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**“Laboratory talk’ in US Sociology, 1890–1930: the performance of scientific legitimacy”**

**Abstract**

This paper examines one aspect of early twentieth century debates over the meaning of scientific methodology and epistemology within the social sciences: the tendency of sociologists to invoke “laboratory” as a multivalent concept and in reference to diverse institutions and sites of exploration. The aspiration to designate or create laboratories as spaces of sociological knowledge production was broadly unifying in early American sociology (1890–1930), even though there was no general agreement about what “laboratory” meant, nor any explicit acknowledgement of that lack of consensus. The persistence of laboratory talk in sociology over decades reflects the power of “laboratory” as a productively ambiguous, legitimizing ideal for sociologists aspiring to make their discipline rigorously scientific.

**Keywords: laboratory; experiment; history of sociology; scientism; value-neutrality**

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***“Laboratory’ in American Sociology, 1890–1930”***

**I. Introduction**

The aspiration to make the study of social life scientific was pervasive in the early years of sociology in the United States. From the earliest institutionalizations of sociological research, teaching and publishing in the last years of the nineteenth century to the heyday of “scientism” in the 1920s, many of the leading practitioners in the discipline took natural science as a methodological model for their own work and regularly proclaimed that sociology had just become or was on the cusp of becoming properly scientific. In this respect they were faithful followers of Comte and Spencer. Contemporary historians have emphasized above all their commitment to value-neutrality (so-called “scientism”) and quantification (e.g., Ross 1991, pp. 390–470; 1993; Bannister 1987; 2003; Camic and Xie 1994), but when early US sociologists embraced the ideals of scientific method and epistemology, it was far from settled what they were embracing. Encyclopedic classification had just as plausible a claim to be the basis of scientific method as counting or quantitative modeling, and the idea that scientific understanding implied certain value commitments was widespread among those whom Andrew Jewett (2012) has called “scientific democrats” from the mid-nineteenth to the mid-twentieth century. This paper aims to recover one underappreciated element of the early sociologists’ contribution to early twentieth century debates over the meaning of science through an analysis of how “laboratory”—as a referring term and as a concept—was discussed and understood in their writings.

Early US sociologists did not have a stable or widely shared image of scientific method or scientific epistemology as they pertained to their discipline or any other. They

did, however, have a broadly shared normative commitment to the ideal of making sociology scientific, one strand of which was the impulse to make the idea of the laboratory “work” in some way for sociology. The writings of the early American sociologists are full of references to “laboratories,” of various sorts, and I will argue that these references—for the performative work that they did and for their modest role in shaping epistemological and methodological practices—were an important part of the ethos of the young discipline. In the period up to 1930, scientific authority and scientific practice remained very loosely defined, but as ideals they were still able to influence argument and method.

After 1930, US sociology was institutionally transformed in a number of ways: through the launching of the *American Sociological Review* in 1936,<sup>1</sup> the diversification of substantive research, the spread of graduate education well beyond initially dominant departments at Chicago and Columbia, the growth of interdisciplinary institutes<sup>2</sup> and the related growth of foundation-funded, large scale collaborative projects.<sup>3</sup> I end my study in 1930 in recognition of these transformations, which roughly mark the end of a period in which American sociology was a highly personality-driven enterprise (see, e.g., Bannister 1987, p. 9; Abbott 1999, pp. 93–96; Hinkle 1980; 1994; Breslau 2007) and in some ways a highly institutionally localized one (see Ross 1991; Camic 1995; Camic and Xie 1994; Turner and Turner 1990; Bulmer 1984). Although there was some continuity from the early “laboratory” rhetoric to these later developments—the experimental studies reported in *The Authoritarian Personality* and the establishment of the Harvard Laboratory of Social Relations, for example—entirely different factors were surely more relevant to shaping the broad institutional contours of mid-twentieth-century sociology, especially the changing

relation of government to social science (Gilman 2003; Solovey 2001; 2004; 2013; Solovey and Pooley 2011) and the complex cultural consequences of the Cold War (Isaac 2007).

Historians of sociology have paid almost no attention to the early sociologists’ numerous invocations of “laboratory.”<sup>4</sup> Nor have the many recent works on laboratories and other sites of experiment in the interdisciplinary science studies literature addressed that lacuna. Because science studies scholars have adopted a (wholly justified) attitude of sustained skepticism towards the self-understandings of their historical and ethnographic subjects, they have largely ignored the ways in which practitioners of laboratory science have used of the word “laboratory” and what they have meant by it.<sup>5</sup> In her study of twentieth century behaviorism and its practitioners’ aspiration to turn the world at large into a laboratory of sorts, for example, Rebecca Lemov (2005) relies on her own implicit definition of “laboratory” and does not give a systematic account of how behavioral scientists’ use of the word changed over time.

The authoritative sources on the disciplinary history of American sociology up to 1930 have tended to assume that, despite the intellectual ferment and disciplinary boundary-crossing that characterized the early twentieth century social sciences, sociologists were able to rely on a relatively stable image of high-prestige natural science as their epistemological and methodological model. In her seminal work on *The Origins of American Social Science*, Dorothy Ross (1991) writes that, as of about 1920, “with science...defined by its method, scientism demanded that the requirements of natural scientific method dominate the practice of social science” (p. 390). While she allows that “the range of sophistication about scientific method varied” (p. 405), Ross does not examine the competing visions of what it meant to be scientific. When any plausible

definition of “science” is admitted, one can compile a bewildering array of claims for when and where enthusiasm for science in sociology was peaking: in the era of “American Spencerians” Albion Small and Franklin Giddings (Hinkle 1980; Turner and Turner 1990; Breslau 2007), in the post-World War Two era of foundation-funded, large scale social research (Turner and Turner 1990; Solovey 2004; 2013; Isaac 2012) or in the scientism of the 1920s (Bannister 1987; 2003; Ross 1991). This paper departs from a point that Ross and others have exhaustively established, viz., for many of the early twentieth century American sociologists, claiming the mantle of “science” was a powerfully legitimating and highly desirable end. I offer new insights into the multiple meanings of “science” that this group worked with by analyzing the various conceptual associations that were made with the word “laboratory” in sociology and the referents (things-in-the-world) for which the word “laboratory” stood. Tables 1 and 2 (p. 11 below) offer an indication at a glance of how much conceptual and referential diversity in their language has been overlooked.

