

The Price of Success: Sociologist Harry Alpert, the NSF's First Social Science Policy Architect

MARK SOLOVEY[†] and JEFFERSON D. POOLEY[‡]

[†]University of Toronto, Toronto, Ontario, Canada

[‡]Muhlenberg College, Allentown, Pennsylvania, USA

Received 2 December 2009. Revised paper accepted 23 July 2010

Summary

Harry Alpert (1912–1977), the US sociologist, is best-known for his directorship of the National Science Foundation's social science programme in the 1950s. This study extends our understanding of Alpert in two main ways: first, by examining the earlier development of his views and career. Beginning with his 1939 biography of Emile Durkheim, we explore the early development of Alpert's views about foundational questions concerning the scientific status of sociology and social science more generally, proper social science methodology, the practical value of social science, the academic institutionalisation of sociology, and the unity-of-science viewpoint. Second, this paper illuminates Alpert's complex involvement with certain tensions in mid-century US social science that were themselves linked to major transformations in national science policy, public patronage, and unequal relations between the social and natural sciences. We show that Alpert's views about the intellectual foundations, practical relevance, and institutional standing of the social sciences were, in some important respects, at odds with his NSF policy work. Although remembered as a quantitative evangelist and advocate for the unity-of-science viewpoint, Alpert was in fact an urbane critic of natural-science envy, social scientific certainty, and what he saw as excessive devotion to quantitative methods.

Contents

1. Introduction	229
2. A Durkheim scholar	232
3. A man of government and public opinion expert	239
4. A social science policy architect	245
5. The price of success	253
6. Historiographical reflections and conclusion	258

1. Introduction

In his 1939 biography of Emile Durkheim, US sociologist Harry Alpert (1912–1977) claimed that the great French scholar, who had died two decades before, was well known among US social scientists, but not known well. One might say today

that Alpert, who died more than three decades ago, is somewhat known but not known well.¹

Though there has been little attention to the development of Alpert's views and career, historical accounts have noted his importance in guiding the development of the U.S. National Science Foundation's earliest efforts to provide funding for the social sciences. From 1953, when he was hired by the National Science Foundation (NSF), until 1958, when he left the agency, Alpert was the social science programme director and the key figure in establishing the agency's basic policy framework for funding social science research and fellowships.²

Historical analysis has emphasised two important points about Alpert's policy work. First, at the NSF Alpert worked under difficult circumstances. During WWII, the early post-war years, and then the onset of the Cold War years the federal government became the most important patron of US science. Natural scientists and especially physicists gained enormous influence in the transformed and greatly expanded federal science establishment. While there were important new opportunities for the social sciences to demonstrate their value to federal funders, these patrons in the civilian and military sectors of the federal government were struggling to define their interest in the social sciences. Much of the difficulty grew out of conflicting viewpoints about their scientific nature, practical uses, and political implications. Influential voices in the social sciences, in the natural sciences, and in US politics disagreed, sometimes rather bitterly, about these matters. Not only were these broad problems of major concern for the National Science Foundation and for Harry Alpert; in addition, various features of the new agency itself assured that they would remain problematic. In this context, Alpert constructed a cautious policy framework, one that placed the social sciences within a unified scientific enterprise but then identified them as junior partners to the better-established and better-funded natural sciences.³

Second, historical accounts have noted that Alpert's successful effort to establish a viable policy framework for social science funding during his short NSF tenure—only five years—was remarkably consequential. Though the agency was rather small at first and its early social science programme very modest in size and scope, the NSF grew in size and stature as time passed. During the post-Sputnik years and even more markedly during the 1960s, the NSF emerged as a major federal science agency, especially in terms of federal support for basic science. For the social sciences, support from the NSF was an esteemed endorsement and especially valued by those social scientists seeking to shore up the scientific legitimacy of their enterprise. Viewed in this manner, it's clear that Alpert's policy work had enormous importance. In 1978, twenty years after Alpert had left the NSF and one year after Alpert's death, two social scientists observed—correctly—that 'to a significant degree, NSF support

¹ Harry Alpert, *Emile Durkheim and His Sociology* (New York: Columbia University Press, 1939), 213.

² On the development of NSF social science programmes and policies during Alpert's years there, see J. Merton England, *A Patron for Pure Science: The National Science Foundation's Formative Years, 1945–57* (Washington, DC: NSF, 1982), esp. 267–72; Otto N. Larsen, *Milestones & Millstones: Social Science at the National Science Foundation, 1945–1991* (New Brunswick, NJ: Transaction Publishers, 1992), esp. 39–57; Daniel L. Kleinman and Mark Solovey, 'Hot Science/Cold War: The National Science Foundation after World War II', *Radical History Review*, 63 (1995), 110–139 (118–24); Desmond King, 'The Politics of Social Research: Institutionalizing Public Funding Regimes in the United States and Britain', *British Journal of Political Science*, 28 (1998), 415–44.

³ Larsen (note 2), 40–1.

for the social sciences rests upon the philosophy and policies established by Harry Alpert.⁴

The present study extends our understanding of Alpert in two main ways. First, this paper examines the earlier development of Alpert's views and career. For Alpert, his NSF policy work was an important chapter in his life's work in the social sciences, but it was only one chapter. Beginning with his 1939 biography of Emile Durkheim, we examine the early development of Alpert's views about foundational questions concerning the scientific status of sociology and social science more generally, proper social science methodology, the practical value of social science, the academic institutionalisation of sociology, and the unity-of-science viewpoint. We also consider developments in Alpert's career during and after WWII. During these years, Alpert acquired extensive experience with government social science programmes, and deepened his interest in questions about scientific methodology, practical value, and political support for social science. At the same time, he became a prominent figure in the field of social survey and public opinion research, a field that was acquiring tremendous significance and attracting major support from a number of federal agencies as well as corporations and private foundations.

Second, this paper illuminates Alpert's complex involvement with certain tensions in mid-century US social science that were themselves linked to major transformations in national science policy, public patronage, and unequal relations between the social and natural sciences. Turning to Alpert's policy work at the National Science Foundation, we see that Alpert found himself dealing with old problems in a new context. Though he was certainly constrained at the NSF, we show that he was also an active agent. He drew upon his long-standing scholarly concerns and administrative expertise in ways that informed his policy recommendations and strategic choices. While his policy work was marked by his personal initiative, it also reflected wider trends in social science funding. Moreover, by following Alpert beyond his NSF years and examining his various writings about the social sciences and federal policy, we show that Alpert's views about the intellectual foundations, practical relevance, and institutional standing of the social sciences were, in some important respects, at odds with his NSF policy work. Although remembered as a quantitative evangelist and advocate for the unity-of-science viewpoint, Alpert was in fact an urbane critic of natural-science envy, social scientific certainty, and what he saw as excessive devotion to quantitative methods. We survey Alpert's nuanced, ecumenical reflections on social science, published throughout his career, in order to highlight the disjuncture with his advocacy for NSF support of the 'hard-core' end of the social research continuum. At the NSF, Alpert downplayed his own, complex views in recognition of the social sciences' tenuous standing there. The irony of Alpert's agency is that it entailed an astute reading of—and deft manoeuvring within—the major constraints he encountered at the NSF in ways that were contrary to the views he expressed clearly elsewhere. Alpert's code-switching was a strategic necessity, though not without its costs.

This analysis of Alpert's policy work and career is especially timely in light of recent criticism of the rapidly growing body of literature on the post-WWII and especially Cold War years. David Engerman and Joel Isaac have both urged

⁴ Richard J. Hill and Walter T. Martin, 'In Memoriam: Harry Alpert, 1912–1977', *Public Opinion Quarterly*, 42 (1978), 141–2 (142).

historians of the social sciences to devote more attention to important links between pre- and post-war developments. Their underlying concern is that it is all too easy to slip into a flat-footed ‘Cold War determinism’, wherein we assume that developments during the Cold War were exclusively determined by the national security imperatives, policies, and pressures associated with that international emergency and its domestic manifestations.⁵ By following Alpert from the 1930s to the 1960s, this essay explores links between pre-WWII and post-WWII developments in US social science in an area where those links might seem especially difficult to locate—namely, in the development of social science funding policies and programmes within an agency established just as the Cold War was getting underway. Our analysis of Alpert shows how pre-WWII views and concerns about the social sciences informed postwar funding policies and—just as importantly—helped to raise basic questions about the wisdom of those policies and the hierarchical relations between the social and natural sciences that they presumed and reinforced.

2. A Durkheim scholar

Born in New York City, Alpert completed his undergraduate studies at the City College of New York in 1932 before enrolling in Columbia University’s graduate programme in sociology the following year. He received a master’s degree in 1935, a doctoral degree in 1938, and had a postdoctoral fellowship at the University of Chicago in 1940–1941. Even before starting his graduate studies, Alpert had developed an interest in French sociology. This took him to France, for studies at the Universities of Paris and Bordeaux in 1932–1933. These studies, which culminated in his dissertation, book, and other writings on Durkheim, reveal that from the beginning of his academic career, Alpert was deeply interested in a range of foundational issues regarding the social sciences. Alpert’s studies aimed to clarify how Durkheim understood the ontology of the social world and especially the relationship between the individual and the collective, social science methodology, relations between the social and natural sciences, and the social relevance of social science. Examining his studies on Durkheim enables us to see how Alpert approached these same foundational issues in ways that would continue to be important throughout his career. Moreover, in some important respects, Alpert’s views as expressed in his writings on Durkheim were inconsistent with the programmatic statements and policy recommendations he later put forth regarding social science funding at the National Science Foundation.

In the early decades of the twentieth century, Durkheim was known among US scholars but not well respected. According to historian of sociology Jennifer Platt, Durkheim was ‘commonly seen as overemphasising the social and underemphasising the psychological and the role of the individual, and in doing so as taking an implausibly “realist” approach to social facts and collective representations which led him to believe in the existence of an impossible “group mind”’.⁶ In the late 1930s,

⁵ Joel Isaac, ‘The Human Sciences in Cold War America’, *The Historical Journal*, 50 (2007), 725–46; David Engerman, ‘Social Science in the Cold War’, *Isis*, 101 (2010), 393–400. For an exemplary analysis of the linkages from the pre-WWII to post-WWII eras in the case of prominent Harvard sociologist Talcott Parsons, see Howard Brick, ‘Talcott Parsons and the Evanescence of Capitalism’, in *Transcending Capitalism: Visions of a New Society in Modern American Thought* (Ithaca, NY: Cornell University Press, 2006), 121–51.

⁶ Jennifer Platt, ‘The United States Reception of Durkheim’s *The Rules of Sociological Method*’, *Sociological Perspectives*, 38 (1995), 77–105 (93).

however, US sociologists expressed increasing interest in Durkheim, in light of two major studies that reinterpreted his views. The first of these was Harvard sociologist Talcott Parsons's 1937 *The Structure of Social Action*, which drafted Durkheim as one of four major social thinkers who were said to converge on Parsons's theory of voluntaristic action. In the post-WWII era, Parsons's analysis would be recognised as a seminal work.⁷ Published in 1939, Alpert's book provided the second main stimulus in the US reinterpretation of Durkheim. Since this book was based upon Alpert's dissertation, which he defended in 1938 but had completed—or nearly completed—in 1937, Alpert noted that he had not had time to incorporate a consideration of Parsons's interpretation.⁸

Alpert was especially keen on refuting the view that Durkheim was a social realist in the strong sense of the term. As Alpert explained, 'the most controversial and vexing problem raised by the Durkheimian conception of sociology seems to be the connection between the theory of *conscience collective* and *représentation collectives* as exposed by Durkheim and the traditional dogmas of group and social minds'. Alpert agreed with previous critics that Durkheim used the 'language of the social realists', but he argued that Durkheim had done this in a polemical way, in the course of his battles with other scholars who favoured reductionistic explanations rooted in psychological interpretations of social phenomena. As a result, US readers of Durkheim had been led 'most unfortunately' and 'quite unnecessarily' to misunderstand Durkheim's actual views, which, Alpert believed, were much more plausible and thus of greater theoretical importance.⁹

To clarify his supportive reading of Durkheim, Alpert made a distinction between two types of 'sociologism'. He defined 'pure sociologism' as the 'literal, unqualified, and substantialist acceptance' of such concepts as 'collective consciousness', 'collective entity', and 'collective representations'. In contrast, 'neo- or moderate sociologism' accepted such concepts only because they were useful as a 'heuristic mode of expressing the reality of associational life'. It was this second, more moderate type of sociologism that Durkheim subscribed to and Alpert welcomed: 'Durkheim was a moderate Durkheimian'.¹⁰ So, though US scholars had taken Durkheim to be a social realist, one could see, after placing his polemical statements in historical context and re-reading his works carefully, that Durkheim 'was not [one] of them'. In fact, Durkheim seemed to be 'explicitly opposed to a "group-mind" interpretation of his position'.¹¹

Consideration of Alpert's discussion of social realism figured prominently in scholarly reviews of his biography, which in turn advanced the wider reinterpretation of Durkheim within US sociology. Howard P. Becker, in a review published in the *American Sociological Review*, observed that after reading Alpert, it was clear that an influential 1915 study by Charles Gehlke had been 'one-sided', as Gehlke had 'made much of collective representations', 'group mind', and related 'flimsy' notions. By

⁷ Talcott Parsons, *The Structure of Social Action: A Study in Social Theory With Special Reference to a Group of Recent European Writers* (New York: McGraw-Hill, 1937). On Parsons's 1937 book, see Charles Camic, 'Structure after 50 Years: The Anatomy of a Charter', *American Journal of Sociology*, 95 (1989), 38–107 (55–60).

