C. Mowery, *Paths to Innovation: Technological Change in 20th-century America* (New York: Columbia University Press, 1998).

³³ Sidney A. Diamond to Louis Guttman, October 6, 1962; Uriel G. Foa to Sidney A. Diamond, October 30, 1962; Sidney Diamond to E. F. Ormsby, IBM World Trade Corporation, November 2, 1962, Box 16, File 67, Guttman Personal Archive, BGA.

³⁴ Louis Guttman to Uriel Foa, October 12, 1962, Box 16, File 67, Guttman Papers, BGA.

“It is difficult to get the impression that education really plays such an important part in the purpose of the Institute,” read the letter of rejection, citing the missing university connection.³⁵

While Foa was trying in vain to obtain a statement from the Israeli Government to the effect that the *Machon* was, in fact, an educational institution, Guttman did not wait. He chose to leverage his own academic connections instead. First, he approached Professor Charles F. Wrigley—“well known as the man who may have done the most to get social scientists into using computers,” was the selling point he gave Diamond—in request of a letter of appeal that they could use with IBM.³⁶ In picking Wrigley, however, Guttman was doing more than paying tribute to the history of mechanized data analysis. Wrigley was then at Michigan State, and he had just persuaded his home institution to buy a \$1.378 million dollar machine from Control Data Corporation, a small but fierce competitor of IBM.³⁷ Wrigley wrote to Sidney Diamond, noting that

Dr. Guttman’s work is internationally recognized to be of the highest originality and power. He had made and continues to make major contributions to the algebraic theory of structural analysis... It is, however, rather ironic, that Dr. Guttman is at present obliged to concentrate upon the algebra and logic of these structural analyses, with rather little reference to their actual use in social science research, just because he has not access to a computer...³⁸

Professor Henry F. Kaiser of the Bureau of Educational Research at the University of Illinois—another SRI variation founded in the late 1940s—made a similar appeal: “Professor

³⁵ J. Cohen, IBM (Israel) LTD. to Uriel G. Foa, “Educational Allowance,” September 27, 1962. Box 16, File 67, Guttman Papers, BGA.

³⁶ Louis Guttman to Sidney Diamond, November 2, 1962. Box 16, File 67, Guttman Papers, BGA.

³⁷ Louis Guttman to Henry Kaiser, October 17, 1962; Sidney Diamond to E. F. Ormsby, “Re: American Committee for Social Research in Israel, Inc.,” November 9, 1962, Guttman Personal Archive, Box 16, File 67, BGA.

³⁸ Charles F. Wrigley to Sidney Diamond, November 2, 1962, Box 16, File 67, Guttman Personal Archive, BGA.

Guttman, in my opinion, is unquestionably the world's most creative psychometrician and to have his creative genius inhabited because of lack of computational facilities would be a catastrophe to scientific psychology.”³⁹ Such solicitations serve as further evidence that Guttman's reputation substituted for an academic institutional framework.

Ultimately, the American Committee coordinated the machinations responsible for the gift of the computer, and in general for the transformation of Guttman's academic connections into resources for the *Machon*. This was an organization that had been designed in 1951 to generate cash flow out of reputation and connections. Its full name was the American Committee for Social Research in Israel; it was incorporated in New York State with the purpose of supplying interested American circles with information on the research activities in Jerusalem and of acting as liaison between the *Machon* and American institutions and individuals, primarily donors and foundations. Samuel Stouffer, Director of the Laboratory of Social Relations at Harvard University, who was a friend and a former mentor of Guttman's, was the first to enlist in the Committee, encouraging other scientists to join. The great WWII military patron of survey research, Major General Frederick Osborn, accepted the chairmanship, the polling internationalist Elmo “Budd” Wilson agreed to serve as president—a capacity he served in for over a decade, and forty top-ranking American scholars, foundation officials, business

³⁹ Henry F. Kaiser to Sidney Diamond, December 4, 1962 and Sidney A. Diamond to Mr. Ormsby, December 6, 1962.

executives, and community leaders, joined the Committee.⁴⁰ Among the scholars were some of the most influential social and behavioral scientists of the era.⁴¹

Many of the committee members were friends, acquaintances, mentors, and collaborators on scientific survey research from the war cohort. Guttman's professional friendships and reputation were the glue that held it together. Although the committee did not have an address, symbolically and socially it functioned as an infrastructure, translating social code into organizational structure and back. Guttman's intention was to keep the Committee informed of the research being done in Israel and to use its backing in getting grants from American foundations. The stated purpose of the Committee was to establish regular ties "between American scientists and the Institute of Applied Social Research in Israel."⁴² This purpose was to be fulfilled by securing access to funds through grants; ties with American scientists and universities; the recruitment of students; diffusion of methods and credibility; and substantiating the frontier lab as a locus of scientific endeavor.⁴³

⁴⁰ Frederick Osborn (1889-1981) was an American philanthropist, military leader, and population scientist best known as the respectable face of eugenic research in the postwar period. He served during World War II as the commanding chief of the Morale Branch of the War Department (later called the Information and Education Division)—and by extension of the Research Branch—in which capacity he and Guttman met. At this point in time, Osborn had just resigned from his role on the UN Atomic Energy Commission, and devoted himself to promoting birth control and population planning internationally, while continuing his support for the social sciences. Osborn was well respected in public policy circles and very well connected. Having him on board was a great asset.

⁴¹ Some of the more familiar names were: Samuel Stouffer, Talcott Parsons, and Clyde Kluckhohn of Harvard's Department of Social Relations; Rensis Likert, George Katona, and Dorwin "Doc" Cartwright of Michigan's Institute of Social Research (ISR); Edward A. Suchman and Robin M. Williams Jr. of Cornell's Department of Sociology and Anthropology; Ernest Burgess and Philip Hauser of Chicago's Department of Sociology; Stuart Chapin of Minnesota's Department of Sociology and Guttman's doctoral adviser; Leonard S. Cottrell, Guttman's former "boss" at Cornell, who was appointed secretary and research director of the Russell Sage Foundation that year; Hans Speier of RAND, Paul Lazarsfeld, and many more. For the full list, see: "American Committee for Social Research, Inc.," 1951, 2513/23/C, Israel's National Council for Research and Development Papers, Israel State Archives, Jerusalem.

⁴² Herut (newspaper), "A Committee of American Scientists for Social Research in Israel," September 15, 1951 (Hebrew).

⁴³ On the Board of Directors served Clyde W. Hart, Director of the National Opinion Research Center (NORC) at Chicago University, Harry Alpert, then Head of Research of the U.S. Bureau of the Budget, and Harry Starr,

The committee's moving spirit was Ruth Ludwig. A social scientist by training and writer by choice, and overall a rather eccentric woman, Ruth Ludwig was the face of the *Machon* in New York. The solidified network of social relations and shared experiences that was the American Committee was set up due in large part to her efforts, and she was the one injecting it with dynamism. She brokered ideas, relationships, and funds between the *Machon* and its U.S. network.⁴⁴ She also represented the *Machon* at three AAPOR meetings in the early 1950s, maintained the connection among Committee members and communicating ideas and interests to American foundation officials and social scientists.

With the Committee's assistance, what recognition the *Machon* did not receive in Israel, it received in New York. Grants were received from various foundations of the U.S. Government (National Institute of Health, National Science Foundation, Office of Naval Research and others), and from various private foundations. These enabled "pure" research to develop alongside the "applied" research. The *Machon* produced studies of the air force and navy of factors in training (1950), new approaches to factor analysis—office of naval research (1953), and the adjustment of new immigrants. The research output was a mixed bag: some of the studies

President and Treasurer of the Lucius N. Littauer Foundation. Nahum Goldman, then the Chairman of the Jewish Agency in the U.S. and President of the World Zionist Organization, was also among the members of the board. Sidney M. Diamond, a New York attorney, who later served as Federal Commissioner of Patents, Trademarks and Copyrights in the Carter Administration, acted as legal adviser to the Committee, Manuel N. Manfield was its Treasurer, and the Committee's unforgettable Executive Secretary.