The argument of this paper is prefigured by Jewett’s major recent challenge to the historiographic emphasis on scientism in the development of the American social sciences. Jewett observes, first of all, that the ideal of value-neutrality was often “honored...only in the breach” (2012, p. 4) by researchers who readily mixed their normative and positive concerns, then goes on to document in detail how science was often understood as a value-producing enterprise with direct implications for American cultural life and democracy. Jewett does not address the laboratory as a methodological or epistemological commitment of the early sociologists, but recent historical studies of experimental psychology (e.g., Danziger 2000; Dehue 2001; Lemov 2005) provide a precedent for my substantive focus on “laboratory.” The ideal of a laboratory science manifested in different ways in sociology

from in psychology, but laboratory talk in sociology nevertheless demands our attention if we wish to understand the attitudes of early American sociologists towards experimentation and science in general.

## II. “Laboratory” in early twentieth century popular and professional language

The broad cultural resonances of the word “laboratory” in the early twentieth century are not the same as those we experience today. The period 1890–1930 saw the founding, for example, of John Dewey’s Laboratory School (established 1896 in Chicago) and “laboratories of social hygiene” like one in Bedford Falls, NY (established 1911), which were rough functional substitutes for today’s institutes for public health, mental hospitals and correctional facilities. “Laboratory” was an expansive category and apparently a widely familiar one. From 1890 onwards the word appeared regularly in the popular press: *New York Times* articles from 1890 to 1900 make reference to specialist laboratories of many types, including marine biology laboratories, psychology or psychical laboratories, bacteriology laboratories, medical laboratories, a “snake laboratory” in Calcutta for the study of snake venom and antidotes (“Snake Laboratory Founded,” 1893) and the Naval laboratory. In each of these cases, “laboratory” is used without explicit definition, on the assumption that the lay reader will sufficiently understand its connotations. So, too, is the lay reader presumed to be able to follow when the newspaper uses the term in a looser, metaphorical sense: e.g., describing a newspaper’s angry coverage of an event as an “inflammation in its vocabulary laboratory” (“Capture of the Bridge,” 1891) or describing a courtroom as having become a laboratory when a witness was asked to make an experimental demonstration (“Courtroom Made a Laboratory,” 1893).

Even before 1900, then, laboratories were culturally recognizable objects, but they were not an invisible background to scientific practice. They were still institutions of recent provenance, and observers of science were attuned to modifications in their design and practice. The *Oxford English Dictionary* dates the origin of “laboratory”—etymologically from “elaborate” and “elaboratory”—to references to chemical and alchemical laboratories in the early 1600s. The Cavendish Laboratory at Cambridge University was established only in 1874, and the first so-called psychological laboratory in Leipzig in 1879 by Wilhelm Wundt. By 1900 in the United States, laboratories commanded popular attention and could be treated as harbingers of cultural change. The popular *NY Times* and the prestigious professional journal *Science* both regularly treated the openings of new university laboratories as newsworthy events, often detailing their physical layout and material apparatus in the process (examples from *Science* include Trowbridge 1885; Jordan 1892; Sharp 1893; Bailey 1900; Smith 1901; McClenahan 1910; Cattell 1928). An 1892 *NY Times* article treats the opening of many new laboratories as evidence that the study of psychology was moving decisively towards a new and more empirical methodology (“Hot Weather Psychology,” 1892), and many of the *Science* announcements stressed the material innovations of new laboratories over those already in existence.

Within the social sciences, the disciplines of psychology and statistics relied most heavily on physical structures called laboratories and on the language of “laboratory.” The first appearance of the word “laboratory” in the leading statistical journal *Publications of the American Statistical Association* (PASA, established 1888) comes in 1889, in an abstract of a talk given at the annual meeting of the Association. The talk addresses the possibility of importing laboratory methods into instruction in statistics:

For students...it is suggested that instruction in statistics be given in connection with courses on Commerce, History of Industry, and Finance, or other subjects of economic history. The mere proving of statistical tables, in connection with United States finance, will be a benefit. In this way the student will acquire some delicacy in reconciling discrepancies, and he will gain independence and judgment. Similar laboratory work in statistics can be applied to the tariff history of the United States (Dewey 1889).

This passage is characteristic of the journal in some important ways. The early *PASA* is to a large extent a pedagogical enterprise, in two senses: many articles instruct readers how to use and teach the new methods of statistics, and many others argue for how the new methods can make subjects that have long been studied in other ways (such as population growth and economic history) more comprehensible. In similar articles to the one just cited, methods are offered for “practical laboratory instruction” in the understanding of “vital statistics” (Dublin and Kopf 1919) and in map-making (Ripley 1899, p. 326). In each of these instances, “laboratory” appears as a place for teaching and learning—and for learning basically simple, mechanical techniques, at that—rather than as a place for new discovery.<sup>6</sup> The notion that laboratory work was not only at the pinnacle of scientific achievement but was also basically transparent and simple would appeal to many sociologists and spur on aspirations to scientificity and rigorous experimentalism in sociology, as it developed roughly in tandem with the discipline of statistics.<sup>7</sup>

As for psychology, descriptions of physical laboratories already in existence (Krohn 1892; Sanford 1895; Martin 1906), reviews of their results (Jastrow 1890; 1891; 1892; Sanford 1893*b*; Kline 1899) and “practical suggestions” for the construction of new ones



(Sanford 1893a; Titchener 1900) appear frequently the pages of the early *American Journal of Psychology (AJP)*. By the end of 1890 there were so-called psychology laboratories at ten universities in North America, and by 1900, 32 more psychology laboratories had been founded in the US, many of them established by students of Wundt or of G. Stanley Hall, who was himself one of Wundt’s first American students.<sup>8</sup> These early institutions, actual and anticipated, were diverse in structure, reflecting ongoing debates over theory and method. The engagement in laboratory talk in psychology cut across the methodological and theoretical divisions that broadly separated so-called functionalists, structuralists and behaviorists.