⁸ Alpert (note 1), 211.

⁹ *Ibid.*, 13.

¹⁰ Alpert, 'Emile Durkheim and Sociologistic Psychology', *American Journal of Sociology*, 45 (1939), 64–70 (70).

¹¹ Alpert (note 1), 14.

examining Durkheim's empirical studies, including his study of suicide—by mid-century considered to be a sociological classic—alongside his more abstract methodological statements, Alpert had shown that Durkheim himself understood that such things as 'collective representations' and 'group mind' are not 'brooding omnipresences in the sky... but observable items making up the commonly accepted "human nature" of the members of a given society'. Reviews by John Mueller in the *American Journal of Sociology* and by W. Rex Crawford in the *Annals of the American Academy of Political and Social Science* were favourable as well.¹² Thanks in part to Alpert's biography and its reception, by the 1950s Durkheim was regularly 'treated as important', and by the 1960s US sociologists commonly considered him to be one of the classical sociological theorists along with Max Weber and Karl Marx—a 'member of the "Holy Trinity"'.¹³

Most importantly here, Alpert's analysis reveals that Alpert himself found Durkheim's moderate type of sociologism attractive, as a middle ground between psychologism and pure sociologism. Like the French scholar, Alpert proposed that a 'psychology of human nature must be largely sociologistic', because 'much of our human nature has a social origin'. Consequently and more specifically, Alpert proposed that a 'comprehensive human psychology' needed to incorporate 'a sociology of values... of thought and knowledge... of mental states... of emotions, and... of language'. At the same time, Alpert warned that scholars had to make sure their studies did not portray the individual as a 'mere automaton impassively receiving and conforming to his social heritage'.¹⁴

While Alpert's discussion about social realism received the most attention by reviewers at the time and subsequently by historians, Alpert's writings on Durkheim also reveal his concern with a variety of other issues of widespread concern to US sociologists, starting with the struggle for academic acceptance. In a 1937 article in the *American Sociological Review*, Alpert considered the difficulties surrounding the establishment of the very first university course in sociology offered in France. Though in France sociology had been offered in other educational settings before, it wasn't until 1887 that Durkheim taught that first university course, at the University of Bordeaux. In recounting the circumstances of that first course, Alpert observed that 'traditional moralists and university philosophers had the greatest contempt for the upstart science of social phenomena. They regarded it not at all as a proper philosophical discipline, but rather as an amiable pastime to be dabbled in by mathematicians, doctors, engineers and physicians. Sociology was indeed *scientia non grata*'. As if that weren't trouble enough, elsewhere in the French university 'economists and jurists... looked down upon this bizarre science of society with

¹² Reviews of Alpert's Durkheim biography: Howard Becker in *American Sociological Review*, 9 (1944), 205; John H. Mueller in *American Journal of Sociology*, 45 (1939), 289; W. Rex Crawford in *Annals of the American Academy of Political and Social Science*, 205 (1939), 204–5.

¹³ Jennifer Platt, *A History of Sociological Research Methods in America, 1920–1960* (New York: Cambridge University Press, 1996), 69. For a stimulating critical analysis of the ways in which US scholars came to identify these European theorists and their theories as 'classical', see R. W. Connell, 'Why is Classical Theory Classical?' *American Journal of Sociology*, 102 (1997), 1511–57.

¹⁴ Alpert (note 10), 67–8. See also Alpert, 'Explaining the Social Socially', *Social Forces*, 17 (1938–1939), 361–5.

disdain, and everyone seemed agreed in regarding it as an essentially philosophical discipline'. In the midst of such controversy, sociology was introduced in France not in Paris but in one of the provincial universities, as a 'branch of philosophy'. And its first teacher (Durkheim) was trained as a philosopher.¹⁵

In later writings Alpert discussed additional difficulties involved in the effort to establish sociology as a legitimate scholarly pursuit. He explained that when Durkheim was a student, a philosophy professor at the Sorbonne had gone so far as to warn him that studying sociology led to insanity. That professor probably had in mind another great French scholar, Auguste Comte, who is often called the 'father' of sociology and best remembered as the founder of 'positivism'.¹⁶ Comte had recommended the institutionalised religious worship of science, which, according to a common view among Comte's commentators, reflected the unfortunate development of a mental imbalance in the author. Alpert explained that when he himself first entered college, he encountered another dismissive viewpoint, for he was 'seriously advised' not to choose sociology as his major because of its poor reputation. At most colleges sociology was still 'equated with Christian Ethics', while many large universities including Princeton and University of California-Berkeley did not even have sociology departments at that time.¹⁷

Alpert's focus on sociology's academic standing is not surprising, since US sociologists of his generation were themselves struggling to establish the legitimacy of their discipline and debating vigorously among themselves what the effort to study society scientifically entailed. Indeed, Alpert's mentors at Columbia University were duelling protagonists in these debates, making Alpert acutely aware of the deep professional tensions the question aroused. The sociologists Robert MacIver, George Lundberg, and Robert Lynd were all on Alpert's dissertation committee, and at least MacIver and Lundberg read and commented on the whole manuscript. The three occupied distinct positions in the lively methodological debates that marked 1930s sociology. Lundberg was a pugilistic advocate of quantitative empiricism and a value-neutral investigative stance. He was also the future author of the scientising polemic *Can Science Save Us?* (1947).¹⁸ MacIver, the department's chairman and theorist, was a vocal critic of Lundberg's style of positivism and argued for a more ecumenical science of sociology, one open to interpretivist approaches.¹⁹ Lynd, like Lundberg, was an avid promoter of quantitative methods, but famously argued in *Knowledge for What?* (1939) that social science inquiry should address questions of values directly. At a time when the anti-democratic forces of militarism and fascism were on the march in Europe and Asia and the USA itself was still struggling to overcome the ravages of the Great Depression, Lynd urged social scientists to reject a disinterested,

¹⁵ Alpert, 'France's First University Course in Sociology', *American Sociological Review*, 2 (1937), 311–317 (313–14).

¹⁶ Alpert, 'Emile Durkheim: A Perspective and Appreciation', *American Sociological Review*, 24 (1959), 462–5 (462).

¹⁷ Alpert, 'Some Observations on the State of Sociology', *Pacific Sociological Review*, 6 (1963), 45–8 (48). It seems that Durkheim's path provided a model that retained significant appeal in France, as evident in the cases of prominent post-WWII sociologists Pierre Bourdieu and Raymond Boudon, who also did their graduate training in philosophy.

¹⁸ Lundberg, *Can Science Save Us?* (New York: Longmans, Green, 1947). On Lundberg's aggressive scientism, see Platt (note 13), 63–105, 212–223.

¹⁹ MacIver's methodological statements are collected in *Social Causation* (Boston: Ginn, 1942).

value-neutral scientific posture in order to focus directly on serious threats to social stability and democracy.²⁰

Reflecting the Columbia faculty's clashing views, Alpert's own stance in this debate was ambiguous, as he carved out a middle position between Lundberg's arch-positivism and MacIver's more catholic perspective. Durkheim, observed Alpert, 'was absolutely convinced... of the possibility of a science of sociology'.²¹ So was Alpert, but what counted for him as proper scientific methodology reflected MacIver's ecumenism, as he revealed in extensive commentary on Durkheim's methodological statements.

Following Durkheim's viewpoint as put forth in *The Rules of Sociological Method* and other writings, Alpert claimed that 'objective' methodology was one of the keys to establishing the scientific status of sociology and social science more generally. In a discussion favourable to what Durkheim meant when he spoke of sociology as an 'objective science', Alpert noted that the adjective 'objective' referred not to the subject-matter of the science but to the 'science itself', meaning the 'body of rules and principles of procedure which we generally call the scientific method'. More specifically, a science is 'objective when its procedural apparatus includes rules for universal verification and validation'. This ensures that 'its empirical results may be accurately and reliably checked by any physically and mentally competent observer'. This emphasis on the communal character of scientific inquiry, rather than the qualities of the individual undertaking the investigation, was, according to Alpert, what Durkheim had in mind when he called for an objective method for sociology. From this perspective, neither sociology nor any other branch of science could be 'subjective', in the sense that it did not fulfil the standards of procedural objectivity—for that would mean it simply wasn't 'scientific'.²²

Complementing this emphasis on objective methodology, Alpert argued that sociology should be a 'natural science'. Again drawing upon his favourable interpretation of Durkheim, Alpert urged scholars seeking to make sociology scientific to keep the following principles in mind: first, 'social facts' exist in 'nature', have 'empirical properties' and are organised into 'real systems'; second, social facts are 'linked by necessary general relations deriving from their very nature', and thus are 'subject to the same principle of determinism... so fruitfully postulated by all the sciences'. These first two principles we might be tempted to call metaphysical. But Alpert would probably have described them as tentative assumptions that were subject to empirical test and verification, as indicated by the additional points he put forth: third, one could discover these 'invariant relations' or 'laws'; fourth, the sociologist could establish 'relations of causation' and 'interdependence'; and fifth, reiterating the centrality of method in science, sociologists had to use the scientific

²⁰ Lynd, *Knowledge for What? The Place of Social Science in American Culture* (Princeton, NJ: Princeton University Press, 1939). On Lynd and social scientific instrumentalism, see Stephen P. Turner and Jonathan H. Turner, *The Impossible Science: An Institutional Analysis of American Sociology* (Newbury Park, CA: Sage, 1990), 42–5, 57–9. On the dispute between MacIver and Lundberg, and between MacIver and Lynd, see Elzbieta Halas, 'How Robert M. MacIver Was Forgotten: Columbia and American Sociology in a New Light, 1929–1950', *Journal of the History of the Behavioral Sciences*, 37 (2001), 27–43 (35–39). Alpert's middle position is elaborated in 'MacIver's Contributions to Sociological Theory', in *Freedom and Control in Modern Society*, edited by Morroe Berger et al. (New York: Van Nostrand, 1954), 286–312 (290–2).

²¹ Alpert (note 15), 315.

²² Alpert (note 1), 111, 112.

methods of 'accurate observation, logical validation, and systematic generalization'.²³

At this point, one might suspect that Alpert was a thoroughgoing, even naïve, scientific enthusiast who had leaped headfirst onto the scientific bandwagon, determined to remake the social sciences in the image of the natural sciences; however, Alpert's views, as hinted above, were more complex. The year before his biography of Durkheim appeared, Alpert published a muted but unmistakable rebuke to one of his Columbia mentors, the quantitative evangelist George Lundberg.²⁴ In a pair of 1936 papers, Lundberg had appealed to physicist Percy Bridgman's argument for 'operationalism' in the physical sciences—the view that concepts are defined by the measuring operations used, rather than their putatively essential properties. Lundberg's version of 'operationalism' was, however, notably partial, deployed as a kind of demarcation device to mark qualitative sociology as 'non-scientific'. For Lundberg, an operational concept was one that could be measured in quantitative terms; anything else was simply not science.²⁵

Alpert, in his 1938 article, complained with explicit reference to Lundberg that operationalism had become a 'metaphysic of its own'—one that must be 'rejected if it assumes an exclusive position and claims that no advances in science are possible without it'. Seeking a more promising alternative, Alpert proposed that the principle of operationalism ought to be a habit of mind, 'not a substitute for thinking'. In addition, Alpert proposed that operationalism should be married to the 'philosophy of probability', which he said is rooted in skepticism and due recognition of the 'tentativeness of our knowledge'. Lundberg's promotion of operationalism was muscular, in the service of scientising boundary work. Alpert, his erstwhile student, recommended humility and the pragmatic use of the operational principle alongside other approaches.²⁶

²³ Ibid., 80.