⁴⁴ Ruth Ludwig was born and raised in Berlin. She earned a doctorate in sociology and economics from Heidelberg University and from 1927-1931, was a member of Werner Sombart's famous Seminar in Berlin. In 1933, she emigrated with her family, first to the UK and then to Palestine. She lived and worked for several years in kibbutz Kinnereth, and later moved to Jerusalem, where she met the Guttman and became a member of the IASR. For all I know, Ludwig never conducted research in Palestine/Israel. During the years she worked for the Institute (roughly from 1949-1959), she was engaged primarily in PR, fundraising, and idea brokering—functions she fulfilled while living in the U.S. for six years out of this period (it is not clear whether her stay was directly related to the Institute). Her writing on Guttman and the work of the IASR appeared in *The New York Times Magazine*, *Commentary*, *Public Opinion Quarterly* and various Israeli periodicals. For background on her life in Germany and pre-State Palestine, see her autobiography: Ruth Ludwig, *Pistolen im Zucker: Ein Leben in zwei Welten* (Frankfurt/M: Ullstein, 1990).

represented American interests (collecting information on local and regional phenomena); some utilized the *Machon* as a lab (in recognition of the fact that certain kinds of research can be done in the frontier that can't be done in the metropole); some simply were a continuation of Guttman's previous work; and some were purely academic (one involving Yale research on the differences in the social relations between *kibbutz* and *moshav ovdim*, and Columbia research on the personality of second generation *Kibbutz* members, as well as Harvard research on the life histories of students).⁴⁵

'Guttman was the Institute'

An article about the *Machon* that appeared in *Omar* observed, "sometimes the institution makes the man and sometimes the man makes the institution. Such a man, who makes the institution, is professor Eliyahu Guttman."⁴⁶ The article continued by making the curious observation that "Guttman reminds in his appearance more a boxer than a professor." He was perceived as someone who possessed power that could not be rationally explained by appeal to his skills, knowledge, and credentials. The boxer image suggests strength, aggressiveness, and sheer physicality, an image that collides with the popular representation of the scientist as ascetic and removed from life. Similarly, a mention of his passion for cream cakes suggests vitality, strong and perhaps unchecked libidinal drive, or even a lack of refinement. The appetite for cream cakes is presented as fuel for "laying equations"—as if mathematical reasoning were some kind of physical sport—rather than as symbol of weakness, i.e. something that needs to be

⁴⁵ The biggest grant application was to the Ford Foundation in 1952 for a five-year period.

⁴⁶ Raphael Toshav, "The Institute for Public Opinion Research: 13 people are finding out what Israel thinks," *Omar*, December 26, 1952.

overcome. Lastly, his largeness of body is mentioned in other places as well, perhaps a reference to the importance of his presence: his power is not vested merely in articulation, writing, etc. but also in his very presence.



Figure 14: Guttman with Institute members, 1980s. Courtesy of the Guttman family, from their personal collection..

First-hand accounts of family and collaborators give further evidence to the centrality of Guttman and his personal qualities to their perceptions and experience of the institute. Asked about whom Guttman recruited and where they came from, his wife Ruth, recalled, “They came to him. Heard about it and came to him. There were a few sociology students, an economics student (Moshe Sandberg), Israel Tor, and a little later, Judy Shuval (“she was already a professional”) and Uzi Peled (“he wasn’t a sociology student, I don’t even know what he studied”), along with a talented group of students and non-student workers...until this very day I bump into people who claimed they worked as interviewers in the *Machon*...people simply came

together, I don't think they had to be sought out."⁴⁷ Moshe Sandberg corroborated this account, remembering that "I didn't know about the *Machon*, but I heard about Guttman."⁴⁸

One critical dimension of Guttman's embodied power was what institute workers repeatedly referred to as his genius. Guttman's family history was frequently brought up to the end of conveying a myth of genius: "Guttman is an American—born here. His parents arrived from Russia before WWI. From his father, a soft drink jobber who in his spare time wrote papers about Einstein's discoveries, he inherited a love of mathematics and skill with figures and equations."⁴⁹ Guttman's innate mathematical understanding or "raw talent" was brought up more often than acquired skills or professional credentials. Also bolstering the image of genius were accounts of Guttman as being misunderstood and uncommunicative. For instance, he had the reputation of a bad lecturer and an even worse teacher. As Aaron Antonovsky put it: "As one who was in daily contact with Louis at work for 13 years: Louis was a genius, while most of us are competent. And geniuses are not easy to work with. They have the truth, *idées fixes*—and make the rest of us uncomfortable. We can try to imitate them, and some try to force us to do so. But others, such as Louis, was so sure of himself that he let us learn and go our way, if so we choose—so long as we did not shame the Institute."⁵⁰

Genius was only one dimension in which scientific credibility capital was stored and built up in Guttman's person. A quality that was remarked upon even more often was Guttman's unusual "integrity," meaning a combination of a scientific ideal and a moral valuation. As

⁴⁷ Ruth Guttman, video interview [date unknown]. The Guttman Family Collection.

⁴⁸ Moshe Sandberg, video interview [date unknown], the Guttman Family Collection.

⁴⁹ Henry Beckett, "Social Science Pioneer in Israel," *New York Post*, March 28, 1954.

⁵⁰ Aaron Antonovsky at a special session of the Annual Meeting of the Israeli Sociological Society, Beer Sheva, February 17, 1988, the Guttman Family Collection.

Lorraine Daston remarks with reference to Kathryn Olesko, the latter “identifies integrity as the cardinal virtue of precision measurement, simultaneously applied to the character of the measures and to the quality of the measurements.”⁵¹ This norm was internalized, disseminated, and standardized as part of scientific discipline in the U.S.—including in programs of mathematical sociology in the 1940s. Unlike Lazarsfeld, whose charisma was vested in his organizational abilities, his indefatigable entrepreneurialism, his energy, and his way with people, Guttman had a very different leadership style based on his image as an uncompromising man of principles. Unlike integrity that is associated with an organization and comes from following the rules and keeping to strict protocol (and in science—procedure and division of labor—that is, integrity or credibility can come from the mode or organization itself, conceived as rational, objective), in this case, the integrity was all vested in the person of Guttman. Thus, an account of the *Machon* as it functioned under his guidance may serve as an alternative to accounts that explain the success of large-scale research operations (or their dissemination) in efficient and rationalized division of labor, strict adherence to protocol, and the primacy of office over person. Taken in all of its complexity, the account of the ‘Institute that was Guttman’ puts into question the argument that by definition, “technologies of quantification” eliminate the local, personal, and idiosyncratic. Following Olesko, I show that precision measurement places a premium on the integrity of the measurer. Going beyond the scope of Olesko’s study, I argue here that scientific or intellectual integrity must be “proved” not only in the act of measurement itself (e.g. in alertness to error), but also by being demonstrated or performed in other arenas.

⁵¹ Lorraine Daston, “The Moral Economy of Science,” *Osiris* 10, no 1. (1995): 2-24. Daston is referring to Kathryn Olesko, *Physics as a Calling: Discipline and Practice in the Königsberg Seminar* (Ithaca: Cornell University Press, 1991), 250-252.

In all accounts, it is clear that the *Machon*'s universal authority, i.e. the unchallenged quality of its product, derived from Guttman's authority as a scientist and expert, and an American one at that: "as director of the Israel institute of applied social science he has worked out such a trap of related questions that most elusive information cannot escape capture. So Guttman said, anyhow, and this was believed by government, industry, university and foundation, in two hemispheres. They pay the Israeli institute for answers and determine their activities by them."⁵² The credibility of the *Machon*'s products depended entirely on him. For the *Machon*, integrity was the most important—and perhaps the only—capital. It was crucial for the credibility of its products and their epistemic status, and since it had no other source of credibility (university, other scientists), the *Machon*'s survival depended on Guttman's integrity, or better put, on the reputation of his integrity.

The project of generating Guttman's personal integrity was an eminently collective one towards which everything from the administrative structure to daily work routines were geared. Each time someone talked about his way with numbers, his powers of abstraction, his confidence verging on religious belief, his zealousness and unwillingness to compromise, they were further marking him as a virtuous person of science: "Louis was a complex man: strong in conviction, mild but dominant in personality; kindly toward his family and friends; tolerant and patient, but would not 'suffer fools gladly'; argumentative sometimes to the point of arrogance...a man who strongly believed in basics and the scientific tradition; he was above all a man of impeccable scientific and mathematical integrity."⁵³

⁵² Beckett, "Social Science Pioneer in Israel."

⁵³ James C. Lingo, Ledyard R. Tucker, and Samuel Shye, "Louis E. Guttman (1916-1987)," *Psychometrika* 53, no. 2 (1998): 153-159.