The behaviorist laboratory was the model dictated most strongly by its accompanying theory. The behaviorist position that observable stimulus and response needed to replace unobserved “consciousness” as the central objects of study in psychology was in effect a declaration of intent to set up laboratory spaces systematically outfitted to conduct the kind of experiments necessary to advance the science. For John Watson, who ran the Psychological Laboratory at Johns Hopkins University, the central behaviorist task was the measurement of conditioned reflexes. Under his leadership the Hopkins laboratory was outfitted to accommodate experimental subjects including rats, chickens, a great horned owl, monkeys, human children and adults. Experimental procedures included the administration of “punishment” (through electrocution and burns), the measurement of respiratory patterns and closely regimented verbal reports from the human subjects (Hillner 1984, pp. 125–133; Lemov 2005, pp. 31–36).

Meanwhile, the leading structuralist psychologist Edward Titchener laid out in papers in *AJP* (1900) and *Mind* (1898) an exacting vision for the physical setting required

to do structuralist “introspective trial” research, down to the necessity of hardwood floors and the prohibition of chalk (Titchener 1900, p. 251). Functionalists William James and John Dewey offered a qualified encouragement for bringing laboratory-like experimentation to the social sciences. James damned with faint praise Wundt’s fixation on laboratory work (James 1875; Hilgard 1987, p. 64)<sup>9</sup> and by his own account, the Harvard laboratory that he had founded in the mid-1870s was nothing much to speak of until a new laboratory was built in 1890 (James 1895; Sahakian 1975, p. 292). Dewey’s forceful critique of behaviorist psychology (most famously in Dewey 1896) confronted the simple and fashionable versions of value-neutrality and experimental control with severe conceptual problems. Nevertheless, both of them, and those who eventually ascribed to “Deweyan philosophy of science” (Jewett 2012, pp. 238–240) stressed practice, experience and experimentation as the touchstones of proper scientific method. Their reservations about the behaviorists’ naïve empiricism did not dissuade them that psychology’s future was as an experimental laboratory science.

Despite the unsettledness of early American psychology’s theory and methods, leading practitioners converged early on to a consensus that the discipline ought to be an experimental, laboratory science and that laboratories needed to be custom-designed in accordance with the imperatives of method. Sociologists would make use of their ideas—especially Franklin Giddings of Columbia, with his strong interest in behaviorist experimentation, and Robert Park, who learned from personal associations with James at Harvard and social psychologist George Herbert Mead at Chicago.

### **III. “Laboratory” as an unstable scientific ideal in sociology**

In the writings of early American sociologists, the numerous examples of laboratory talk include Charles Henderson’s call for the establishment of “prison laboratories” to make prisons better instruments of social reform (1900); the designation of observatories and museums as laboratories (Zueblin 1899); Ulysses Weatherly’s (1923) depiction of the “West Indies as sociological laboratory”; Robert Park’s essay on “The City as a Social Laboratory” (Park 1968[1929]); and several early textbooks and pedagogical articles that present themselves as “laboratory guide[s]” to sociology (Small and Vincent 1971[1894], p. 15) or otherwise present significant discussions of the so-called laboratory method in sociology (e.g., Cutler 1911; Taylor 1923; Eliot 1924; Melvin 1925; Jocher 1928; Carr 1929; Harper 1930). From the beginning to the end of the period we find that laboratory references range from referentially ambiguous to concrete, from conceptually very imprecise to exact, as the tables below illustrate:

TABLE 1 HERE

TABLE 2 HERE

“Science,” and the idea of “laboratory science” in particular, were broadly unifying aspirations in the early discipline of sociology, although there was considerable variability in the conceptual associations “laboratory” carried when it appeared in print. The diverse and overwhelmingly positive references to “laboratory” catalogued above should make this point clear. Disagreement with regard to conceptual meaning of “laboratory” rarely produced explicit reflections on the meaning of scientific method or epistemology. However, it did produce at least a semblance of common value-orientation and common standards of legitimacy in the discipline. The ideal of sociology as a laboratory science was not universally shared (although it had few vocal detractors—perhaps the most notable

was C.H. Cooley<sup>10</sup>), and the use of common terminology papered over some major substantive disagreements. But *as* the expression of an ideal, laboratory talk in sociology nevertheless gave some coherence to the disciplinary project.

One rendering of sociology’s early history has it that social workers and professional sociologists were generally antagonistic to each other, particularly the latter to the former (see especially Deegan 1988; see also review of the literature in Lengermann and Niebrugge 2007). But several articles in the *American Journal of Sociology* (*AJS*, founded 1895) and *Social Forces* (founded 1922) integrate the scientific aspirations of laboratory talk with a vision of sociology as firmly rooted in a reformist tradition (Henderson 1900; Anderson 1923; Eliot 1924). They could rely not only on the use of scientific language and concepts in the work of their professionalizing colleagues, but also on the explicit talk of the social settlement as laboratory that originated with activists and civic leaders like Robert Woods of the Boston South End settlement house and James Reynolds of the New York City University Settlement (Recchiuti 2007, pp. 92ff.). Henderson’s, Anderson’s and Eliot’s articles all rely on the reformist potential of social science to motivate the significance of the laboratories in question—for example, by pushing for “laboratories” in prisons to track the life histories of inmates (Henderson 1900). In doing so they reveal a symbiotic relationship between social work and professional sociology that extended into the very heart of professional sociology’s grandest and ostensibly most disinterested ambition: to turn sociology into an objective science. The same melding of reformist hopes and scientific language can be seen in Albion Small, who was both the first to use the language of “laboratory” in Chicago’s Sociology Department and a deeply religious man

whose ethical vision of sociology that entailed a clearly voiced opposition to unfettered capitalism (Small 1913).