²⁴ The existing historiography, which treats Alpert only fleetingly, erroneously groups Alpert's qualified embrace of operationalism with Lundberg's aggressive advocacy, perhaps because Alpert's later NSF promotion of scientism made this kind of retroactive characterization plausible. See for example Roscoe C. Hinkle, *Developments in American Sociological Theory, 1915–1950* (Albany: State University of New York Press, 1994), 44.

²⁵ Lundberg: '... I define the concept [of generalization] in terms of the operations by which I arrive at it, in conformity with the accepted requirements of science. Is this or is it not what we very scientists today mean by generalization? If you accept this definition, the question as to whether all scientific generalization is necessarily quantitative at once disappears, for quantification is implicit in this definition'. 'Quantitative Methods in Social Psychology', *American Sociological Review*, 1 (1936), 38–60 (44–5). See also Lundberg, 'The Thoughtways of Contemporary Sociology', *American Sociological Review*, 1 (1936), 703–23. Bridgman outlined his principle of operationalism in *The Logic of Modern Physics* (New York: Macmillan, 1927). On Bridgman's discomfort with the uptake of his thought in sociology and, especially, behaviorist psychology, see Maila L. Walter, *Science and Cultural Crisis: An Intellectual Biography of Percy Williams Bridgman (1882–1961)* (Palo Alto, CA: Stanford University Press, 1990), 162–88.

²⁶ '... science has endeavored in recent decades to become more humble; humility and modesty are making headway. The new attitude was well expressed by Justice Holmes when he remarked that "certitude is not the test of certainty. We have been cocksure of many things that were not so"', in Alpert, 'Operational Definitions in Sociology', *American Sociological Review*, 3 (1938), 855–861 (856–8). In a 1942 letter to the editor, in response to another Lundberg essay promoting a narrow, quantitative reading of operationalism—Lundberg, 'Operational Definitions in the Social Sciences', *American Journal of Sociology*, 47 (1942), 727–45—Alpert staked his differences with his onetime mentor in starker terms: 'If we adopt what seems to be Professor Lundberg's position, namely, that clear and precise terms are to be valued per se, we place ourselves in a realm of scientific anarchy. Choice becomes a matter of taste, with every operational anarchist sponsoring his own preferences. Clarity cannot be the sole standard of scientific conceptualization . . . Are not organizing ability and utility, and even meaningfulness, as important in conceptualization as clarity?' Alpert, Letter to the editor, *American Journal of Sociology*, 47 (1942), 981.

Just as significant for appreciating his complex position on the nature of social science is Alpert's support for historical and philosophical approaches. He presented his biography of Durkheim as a 'contribution to the history of social theory'. Such a study seemed important, in Alpert's view, because in order to 'understand a doctrine [of social theory], one must at least know its history', including the 'personality of its author, his life, experience, and influences'. Taking stock of the past was thus not merely an entertaining pastime but was for Alpert a crucial basis for scientific progress, in the social and natural sciences. Invoking a phrase that he would repeat many times throughout his career, Alpert observed that 'as an army marches on its belly, so a science progresses on its mistakes'. Alpert understood that the tendency within sociology and other social sciences to emphasise new discoveries as the basis of progress too often resulted in impressive-sounding but ultimately groundless claims. Thus he urged his fellow social scientists that often they would be better off not by seeking 'the new, the novel and the startling' but by reexamining and rethinking 'the old'.²⁷

As for the value of philosophy, Alpert proposed that the search for facts and the accumulation of facts were crucial for scientific progress, yet they were far from sufficient. Beyond seeking 'the fact-stuff that is its life-blood', sociologists also and 'without trepidation' had to 'soar into the higher regions of rational thought whence alone come proper synthesis and integration'.²⁸

Yet additional complexity emerges in Alpert's conception of sociology as the study of subjectivity, values, and ends. Not only did this type of study have no close counterpart in the natural science, but also, Alpert asserted, required special methods of study. In discussing Durkheim, Alpert wrote that the Frenchman's effort to develop the science of sociology grappled with a 'dilemma', namely, 'that social phenomena are both natural, hence determined, and axiological, hence involve consideration of human ends and values'. Thus Durkheim evidently recognised the need for sociologists to study the origins of social institutions in order to discover 'to what useful ends they correspond'. While agreeing with these points, Alpert charged that Durkheim had not recognised the need for interpretive investigation, or *Verstehen* analysis, as identified by another great late-nineteenth and early twentieth-century social scientist, Max Weber. It was Weber who taught Alpert's generation that, in addition to studying 'causes and functions' in social life, social scientists needed to pursue the sort of knowledge 'summed up in the concept of *Verstehen*'. This type of study, peculiar to the social sciences, meant investigating, as Alpert put it, the 'social situation or process from the viewpoint of the motivations of the human agents involved', analysing their actions in relation to 'their motives', and establishing 'what ends the agents pursue and how they relate means to ends in their social activities'.²⁹

Last but not least, Alpert emphasised in an approving manner that Durkheim's sociology as a scientific pursuit had tremendous social relevance. While sociology might be institutionalised in the university, Alpert noted that for Durkheim, its *raison d'être* rested upon its potential to guide practical social reforms—not unlike Lynd's desperate call for socially engaged scholars in *Knowledge for What?* In his review of

²⁷ Alpert (note 1), 13–14.

²⁸ *Ibid.*, 13.

²⁹ *Ibid.*, 108–9. MacIver also drew on Weber's *Verstehen* sociology.

Alpert's biography, Mueller asserted, erroneously, that Alpert had omitted an analysis of Durkheim's 'reformistic [sic] political implications'. Alpert explained that Durkheim 'always endeavored to use the beacon of sociological knowledge to cast light upon practical problems'. The tasks of solidifying the foundations of the Third Republic were especially compelling.³⁰ Two years earlier, Alpert had already explained at some length that the development of French sociology in the late nineteenth century was 'integrally bound up with the advent and the problems of the Third Republic', including 'the collapse of the Empire, the ignominious defeat in the Franco-Prussian war, [and] the Commune'. Such developments, Alpert summarised, 'left France in a state of social and moral confusion out of which order had to be established'. For this daunting task of reconstruction, the nascent scientific study of social order held tremendous promise, at least from the point of view of Durkheim and 'many Republicans' who believed a stable social order had to be 'founded in the nature of things'. In order to discover 'what that nature is', they turned to what they thought was the 'only valid guide', namely, objective scientific knowledge. Sociology for these Frenchmen thus became the key to 'social reconstruction and moral regeneration'.³¹

In sum, Alpert was sometimes critical of Durkheim, for example, for being misleading in his discussion of social realism, and for not appreciating the importance of interpretive knowledge. But by and large Alpert's analysis presented a favourable reinterpretation that drew attention to the 'advantages of Durkheim's broad view of sociology as a positive science of social behaviour, as a rejuvenating method of social investigation, and as a solid foundation for an integral social philosophy'.³² During WWII and the early post-war years, Alpert's career moved in new directions that drew his attention to major scholarly developments and dramatic changes between the federal government and US social science. The controversial relations between social scientists and Washington politics as well as their problematic position in the rapidly changing federal science policy system provided Alpert with new contexts for thinking about basic questions concerning the social sciences that he had first taken up vigorously in his studies of Durkheim. As we will see, Alpert downplayed some of these views in his successful effort to establish a foothold for the social sciences at the NSF in the 1950s.

3. A man of government and public opinion expert

After Alpert received his Ph.D. in 1938, he worked as an assistant professor of sociology at the City College of New York and then, during the 1940s, was among the great number of social scientists who became involved in the war effort. During the early postwar and Cold War years, his wartime work became the basis for further connections to the federal government, again a common phenomenon in the careers of social scientists from his generation. Alpert worked in the Office of War Information from 1943 to 1944 as a public opinion analyst, studying the social and political structures of enemy and occupied countries; in the Office of Price Administration from 1944 to 1945 as a public opinion and rationing statistics

³⁰ Mueller (note 12), 289; Alpert (note 1), 29–31, 57–61.

³¹ Alpert (note 15), 311–12.

³² Alpert (note 1), 3.

analyst; in the Bureau of the Budget from 1945 to 1948 and 1950 to 1953 as a researcher and coordinator of research statistics; and as a consultant on manpower problems for the U.S. Air Force's Research and Development Board from 1948 to 1950. The government work helped to make statistics and public opinion research central interests for Alpert, and to establish him as an important figure in these areas. While he did not do original research in either field, Alpert acquired administrative experience that allowed him to cultivate a broad view of their intellectual and practical challenges. For this, Alpert obtained a measure of recognition from his academic peers, as indicated by his election as president of the Washington Statistical Society (1954–1955), and, more importantly, his election as president of the major national scholarly society for public opinion specialists, the American Association of Public Opinion Research (1955–1956). To help establish the wider context for Alpert's postwar work and concerns, we briefly discuss federal social science patronage and the emergence of public opinion research.

The period from WWII through the early 1950s proved to be an especially dynamic and consequential one, as histories of US social science, of the individual social science disciplines, and of social scientists' entanglement in various public policy issues have shown. Among the most significant developments was the emergence of the federal government as a major patron of US social science, marked by five significant features: the development of a variety of new public funding sources for the natural and social sciences, including military, intelligence, and propaganda agencies that had deep roots in WWII developments and became central to US national security interests during the Cold War; significant new opportunities for the social sciences in civilian science agencies as well, like the National Institute of Mental Health (f. 1946) and the National Science Foundation (f. 1950); persistent questions about their utility and scientific character, often tied to a partisan political context; the dominance of the natural and especially physical sciences in a greatly expanded federal science establishment, which placed additional pressures on social scientists to prove themselves before their natural science colleagues; and institutional instability in postwar federal social science programmes, making it clear just how difficult was the multi-levelled challenge of establishing their scientific credentials, practical utility, and national value.³³

The case of the national science debate, which led to the creation of the National Science Foundation, is revealing and especially relevant in the context of Alpert's work at the new agency. This debate began shortly before the end of WWII, became most intense following the publication of *Science—The Endless Frontier* (Vannevar Bush's landmark 1945 science policy report) and the emergence of a number of competing legislative proposals to create a new science agency, and then continued throughout the rest of the decade, until a successful bill was signed by the President in 1950. Among the controversial issues in that debate was whether the social sciences belonged in an agency devoted mainly to the advancement of basic scientific research in the natural sciences. An effort led by the Social Science Research Council (SSRC) made the case that the social sciences should be included, based primarily on the argument that they were part of a unified scientific enterprise and had much to

³³ Two rich bibliographic overviews of the historiography of postwar social science are Isaac (note 5); and Hunter Crowther-Heyck, 'Patrons of the Revolution: Ideals and Institutions in Postwar Behavioral Science', *Isis*, 97 (2006), 420–46. Also see the excellent volume edited by Roger E. Backhouse and Philippe Fontaine, *The History of the Social Sciences since 1945* (New York: Cambridge University Press, 2010).

contribute to national affairs in the new, anxious atomic age. On the other side, prominent voices in the natural science and political communities expressed serious doubts about the scientific status and political implications of the social sciences. These doubts were strongest among conservative politicians and scientists who portrayed the social sciences as ideological and political in nature rather than strictly scientific. Additionally, while liberal politicians and scientists tended to view the social sciences more favourably, their support faded during the protracted and often partisan struggle to create the National Science Foundation.³⁴