Guttman's integrity—people pointed out, either admiringly or less so—apparently corresponded with an uncompromising approach to the *Machon's* work. In the words of one colleague, looking back: “He would look at these models, and say, ‘now, take this apart and do it my way.’ There were people who weren't willing to accept that. Also in the U.S., not only in Israel. There were no fights, people simply said, ‘Louis, you're a genius, do it your way, we'll do it our way.’”⁵⁴ Conflict was a common feature of Guttman's professional life—he waged quixotic wars against the university, commercial pollsters, and fellow social scientists. In his rapport with colleagues from the HU, for example, integrity often had a divisive and alienating effect: Guttman corrected an esteemed colleague in the middle of a lecture, shaming him in public, attacked another in a newspaper, and tarnished a third in an article published in a high profile scientific journal. He alienated people left and right due to the manner in which he voiced his reservations or critique by publishing op-eds in the newspaper, appearing on TV, sending angry letters, correcting colleagues in public—evinced complete disregard for norms of collegial conduct and the “social ethic” more generally. People either admired and “followed” him or could not get along with him, marking him as “difficult,” uncompromising, and belligerent.

All these personality traits—being stubborn, uncompromising, bad with people (“no politician,” incommunicative), arrogant and often argumentative—which on the face of it seem to constitute a real hazard for an organization like this, were actually instrumental in the *Machon's* survival; in fact, necessary for securing its scientific standing. These qualities of his were scientific virtues (were associated with scientific and mathematical integrity, precision, impartiality, fairness).

⁵⁴ Ruth Guttman, video interview [date unknown]. The Guttman Family Collection.

The Split Director

The *Machon*, much like its contemporaries in the United States, marked the beginning of “big” social science. In addition to the novel mix of “applied” work for hire for government or industry and “basic” university science, the survey institute also came to stand for the emergence of administrative forms of organization and a bureaucratic ethos in the social and behavioral sciences.

Splitting the leadership of the *Machon* achieved the internal separation of the administrator and the expert, divorcing of the power of knowledge from administrative power. Foa was the *Machon*'s executive director, while Guttman served in the capacity of a scientific director and “chief consultant.” Among social research institutes in general, one of the emergent features was the creation of a new kind of person: the research director. Responsible for coordination, non-academic hierarchy and control, and fundraising, the scientist had to learn to become an administrator.⁵⁵ In a paper he presented on the occasion of the dedication ceremonies for the new building of the Institute of Behavioral Science at the University of Colorado in 1962, Paul Lazarsfeld, the founder and director of the Bureau of Applied Social Research at Columbia University and great guru of organized social research, spoke at length about the novelty and significance of the director role:

One institutional consequence of research institutes is that they necessarily train men who are able and not embarrassed to combine intellectual and administrative leadership. The functions of an institute director confirm this. ...A brilliant scholar may not be experienced in hiring and supervising participant-observers; a fine analyst may not know the most economical way of using tabulating equipment or the most expert form in which to present statistical returns in a written report...[hence] the collective experience of the institute staff exceeds the skill of the individual faculty

⁵⁵ Converse, *Survey Research in the United States*. Christian Fleck argues that the role of research director was not simply a practical necessity, but a structural component of empirical social research. Christian Fleck, *A Transatlantic History of the Social Sciences: Robber Barons, the Third Reich and the Invention of Empirical Social Research* (London: Bloomsbury Academic, 2001), Chapter 5.

member. ... the institute director brings men and money into contact; this is not badly described as the role of *idea broker*...I have dwelt at some length on the role of the institute director because so much depends upon him, and because the job is so difficult to fill. Or rather, it is so difficult to fill it with the right kind of man or woman.⁵⁶

Having created the IIASR out of thin air, Guttman clearly belonged to the new breed of social science research entrepreneurs that Mills described in his typically elegant and virulent manner.⁵⁷ And yet, the IIASR had two directors. The executive director's role was to coordinate the work of the different sections of the institute (including field work, sorting and tabulation of data, publishing, and secretarial or administrative), while the scientific director was tasked with "supervising the research planning and analysis," thus preempting the criticism of the institute director as the embodiment of the clash between business orientation and scientific ethos and values.

Foa and Guttman met when the latter was setting up the research unit on morale in besieged Jerusalem. Desperately seeking volunteers with relevant background to assist in the design and execution of surveys, Guttman was introduced to Foa. Upon Guttman's arrival to Jerusalem, it was proposed that the two would collaborate on further studies of this kind, but the eruption in late 1947 of inter-community violence and then an all-encompassing civil war in Palestine following the U.N. General Assembly's historical vote in favor of partition, disrupted their plans. Instead, Foa joined Guttman and together they headed the volunteer group of researchers that constituted the core staff of what later became the *Machon*.

⁵⁶ Paul Lazarsfeld, "Some Problems of Organized Social Research," in *The Behavioral Sciences: Problems and Prospects: Three Papers* (Institute of Behavioral Science, University of Colorado, 1964), 18-19.

⁵⁷ C. Wright Mills, *The Sociological Imagination* (New York: Oxford University Press, 2000).

From that moment on, they worked side-by-side, day in, day out, for eighteen years. During the war, Foa was made army Captain and served as Guttman's second-in-command in the Psychological Research Unit, and in its aftermath moved with him back to Jerusalem to found and manage the IIASR. People often wondered what the glue was that kept them together as they were considered different in almost every respect. There seems to have been a general agreement among the *Machon* workers that Foa was a mediocre scientist, but a bit like a sponge: absorbing and then diligently expressing ideas that were not his own and to which he had little to add. After his affair with Dodd's "S-system," he adopted Guttman's methodological inventions (scale and intensity analysis, facet theory) and for the rest of his career engaged in their theoretical explication and the demonstration of their viability and usefulness through a series of application studies. Bright and quick on his feet, clear thinking on theoretical issues and with the gift of the ability to communicate complex ideas in simple language, he was nonetheless regarded as lacking the stamina or discipline for the arduous and often tedious routines of experimental science. Describing his typical workday, his daughter recalled:

My dad would get up in the morning, wear a suit and a tie and well-shined shoes; he would leave the house like a typical Italian man...he hated these clothes, but this is how he got dressed for work. When he got back home, he would immediately take off the suit, wear instead his "home rags," and start fixing something around the house. He wasn't a man of long hours... At our place, there were three meals a day, like in Italy. Lunch was important. So, he would arrive in the middle of the day to eat. He often brought guests: would come home early and bring people with him for lunch. Sometimes he went back to the *Machon* later, but usually these became what he called his "short days."⁵⁸

⁵⁸ Ora Foa, interview by the author, September 27, 2012.

Reports characterize them as working like two halves of the same person: sitting on two sides of the same table and complementing each other's strengths and weaknesses.⁵⁹ Foa managed the day-to-day work while Guttman dealt mainly with quality control and methodological troubleshooting—he occupied the position of expert advisor, dissociating himself from administrative power and business interests. His advising status signified both impartiality and quality that would not be compromised for the sake of efficiency or external considerations or interests. Foa handled the personnel matters, including hiring and firing, discipline, and control. Guttman, on the other hand, was responsible for training and scientific-technical advising. In other words, he was not a boss, but the representative of truth and rigor. Foa moved money around while Guttman retained an external disposition - he raised funds but didn't negotiate. Between them, “Competition was not the dominant thing. They collaborated; ‘made it’ together... They were scientifically involved for many years.... the relationship [between them] was based on deep mutual appreciation, if not straightforward admiration, that lasted all those years, very big harmony.”⁶⁰

Despite the harmony and the friendship, and unlike in other scientific duos of the era, their collaboration was not based on equal partnership, but on some version of the master-slave dialectic. The relationship between Lazarsfeld and Merton, the ‘first couple’ in the social sciences, for instance, was based on different yet complementary intellectual styles (often described in the literature as “empiricist” and “theoretical”), rather than on a clear division of labor. In the case of Guttman and Foa, such a division of labor existed and it served to maintain

⁵⁹ Uzi Peled at a symposium on the Institute for Applied Social Research, the Israel Academy of Sciences and Humanities, Jerusalem, July 8, 2001, the Guttman Family Collection. (Hebrew)

⁶⁰ Ora Foa, interview by the author, September 27, 2012.

an internal separation between the scientist and the administrator, the expert and the executive. This division was internal to the director role, not a simple separation of functions. Despite what seems like the creation of two separate and clearly defined leadership positions, each covering one function of the institute director role, this was not the case. As the personal embodiment of the institute, the research director had to be one. This unity, I argue, was responsible for the *Machon*'s identity. At the same time, securing the *Machon*'s epistemic authority depended on the generation of integrity, itself requiring the separation of the person of science from the administrator. The director of the IIASR could show up either as split or as unified, according to need.⁶¹