In her widely read *Twenty Years at Hull-House*, Jane Addams explicitly rejected the notion that the settlement should be thought of as a laboratory, on the grounds that “Settlements should be something much more human and spontaneous than such a phrase [as ‘sociological laboratory’] connotes” (Addams 1910, p. 308). Her resistance, of course, suggests that the idea of settlement as laboratory had a wide circulation, and even in Addams’s case the resistance seems driven by a narrow reading of “laboratory”—as a place where human emotion and human experience are suppressed and cannot be understood—rather than by skepticism that the settlements ought to adopt some aspects of experimental scientific method. Addams and her fellow settlement workers at Hull House engaged in mapping work and social observation that was clearly meant to make a contribution to sociological understanding (*Residents of Hull-House*, 1895). The notion of sociology as place-specific, observational study, and more specifically as a laboratory science, was an ideal that bridged the growing divide between social reform work and sociology.

Two recurrent and especially prominent conceptual associations with “laboratory” were, on the one hand, the laboratory as controlled experimental space and, on the other, laboratory as found natural experiment.<sup>11</sup> In Thomas Gieryn’s account of Chicago sociology, the latter idea is more associated with the term “field site” than with “laboratory,” and Park, Burgess and their students navigated between the two descriptions of the city depending on the context. Gieryn argues that “laboratory” meant something quite specific to Park and his students: a “truth-spot” that puts the experimenter in total control of the environment,

as opposed to a field site, where knowledge claims are legitimated by a researcher who can claim to have observed “unadulterated reality” (Gieryn 2006, pp. 5–6). Gieryn and Bruno Latour, rare examples of contemporary science studies scholars who have interrogated historical invocations of “laboratory” in the social and biological sciences (Gieryn 2006; Latour 1984, pp. 81–83; 1987, pp. 93, 249–250; 1999, pp. 121–127), both assume that such invocations were legitimizing moves that could rely on the already high status of so-called “laboratories” and of the natural sciences. This claim looks partially correct, but it carries the implication that the conceptual sense of “laboratory” was clear and univocal across both the social and natural sciences. This position cannot be supported with reference to early twentieth century American sociology.

The myriad “laboratory” references among early sociologists, especially those that attempt to balance between the idea of experimental control and the idea of unmediated access to the world, are best understood as efforts to establish scientific authority performatively—in J.L. Austin’s sense of using language to enact some state of affairs other than merely stating a proposition. In focusing on this performative work of enacting, by writing alone, their own scientific legitimacy, many of the early sociologists neglected to elaborate the actual conceptual content of their laboratory references. Taking a longer view of developments at Chicago than Gieryn does, we find that the notion of sociology as a laboratory science began with Albion Small decades before Park’s arrival, and we find the same range of conceptual and referential clarity that existed in the discipline at large. Writing in a context in which the basic notions of “science” and “laboratory” were both unsettled, sociologists at Chicago and elsewhere sought to establish the legitimacy of their discipline, at least in part, by calling their sites of knowledge production “laboratories” and

by calling sociology a “laboratory science.”

Park himself is an example of the relative importance of the performative dimension of laboratory references. Although Park remains very strongly associated with the notion of the found natural experiment—particularly the city—as a laboratory (Park 1968[1929]), his version of “laboratory” is really conceptually thin. In his personal notes and correspondence, Park was much more invested in the Chicago school’s other well-known heuristic, the city as “urban ecology.”<sup>12</sup> A question for students from Park and Burgess’s textbook *Introduction to the Science of Sociology* (1969[1921]) suggests that the authors were ambivalent about the status of their own best-known use of the laboratory idea: “Is the description of great cities as ‘social laboratories’ metaphor or fact?” (p. 32). When he served as president of the American Sociological Society in 1925 and organized the annual meeting around the theme of “The City,” Park organized sessions that took a variety of perspectives on the city, but “the city as a social laboratory” did not appear as either a session or a sub-theme (Burgess 1926).<sup>13</sup> In an unpublished manuscript on “The Sociological Method,” Park differentiates between “expert” knowledge and “scientific” knowledge in a way that pushes against the idea of the proper domain for scientific work as a particularistic, controlled and contained environment:

Science may be described as that form of knowledge which has been reduced to such a formulation that it can be accurately communicated and applied flexibly to many cases, not limited to such cases as may be encountered at a certain time and place. The knowledge of the scientist may be contrasted with the knowledge of the expert. The expert has highly developed and cultivated knowledge, but unless he is also a scientist, his knowledge is likely to be particularistic, limited to the materials

of his own experience (Park and House, 1931–1934).

Park’s student Robert Redfield<sup>14</sup> did much more with the ideas of “laboratory” and experimental manipulation than Park. Redfield became involved in the 1930s with the Laboratory of Anthropology in Santa Fe, New Mexico, which had been established as a “first social science field station” (in Redfield’s words) in 1927 (Redfield, “Laboratory of Anthropology,” no date).<sup>15</sup> He did not abandon the natural experiment idea of his teacher Park, stressing the importance of the Anthropological Laboratory being “on site” and near at hand to the phenomena of interest (in this case, Native American social life) (Mekeel 1940; Redfield to Mekeel, no date; Redfield, “Outline of a Series,” no date). But by applying Park’s idea of the city as a social laboratory to the existing physical structure of the Santa Fe Laboratory of Anthropology, Redfield mobilized in organizational design and methodological practice a concept that with Park never really guided method (Redfield 1927; Redfield 1929).