Consequently, when the SSRC-led effort, including the testimony by a number of leading social scientists from the various disciplines, proved to be unsuccessful, the social sciences found themselves on the sidelines. The NSF's 1950 charter did not refer directly to the social sciences, but a passage referring to the 'other sciences' meant that the decision about whether to support them would be left open; thus, the charter was permissive but not mandatory regarding social science funding. If social scientists were to obtain funding from the new agency, surely there would be many obstacles to overcome.³⁵

Also of special relevance to Alpert's story is the growth of public opinion research during these years, as it became one of the most exciting areas of social science inquiry (especially for sociology, political science, and communications research). Scholarship in this area, which had roots in 1920s psychological studies of attitudes and in interwar market research, coalesced around new sampling procedures legitimised by pollsters in the 1936 presidential contest. The pollsters' success attracted the attention of entrepreneurial social scientists in sociology, psychology, and political science who recognised and exploited the polls' and polling organisations' potential. The field came to be organised around the refinement of sampling methods and interview techniques, while certain topical areas received special scholarly attention, notably the study of media audiences and effects. These topics were, not coincidentally, also of great interest to media firms, commercial pollsters, and the Rockefeller Foundation, which provided researchers with generous funding.³⁶

The Rockefeller Foundation's involvement evolved in the context of growing tensions in Europe, culminating in its sponsorship of a 'Communications Seminar' in 1939 and 1940, and proceeded at a time when explicit government efforts in this field were not politically feasible. From early 1940 until the US entry into the war, the Foundation served, in essence, as an unofficial arm of the state, while the Roosevelt administration was hampered by a public culture still wary of propaganda and a somewhat isolationist Congress. But after the US entered the war, the Rockefeller

³⁴ Solovey, 'Riding Natural Scientists' Coattails Onto the Endless Frontier: The SSRC and the Quest for Scientific Legitimacy', *Journal of the History of the Behavioral Sciences*, 40 (2004), 393–422; Larsen (note 2), 1–18; *The Nationalization of the Social Sciences*, edited by Samuel Z. Klausner and Victor M. Lidz (Philadelphia: University of Pennsylvania Press, 1986).

³⁵ Solovey (note 34).

³⁶ Brett Gary, 'Communication Research, the Rockefeller Foundation, and Mobilization for the War on Words', *Journal of Communication*, 46 (1996), 124–47. For the history of public opinion research as an interdisciplinary social science field before, during, and after the war, see Jean M. Converse, *Survey Research in the United States: Roots and Emergence 1890–1960* (Berkeley: University of California Press, 1987); Sara Igo, *The Averaged American: Surveys, Citizens, and the Making of a Mass Public* (Cambridge, MA: Harvard University Press, 2007); Jefferson Pooley, 'An Accident of Memory: Edward Shils, Paul Lazarsfeld and the History of American Mass Communication Research' (unpublished doctoral thesis, Columbia University, 2006), 179–299.

projects were incorporated into rapidly expanding wartime bureaucracies. By the summer of 1942, most of the major government propaganda-related initiatives were in place, and staffed to a large degree by the main figures in interwar public opinion research. The government effort spanned dozens of agencies and employed thousands of social scientists.³⁷

Despite the embarrassing and controversial failure of public opinion polling techniques to accurately predict the winner in the 1948 presidential election, opinion research became an enormously important field in mid-century America. The area became the single largest recipient of government support for social science during the 1950s. Public opinion researchers, especially sociologists and political scientists, emerged from the war flush with substantive and methodological gains, exemplified by the four-volume *American Soldier* (1949), which was based on extensive wartime research in the Army's Research Branch and supported by the Carnegie Corporation.³⁸ With extensive funding from government agencies as well as support from private foundations and corporations, research shops like Paul Lazarsfeld's Bureau of Applied Social Research at Columbia University established themselves, improbably, at the centre of postwar sociology.³⁹

Alpert had already declared his support for statistical work and probabilistic reasoning before the war, but it was only in the 1940s and 1950s that he elaborated on the great practical benefits of social science research in this vein, especially public opinion polling, and especially for modern societies like the US. Following in a sociological tradition of theorising the transition to modernity to which Durkheim contributed, Alpert began by asserting that at the level of social interactions, there was a marked difference between the simpler rural communities that predominated in the past and the more complex urban societies of recent times. Whereas in 'small, simply structured communities' people can get along fine with 'individual, personal types of knowledge', in complex urban societies people turned to 'impersonal methods of fact-finding and fact-dissemination'. Urban societies cannot depend on the 'everybody-knows', 'it's-obvious-to-all' or 'I-can-tell-you-from-personal-experience' sort of knowledge. From this functionalist perspective, Alpert claimed that 'elaborate systems of social bookkeeping' and 'statistical systems' were 'necessary institutional adjustments to the conditions of large-scale, urban, mass organisation'.⁴⁰

³⁷ The best overviews of opinion researchers' government propaganda service are Converse (note 36), 162–228; Christopher Simpson, *Science of Coercion: Communication Research and Psychological Warfare, 1945–1960* (New York: Oxford University Press, 1994), 15–31; and Timothy Glander, *Origins of Mass Communications Research during the American Cold War: Educational Effects and Contemporary Implications* (Mahwah, NJ: Lawrence Erlbaum, 2000), 41–60. There were, in the immediate aftermath of the war, a few general accounts. See, for example, John McDiarmid, 'The Mobilization of Social Scientists', in *Civil Service in Wartime*, ed. Leonard D. White (Chicago, IL: University of Chicago Press, 1945); and Daniel Lerner, *Sykewar: Psychological Warfare against Germany, D-Day to VE-Day* (New York: G. W. Stewart, 1949).

³⁸ *The American Soldier: Adjustment During Army Life*, edited by Samuel Stouffer and Edward A. Suchman (Princeton, NJ: Princeton University Press, 1949). See Daniel Lerner, 'The American Soldier and the Public', in *Continuities in Social Research: Studies in the Scope and Method of The American Soldier*, edited by Robert K. Merton and Paul F. Lazarsfeld (Glencoe, IL: Free Press, 1950), 212–51, for a study of the volumes' reception. On the 1948 polling fiasco, see Frederick Mosteller et al., *The Pre-Election Polls of 1948* (New York: Social Science Research Council, Bulletin 60, 1949); also Igo (note 36), 152–6.

³⁹ From 1946–48, Alpert served as a research consultant to the Bureau.

⁴⁰ Alpert, 'The Federal Statistical System', *American Journal of Sociology*, 56 (1951), 468–75 (468).

It is precisely because of his belief in the value of statistical work, perhaps, that Alpert was not shy in expressing his criticisms of certain tendencies in the public opinion field. In a mid-century symposium on the 'Processes of Public Opinion Formation', he highlighted some fundamental challenges facing scholars, starting with the very basic problem of defining and measuring public opinion, a problem that echoed his earlier concerns about operationalism and reductionism in social explanation. Other scholars in this symposium had already asserted that public opinion should be understood to be a 'group product', that it 'deals with issues... relevant to group interests', and that since public opinion is 'part of society's problem-solving process', it should be seen in relation to a 'specific social situation'. Viewed in this light, Alpert found much of the research on 'public opinion' to be 'irrelevant, immaterial, incompetent, and impertinent'. What was the reason for this unhappy state of affairs? Alpert answered that it was because researchers typically recorded 'individual, private, verbal responses, privately expressed to a stranger in an artificially contrived social situation'. Having identified the underlying problem, Alpert argued that scholars had a choice to make: either they could 'assert mastery' over their studies by adapting their techniques to the problems at hand, or they could remain 'methodological slaves, permitting a presumed technical proficiency' to determine their 'interests and concerns'.⁴¹

This last remark by Alpert reflected his growing worry that the dramatic expansion in the use of quantitative methods in survey research and social science more generally was not accompanied by sufficient scholarly awareness of the limitations of quantitative data. An oft-repeated statement by the famous nineteenth-century British scientist Lord Kelvin went, in Alpert's rendition, something like this: 'When you can measure what you are speaking about and express it in numbers you know something about it, but when you cannot measure it, when you cannot express it in numbers, your knowledge is of a meagre and unsatisfactory kind'. In Alpert's view, Kelvin—much like George Lundberg—had 'unquestionably overstated his case', for his statement ignored the great 'progress made through qualitative studies'. Kelvin's statement also ignored the point that 'numbers are sterile unless they are brought into relation with one another in terms of some meaningful system'.⁴²

Turning directly to government-sponsored social research, Alpert identified additional limitations. To begin with, the statistical survey data collected by some agencies had limited value. Scholars working in this area had questioned the Census Bureau's decision in 1946 to stop its annual collection of state judicial criminal statistics. While he acknowledged that this annual collection was potentially valuable, Alpert also pointed out that the reasons for the Bureau's decision included the recognition that, when viewed as a statistical product, the series had 'serious limitations'.⁴³

⁴¹ Alpert, 'A Comment', *Public Opinion Quarterly*, 14 (1950–1951), 685–6. Also see Alpert, 'Some Observations on the Sociology of Sampling', *Social Forces*, 31 (1952–1953), 30–3.

⁴² Alpert (note 40), 475. On Kelvin's statement, see Robert K. Merton, David L. Sills, and Stephen M. Stigler, 'The Kelvin Dictum and Social Science: An Excursion into the History of an Idea', *Journal of the History of the Behavioral Sciences*, 20 (1984), 319–31.

⁴³ Alpert, 'National Series on State Judicial Criminal Statistics Discontinued', *Journal of Criminal Law and Criminology*, 39 (1948), 181–8 (182).

Another significant problem that concerned Alpert was the uneven political support and tentative public acceptance of public opinion statistics. On the positive side, by that point many national legislators recognised that public opinion had a 'significant role' in the 'development, formulation, and resolution of public issues'. They were beginning to see that the public opinion survey could be a 'fruitful instrument for the measurement and understanding of public attitudes, opinions, motivations, information, and expectations'. Yet Alpert warned that too often the conduct of such polls did not adhere to the 'best accepted standards of good survey practice'. The harmful consequences were many, including 'inaccurate and misleading poll results', 'unfavorable attitudes toward polls on the part of the general public', 'unfavorable attitudes by Congressmen toward requests from Executive agencies for support of opinion and attitude surveys', and the 'misuse of polls as a guide to public policy because of unwarranted reliance on responses to questionnaire items'.⁴⁴ Alpert, unlike many of his colleagues, was anything but an uncritical booster of the new survey methods.

Alpert's engagement with statistical work during this period reveals a similar pattern: a strong commitment to the improvement of quantitative measurement together with a skeptical admission that the ultimate value of these methods was limited—an interesting combination of views consistent with Alpert's earlier writings from the 1930s. As represented by Kelvin and Lundberg, scientific enthusiasm for quantitative precision has often been associated with a certain arrogance—my (or our) quantitative knowledge, obtained by using rigorous techniques of scientific measurement and precise analysis, is superior to your qualitative understanding, which, however it was obtained, is by comparison imprecise and fuzzy. At the same time, quantification has often been a strategy taken up by groups, including scientists, with weak status, as historian of science Ted Porter has shown.⁴⁵ In the case of social scientists trying to establish the authoritative nature of their work to skeptical natural scientists, doubting politicians, and potential patrons not wanting to squander their money, it's easy to see how a commitment to statistical methods in the new studies of public polling and opinion research would have been attractive, as it was to Alpert. This isn't to say scholars like Alpert weren't enthusiastic about such methods for other reasons as well, for example, because statistical analysis could reveal patterns in large data sets that otherwise remain hidden, or because it could indeed add valuable quantitative precision to vague claims about public opinion and consumer preferences. Although Alpert certainly saw the scholarly benefits of statistical work, he was also sensitive to two dangers that informed his strikingly humble stance: first, social scientists might oversell the value of their work, resulting in a backlash led by more powerful interests that social scientists were cultivating as allies, thus undermining their efforts to strengthen trust in their work; second, excessive enthusiasm for quantitative precision would prevent researchers from recognising the limits of their analysis and, presumably, inhibit them from pursuing promising lines of investigation that employed qualitative types of analysis.

⁴⁴ Harry Alpert, 'A Critical Introduction to "Congressional Use of Polls: A Symposium"', *Public Opinion Quarterly*, 18 (1954), 121–3 (121–3).