The splitting of authority was well reflected in how the work of representing the *Machon* to the outside world was divided between the two. A quick review of official Institute publications reveals that Foa is signed on almost all of them; from project reports prepared for clients or sponsors, to communiqués on Institute research activities sent to professional journals. Guttman usually features in these texts in the third person, thanked for his advice and guidance. He would appear as author only in two discursive circumstances: in scientific publications on survey research methodology and measurement theory, which contained ideas that were developed in the course of daily work on commissioned studies, but represented a sublimated abstraction of that work (as I noted earlier, Guttman has published little to none under his name about the content of specific studies); and in newspaper articles (and later also TV shows) about election results projections, social problems and so on, which Guttman either published on his own initiative in the form of op-eds, or was interviewed for. These popular appearances were

⁶¹ The reference here is Jacques Lacan's "split subject." For the purpose of the point I am trying to make it suffices to say that Lacan offers a structural theory of subjectivity according to which the Subject is constitutively divided into two parts or aspects, irreconcilable in its/their difference but nonetheless unified.

often tour de force authority displays: Guttman would scold commercial pollsters (which he often regarded as amateurs and charlatans), argue with fallen colleagues, pontificate on the importance of scientific rigor, and set the standard straight. A good example is a 1963 op-ed published in the economic supplement of Haaretz. In the guise of an informative review of market research, Guttman speaks about proper sampling, reliability of interviews, and mainly who is authorized to design a study and who is not.⁶² In all these texts Guttman wears the expert hat, never that of the executive. In semiotic terms, Guttman only appears in print in full scientific armor, the ultimate authority, never a representative of anything but scientific authority, which he embodies.

While this structure succeeded in bolstering Guttman's cache of credibility on which the institute depended, it also secured Foa's subordinate position, and diminished Foa's authority with the staff, who often appealed his decisions with Guttman. The loan story is a good example: when it was Foa who needed money, it was understood as an administrative matter, requiring procedure, paperwork, formal guarantees, etc. And when Foa made financial decisions, they became subject for suspicion and scrutiny.

Foa asked me to sign the [loan] check and I of course refused to do so without getting your permission first. He [Foa] hinted that I didn't hesitate to approve large sums of money for you when you moved and that everything is allowed when it comes to you. Foa checked the Institute's last balance and asked me for an explanation of your debt. I told him that this is instead of the study that was approved for you [unclear] and that you plan to pay it back very soon. He also saw the check you've sent and said it doesn't seem okay to him.⁶³

⁶² E. L. Guttman, "How to Study Markets?" *Haaretz Economic Supplement*, June 3, 1963. (Hebrew).

⁶³ Rachel to Guttman, October 15, 1962. Box 16, File 67, Guttman Papers, BGA.

Things took a turn for the worse in 1962, with Foa's appointment as professor. In 1962 the *Machon* switched from a civil service pay grade to an academic pay grade. As a result, scientific workers were ranked according to their academic credentials rather than professional function and seniority. Foa not only advocated for the change to academic ranking, he even claimed the status of full professor while Guttman was abroad. As one institute worker, Antonovsky, explained to Guttman: "Since the agreement is unclear...he [Foa]—in the name of the *Hanhala* (management)—proposed a supplement which includes '...one's scientific contribution will be measured by...the rank which the worker has or had at a university or other recognized academic institution,'" a condition to which Foa claims his former employment at Stanford University. Antonovsky, writing in his capacity as member of the "workers union," explains his objection to Foa's suggestion first by referring to what he perceives as Guttman's determining criterion for rank—an academic position at an Israeli institution, and then by elaborating on the adverse effect Foa's promotion would have on both the internal stability and order at the *Machon*, and its reputation:

As a staff member, I feel that if he is ranked as a full professor this puts into question the serious meaning that ranking at the Institute should have; it may well make us a laughing stock; and it will create further trouble, e.g., Judy will demand that if Uriel deserves full professor, she deserves associate. In answer to this, Uriel called upon the evidence of the responses to his papers in the American academic world to show that he does deserve full professor. I still don't agree.⁶⁴

As evidenced by the organization of early survey research institutes in the United States, applied social research usually demanded a fusion between the entrepreneur, civil servant, and scientist in its leading practitioners. In the case of the *Machon*, where scientific authority was not secured by affiliation with a university and had to be stored in Guttman's character, a division

⁶⁴ Aaron Antonovsky to Louis Guttman, October 9, 1962. Box 16, File 67, Guttman Papers, BGA.

had to be maintained between scholarship and everyday research work. Research and writing were severed from mundane business routines, money, work relations, etc. through a division of labor at the level of the director. Foa needed to handle the dirty work so that Guttman could keep his hands clean.

Unlike Lazarsfeld and the other American institute-builders, Guttman could not assemble around him people he trusted in the professional sense. He knew very little about their training and skills, partly because they were trained in different parts of the world and he didn't have tools to compare, and partly because most of them had very little training, so that the question of what they know could not really play a part in their selection. Where professional trust was lacking, personal trust and obligation functioned as substitutes. The consequences of this dynamic were such that Guttman ended up working with people who didn't have their own scientific opinions or agenda, who wouldn't dispute or undermine him, who felt obligated to him personally and emotionally committed to the *Machon* (rather than to science or their career) and would therefore remain loyal no matter what. The rhetoric and practices around relations in the *Machon* were more akin to that of family, based on relations of mutual dependency and personal commitment that went beyond the professional "call of duty." If bringing the institute people into the family, so to speak, was one powerful means of building internal trust in the absence of institutional controls, another was bringing family into the institute. The virtue of the person of science is constructed not only on what happens in the lab, but also on how the boundaries between lab and life are negotiated and what happens outside those boundaries. In the next and final section of this chapter, I will show the importance of marriage and family to the way in which Guttman's scientific credibility was fashioned.

‘They practically had a Joint Career’

The *Machon*, employing highly specialized scientific workers organized in project-based research teams, and characterized by an efficient, standardized labor process, functioned more like an industrial operation than a traditional research organization, such as the laboratory or the university department. A significant adaptation in the process of domestication in a foreign locale was in the substitution of family-like dynamics for administrative hierarchies and formalized social relations. The new state’s inhospitable conditions for large-scale, labor-intensive and costly research meant that much of the work had to be done by actual family members. As the *Machon* evolved by the early 1960s into a successful national research organization, familial patterns of interaction and relating persisted, securing knowledge production by operationalizing emotional commitment and intimacy in place of “work-discipline” and a vocational “ethic of responsibility.”

In an interview, one of Guttman’s daughters observed with respect to her parents’ marriage that, “They practically had a joint career. Their lives were intertwined in many ways.”⁶⁵ Ruth and Louis almost always traveled together, and when they didn’t, he would keep her updated. In a letter in which he reports going to a conference on her behalf (genetics of mental health), he summarized for her the setting and central lectures. The account is shorthanded and judgmental, suggesting that they shared the same intellectual universe, if not the same opinions.

⁶⁵ Daphne Guttman, interview by the author, date unknown.



Figure 15: Ruth Guttman with her children, 1958. From the Guttman family collection.

To understand all this, we need to realize that Ruth Halpern (later, Guttman), despite her unusual life story, was in many ways an exemplar of the new woman scientist. Fleeing Nazi Vienna in late 1938, the Halperns, like many other Jewish refugees from Europe, landed in New York and began a new life. The fifteen-year-old and very angry Ruth—an educated and opinionated young woman with a poor command of English and a deep distaste for American culture—could not forgive her father’s choice of refuge. A devout Zionist, she set course to reach Palestine, and after one year at Brooklyn College, applied to the New York State College of Agriculture at Cornell University. “I thought one needed to study agriculture in order to immigrate to Palestine.” At Cornell, taking introductory classes in physics, chemistry, and biology, she discovered that her true interest lied in basic, rather than applied, science: “I did all the required courses, and one of them was ‘introduction to heredity’. I fell in love. [After that, I focused on genetics].” The beginning of a life-long affair with the biological study of heredity also marked the beginning of romantic involvement with Louis Guttman, then an up-and-coming professor of sociology at Cornell. A labor Zionist and active member of HaBonim, Louis was an appropriate *shidech* (match) and the two married. Ruth continued on into a Masters program in genetics. Eventually, the couple decided they would immigrate after Ruth had completed her Ph.D., a relatively recent possibility for women at Ivy League universities. The life sciences, however, were still considered a gendered domain, and Cornell rejected her application. “They told me: ‘women can’t carry all these heavy plants around.’” Ruth applied instead for a Ph.D. program at the HU, and with the cohort of 1947, she began her doctoral studies in plant cytology—the only botanical field in which research degrees were offered at the time—at the small Jerusalemite university.⁶⁶

⁶⁶ Ruth Guttman, video interview [date unknown]. The Guttman Family Collection.