In another context, we find a set of “laboratory” references that were primarily heuristic but were nevertheless conceptually sharp—far more so than some of the primarily performative references. “Laboratory” as heuristic appears in many textbooks, and pedagogical claims—those made in the context of discussions over how to teach sociology in the classroom—turn out to be among the most detailed expositions of “laboratory” and its conceptual associations (e.g., Vincent 1905; Cutler 1911; Taylor 1923; Eliot 1924; Melvin 1925; Weatherly 1926*b*, pp. 333–337; Hart 1927, p. 10; Jocher 1928; Carr 1929; Harper 1930).

In his paper on “Laboratory Work in Rural Social Problems,” Melvin (1925) distinguishes between types of laboratory and the work that can be done within them, as



well as between a “narrow” and broader (i.e., heuristic) sense of “laboratory method.” Ulysses Weatherly’s pedagogical discussion appears in a passage on formal education embedded in a book (*Social Progress—Weatherly 1926b*) that is not itself a textbook, but it is still a clear example of pedagogical writing. Weatherly criticizes the tendency of methodological teaching to induce rote behavior and uncritical thinking on the part of students while acknowledging the “high achievement in research” of “the laboratory method” (p. 334). This attitude is quite distinct from Weatherly’s two *AJS* papers (1923; 1926a). Neither of those papers engages critically with the epistemological or practical limitations of “laboratory method” as such, although both embrace the notion of the found natural experiment as an object of laboratory study.

On the occasion of the completion of the “first year of experimental sociology” at the University of Michigan (Carr 1929, p. 63), Lowell Carr observes that laboratories create epistemological problems quite apart from any that they might solve. He asks rhetorically, “how can you induce ‘normal’ interactions under the highly artificial conditions of a laboratory?” (p. 66). Building on the premise that sociology is the study of human interactions, which are instrumental when naturally occurring, Carr continues, “no other question points so clearly points to the contrast between a psychological experiment and a sociological one.” Face-to-face interaction cannot be recreated in a laboratory as it naturally occurs, and group interaction only compounds the problem. Carr balances a serious respect for the disciplinary project of formulating laboratory methods (particularly the behaviorist psychology-inspired study “interstimulation and response,” p. 65) with awareness that laboratory methods are not only limited but also inherently limiting in ways that other heuristics need not be. He notes that “empiricists are in full cry and

behaviorism is a power in the land” at the time of his writing (p. 65). While he embraces the application of behaviorist-experimental results to the experimental classroom in sociology, he does not fully commit to the ontological priority or epistemological soundness of what this approach can produce.

There are striking differences in clarity and detail between such pedagogical renderings and the ambitious, early attempts to Small and Giddings to advance general social theory through deployment of the laboratory concept. Giddings adopts a causal and teleological view of society (1914[1896]), and he infers that the correct analytic lens for sociology is a search for causal mechanisms aiming at some teleological end. He explicitly invoked the laboratory ideal in response to a survey by Luther Bernard, published in 1909 in *AJS*: “we need men...to give us *exact studies*, such as we get from the psychological laboratories, not to speak of the biological and physical laboratories” (Giddings, quoted in Bernard 1909, p. 196). Reproducibility of experiment and the systematic reduction of complexity—such that sociology is neatly continuous with behaviorist psychology—are both central to his vision of laboratory work in sociology, but he is unable to translate this expanded view of the laboratory context into innovative experimental designs. His “scheme of inductive method” assumes both the epistemic validity of empirical induction and inferential leaps from “behaviors” observed in the lab to the nature of individuals to the organismic social whole. Small was equally enthusiastic about analogy to the natural sciences, albeit without the enthusiasms for predictive power and atomic reduction that motivated Giddings. The notion of society as an organism and the pursuit of social control are persistent themes, but he has little to point to in the way of really promising developments towards laboratory social science. Even Small’s pedagogical contribution is

less compelling than many others on this front: his textbook with George Vincent is expressly presented as a “laboratory guide” to sociology (1971[1894], p. 15), but there is little content to this reference beyond the notion that, since the exact sciences are practiced in laboratories and sociology is on its way to becoming an exact science, sociology will be a laboratory science.

In the preceding paragraphs I have stressed the low level of substantive agreement around the meaning of “laboratory” in sociology but the high level of agreement in principle that making sociology into a laboratory science was a worthy undertaking for the discipline. I have focused on three features of the intellectual landscape of early sociology as related to the discipline’s reliance on laboratory talk: the increasing specialist separation of social reformers and professionalizing sociologists; the differences and occasional rhetorical movements between the notions of controlled experimental space and found natural experiment; and the use of “laboratory” as a teaching concept in pedagogical texts. I have noted the apparently incongruous fact that the broadly shared enthusiasm for the idea of sociology as a laboratory science did not produce detailed conceptual expositions of the meaning of “laboratory” or “scientific method”—the most we have in this respect are some pedagogical texts that offer in-depth explorations of the conceptual associations of “laboratory” and discussion of how to implement laboratory work in the classroom.

The simultaneous enthusiasm for the laboratory concept and lack of careful exposition of it can be best explained with reference to the performative power of language. As Gieryn and Latour suggest, there was something legitimizing about talking about objects of social research as existing within “laboratories,” even if there was no clear consensus as to what “laboratory” meant. As Donald Levine (1985, pp. 217–220) has noted

in another context, key concepts in scientific discourse have often been productive because of, not in spite of, their basic ambiguity. That there is no prominent text anywhere in early American sociology questioning the coherence or the central meaning of “laboratory” or “science,” even while “laboratory” and “science” were continually invoked by scholars in divergent ways, is a testament to the power of linguistic signs to shape behavior even when they are not fully understood.

A skeptical reader may question whether early American sociologists really intended to make their discipline into a laboratory science, or whether on the other hand I have taken an unusual turn of phrase too seriously. The answer to this question of course depends on how we define “laboratory” and “laboratory science,” but under a sufficiently diffuse definition of “laboratory”—one that respects the diversity of conceptual associations that the early sociologists themselves worked with—many of them really did intend to make their discipline, in one sense or another, into a laboratory science. The authors of the texts discussed in this paper give no hint of being insincere, even when they were inattentive to the conceptual ambiguities of “laboratory.” Attention to the widespread use of the word and its several associated concepts helps us to understand better their scientific ethos and the many live questions that, for them, still surrounded scientific method and epistemology.