⁴⁵ Theodore Porter, *Trust in Numbers: The Pursuit of Objectivity in Science and Public Life* (Princeton, NJ: Princeton University Press, 1995). Also relevant: Ian Hacking, *The Taming of Chance* (New York: Cambridge University Press, 1990); and Gerd Gigerenzer, et al., *The Empire of Chance: How Probability Changed Science and Everyday Life* (New York: Cambridge University Press, 1990).

Before he arrived at the National Science Foundation, then, Alpert's experiences with public opinion research, government social science programmes, and the wider political, institutional, and intellectual contexts that shaped the advance of the social science enterprise were considerable. The concerns he expressed about the problems of political support, public acceptance, and sound scientific methodology were not entirely new concerns. They were rooted in his scholarly studies of Durkheim. Nevertheless, during WWII and the early post-WWII years, major changes in what another sociologist, Gene Lyons, later called the 'uneasy partnership' between social science and the federal government transformed the context for his thinking about these matters.⁴⁶ At the NSF, he would confront a difficult environment that tested his ability to establish a viable policy framework for social science support. His own complex views on social science were also put to the test.

4. A social science policy architect

After WWII, and in addition to his governmental work, Alpert continued his overlapping career as a university scholar. He remained at the City College of New York until 1947, served as a research consultant to Columbia University's Bureau of Applied Social Research from 1946 to 1948, as a lecturer and adjunct professor of sociology at American University in 1947 and again from 1950 to 1953, as an associate professor of sociology at Yale University in summer 1947, as an associate professor of sociology and the chairman of the anthropology and sociology department at Queens College from 1948 to 1950, as a researcher in social psychiatry at Cornell University Medical College from 1951 to 1956, and as a professor of sociology at the University of Washington in 1955. After returning to the federal Budget Bureau for three years until 1953, Alpert took up the job at the NSF for which he is best remembered.

Historical studies have emphasised that a number of pressures shaped Alpert's NSF policy work for the social sciences, starting with partisan controversy about their scientific status and political valence and which, as we saw, was already present in the agency's legislative origins. After the NSF was up and running in the early 1950s, the agency's General Counsel determined that the agency did indeed have the 'authority' to provide the social sciences with research grants and fellowship awards but added that, as the agency's legislative history revealed, 'Congress intended the Foundation to exercise a fair amount of restraint in the use of this authority'. The natural-science orientation of the new agency presented additional obstacles. The agency's leaders included a number of prominent natural scientists and science administrators but no active social scientists. These leaders, including the physicist and first NSF Director Alan Waterman, concentrated on providing support for basic research and fellowships in the natural sciences. Moreover, some of them were downright skeptical about the social sciences.⁴⁷

⁴⁶ Gene M. Lyons, *The Uneasy Partnership: Social Science and the Federal Government in the Twentieth Century* (New York: Russell Sage Foundation, 1969).

⁴⁷ For the General Counsel's Memorandum of March 17, 1953, see Harry Alpert with the assistance of Bertha W. Rubinstein, 'Role of the Foundation with Respect to Social Science Research National Science Foundation', April 15, 1954, Appendix A, 36-40, Folder: The Role of the Foundation with Respect to Social Science Research, Alpert-Rubinstein, 1954, NSF Historian's File, National Science Foundation headquarters, Arlington, Virginia. The works cited earlier by England (note 2), by Larsen (note 2) and by Kleinman and Solovey (note 2) all provide brief overviews of the challenges Alpert faced in developing a framework for NSF's social science funding during the 1950s.

Making matters even more difficult, during the early to mid-1950s, conservative political criticism of the social sciences remained pervasive and influential. A series of McCarthyite attacks on the social sciences and their patrons, especially the big private foundations (i.e. Rockefeller Foundation, Carnegie Corporation, and Ford Foundation), made NSF leaders fear that providing support for the social sciences would lead the agency into hot water. Lastly, the fact that the new agency was rather small at first, with little funding even for its natural science programmes, made its leaders especially wary of getting involved in the controversial social sciences; such involvement might provoke damaging attacks. It was thus under difficult conditions that Alpert set out to craft a strategy for supporting the social sciences—a strategy that would, slowly but surely, succeed in winning them an important measure of scientific legitimacy and public funding.⁴⁸

But what exactly did Alpert himself contribute to the development of that strategy? It is clear that he did not have much room to manoeuvre. Certainly there was no opportunity to make the social sciences a major part of the agency's overall programme. He needed to reassure the agency's nervous leaders that supporting the social sciences was worthwhile. This meant he would have to address problematic questions about their scientific status and political valence. Having explored Alpert's earlier writings and career, we can now see that in his NSF policy work Alpert found an opportunity to address long-standing scholarly interests and practical concerns; for him, the NSF provided a new arena to work on old problems. While he was certainly constrained by various pressures beyond his control, his policy work would also reflect his own initiative, as he drew upon certain ideas of his from the past and incorporated them into palatable policy recommendations.

In addition, we'll see that Alpert regularly incorporated statements from prominent figures in the social and natural sciences at the time, thereby suggesting that his strategy was broadly consistent with common wisdom. In important ways, his selections were indeed consistent with other important efforts in mid-century America by social scientists and their patrons (including the Ford Foundation and the Department of Defense) to promote their work in ways that would win them support and influence while undermining the charges of their many critics. At the same time Alpert glossed over or ignored certain points about the differences between the social and natural sciences in the areas of ontology, methodology, and social relevance that he had previously identified. In doing so, Alpert had to be selective in the way he made references to other scholars, citing only those remarks that served his purpose.

Alpert's strategy for winning support for the social sciences emphasised, above all, the place of the social sciences within a unified scientific enterprise, a theme

⁴⁸ Again, see the works by England, by Larsen, and by Kleinman and Solovey (note 2). It should be mentioned that during these early years of the agency, a few individuals on its twenty-four-member governing board had a significant interest in the social sciences. This group includes Charles Dollard, Chester Barnard, Sophie Aberle, and Frederick Middlebush. Some of them—including Barnard whose views are discussed briefly below—contributed to discussions about the agency's involvement in the social sciences. But there's no evidence indicating that any of them assumed more significant roles in promoting the social science activities of the young NSF. They were all appointed because of their expertise in other areas, including the political scientist Middlebush, who had written a popular political science textbook and was the only board member with a social science doctorate. After moving into university administration many years before his NSF appointment, however, he had long since been active as a scholar; his appointment was based upon his high national profile and expertise in university administration.

central to Alpert's thinking ever since his graduate studies on Durkheim. It was also a theme that interested a few members of the National Science Board (the NSF's 24-member governing council), some of whom had a sincere interest in the social sciences. Of special importance was a discussion with the NSB chairman Chester Barnard. Known among scholars for his writings about the nature of the modern business executive, Barnard was former chairman of the board of directors at the Rockefeller Foundation, and in that capacity had been involved in an extended controversy about the nature of the social sciences and their post-WWII funding. Alpert reported, in one of his earliest policy papers to Director Waterman, that Barnard emphasised the 'importance of drawing a sharp distinction between the application of scientific methods in the social sciences, and the essentially political, ethical and welfare activities which frequently pass for the social sciences'. To help make this distinction, Barnard suggested that social research could be viewed in terms of a continuum, with 'softer' studies falling at one end and 'harder' studies at the other. Barnard then recommended that the NSF make a 'sharp delimitation... of the "hard science core" of the social sciences' and 'very carefully limit itself to this hard core'. Alpert indicated to Waterman that he agreed with Barnard. But what, more specifically, did Alpert mean by the 'hard core'?⁴⁹

In internal NSF documents and in published writings for the social science community at large, Alpert identified the hard core according to the type of scientific methodology used. The NSF, he told Waterman, should only support 'basic research which conforms to the highest standards of scientific inquiry and fulfils the basic conditions of objectivity, verifiability, and generality'. Significantly, this trio—objectivity, verifiability, and generality—had appeared in Alpert's earlier writings. This trio also acquired great staying power at the NSF, as the words appeared time and time again in agency documents, like a mantra whose brevity and precision captured the very essence of the agency's preferred style of social research.⁵⁰ Elsewhere, Alpert elaborated on the fundamental importance of methodology. In answer to the question 'What are the Social Sciences?' Alpert said that the term referred to the 'application of the methods of science to the phenomena of human social behavior', then explained that 'the phrase "methods of science" refers to that common core of logical organization, experimental procedures, mathematical and quantitative techniques, and conceptual cumulations which constitute the unity of science and insure the objectivity of its findings'.⁵¹

As he did with Barnard, Alpert made this case about common methodology as strong as possible, by citing corroborating statements by well-known figures from the scientific community. In his earliest published article on NSF social science

⁴⁹ Barnard as reported in Memo from Alpert to Waterman, May 22, 1953, in Office of the Director, Alan T. Waterman, Subject Files 1951–56, Box 20, Folder [on social science programme], Record Group 307, National Archives (Washington, DC). While Alpert drew heavily upon Barnard, Alpert, as noted by Henry W. Riecken (Alpert's successor at the agency), was 'strongly advised by the friendlier members of the National Science Board, as well as by allies in the rest of the scientific community, to adopt a strategy of stressing the 'hard science' aspects of the social disciplines.' Riecken, 'Underdogging: The Early Career of the Social Sciences in the NSF', in *The Nationalization of the Social Sciences*, edited by Klausner and Lidz, 209–225 (215).

⁵⁰ Alpert to Waterman, Nov. 1, 1953, Position Paper No. 2, 'basic research' at 2, Folder: [Reports on Social Science Program], NSF Historian's Files (NSF HF).

⁵¹ Alpert, with the assistance of Bertha W. Rubinstein, 'The Role of the Foundation with Respect to Social Science Research', April 15, 1954, 59, Folder: Role of the National Science Foundation with respect to Social Research (Alpert-Rubinstein, 1954), NSF HF.

policies, Alpert said he was following the viewpoint of Elbridge Sibley, the author of a major study on the recruitment and training of social scientists that had been sponsored by the Social Science Research Council and the Carnegie Corporation. In particular, he cited Sibley's assertion that social scientists needed to formulate 'hypotheses' that could be 'tested and verified by experiments or systematic observations' and develop their predictive capacities. In brief, social science had to 'conform to descriptive analyses of science generally'. Alpert also cited the accomplished chemist, former Harvard University president, amateur historian and philosopher of science, and first NSB chairman James Conant—Conant preceded Barnard in this position—when he recommended that the social sciences 'must progress toward firmer theoretical formulation and conceptualization'. And one final example: agreeing with the sociologist, Carnegie Corporation president, and National Science Board member Charles Dollard, Alpert said that social scientists needed to 'become scientists in fact, dealing in fundamental theory which can be tested, and struggling to achieve laws and generalizations which will enable us to predict what men or groups of men will do or not do under stated conditions'.⁵²

Alpert went further, and promoted the social sciences as part of a unified scientific enterprise by proposing that the NSF provide support for 'convergent research'. Alpert suggested to Director Waterman that, at the NSF, 'accent should be placed on the areas of convergence of the natural sciences and the social sciences'. Here, again, Alpert turned to authoritative voices for assistance, in this case to Vannevar Bush, the main author of the landmark 1945 national science policy report *Science—The Endless Frontier*. In his 1946 congressional testimony about the pending NSF bill, Bush said the proposed agency's involvement with the social sciences should be guided by the goal of 'effective integration and partnership' with the natural sciences. Alpert explained that by focusing on convergent research, defined by significant overlap between social and natural science at the levels of methodology and subject matter, the agency would help to achieve the important goal Bush had set forth.⁵³ In the apt characterisation offered by social psychologist Henry Riecken, Alpert's successor at the NSF, Alpert was following the 'strategy of protective coloration, of allying one's cause with stronger others, a strategy that has been used by countless minorities and other underdogs to secure a share of power and position'.⁵⁴

Alpert's policy work was marked by his sensitivity to the problems of political support and public acceptance as well. At the NSF, Alpert focused on establishing modest and stable social science funding, aided by the deliberate avoidance of topics most likely to provoke public controversy. To justify this stance, Alpert invoked the common view at the time that called for a division of labor between public agencies and private foundations, based upon the (allegedly) greater susceptibility of the former to political pressures. Thus he proposed that the large private foundations (i.e. Rockefeller, Carnegie, and Ford) needed to be the 'major source' of 'risk capital' for

⁵² Harry Alpert, 'The National Science Foundation and Social Science Research', *American Sociological Review*, 19 (1954), 208–11 (210).