Ruth regulated the scholarly life of her husband, serving as an in-between who guarded the domestic and scientific division of labor: “they had met at Cornell and now, while he lectures at Harvard, she is in Jerusalem, looking after their two small children and going on with her own work, which includes learning the reason for a declining birth rate in Israeli cows.”⁶⁷ Reflecting on her role as mediator and editor, she said:

I was so involved. He shared with me, with us. He would get mad about this thing or other, and write a letter. Then he would let me read it, and my job was to be the censor because often his tone was too harsh. I would tell him: “you can’t send a letter like this; this is too much.” He couldn’t help himself but saying the truth, and you can’t always do that.⁶⁸

Ruth watched over her husband and guarded his creative space. On the other hand, there is also evidence of the blurring of the boundary between home and work life. Ruth facilitated the consolidation of the *Machon* community, by hosting and entertaining alongside Louis at their house, and also by making friends with the scientific workers and their wives. In this sense she functioned in the traditional role of the scholar’s wife.

The childhood and adolescent memories of Daphne Guttman, the youngest of the Guttman children, serve well to illustrate both the extent of family involvement in the work done at the *Machon*, and the variety of forms in which they contributed to the production and reproduction of scientific knowledge:

My father's professional life was always a natural part of our family life. [Some of the people sitting here worked closely with my father for many years and for me they are an inseparable part of my personal history, an extended part of my growing up in the Guttman family.] [He] shared his life with us, his family as a whole...made me personally feel part of all these aspects of his life...so I suppose I just grew up into them. It was natural for me to have discussions with him about his work and ideas, alongside hearing his old family stories, telling him about my ideas or just talking

⁶⁷ Beckett, “Social Science Pioneer in Israel.”

⁶⁸ Ruth Guttman, video interview [date unknown]. The Guttman Family Collection.

about anything. The words "mapping sentence and data analysis" were part of my growing up vocabulary. It was natural for me and a lot of fun to be tried on by both my parents the latest developed test by their good friend Wechsler from Cornell University, alongside talking about my next scout's trip, or playing games together. It was natural to see my father's growing piles of hand written endless equations alongside a pile of reading books or today's newspapers.⁶⁹

Daphne recalls spending her school vacations in the *Machon*, serving as a guinea pig for new tests, and the image of her parents as sharing “facet theory for breakfast, for lunch, and for dinner.” On the other hand, regarding his qualities as a father, Daphne recalled: “He would play with us, invent games, do magic tricks, move his ears...he’d always be telling funny stories...he was simply a wonderful father. From that perspective, he was truly a warm, genial, humane person. On the other hand, he was very strict, pedantic. And when it came to his scientific integrity, he was ‘*kapdan ad zov-dam*’ [literally: strict to the extent of bleeding].”⁷⁰ Guttman’s scientific research and the *Machon* were part of their domestic life. Home was an extension of the institute, or more accurately, a space of research and shared reasoning.

By the same token, the *Machon* was in some ways an extension of home. Daphne recalls spending all her school vacations in the *Machon*, assisting with the work. Later, during her college years, she served as a field interviewer for pay: “They also conducted all their tests on me [Wechsler Adult Intelligence Scale]; I was their guinea pig [laughing]. Every experiment they first tried on us; it was fun!”⁷¹ Among Guttman’s domestic relationships, his marriage played an especially central role in the migration and reproduction of knowledge, both at the material and epistemic levels. It was not about doing work, as Ruth didn’t contribute at all to the

⁶⁹ Daphne Guttman, Guttman Memorial.

⁷⁰ Daphne Guttman, interview by the author, date unknown.

⁷¹ Ibid.

collection or processing of data, or to the administrative aspects of research (recruiting people, managing funds and people, finding and negotiating with clients, etc.). She did however join the *Machon*; Louis was able to “seduce” her with an opportunity to pursue her own research interests within the framework of two existing projects that were commissioned by the government, involving genetic analysis in tomatoes and in mice.⁷² She was later also involved in studies of the human genetics.⁷³

On the face of it, offering her the opportunity to join these two ongoing projects reads like another act of love and support on Louis’ part, who, according to various accounts, was deeply committed to advancing Ruth’s scientific career from the very beginning. The “proposition” to join the *Machon* came at a moment of stagnation in Ruth’s career, when, having completed her post-doctorate at Berkeley—where she learned about the new and exciting developments in genetics—she had returned unwillingly to the routines of cytological lab work at the HU’s Medical School. Joining the *Machon* as a full-time researcher was perceived primarily as a way out of a scientific field that was rapidly losing relevance.

What is worth noting in this context is the epistemic-authority value of Ruth’s use of social research methods of empirical data collection and analysis in more traditionally empirical data-driven disciplines, such as plant, animal and human genetics. This lent credibility to survey research as a methodological enterprise and tool that could be applicable the entire domain of human behavior. It also provided a means of connecting survey research with human genetics,

⁷² “A genetic Analysis of 8 Different Traits in 80 Lines of the Cultivated Tomato and in the Hybrids Resulting from the Crosses Between Them” and “The Inheritance of Behavior Sequences in Mice” (The Institute of Applied Social Research, Annual Research Report 1965-1966).

⁷³ One report, “Test for a Biological Basis for Correlated Abilities”, was published in *Human Biology*. Curiously enough, some of these studies, involving familial correlation of physical and behavioral traits, caught the eyes of the American Eugenics Society (Correspondence, 1962).

and helped her husband to buff up the scientific research profile of the *Machon* by showing that it did more than merely applied policy research.

In the Guttmans' correspondence, there is a mix of the professional, scientific, domestic, and familial. Louis writes only about academic and scientific matters. In one letter, he remarked, "I've been experimenting with different techniques on the computer for testing out configurations in matrices...we should get results on your matrices this week, at least for the first approximations..."⁷⁴ Clearly science was where they connected first—in one letter he even asks how her course is going before asking how the kids are. Only in the last part of the letter does he adopt a more personal tone: "I miss you and the kids probably more than you miss me. Maybe that's why I've been driving on this new stuff, and am busy day and night. Anyway, it's been a chance to get a lot of things out of the way..."⁷⁵ Ruth's letters reflect her scientific marital duties. In a letter from Oct 23, 1962, she notes, "I am very involved with the setting up of the mouse lab, films, room, etc. At home there is still no hot water and the garden needs lots of time. So my system now is to garden in the afternoon and work evenings when it's quiet, until about 11."⁷⁶

If the Guttman's home was pervaded by the *Machon*, so too was the *Machon* a site of extended family. In the absence of institutional controls, family ties were essential for nurturing and solidifying commitment. Work relations blended into relations of friendship and community, even kinship; there were also constant social activities that turned the workers into a tightly knit community. Guttman certainly didn't invent such practices—this blurring characterized the

⁷⁴ Ibid.

⁷⁵ Louis Guttman to Ruth Guttman, November 14, 1962. Box 16, File 67, Guttman Papers, BGA.

⁷⁶ Ruth Guttman to Louis Guttman, October 23, 1962. Box 16, File 66, Guttman Papers, BGA.

workspaces of industrialists as well—but it was not common at all in academia. Colleagues at the *Machon* spent weekends and holidays together, celebrating birthdays, garden parties, bringing spouses and children into the fold, and constantly bridging (or perhaps transgressing) the boundary between the private and professional, home and office. “Family rhetoric”—supported by appropriate practices—suggested care, protection, obligation, and commitment that went beyond the “call of duty.” Expectations could thus be framed in personal rather than professional terms and pressure accordingly applied. Ruth and Louis also showcased their collaborative creative life, as if providing a model or exhibiting a standard that should apply for the entire *Machon*.

CONCLUSION

In choosing to tell the story of American-behavioral-science-goes-to-Palestine through the life of a particular scientist, this thesis aims, above all else, to insist on something so immediate and basic that it can be easily overlooked: namely, that knowledge migrates and mutates with people, and how it takes root elsewhere and develops is fundamentally shaped by the lived experience, virtues, and personal ties of the individuals who carry it. Efforts to analyze the global circulation of scientific ideas and tools might feel at first sight as though they need to be carried out on a dauntingly large scale and abstract levels, but as I have attempted to show, there are persuasive ways to write such a history that require us to foreground *the role of the personal and of the embodied*.