#### **IV. Conclusion: Scientific ideals and the historiography of American sociology**

I began this paper by addressing a lacuna in the history of American sociology: the lack of attention to the many invocations of “laboratory” in the discipline’s early history. The work I have done to fill this void invites several avenues for further research. First and perhaps most obviously, this paper invites us to think about the role—or lack of a role—of

a laboratory ideal in the other social scientific disciplines. As they developed through the early years of the twentieth century, the professional identities of the social science disciplines were interdependent and unsettled. Although I have focused here on sociology, scholars could do important work by looking at the other disciplines. The absence of laboratory talk in economics (which nevertheless managed to gain a widespread reputation as the most scientific of the social sciences) and the emergence of laboratory-based social psychology on the boundary of psychology and sociology, for example, might both be explained by expanding on the thematic work of this paper.

Second, the performative significance of other key words in social scientific discourse might be productively analyzed. I have argued here that the laboratory ideal in sociology shaped behavior, but it did so only in a circumscribed way. Laboratory talk served as a unifying ideal and as a legitimation tactic in most instances, but only when the “laboratory” referent was clear and the description was oriented to practical work or to teaching—as with Redfield writing about the Anthropology Laboratory in Santa Fe or Carr’s paper in *Social Forces*, for example—were there immediate consequences for method. As Jennifer Platt (1996) shows in her history of sociological research methods from 1920 to 1960, there was often significant disparity between methodological precepts and methods in practice. Other key words might turn out to be more significant for the history of social scientific practice. Jewett (2012) has already demonstrated that the definition of “science” adopted by the “scientific democrats” had major practical consequences, and Daston and Galison’s (2007) investigation of “objectivity” in the natural sciences comes to an analogous conclusion. Sustained examinations of the performative

efficacy of “experiment,” “model” or “objectivity” in social sciences, for example, would be of value to historians.

Third and finally, my paper invites researchers to reassess the standard historiographical periodization of American sociology. The discipline certainly looked very different in 1930 as against 1890, and there are few generalizations that can be sustained across the entire period. But a persistent use of the rhetoric of “laboratory” is one of them. The first references to New York City as sociological laboratory come no later than 1894 (Recchiuti 2007), and Park wrote “The City as a Social Laboratory” in 1929 (inaccurately but successfully presenting this heuristic as a new idea). Small and Vincent introduced the idea of the textbook as a laboratory guide in the very first textbook in American sociology in 1894, and papers on classroom instruction in sociology employed the “laboratory” motif throughout the 1920s (Taylor 1923; Eliot 1924; Melvin 1925; Jocher 1928; Carr 1929; Harper 1930; cf. Chapin 1920, which focuses on field work rather than the laboratory). The notion of the social settlement as social laboratory developed contemporaneously with, and was mutually reinforcing with, the notion of the city at large as a laboratory. Laboratory talk and certain key conceptual associations with the word “laboratory” were present in US sociology consistently from its beginnings up to 1930. Large-scale numerical data supports the same point. In a keyword analysis of *AJS*, the word “laboratory” appears at a steadily increasing rate from 1895 to 1930 (with spikes in 1895, 1902, 1904 and 1921, corresponding to individual articles that use the word at a much greater frequency than average). The percentage of articles per year in which the word “laboratory” appears at least once remains roughly constant over the period, with fluctuations early on that flatten out in the 1920s (see Appendix I).

Many historians have seen the 1920s as a historiographic turning point for American sociology, citing a growing concern with scientism that flourished in that decade (see especially Ross 1991). Jewett (2012) has shown that intensive debates around the meaning of science within the social sciences were continuous over a very long time and were far more multivalent than a debate about value-neutrality alone. My own narrative supports this broad perspective on the debate—questions about the meaning of science were not new in 1920 and nor were the conceptual solutions proposed.

More dramatic changes from 1890 to 1930 are to be found in the technologies and material practices of sociology than in the conceptual associations of “laboratory” and its rhetorical significance. In the period 1890 to 1930, the terms of debate about “laboratory” and its referential uses remained relatively constant, but important innovations were made in the actual practice of sociological work. Statistical techniques grew steadily more sophisticated, and the 1920s saw at least the beginnings of participant observation and survey research as codified methodologies (as against the earlier social survey work of Charles Booth in London and the Pittsburgh Survey, for example) (Platt 1996, pp. 44–47). The concept of “social control” appeared prominently in the writings of E.A. Ross as early as 1901 (Ross 1901; 1905, pp. 267–271; see also Ross 1991, p. 230ff.; Jewett 2012, pp. 120–123), but the idea was not exploited for its potential experimental implications in the United States until Floyd Allport’s social psychology experiments of the 1920s (Allport 1920; 1924; Danziger 2000, p. 332ff.). We must indeed be sensitive to changes over time in early sociological research practices, especially those that might broadly be called “experimental.” But discussions of so-called “laboratories” in the writings of early American sociologists retain much the same character from 1890 through 1930.