⁵³ Alpert to Waterman (note 50), 1.

⁵⁴ Henry W. Riecken, 'The National Science Foundation and the Social Sciences', *Social Science Research Council Items*, 37 (1983), 39–42 (40).

the social sciences. These foundations had to ‘bear the major responsibility for supporting the unorthodox, the unusual, and the “big gamble”, as well as areas, like sex and politics, which can easily lead to public controversy’. From this politically savvy standpoint, an agency like the NSF could, justifiably, leave such sensitive topics aside.⁵⁵

On a related point, Alpert called for a clear separation of basic research, which NSF supported, and the applications of social research, which Alpert presented as a rather distinct activity. By advocating the ‘stimulation of basic research’ in the social sciences Alpert was adhering to the broad mandate of the agency, namely, to support basic science but not applied studies. Yet there was more at stake here. Most obviously, by concentrating only on basic studies, Alpert was proposing a way for the agency to avoid engagement with research plainly linked to practical matters and thus potentially controversial—that would be left for the private foundations.⁵⁶ Equally important, Alpert was advancing a claim about the similarity of social and natural science research that was questionable at the time. Many scientists and their supporters in that era subscribed to the notion that one could draw a meaningful distinction between basic and applied research in the natural sciences. The case of seemingly esoteric research in physics paving the way for the development of atomic weapons and atomic energy was often invoked, but it was only one of many such examples. But the question of the relevance of this argument to the social sciences was controversial.

A main contributing factor in this controversy was the worry—or belief—that values could not be kept apart from social inquiry in the way that the notion of basic research in the natural sciences seemed to require. Perhaps particular ethical, social, or political concerns that motivate social research might also shape the course of research and the interpretation of results in ways that are value-laden and suggest a particular course of action.⁵⁷ For example, in the public and scholarly controversy over Alfred Kinsey’s contemporaneously published studies of human sexuality, some critics claimed that Kinsey’s unorthodox views about human sexuality influenced his research in value-laden ways, thereby undermining its objectivity and compromising, in their view, its scientific value. The same type of criticism appeared in (largely conservative) attacks on (mainly liberal) psychologists and social scientists like Gunnar Myrdal who claimed that scientific research refuted the notion that there were significant inborn mental differences among the races of people. Both Kinsey and Myrdal argued that their research findings together with widely accepted democratic ideals implied the need for major reforms, in the areas of legislation and social practises regulating sexual behavior and race relations, respectively. In addition, Myrdal argued that social research informed by specific value commitments, i.e. certain democratic ideals, was perfectly consistent with the demands of scientific objectivity. But, according to their harshest (and typically conservative) critics, Kinsey and Myrdal were immoral, even dangerous

⁵⁵ Alpert to Waterman (note 50), 3.

⁵⁶ Alpert (note 51), 11.

⁵⁷ One could make a similar argument about so-called basic research in the natural sciences, and some have. But this was not a common argument at that time. Nor was it an argument Alpert advanced or addressed.

revolutionaries who were using a perverted form of social inquiry that was more ideological than scientific as a tool for advancing unseemly, anti-American causes.⁵⁸

In the context of this highly charged debate about the role of values and ideology in social science, Alpert turned to the basic-applied distinction as a means of placing further distance between social science and potential controversy at the NSF, while simultaneously reaffirming the unity-of-science theme. He claimed that the social sciences were no different than the natural sciences when it came to the development of basic scientific knowledge and its 'utilization'. Just as research on the atom could be used for the purposes of 'peace or destruction' and research on bacteria for the purposes of 'health or disease', studies of human motivations could be used to 'provide happiness or sell soap'. But such alternatives were not for the scientist as a 'seeker of knowledge and truth' to determine.⁵⁹

Following such reasoning, Alpert adopted an engineering model to explain how basic social research that allegedly had no value-orientation would become useful in the long run. Like the engineer who 'continually relies on' the results of basic studies in the physical sciences and mathematics, 'practitioners of applied social science' depend on the findings from 'basic research on the fundamental working of human behavior'. Thus it would be left to the practitioners to figure out how to use the allegedly neutral findings of social science to achieve the practical ends they desired.⁶⁰

On these issues Alpert offered advice directly to social scientists as well, to help them avoid the sort of unwanted political scrutiny that could bring the entire social science enterprise and their patrons into disrepute. In a manner reminiscent of Barnard's position, Alpert encouraged his fellow scholars to 'eschew identification with social reform movements and welfare activities, and especially, the unfortunate phonetic relationship to socialism'.⁶¹ Though he didn't tell social scientists to eschew Marxism explicitly, this point would have been obvious, and his comments about the deficiencies of Marxism clearly implied so. Whatever Marxism was it was 'not science'. In fact, 'the science of sociology' represented the 'very antithesis of Marxism', asserted Alpert, for Marx knew 'in advance' what the answers were. More generally, Alpert accused 'historical materialism' of being 'a dogmatic, monistic, economic-deterministic philosophy'.⁶²

⁵⁸ There is a large literature on the controversies over the works by Kinsey and Myrdal and their social implications. See, for example, James H. Jones, *Alfred C. Kinsey: A Life* (New York: Norton, 2004); David W. Southern, *Gunnar Myrdal and Black-White Relations: The Use and Abuse of An American Dilemma 1944-1969* (Baton Rouge: Louisiana State University Press, 1987). For a stimulating essay about how the controversy over values and objectivity has shaped historical accounts about the roles of social scientists and especially psychologists in the landmark 1954 Supreme Court desegregation case, see John P. Jackson, Jr., 'The Triumph of the Segregationists? A Historiographic Inquiry into Psychology and the Brown Litigation', *History of Psychology*, 3 (2000), 239-61. It is also relevant that leading quantitative social scientists—Alpert's peers—insisted on the inherent and mutually beneficial connection between applied and basic research. See Paul F. Lazarsfeld, et al., *The Uses of Sociology* (New York: Basic Books, 1967), and the discussion in Crowther-Heyck (note 33), 433-5.

⁵⁹ Appendix B in Memo from Program Director for Social Science Research to Director, July 1, 1958, quotes at 20, Annual Review of Soc. Sci. Program for FY 1958, Folder: [Reports on Social Science Program], NSF Historian's File. A similar viewpoint appears in Alpert, 'Congressmen, Social Scientists, and Attitudes Toward Federal Support of Social Science Research', *American Sociological Review*, 23 (1958), 682-7 (685).

⁶⁰ Alpert, 'The Knowledge We Need Most', *Saturday Review*, 1 February 1958, 36-8 (37).

⁶¹ Alpert (note 52), 210-11.

⁶² Alpert, Review of *Sociology: The Science of Society* by Jay Rumney and Joseph Maier, *Social Forces*, 32 (1953-1954), 195-6 (195).

Alpert's recommendations along all of the above lines were implemented at the NSF. In 1954, the agency's governing board approved a 'modest' programme to support social science research, noted that it would be 'exploratory, experimental, and cautiously developed', and recited Vannevar Bush's words about the need for 'effective integration and partnership' between the social and natural sciences. This outlook informed the establishment of two programmes for 'convergent' research, one in the agency's Division of Biological and Medical Sciences and the other in its Division of Physical Sciences.⁶³ After three years, Alpert reported these exploratory programmes had demonstrated that it was 'possible to identify significant and scientifically meritorious projects in selected social science areas'. Based upon this success, Alpert, following the position expressed by a recently established NSF Advisory Panel for Social Science Research, recommended 'a limited broadening' of the agency's funding criteria, to go beyond convergent terrain.⁶⁴

Upon agreeing to drop its convergent criteria, the agency created new disciplinary-based programmes (for sociology, economics, anthropology, and history and philosophy of science), first under the auspices of a Program (1957), then an Office (1958), and later, after Alpert had left the agency, a Division (created in 1961). With divisional standing, the social sciences, somewhat remarkably, had acquired the same-level of organisational standing as the natural and biological sciences, which had enjoyed divisional status from the agency's beginning. These developments were accompanied by the expansion of financial support.⁶⁵

However, this stronger commitment and expansion took place within carefully defined limits, limits that continued to bear Alpert's imprint. As Alpert noted in his 1958 internal annual review to Director Waterman, NSF support was still 'confined to the hard-core scientific end of the continuum'.⁶⁶ Alpert added that he approved of this. For many years to come, agency documents continued to specify that it only funded social research that fulfilled the methodological criteria of objectivity, verifiability, and generality. A close examination of the studies that received such NSF funding during the 1950s and beyond lies outside the scope of this paper. Suffice it to say that the agency was particularly supportive of quantitative studies that sought to test specific and limited hypotheses. On the flip-side, the agency provided little or no support for historical and philosophical inquiries, with two revealing exceptions. The first is the history and philosophy of science, which the agency supported from its early days in part because some agency administrators and board members as well as practitioners in these fields saw such studies as a means to illuminate the unity of the sciences. The second exception was historical studies in anthropology, including archaeology, which had special appeal in part because of their apparent remoteness from current events—they therefore seemed unlikely to arouse public controversy.⁶⁷

⁶³ Appendix C: Recommendations Approved by National Science Board, August 1954, in Alpert with the assistance of Bertha W. Rubinstein, Progress Report No. 5 (Revised), Feb. 1. 1956, Folder: [Reports on Social Science Program], NSF HF.

⁶⁴ Alpert to Director [Waterman], June 14, 1957, Box 40, Folder 'Social Science Research Program', Office of the Director, Alan T. Waterman, Subject Files 1951–56, Record Group 307, NA.

⁶⁵ Basic information about the scope, organization, and funding of NSF's social science programmes can be found in the agency's published annual reports.

⁶⁶ Appendix B to Memo from Program Director for Social Science Research to Director, July 1, 1958, 'confined' at 20, Annual Review of Soc Sci. Program for FY 1958, Folder: [Reports on Social Science Program], NSF HF.

⁶⁷ On NSF support for history and philosophy of science, see Margaret Rossiter, 'The History and Philosophy of Science Program at the National Science Foundation', *Isis*, 75 (1984), 95–104; and on its support for anthropology and especially archaeology, see John E. Yellen and Mary W. Greene, 'Archaeology and the National Science Foundation', *American Antiquity*, 50 (1985), 332–41.

Throughout the 1950s and well into the 1960s the NSF remained unsupportive of research that had an explicit normative component or policy orientation, and hesitant to fund research on controversial topics like sex, religion, and politics. In his 1958 annual review, Alpert noted the continued concern among agency leaders that the social sciences 'involve areas of public controversy which might jeopardise the future growth and development of the Foundation itself'. Such a concern was, said Alpert, reasonable, for the social sciences did 'unquestionably involve areas of public controversy'. Fortunately, the NSF had been successful in 'defining its social science research program so as to omit such controversial areas'. Alpert proposed that the agency should therefore remain firm in this policy, 'no matter how significant such research might be'.⁶⁸

During and after his NSF years, Alpert often wrote about its social science programme in positive terms. He pointed out that this programme had taken shape after 'careful and deliberate study', and was 'characterized by selective, cautious, but continuous and essentially sound growth'. The NSF had 'demonstrated clearly that reliance on scientific methods and the scientific integrity of investigators can insure freedom from involvement in controversial issues'. Speaking about public attitudes towards the social sciences more generally, he asserted that the 'exigencies of social living in contemporary society' were undermining 'resistance' to the social sciences 'at a rate which would have seemed incredible a short decade ago'.⁶⁹ While the 1950s had begun with a 'roaring antipathy' towards the social sciences, the decade seemed to be ending with 'attitudes of positive interest and quiet acceptance'. Using a boxing metaphor, Alpert stated that for several years the social sciences had been 'rocky and punch drunk', but when the fight was over, they were 'still on their feet'. They had survived the McCarthy Era investigations, endured 'pariah status and innumerable reorganizations', and weathered 'appropriation storms'. Alpert even went so far as to say that the social sciences had 'never had it so good'.⁷⁰ The development of federal support from a 'multiplicity of organizations' now insured a 'healthy growth by sustaining diversified channels for exploration of new areas and untried methods'. In this wider context, the NSF played a valuable role as the only agency 'devoted exclusively to the support of basic research'.⁷¹

Such comments fit well with the claim, made two decades later by sociologists John Galliher and James McCartney, that Alpert's position represents an example of the 'benign viewpoint' about government support, which holds that 'government has simply facilitated the realization of central scientific tasks while preserving scientific autonomy'.⁷² Further consideration of Alpert presents a more complicated picture, however, for his NSF policy work rested on certain policy principles and claims that Alpert recognised as problematic. In Alpert's published writings while representing

⁶⁸ Appendix B (note 66), 18, 19.