It is true that much discussion in the scholarly literature has been devoted to brokers and mediators—explorers and travelers, missionaries and merchants, colonial collectors and native translators, diplomats and spies—and their role in cross-cultural scientific transmission and exchange.¹ However, these *go-betweens*, as Schaffer, Roberts and al. have dubbed them, are often portrayed not as knowledge-makers so much as simple carriers of knowledge which is already codified or otherwise explicated; whether that be in the form of scientific texts, maps, diagrams, or numbers.² It is usually some quality of the mobile piece of knowledge—its “flatness,” for instance, to use a Latourian idiom—that is assumed to determine whether it will successfully travel across many contexts. More recently, historians have been successful in

¹ Some notable such studies include: Marwa Elshakry, *Reading Darwin in Arabic, 1860-1950* (Chicago, IL: The University of Chicago Press, 2013); Fa-ti Fan, *British Naturalists in Qing China: Science, Empire, and Cultural Encounter* (Cambridge, MA: Harvard University Press, 2004); Neil Safier, *Measuring the New World: Enlightenment Science and South America* (Chicago: Chicago University Press, 2008)

² Schaffer, Simon et al., eds. *The Brokered World: Go-Betweens and Global Intelligence, 1770–1820*.

retracing the itineraries of epistemic things – namely, the constitutive elements or “stuff” of science, from specimens and artifacts to genetic material to raw data; studies that do well to demonstrate the fundamental instability of *all* scientific meaning when set in motion.³

Nevertheless, these traveling particulars are often conceived as being essentially separate from the human actors who manipulate and trade in them, and the artisanal know-how required for making them work in new settings is simply not mentioned.

In my dissertation, however, I attempted to make a case for an argument, which is not new and is not mine, but remains marginal in the global history of science. That is to start with so-called immutable mobiles in order to ask questions about knowledge transfer and cultural transformation (and its durability) or to embark on the historiographical quest of finding “reliable” bits of knowledge in order to account for the apparent universality of modern technoscience, as well as for the alleged homogeneity of the world we live in today, is simply the wrong way to go.⁴ As I have attempted to show throughout the dissertation, facts, (as well as concepts, instruments, measurement techniques and so on), however reliable, do not travel so well on their own. As they wander away from their cultural homelands and move into new contexts, their credibility is often contested and their value or capacities doubted. Some meet with indifference, incomprehension, even ridicule. Others are appropriated by powerful sponsors to promote other projects and interests. And all along they must compete with other ways of knowing and doing over resources, territory and authority. On their own, even if neatly packaged

³ See, for example: Lukas Rieppel, “Prospecting for Dinosaurs on the Mining Frontier,” *Social Studies of Science* 45, no. 2 (2015), 161-186

⁴ Bruno Latour, “Drawing Things Together,” in *Representation in Scientific Practice*, ed. Michael Lynch and Steve Woolgar (Cambridge, MA: MIT Press, 1990), 19-68, and *Science in Action: How to Follow Scientists and Engineers Through Society* (Cambridge, Mass.: Harvard University Press, 1987), 223-28; Mary S. Morgan and Peter Howlett, eds. *How Well Do Facts Travel? The Dissemination of Reliable Knowledge* (New York: Cambridge University Press, 2011)

and carefully labeled, they lack a blueprint on how to practice science and how to live a scientific life – at home, in the lab, or in public.

As I argue, these moral and experiential aspects of science, which Polanyi famously termed “tacit knowledge,” are either pre-reflective, or else cannot be adequately verbalized and are therefore difficult if not impossible to transmit by means other than regular interaction and extensive personal contact.⁵ Inarticulate or unarticulated, mundane and repetitive, and seemingly without much character or importance, but it is precisely this kind of “craftsmanship” that only skilled practitioners can embody, with all the technical skills, mental habits, ‘rules of thumb,’ learned intuition and sensibilities, as well as values and ideals, that give scientific claims and methods their orientation and meaning.⁶ Without taking them into account, I argue, it is very difficult to make historical sense of the acculturation processes this thesis is concerned with.

The experience that Guttman had gained during his World War II service as expert-consultant for the U.S. War Department is a good example of that tacit or personal knowledge that helps scientific claims and methods feel at home away from home. Spending time at the Pentagon taught him how to negotiate with public officials, what problems to pick and also how to practice science in a military environment. The U.S. Army Research Branch itself served as an organizational platform and model for the research unit he had established in the Haganah, and on the basis of which the civilian institute was later built. Furthermore, getting a handle on the operation of big social science and learning how to produce useful knowledge that could be applied in guiding effective policy-design was crucial for turning the Israeli institute into a viable

⁵ Michael Polanyi, *Personal Knowledge: Towards a Post-critical Philosophy* (London: Routledge, 1962).

⁶ Fleck also supported the notion that the “technical skills required for any scientific investigation” cannot be “formulated in terms of logic.” Ludwik Fleck, *The Genesis and Development of a Scientific Fact* (Chicago: University of Chicago Press, 1979), 35.

enterprise under continued government sponsorship. Grant writing and fundraising skills—a new must for every professional social scientist in postwar American academia—and the experience of procuring both governmental and private funding for research independently of a university, proved as essential for ensuring the institute’s academic identity in such circumstances. Guttman brought with him, in other words, not only a set of scientific tools, but also the knowledge of how to recreate the material and institutional milieu in which these tools properly functioned.

Guttman also brought with him a “feel” for qualitative data and a “knack” for solving problems – propensities that could not be formalized. Survey research was hard to teach and much of it at that time could only be learned on-the-job. Asking unambiguous questions was considered an art whose secrets no handbook could fully capture and Guttman’s technique of scale analysis, though it was described in several articles, was infamously difficult to grasp and execute. A big part of it, as I explained in Chapter Two, was the method’s reliance on the trained capacity to see visual patterns in large and rather messy datasets. This dimension of the method was an embodied skill that Guttman, who was not the most articulate scientist to begin with, could only demonstrate. Yet, his data connoisseurship, fostered, as it was typical for this pre-computer era, by many hours spent running sets of punched cards through the counter-sorter himself, as well as what could only be described as his unusual analytical dexterity, enabled flexibility and improvisation in face of changing conditions and emergent problems in the new setting.⁷ Such abilities became particularly handy, so to speak, in the early studies, when the data

⁷ Generally, Guttman was able to take advantage of computer technology to further and develop his visual approach to data analysis. Said on the SSA program: “a pleasing related feature of the techniques discussed is that the results achieved are directly and intuitively interpretable by relatively untutored persons, as well as by the scientist who takes responsibility for his project in its entirety.”

collected included a lot of “statistical noise,” and much ingenuity was required to control for obscuring factors.

Finally, Guttman embodied what could be called, after Olesko, an “ethos of exactitude,” which accounts for the role of professional beliefs, ideals or aspirations, as well as legitimate modes of affective expression (or its absence) in the transfer of science.⁸ As I explained in Chapter One, Guttman, like Chapin before him, reflected in his interests the narrowing effect of the new specialized training in sociology; but he also, and perhaps more significantly so, absorbed and internalized an extreme version of the ethos of objectivism and its strict scientific discipline. This ethos, which formed what Fleck had called the “professional habits needed to become a trained person” in American sociology of the 1930s and 1940s, manifested in an epistemic value-system and attendant set of behaviors—uncompromising demand for rigorous empirical proof, aversion to conceptual ambiguity, and the effort invested in achieving maximal statistical accuracy—that Guttman adopted during his studies and continued to characterize his work. Such self-control, as Burnett argued, was essential in settings and circumstances where one needed “more than a good instrument” in order to obtain good observations and guarantee the quality of results.⁹

⁸ Olesko 1991. Daston and Galison define mechanical objectivity as “an attempt to capture nature with as little human intervention as possible.” I prefer the characterization of the notion provided by Ted Porter, who insists on the close connection between the meanings of objectivity in science and in moral and political discourse. For Porter, mechanical objectivity is the kind of objectivity that is allegedly guaranteed by rule following, in contrast to “disciplinary objectivity” which is based on consensus among experts. It is “mechanical” because it substitutes procedure or method for judgment and will. For both Daston/Galison and Porter, mechanical objectivity is associated with self-restraint and self-discipline, that is, with the ability to follow the rules regardless of personal preferences, understanding or feeling.