V. Appendix I

Figure 1: Rate of “laboratory” references per 10000 words

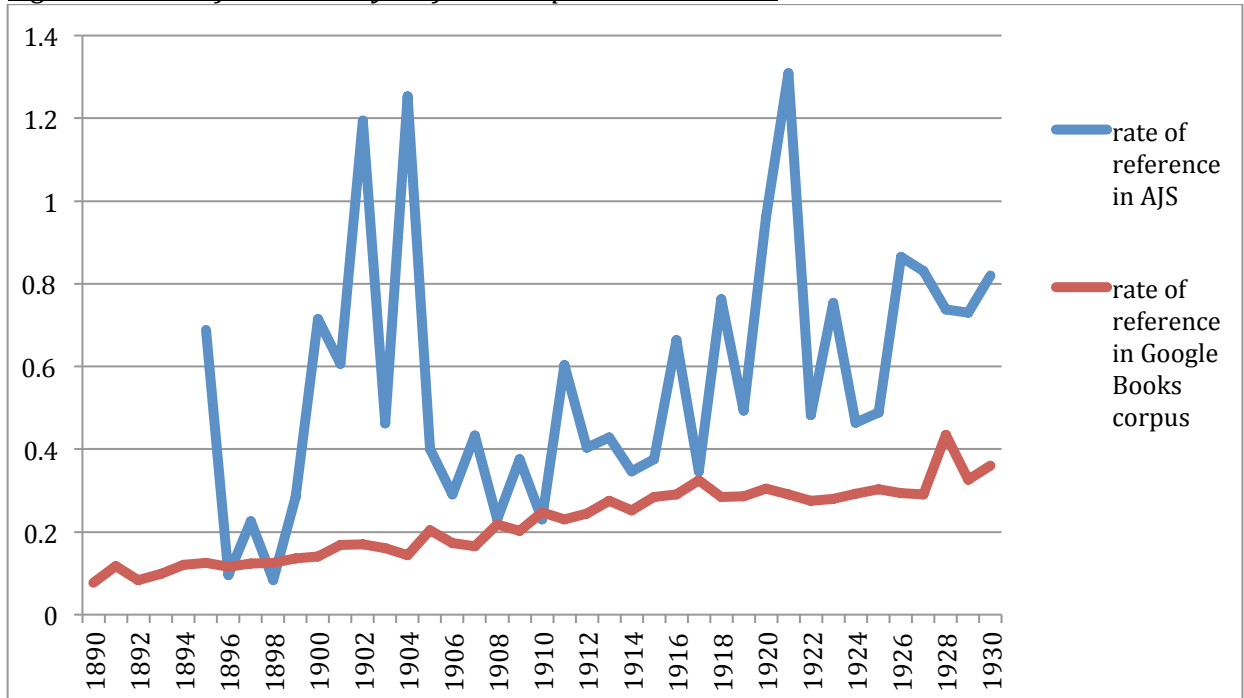
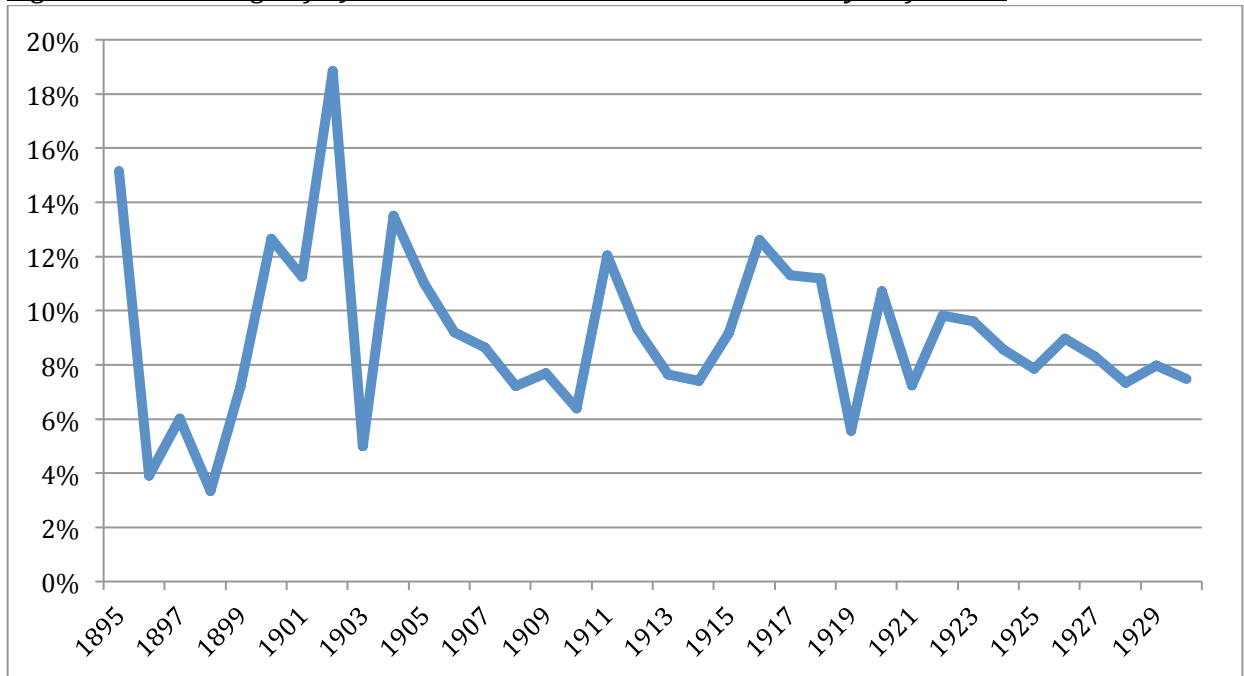


Figure 2: Percentage of AJS articles with at least one “laboratory” reference



AJS articles were culled from the JSTOR database. Total word count was estimated from total page count by year and an estimate of words per page. With a few exceptions,



*AJS* articles as stored in JSTOR are text-searchable, so accurate counts of word occurrences in the text are available for most years.<sup>16</sup> Figure 1 shows the aggregate rate at which keywords “laboratory” and “laboratories” appear in *AJS*.

Data from the Google Books corpus—a database of millions of English-language books from all genres, which gives an estimate of the overall rate of use of particular words and phrases—is available at <https://books.google.com/ngrams>. The Google Books trend line in Figure 1 was produced by aggregating the case-sensitive records for “laboratory,” “Laboratory,” “LABORATORY,” “laboratories,” “Laboratories” and “LABORATORIES.” Common OCR errors (e.g., “laboratoy”), misspellings and unusual capitalizations are excluded but would change the results only trivially.