⁶⁹ Alpert, 'The Social Science Research Program of the National Science Foundation', *American Sociological Review*, 22 (1957), 582-5 (582); Alpert (note 60), 38; Alpert, 'Congressmen, Social Scientists, and Attitudes Toward Federal Support of Social Science Research' (note 59), 685-6.

⁷⁰ Alpert, 'The Government's Growing Recognition of Social Science', *Annals of the American Academy of Political and Social Sciences*, 327 (1960), 59-67 (60, 62).

⁷¹ Alpert, 'The Growth of Social Research in the United States', in *The Human Meaning of the Social Sciences*, edited by Daniel Lerner (Cleveland, OH: World Publishing Co., 1959), 73-86 (73, 76-7).

⁷² John F. Galliher and James L. McCartney, 'The Influence of Funding Agencies on Juvenile Delinquency Research', *Social Problems*, 21 (1973), 77-90 (78).

the NSF, he tended to downplay or ignore these differences or contradictions. But before and after—and even during—his NSF years he spoke about them forthrightly.

5. The price of success

In Alpert's post-NSF career, he was a senior scholar, high-level academic administrator, and respected voice within US sociology and social science. In 1958, Alpert went to the University of Oregon as professor of sociology and dean of its Graduate School from 1958 to 1964. Later he became Dean of Faculties from 1964 to 1971, and then Vice President for Academic Affairs and Provost from 1971 to 1976. He was also the vice president of the American Sociological Association in 1958, president of the Sociological Research Association in 1961, president of the Pacific Sociological Association from 1962 to 1963, editor of the *American Sociological Review* from 1960 to 1962, fellow at the Center for Advanced Studies in the Behavioral Sciences from 1963 to 1964, and director of UNESCO's Department of Social Sciences from 1967 to 1970. In this section, we will consider four basic concerns Alpert raised regarding the nature and direction of US social science. Taken together, these concerns suggest his success in establishing a viable framework for NSF social science support came at a price.

First, Alpert raised questions about the methodological unity of the sciences. In a glowing 1954 portrait of the sociologist Robert MacIver—one of his Columbia mentors—Alpert wrote that MacIver's 'humanistic' sociology had 'reaffirmed the view of man as a creative human being', as an 'idealistic, goal-seeking, value-creating being who is especially distinguished by his capacity to impute meanings to events, objects, and people'. In contrast, Alpert found that 'strictly positivistic writing in sociology' offered an impoverished 'image of man as essentially a passive, mechanical creature' who responded 'automatically to external stimuli' even though they had 'no particular meaning to him'. There was thus wisdom in MacIver's warning that the discipline risked 'becoming "too scientific"' by 'ignoring the art of interpretation', which, asserted Alpert, is the 'soul of the sciences of man'. In the quest for a 'spurious objectivity', sociology might jettison its 'true subject matter'. In this way, Alpert praised MacIver's 'humanistic and "*verstehende*" sociology', much like he had praised Max Weber (and criticised Emile Durkheim).⁷³

In support of this general skepticism about methodological unity, Alpert also cited Vannevar Bush, which is most surprising in light of Alpert's NSF policy work and because Bush's statements were often invoked by the agency to explain that it supported the social sciences with the goal of establishing a healthy partnership between them and the natural sciences. Yet what was not often noted at the time—nor well-remembered today—is the fact that Bush was, in fact, disdainful of slavish efforts to follow in the natural sciences' footsteps—as was Alpert, as we saw earlier. Bush's view, as Alpert summarised it, held that the 'alleged differences' between the natural and social sciences, 'namely that the former are experimental and precise, while the latter are observational and imprecise', rests upon a 'confusion' and fails to consider the 'whole gamut of natural science research'. To wit, some sciences like 'astronomy and geology' were 'no less observational than anthropology or sociology', while the 'data of genetics scarcely approach in precision much of the

⁷³ Alpert (note 20), 287–90.

social science materials collected in a national census'. Bush further observed that 'great' scientific work had often involved 'very little in the way of measurement and mathematical formulation'. In general, Bush warned, the effort to bring 'methods of approach from one branch of science to any other' always encountered 'difficulties', as one would expect, 'for the method must be based on the problem in hand and not on *a priori* consideration'. Bush, as Alpert presented him, seemed to be recommending that the social sciences needed to find their own methods appropriate for their particular subject matters. Whether the particular methods used in the natural sciences would be helpful was an open question, and thus presumably not one that a funding agency should try to determine ahead of time.⁷⁴

Second, in a related point Alpert complained about a 'special fascination, if not obsession' among sociologists 'with methodology as an end in itself'. Alpert never wavered in his belief that better methodology was a key to establishing the 'superiority' of social science 'findings over common-sense judgments, folklore, everyday observations, popular conceptions, and ingrained stereotypes and prejudices'. Yet Alpert found value in the remark, attributed to an unnamed French scientist, that 'sociology is the science with the most methods and the least results'. Similarly, Alpert repeated with approval another quip, this one attributed to the University of Chicago economist Frank Knight, that 'a concern with methodology is like playing the slide trombone: no one seems to enjoy it as much as the fellow who's doing it'. Alpert thus reminded his fellow sociologists that they should refrain from indulging in the use of 'devices, gadgets, mathematical formulae, and the like', until they had clearly understood 'their appropriateness to the ends in view'. Echoing a concern he had expressed about public opinion research, Alpert emphasised that one's concern about scientific methodology must not 'determine the topics' selected for study.⁷⁵ He worried that it had become all too common for researchers to focus on 'concrete, specific, delimited problems of social investigation', while losing sight of the need to ask, as had Robert Lynd—another one of Alpert's mentors—'knowledge for what?'⁷⁶

Elsewhere Alpert made an observation about historical change in scientific methodology that seems startling when placed alongside his NSF policy work, though sensible in light of the more catholic views he expressed elsewhere both before and after his NSF years. 'Science', Alpert pointed out, is 'pluralistic and dynamic'. At 'different moments' in time, one encounters 'differing conceptions of scientific method'. These are so different, in fact, that what one generation counts as 'science', the next generation may count as 'superstitions'.⁷⁷ These are not the words of a scientific monist.

Alpert also resisted the quantitative evangelism then ascendant in US social science. Again recommending MacIver as a wise guide, Alpert reminded his scholarly peers that they needed to treat quantitative data as 'guideposts to understanding rather than as ultimate goals'.⁷⁸ Though the 'use of mathematical formulae or rating scales' helped to give one's research a 'scientific aura or tone', Alpert opined that

⁷⁴ Alpert, 'Public Opinion Research as Science', *Public Opinion Quarterly*, 20 (1956), 493–500 (494–5).

⁷⁵ Alpert (note 15), 46; Alpert, 'Sociology: Its Present Interests', in *The Behavioral Sciences Today*, edited by Bernard Berelson (New York: Harper & Row, 1964), 52–64 (62).

⁷⁶ Alpert (note 20), 288–9.

⁷⁷ Alpert (note 74), 494.

⁷⁸ Alpert (note 20), 292.

‘aroma or stink’ would often be the ‘more accurate designation’. Hence this memorable recommendation: ‘Measure, by all means, measure, and count, by all means, count. But let us count and measure the things that count’. Immediately following his complaints about methodological excesses, Alpert mentioned the lure of funding. Though he did not explicitly link the two, it is reasonable to conclude that they were closely related in his own mind. As he put it, ‘there is the tendency, not limited by any means to our own discipline, to undertake research “where the money is” rather than because one has a firm conviction that the topic has theoretical or scientific importance’.⁷⁹

Third, recall that, while working at the NSF, Alpert had emphasised the separation of basic social science from applied research. Yet elsewhere he recognised that this was only one way in which social science had practical value. His early work on Durkheim commented favourably on many practical uses of social science, including as a guide for social reconstruction. Alpert’s biography of Durkheim appeared in print the same year as Lynd’s *Knowledge for What?*, wherein Lynd criticised scholars who were hesitant to pursue the practical implications of their work at a time when democracy, freedom, peace, and prosperity were besieged by menacing foes. In 1963, Alpert recalled Lynd’s attack on the ‘concept-formers and library researchers who were discussing theories of navigation while the ship was going down’, and found wisdom in Lynd’s criticism of the disinterested scientific posture (which, as we have seen, was embedded in the NSF’s promotion of ‘hard-core’ social research). ‘A discipline is in good health’, Alpert told his fellow sociologists, when it could produce and welcome ‘systematic theoreticians, skillful and imaginative empirical researchers, and social critics strong in intuition and insight and courageous in expressing unpopular points of view’.⁸⁰

Studies of US anti-Semitism provided Alpert with a case in point about the need for social research oriented toward solving rather than merely understanding. Surely, this was also a matter of special interest to Alpert since he was Jewish and, according to his long-time NSF assistant Bertha Rubinstein, had struggled in his scholarly career with anti-Semitism. Reviewing a 1948 book by Robert MacIver, Alpert found grounds for a ‘serious indictment’ of the field of research on ‘inter-group relations’, for the ‘existing literature’ failed to offer much ‘scientific guidance’ in the effort to combat ‘discrimination and prejudice’. It was a great disappointment that researchers had carried out only a few ‘scientifically designed studies’ to evaluate the ‘effectiveness of specific action programs’. Returning to the dangers of ‘crude empiricism’, Alpert charged that in this important area, ‘prodigious amounts’ of social research concerned with method and technique had failed to offer much guidance to the ‘framers of social policy’.⁸¹

In light of Alpert’s worry here, perhaps it is not surprising that during the 1960s he came to question at a more general level the principle that social scientists should take a value-neutral stance in order to make their work free from bias, to establish that they were rigorously scientific, and to separate the roles of the social scientist

⁷⁹ Alpert (note 15), 46–7.

⁸⁰ Ibid., 45, 48.

⁸¹ Alpert, Review of *The More Perfect Union* by R. M. MacIver, *Jewish Social Studies*, 11 (1949), 189–91 (190–1). Regarding Alpert’s concerns about anti-Semitism, also see his review of *How Secure These Rights?* by Ruth G. Weintraub, *Jewish Social Studies*, 11 (1950), 261–33; and his review of *A Mask for Privilege: Anti-Semitism in America* by Carey McWilliams, *American Journal of Sociology*, 54 (1948), 177.

and the citizen. Again using one of his Columbia mentors to help frame the issue, Alpert, in his last published essay, from 1968, identified George Lundberg as one of the most forceful proponents of value-neutrality. By this point during that tumultuous decade, a growing chorus of critics inside and outside of academia were from various viewpoints calling for social scientists to incorporate humanistic values (e.g. freedom, democracy, justice) into their frameworks of analysis. Among scholars supporting liberal and more radical social changes, a common criticism of 'value-neutral' social researchers was that they really weren't: their purported value-neutrality meant that they were unable to raise direct challenges to the status quo and thus their research often displayed a conservative bent. Alpert's own position was a bit tentative, but clearly he saw merit in this charge, as he asked: 'Does the recourse to scientism correlate positively with political conservatism?' While admitting that he did not know the answer for sure, he found the charge that a '*wertfrei* view of science' is related to a 'conservative philosophy' to be a 'reasonable hypothesis'. It seemed to him that the position that says 'scientific propositions are ethic-free provides one with a ready-made rationalized justification for rejecting social and political causes'. And later in the same essay, Alpert repeated his worry that as sociology had become 'more scientific', it had unfortunately become 'more irrelevant to the social issues of the day'.⁸²

Fourth, though Alpert's NSF policy work supposed that the social sciences needed to advance by emulating the more mature natural sciences, Alpert found this pecking order problematic, not just because of the dubious assumption about there being a universal scientific methodology, but because this pecking order seemed to have a corrosive influence on the climate of professional respect and support. Shortly before Alpert would leave the NSF in 1958, an article of his in the popular magazine *Saturday Review* included a sarcastic chart on social science funding. Though here as in many other places Alpert used humour to make his point, the real problem was no laughing matter, neither for social science nor for society. Alpert called for measures to establish in the long run 'among natural scientists and social scientists a sense of mutual respect and understanding and a community of interest'. This meant abolishing the 'invidious hierarchical distinctions among specialists of the various disciplines', and, more positively, creating an 'institutional environment' to encourage 'all scientists' to work together toward the 'common goal' of advancing our 'understanding' of 'nature, man, and society'. Only months before, in the fall of 1957, the launching of the Soviet Sputniks had heightened US fears about an impending reign of terror from the skies. Making implicit reference to this awful possibility, Alpert concluded that 'man's mastery of space... must quickly be matched by man's mastery of himself. This is a fundamental condition of survival'.⁸³

In other writings from his post-NSF years Alpert again drew attention to the harmful impact of invidious hierarchical distinctions, declaring that the social sciences had 'suffered' due to their 'minority group status' within the scientific community. As with other such groups, social scientists were subject in the labor market to the 'rule of "last hired, first fired"'. Within the federal government, social science research units continued to suffer from attacks, cuts, and abolition, even

⁸² Harry Alpert, 'George Lundberg's Social Philosophy: A Continuing Dialogue', in *Behavioral Sciences: Essays in Honor of George A. Lundberg*, edited by Alfred de Grazia (Great Barrington, MA: Behavioral Research Council, 1968), 48–62 (58, 61).