⁹ D. Graham Burnett, *Masters of All They Surveyed: Exploration, Geography, and A British El Dorado* (2000), 99, 103

Note too that there were other Jews on the move in Palestine at that time: European refugee intellectuals, Zionist technical experts, Jewish-American developers and visiting professors, Institute research workers and guest scientists. Like him, they carried ideas, but also a whole lot of ‘know-how,’ both regarding the very practice of social science and regarding its normative order. In other words, the large-scale migrations of Jews into and through Palestine during the postwar period also represented a migration of a huge diversity of experiences that sometimes meshed and sometimes clashed. The transmission and acculturation process of American social science was thus buffeted, honed, and shaped by multiple encounters that in the end cannot be reduced to the question of how a certain theory or research methodology was “received.” For instance, despite the common historical assumption that humanistic or interpretive sociology was averse to the application of quantitative methods in the study of social phenomena, Buber and Co. were not opposed in principle to statistical analysis and were certainly not against empirical inquiry. They did not hold surveys of opinion in high regard due to their high use-value (and little truth-value). But neither did Guttman for whom it was a method for observing society, not an end in itself; something they did not understand. Though the hostile, decade-long *Methodenstreit* at the world-only and refugee-filled Jewish university seems like a classic case of “history repeats itself, first as tragedy, then as farce,” as I have tried to argue, what decided the fate of academic sociology in Israel was not (or at least not only) the politics of method, but the *scholarly habitus* of the mandarin intellectuals in Jerusalem and their exiled brethren in London, New York and Chicago: habituated and durable patterns of thinking and doing, both practical and normative—starting from the way they write letters and memos, to how they judged the quality of a candidate, to the institutional survival strategies they employed—which were inculcated into the minds and bodies of these twice-displaced Central

European Jews during their academic socialization in the German-language cultural milieu of the 1920s and 1930s and then transferred and reproduced at the Hebrew University, determinately shaping their ideal of the “man of science,” and leading eventually to their decision to reject Guttman and the distinctively American approach to sociology that he represented.¹⁰

Focusing on the personal and the embodied nature of knowledge on the move also helps us think in new ways about scientific credibility. The claim that trust is imperative for constituting every kind of knowledge has become a commonplace in the history of science.¹¹ I argue in this thesis, however, that the question of trust powerfully resurfaces when science is practiced away from its native lands. As I have shown, behavioral survey research was an unknown quantity in Jewish Palestine, and as a result, the kind of knowledge claims it generated – statistical representations of aggregated individual attitudes or preferences – lost the established public credibility that they had in the American context. Furthermore, in the absence of institutionalized guarantee for professional skill in the new state (along with a culture founded on familiarity and personal authority), teaching trust in numbers to people who trusted only those they knew proved challenging. Being able to trust the man behind the data became much more crucial – indeed, a limiting condition for the successful localization of survey research, and more generally, for social scientific knowledge to be generated, communicated and used.

Great importance was therefore placed on qualities or characteristics that historically code for reliability. One such virtue, which served as proxy for scientific integrity and helped to

¹⁰ Bourdieu examined the notion of the scholarly habitus (or the durable habits of modern academic intellectuals) mostly in *Homo Academicus* on the basis of theoretical work he did previously in *The Logic of Practice* and in *Distinction*. For a wonderful historical analysis of the scholarly habitus, see: Gadi Algazi, “Scholars in Households: Refiguring the Learned Habitus, 1480-1550,” *Science in Context* 16, 1-2 (2003), 9-42.

¹¹ Steven Shapin, *A Social History of Truth: Civility and Science in Seventeenth-Century England* (Chicago: University of Chicago Press, 1994)

overcome ASRI's credibility problems, was personal integrity. Guttman's moral uprightness was displayed in various ways: he became known as a difficult, uncompromising man, who would not bend or make concessions for the sake of expediency. Brutally honest and "unforgiving of fools," he would often get into ugly fights over matters large and small, insult and intimidate. And yet, those who worked with him considered it a privilege. They would mention his seriousness and his decency, as well as the exacting standards of conduct that he had set, primarily for himself, but also for the other researchers employed at the Institute in terms of both their practice and ethics. As I tried to show, in the same manner that the office was not distinguished from the home, so to speak, in the way Guttman ran the Institute, also the distinction between professional and personal conduct was oftentimes blurred. It was all personal and all scientific, all the time. Perhaps the clearest example of such blurring of boundaries, which, I argued, was also quite telling of this moral economy of integrity, was the Foa affair. For Guttman, who was as adamant about the commitments and obligations of family life as he was about hypothesis testing, betraying one's role as husband and father was indicative, if not directly related to making claims which are not supported by empirical evidence.

Guttman's charisma on all these fronts acted to catalyze and advance organized social science inside and outside Israel. Others have also called attention to the importance of charisma in other pioneer survey research institutes, Lazarsfeld being perhaps the most iconic example. But as I argued, the personification of ASRI in the person of Guttman was an unusual case in two senses: the degree to which the reputation of the institute and its products depended on the reputation of its founder and scientific director; and the orchestrated manufacture of charisma, it being to a large extent an institutional effect and one of the 'products' that the ASRI delivered.

Finally, Guttman's personal ties – friendships and family relations – were crucial, financially and symbolically, for sustaining everyday science on the frontier. These ties included the war cohort, the people who made TAS, and the membership of the new survey research community that came into being in the war's aftermath. Many of these social scientists, who were mentors and colleagues and went on to occupy key positions in top universities and research organizations, as well as in private foundations and federal sponsoring agencies, enlisted for the *American Committee for Social Research in Israel, Inc.*, which as I showed served not only as a fundraising mechanism, but in many ways also the scientific network through which this knowledge circulated and was made durable (through frequent travels to lecture tours, research stays, the presence of invited guests, collaborations and correspondence). Lastly, as I explained in Chapter Four, family relations played an important role in the daily life of the institute, and, interestingly, also in the shaping of the collegial relationships among its leaders.

In placing Louis Guttman front and center, this dissertation makes a point of showing that the biographical is a surprisingly productive way of understanding or making sense of structural processes in the history of science, such as knowledge migration and indigenization. Unique though his life may have been, Guttman's career trajectory and professional identity were in many ways exemplary of a scientific life in American social science of that era: a product par excellence of sociological objectivism and the rise of large-scale applied research, Guttman stood for a firm belief in the power of social science to control and predict, rationalize and modernize social action. More specifically, Guttman's journey illuminated the many ways by

which individuals are what James Clifford called “traveling cultures.”¹² As several innovative biographies have successfully demonstrated, the idiosyncrasies of the most eccentric scientists often exemplified the spirit of the culture they sometimes came to symbolize. RAND nuclear strategist Herman Kahn (who was the inspiration for Kubrick’s Dr. Strangelove), for instance, with the swimming pool he had installed inside his fall-out shelter, the experimental doses of LSD that he took, and the droll monologues on a post-holocaust radioactive existence, epitomized “the rational irrationality” of American Cold War culture that believed in a survivable and winnable nuclear war.¹³ Arriving in Palestine equipped with a brand new Super Buick, a portable 16mm film camera (the only one in the country, if not the entire region) and his truth-telling machine—technoscience packaged as consumer goods—Guttman similarly exemplified postwar American culture, with its consumerism, technological enthusiasm, and developmental evangelism. He was also a man of his time politically: the instrumentality, parochialism, and dangerous naiveté of the “quiet American” who intervenes in the affairs of other countries without having an inkling of the complexity involved.

Unanswered Questions

The dissertation has suggested that transnational knowledge migration in the postwar social sciences—specifically the successful indigenization in Israel/Palestine of American-style large-scale survey research—is crucial for providing a complete historical account of the rise and

¹² James Clifford, “Traveling Cultures,” in *Routes: Travel and Translation in the Late Twentieth Century* (Cambridge, MA: Harvard University Press, 1997), 17-46.

¹³ Sharon Ghamari-Tabrizi, *The Worlds of Herman Kahn: The Intuitive Science of Thermonuclear War* (Cambridge, MA: Harvard University Press, 2005). This point was beautifully illustrated in a review of the book by Charles Thorpe. See: Charles Thorpe, book review, *Journal of Historical Biography*, 134-140

spread of a behavioral idiom and its attendant modes of representation and intervention. Or in more general terms, that the traveling of the modern social sciences was what made them what they became. One set of questions, therefore, which remains to be explored, concerns *the concrete influence the work done in Israel during the 1950s and early 1960s had on mainstream social science*. To what degree has behavioral science been transformed—beyond the localized intermingling with vernacular nomenclature and understanding that I described—by these encounters and exchanges on the eastern frontier? What happened to Guttman’s experimental techniques of questionnaire design and data analysis when deployed in the study of large heterogeneous populations on a national scale? Did moving into the new contexts of postwar nation building and state-formation and being applied to new problem areas, result in methodological changes? In what ways did this transplantation project, which was itself an early example of the traveling of socio-psychological facts to the global south, contribute in turn to the stabilization of knowledge claims and overall authority of academic institutions that emerged from the war as global centers of behavioral research and innovation?

One highly significant area for which the ASRI was known throughout the scientific world was basic theoretical research in methodology, primarily in *research on the structure of attitudes and opinions*. Finding empirical ways of explaining what attitudes are, how they “behave” (or what is the psychological and sociological meaning of survey data patterns) and how to measure them better was Guttman’s main scientific focus when the institute was conducting its first large surveys. It was a combination of his wartime work on scale analysis, with the new research program he began in 1944-45 on the higher components of scalable attitudes (in the context of which he and Suchman have first described the “intensity function in scale analysis”) that helped establish that attitudes have both a “direction” and a “volume”

(namely the intensity with which an attitude is held), and which he then took with him to Palestine/Israel.¹⁴

In the course of the last large study Guttman's military research unit conducted in the nascent Israeli Army in 1949 (which concerned the plans of soldiers after demobilization), another "principal component" was discovered. The study was a near identical reenactment of the "postwar plans" survey the ARB carried out among U.S. Army soldiers in June 1944, except for one key difference: the Hebrew schedule (questionnaire) included a series of additional questions meant to probe into what has thereafter been called the "closure" of attitudes. Questions such as "have you made up your mind as to what you'd like to do?" yielded, the analysis of data revealed, three types of reply: one came from those who were "closed": that is those who knew very well what they wanted to do in the future. Then there were those who were "open": they knew that they did not know what they wanted to do. And lastly, there were those who were unsure. In analyzing these results—tabulating the degree of closure against each alternative plan separately—Guttman and his team received a surprising result that had never been seen before in attitude research: the relation of closure to attitude on a particular topic was not linear, not even curvilinear, but "polytone" (forming an N shaped curve). Those most positive toward staying in the Army were also the most closed: they knew what they wanted to do. Those somewhat less positive were more open. Those even less positive were completely open. But as the zero point (not positive and not negative or neutral) was approached, the closure began to increase again. Crossing the zero point to being somewhat negative led again to high closure: here were people who knew that they did not want to stay in the army. But there were

¹⁴ The theory of principal components was both a way of describing and a theoretical argument regarding the structure of survey-based observations and their psychological meaning.

people even more negative, and they were “open”: The most negative people did not know what they wanted to do, but they’ll be damned if they have to take this particular alternative. In simple psychological terms, it meant that indecision was no guarantee for open-mindedness. This phenomenon was labeled “the prejudice principle.”

Identifying this response pattern enabled a more accurate prediction of what would actually happen with respect to the likelihood of volunteering to the postwar army and has generally proven indispensable for properly interpreting survey results. But it also allowed Guttman to make theoretical claims regarding the nature of human behavior on the basis of observations made in Israel. Since Volume Four of TAS (‘Measurement and Prediction’) was then still in press, Guttman was able to include a mention of this finding.¹⁵ “Closure” was subsequently used in further studies and entered the general theory of social attitudes.¹⁶ The theory of principal components—the idea that any opinion involves certain components that determine its modality—was considered then of great significance in many areas of social science where expressions of opinion, attitudes, or values were important variables. As Samuel Stouffer argued in a 1953 grant application to fund a joint Harvard-ASRI study of the third and fourth components, “[it] is well known, [that] such expressions vary greatly in strength and in the extent that they are likely to ‘stay put’ in the face of pressures to change them.”¹⁷

¹⁵ “The third principal psychological component of scalable attitude,” Vol. 4, p. 313

¹⁶ An immediate consequence of this discovery was the deduction of the fourth principal component of attitudes, namely, “involvement” (the extent to which an individual was personally and deeply involved). The empirical evidence for substantiating this hypothesis was found in a large commissioned study on listening habits to *The Voice of Israel* (Israel’s national broadcasting system), as well as in a study conducted by the Harvard Laboratory of Social Relations among college students. The limited scope of this review does not allow me to elaborate on the nature of the collaboration between the two “labs.”

¹⁷ “Request for grant-in-aid for research on the structure of opinions,” The Laboratory of Social Relations, Harvard University to the Social Science Division, Rockefeller Foundation, December 15, 1953

While spending a term at Harvard in 1954 for the purpose of said collaboration, Guttman began working on a new approach to measurement he called “facet theory,” by means of which verbal formulations could be shown to guide empirical observation and constitute a basis for examining the resulting statistical structure. Trying early on to demonstrate the usefulness of facets, Guttman boldly attempted to put some of Talcott Parsons’ formulations in facet terms, which arguably rendered the latter’s impossibly dense and notoriously opaque action theory empirically testable.¹⁸ The breakthrough for facet theory occurred in the mid-1960s when Guttman’s nonmetric techniques were incorporated into a computer program.¹⁹

A second area of research in which work done at the Institute had shaped social science was the study of personal adjustment in new immigrants. Unlike the principal components theory, which was arguably an accident of place (Guttman happened to be in Israel, and used large samples surveyed for Israeli government offices or private clients to test the theory), the social psychology of immigration and displacement was developed on the basis of a uniquely Israeli experience. There were several studies I’m interested in, but the largest and most significant was a 1949-1950 survey of attitudes and behavior patterns of immigrants during the first year in Israel. The data was collected in nine different languages from nearly 2000 men and women who were living in 19 temporary transit camps. And it was a pioneer attempt at applying Guttman scaling to such a large sample. As Judith Tannenbaum-Shuval, a Harvard graduate and a senior researcher at the Institute who led the study, wrote in 1951, “the phenomenon of a

¹⁸ Louis Guttman to Leland C. DeVinney, February 24 1954 and “Change in topic for social relations 278 – Louis Guttman’s seminar. The topic for Tuesday evening, February 23 (8pm – Emerson 323) will be ‘A possible Metatheory for the Parsonian Theory of Action: The Theory of Facets.’” RAC

¹⁹ Guttman and Lingoes worked together on Smallest Space Analysis (SSA), which was among the first (if not the very first) computer program for non-metric multidimensional scaling (MDS).

country's developing under circumstances so unique and at such a rapid pace, is bound to produce social situations which have never before existed in other countries." One such situation was mass immigration, which offered the social scientist "an opportunity to observe the process of large scale acculturation [usually] operating over a period of several generations." In the introduction to Shuval's 1963 celebrated book, *Immigrants on the Threshold*, which was based on the results of this study, Guttman went even further, and justifying the general scientific interest in mass immigration to Israel, argued that the situation of the hundreds of thousands of immigrants—held in barely habitable refugee transit camps and tent cities—was akin to artificially-induced adolescence on a large scale, which made it especially suitable for studying adjustment processes – usually studied in individual adolescents – in relatively controlled conditions. "Immigrants constitute a class of people for whom a relatively sharp division between past and future can be made. There is a fairly clearly demarcated point at which they physically enter their country of destination. Given such a physically clear transition point, one can begin to inquire about what it is in the past that helps them adjust to facing an unknown future."

Employing this kind of reasoning, and the scientific arsenal of TAS—Shuval solidified the vernacular use of the concept of personal adjustment, which Guttman first invoked in regards to soldiers during the 1948 War, and also successfully introduced the idea of "relative deprivation"—the particular historical circumstances of immigration in early-day Israel and the international politics of postwar displacement were generalized. Shuval was consequently appointed as a special advisor for UNESCO, and the organization also provided support that enabled the Institute to complete the analysis of the data. In future research I'm planning to look

into the effect absorption research in Israel had on the conceptualization of immigration and displacement, as well as on the policy of relevant organizations, like the UN.

Another set of questions left unanswered in the dissertation concerns the influence that ASRI, as an international pioneer in the application of social science to the building of a new nation, may have had beyond the bounds of the Israeli state and its nation-building project, mainly on the adoption of public opinion polling and survey research in the global south. Having been successful in making state bureaucracy “more sensitive to the popular will,” as it was argued on several occasions in the early 1950s, Guttman’s research institute was hailed as “a model for others to follow” in the utilization of research findings in public policy and governance. The veracity of this reputation notwithstanding, had it indeed been imitated elsewhere and in what ways? In more critical terms, was the model Guttman created flexible enough or was it too infused with nationalism to travel elsewhere? An undated photograph, likely from the mid-1960s, shows Guttman lecturing at a seminar in Cuba; he was twice the president of WAPOR, the international counterpart of AAPOR. How far was he further seeding new social science initiatives through these efforts? What we know is that the emergence of social science in Palestine/Israel happened at a fraught and politically liminal moment in both the history of global international relations and the history of the social sciences. It happened in a time coincident with the formation of a new world order. In this sense, the use of surveys in Israel can fairly be seen as part of a larger story about the place of social scientific knowledge in postwar world history.

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