⁸³ Alpert (note 60), 37–8.

though the overall climate had become more favourable over the 1950s. Citing problems at the Division of Research of the Housing and Home Finance Agency, the Air Force Human Resources Research Institute at Maxwell Field, and the Air Force Personnel and Training Center at Lackland Air Force Base, Alpert complained about the ‘on again, off again’ pattern of government social science activities. Like it or not, Alpert recognised that the social sciences had ‘prospered best’ where they were ‘included under broad umbrella classifications of the scientific disciplines such as the agricultural sciences, military sciences, medical sciences, and health sciences’. When placed in ‘close company’ with the higher-status biological or physical sciences, the controversial social sciences enjoyed ‘protection and nourishment’ that they normally did not have ‘when they are identified as such and stand exposed, “naked and alone”’.⁸⁴

A decade after Alpert left the NSF, political and scholarly debate over a legislative proposal to create a National Social Science Foundation (NSSF) centred on many of the issues that Alpert had struggled with. The major architect and promoter of the ill-fated NSSF proposal was Oklahoma Democratic Senator Fred Harris. He argued that a new agency for the social sciences was needed in order to overcome an entrenched pattern of marginalization and relative neglect at the NSF and in the federal science establishment more widely, a pattern which had left the social sciences with meagre representation, status, influence, and funding. Controversy over Harris’s proposal reached a high point in 1967 when Harris, as the head of the Senate Subcommittee on Government Research, oversaw national hearings on his proposal. Those who argued against Harris’s proposal claimed that the social sciences were doing reasonably well in the NSF, that funding for hard-core research had strengthened their scientific standing and public acceptance, that the unity of the sciences remained a valuable ideal, and that keeping close company with the natural sciences served to protect social scientists and their patrons from harmful public controversy. Harris and pro-NSSF voices claimed, on the contrary, that the social sciences were receiving only crumbs of support, that they were being mistreated by natural scientists who often didn’t understand them well, and that, consequently, the social sciences were not able to develop their full intellectual potential and practical value to the nation. As long as they were ‘integrated’ as ‘second-class citizens’ at the NSF, it would be better for them to have ‘separate but equal’ facilities—as in a new social science agency, reasoned Harris.⁸⁵

As this brief consideration of the late-1960s NSSF debate suggests, the sort of concerns Alpert expressed about the hierarchical relations between the social and natural sciences informed national science policy discussions and debates in the coming decades. Ever since Alpert’s early policy work, the NSF stood out in the nation’s capital as an especially visible proponent of the unity-of-science viewpoint when it came to thinking about the nature of the social sciences and how public funds could be best used to advance their progress as part of the nation’s scientific

⁸⁴ Alpert (note 70), 61, 64; Alpert (note 71), 82.

⁸⁵ U.S. Senate, Committee on Government Operations, Subcommittee on Government Research, *National Foundation for Social Sciences, Hearings*, 90th Cong., 1st Sess. (Washington, DC: GPO, 1967), 90. On the debate over Harris’s NSSF proposal, see Gieryn, ‘The U.S. Congress Demarcates Natural Science and Social Science (Twice)’, in *Cultural Boundaries of Science: Credibility on the Line* (Chicago: University of Chicago Press, 1999), 65–114 (84–93, 101–8); Mark Solovey, ‘Senator Fred Harris’s National Social Science Foundation Proposal: Challenge to the Federal Science Establishment’, author’s manuscript.

enterprise. Because of the agency's prominence in this respect, the NSF also became a predictable target of attack by critics of the unity-of-science stance and the hierarchical social science-natural science relations that continued to inform and sustain that stance at the NSF and elsewhere in the federal science establishment.⁸⁶

As we have shown, Alpert's advocacy for the 'hard core' at the NSF was in some tension with his critical, career-long reflections on these issues. The explanation lies in the highly constrained opportunity structure that Alpert was forced to manoeuvre within at the NSF. He was acutely aware of the fragile standing—and contentious legislative history—of the social sciences at the agency, and only established a lasting presence for social science by walking the tactician's tightrope. A measure of fealty to the unity-of-science ideal and to quantitative empiricism on 'safe' topics was, in effect, the price of success. Of course Alpert's adroit code-switching had major consequences for the postwar social sciences, given the development of NSF funding in the decades following his departure and the high scientific prestige accompanying NSF grants.

6. Historiographical reflections and conclusion

At this point, we might ask why Alpert has received relatively little attention in historical accounts so far, not only because this question helps us to reassess Alpert's significance but also because it helps us to reflect more broadly upon historical frameworks of analysis. Much of the history of sociology has been written from a disciplinary perspective, whether by sociologists or by historians, and the predominant focus has been on the major developments and debates in theory and methodology, along with close attention to the major participants in those developments and debates.⁸⁷ When one thinks of important sociologists during the second third of the twentieth century—Alpert's era—the following figures readily come to mind: Paul Lazarsfeld, George Lundberg, Robert Lynd, Robert Merton, C. Wright Mills, William Ogburn, Talcott Parsons, David Riesman, Pitirim Sorokin, Louis Wirth, and perhaps a few others. To be sure, Alpert didn't contribute as much to the theoretical and methodological developments and debates as they did. In his favourable article about Robert MacIver, Alpert proposed that MacIver was underappreciated and suggested why this might be the case: 'there is no MacIver Neurotic Inventory, no MacIver Index of Morale, no MacIver Hidden Attitude Analysis; nay, not even a MacIver Scale'. Of Alpert one can say the same.⁸⁸

In addition, in histories of the social sciences (and histories of science more generally), the work of policy and programme administrators has received little historical attention, the exceptional case being the unusually dynamic figure who had a significant degree of freedom and power to shape those policies and programmes. Of those from Alpert's era, one thinks of Talcott Parsons at Harvard's Department of Social Relations, Paul Lazarsfeld at Columbia's Bureau of Applied Social Research,

⁸⁶ It would be interesting to know what Alpert thought of Harris's challenge; unfortunately, existing documents do not indicate if he commented on it.

⁸⁷ An interesting recent book that provides a good example of this orientation in the history of sociology is David P. Haney, *The Americanization of Social Science: Intellectuals and Public Responsibility in the Postwar United States* (Philadelphia: Temple University Press, 2008). Since this book is really about sociology and sociologists, rather than social science more broadly, the title is somewhat misleading.

⁸⁸ Alpert (note 20), 292.

or, moving beyond sociology, the polymath Herbert Simon at the Carnegie Institute of Technology (later Carnegie Mellon University). Compared to them, Alpert did not display the same dynamism or have the same degree of freedom and power.⁸⁹ Since Alpert's efforts at the NSF were constrained heavily by historical conditions and immediate pressures, it might be thought that those factors determined the character of his efforts to such an extent that there is not much more to be said. However, this viewpoint obscures much about Alpert and his historical importance. In his work on Durkheim, Alpert drew attention to the problem of viewing individuals as passive subjects whose actions are determined by an all-powerful society. Viewing the evolution of Alpert's views and career from the interwar to WWII to postwar periods has allowed us to see Alpert as an individual, even during his NSF years, and thus to understand him as an active agent.

Alpert explained at one point during his NSF years that, 'from Comte's day on, it has been well nigh impossible for sociologists, psychologists, anthropologists, and other students of human social behavior to avoid such questions as how "scientific" the social sciences are, whether free will or something else makes it impossible to apply scientific method to human behavior, or whether there is a fundamental incompatibility between the natural sciences and social knowledge'.⁹⁰ At the NSF Alpert certainly could not avoid such questions as he set out to craft a viable policy framework. He did so by emphasising support for the 'hard-core' end of the social science continuum. As he defined it with the help of supportive advice from Chester Barnard and in accordance with broader trends in mid-century US social science funding, this position assumed the presence (or goal) of a robust scientific unity defined by rigorous scientific methodology alongside the criteria of objectivity, verifiability, and generality (criteria Alpert had defended as early as the 1930s in his studies on Durkheim). With a keen sense of political and institutional realities, Alpert linked this strategy of protective colouration with a cautious approach that promised to help the social sciences steer clear of criticism inside and outside the agency, based upon a programme of modest and stable growth that began with areas of 'convergent' research, avoided controversial topics, and made ineligible social science that was associated with social criticism, value-laden studies, and social reform.

Remarkably, Alpert's strategy for funding the social sciences drew upon and reinforced certain features of mid-century US sociology and social science that he had expressed concerns about elsewhere—in his studies on Durkheim, in his engagement with debates over foundational issues that involved his Columbia mentors Lynd, Lundberg, and MacIver, in his writings about statistics and public opinion research, and in his commentaries about government-social science relations. These features that concerned Alpert included the unity-of-science thesis, the obsession with social science methodology and quantitative analysis, the clear separation of basic social science from practical uses and social criticism, and the

⁸⁹ For a recent and thoughtful discussion about the significance of entrepreneurial social science researchers and programme builders in the post-WWII years, see Hunter Crowther-Heyck, 'Herbert Simon and the GSIA: Building an Interdisciplinary Community', *Journal of the History of the Behavioral Sciences*, 42 (2006), 311–34.

⁹⁰ Alpert, 'The Social Sciences and the National Science Foundation, 1945–1955', *American Sociological Review*, 20 (1955), 653–61 (653).

scientific pecking order that made social scientists second-class citizens at the NSF and within the federal science establishment more generally.

Since the changes in US national science policy, science patronage, and the social sciences that occurred during WWII and the early Cold War period were in many ways dramatic, historians of science can easily neglect important linkages and continuities between this later period and the interwar era. Examining the cases of individuals like Alpert's whose careers spanned these two eras is one good way of illuminating these linkages. Such work can reveal if and how national science institutions, policies, and programmes created and shaped in powerful ways by preoccupations of the post-WWII and Cold War years were, nevertheless, also shaped by concerns and developments that long preceded that era. This is obviously so in the general yet still very important sense that problems in defining the topical scope, methodological foundations, scientific character, and practise value of the social sciences are foundational ones whose roots can be traced back to the very origins of the modern social sciences in the seventeenth and eighteenth centuries—and whose persistent prominence is clear during the period of early professionalization of US social science in the late nineteenth and early twentieth centuries. But it is also true in the more specific sense that important players in the post-WWII era carried with them pre-WWII (and WWII) experiences and views that then influenced their actions in specific ways. Harry Alpert's story is one revealing case.

Acknowledgements

It is a great pleasure to thank Michael Bycroft, for superb research assistance; Hunter Heyck, Philippe Fontaine, Marga Vicedo, Sarah Igo, Neil McLaughlin, and an anonymous referee, for helpful comments on earlier version; and the National Science Foundation Sponsored Project Award 9810635, and the University of Toronto Institute for the History and Philosophy of Science and Technology, for financial support.