Counts of *AJS* articles with at least one “laboratory” reference and counts of total articles per year for Figure 2 are again taken from the JSTOR database.

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Table 1: Conceptual associations with “laboratory”

<b>“laboratory” conceptual associations</b>	<b>examples and sources</b>
control over objects	“The laboratory affords an ideal condition for experimentation and investigation. Variant and disturbing factors can be eliminated or controlled” (Melvin 1927, p. 206)
control over subjects	<p>“How can you induce ‘normal’ interactions under the highly artificial conditions of a laboratory?” (Carr 1929, p. 66)</p> <p>“There are...a number of laboratory experiments on the personal responses to emotional expression of the face” (Young 1927, p. 955)</p>
accessibility of “natural experiments” to observation	“The West Indies as a Sociological Laboratory” (Weatherly 1923)
constructed, artifactual sites of observation	“A workshop par excellence it [the Social Science Research Building, University of Chicago] certainly is from basement to roof, and it includes specifically a laboratory for anthropometrics and archaeology...a psychological-psychiatric laboratory, and an extensive statistical laboratory” (Smith and White 1968[1929], p. 25)
methodological transparency and precision	“The manual has been referred to in another place as a ‘laboratory guide.’ This description indicates the conception of the authors, and the nature of the recommendations which they would offer to teachers” (Small and Vincent 1971[1894], p. 15)
value-neutral method	“The sociologist must proceed with the same spirit of disinterested objectivity with which the natural scientist works in his laboratory” (Wirth 1926, p. 464)
objective knowledge production	“The laboratory affords an ideal condition for experimentation and investigation...Laws and norms are derived from facts secured under these controlled conditions” (Melvin 1927, p. 206)
exhaustive or “total” knowledge production	“The recent introduction of the “laboratory method” in sociology is a guaranty that no department of human life will be



	<p>neglected” (Henderson 1896, pp. 396–397)</p> <p>“A [child-guidance clinic] can enter directly into the lives of only a limited number of children. As a separate entity, it is at most a small laboratory working with a few children, but working with them in a more comprehensive way than any other organization” (Truitt 1926, p. 335)</p> <p><i>Cf.:</i> “The psychoanalytic psychology owes its vogue to its boldness in disregarding the rather narrowly spatial methods within which laboratory psychologists were confining themselves” (Cooley 1926, p. 70)</p>
<p>reliance on findings, methods or standards of proof in the natural sciences</p>	<p>“Sociological research is at present in about the situation in which psychology was before the introduction of laboratory methods, in which medicine was before Pasteur and the germ theory of disease” (Park and Burgess 1969[1921], p. 44)</p> <p>“An introductory course in sociology is not going to be more scientific...if it merely apes the ritual of the laboratory disciplines” (“Review of <i>An Outline and Notebook,</i>” 1927, p. 863)</p>
<p>predictive power</p>	<p>“From the results of this laboratory work the school has announced what it defines as a criminal type, and asserts that all born criminals have characteristic anomalies either physical or mental” (Kellor 1899, p. 517)</p>

Table 2: Selected “laboratory” referents<sup>17</sup>

selected “laboratory” referents	sources
New York City	“Report on a Department of Social Science at Columbia College,” 1894 (quoted in Recchiuti 2007, p. 3)
“The nation” (as “meeting ground of varied races and nations”, a laboratory for the ethnologist)	“The American University” (Hirsch 1895)
Chicago	“Scholarship and Social Agitation” (Small 1896)  “The City as a Social Laboratory” (Park 1968[1929])
Edinburgh Outlook Tower observatory	“The World’s First Sociological Laboratory” (Zueblin 1899)
The social settlement	Settlements as “laboratories in Social Science” (Robert Woods, quoted in Recchiuti 2007, p. 92)  <i>Cf.</i> : “The settlement should never be looked upon as a statistical laboratory.” “Scientific value of the social settlements” (Hegner 1897)  <i>Cf.</i> : “I have always objected to the phrase ‘sociological laboratory’ applied to [Hull-House], because Settlements should be something much more human and spontaneous than such a phrase connotes.” <i>Twenty Years at Hull-House</i> (Addams 1910, p. 308)
West Indies	“West Indies as a Sociological Laboratory” (Weatherly 1923)
The Preschool Psychological Laboratory at the University of Iowa	“Topical Summaries of Current Literature: the Behavior of the Preschool Child” (Pearson 1926, p. 803)
The Social Research Laboratory of the University of Southern California.	“Measuring Geographic Concentration by Means of the Standard Deviational Ellipse” (Lefever 1926)
“The modern cultural situation”	“Recent Literature” (Goldenweiser 1926)
“The child-guidance clinic”	“Recent Literature” (Truitt 1926)
“The rural community”	“Research in Rural Sociology” (Taylor 1927, pp. 218–219)
A class of sociology students	“Review of <i>Children’s Thinking</i> ” (Pearson

	1927)
Rooms within the Social Science Research Building, University of Chicago	<i>Chicago: An Experiment in Social Science Research</i> (Smith and White 1968[1929])
The Santa Fe Anthropology Laboratory	“Laboratory of Anthropology” (Redfield, no date)
Experimental sites for psychology (behavioralist or structuralist), psychiatry and statistical work	Numerous

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<sup>1</sup> The founding of *ASR* as an official journal of the American Sociological Association negated the University of Chicago Sociology Department’s position in control of the discipline’s sole flagship journal, the *American Journal of Sociology*.

<sup>2</sup> Prominent examples include the Yale Institute of Human Relations (established 1929), the Columbia Bureau of Applied Social Research (1942) and the Harvard Department of Social Relations and associated Laboratory of Social Relations (1946).

<sup>3</sup> Examples include *The Authoritarian Personality* (Adorno et. al. 1950), a study commissioned by the American Jewish Committee, and four volumes of “studies in social psychology in World War II” prepared by members of the Social Science Research Council and funded by the Carnegie Corporation, now remembered as the *American Soldier* study by Samuel Stouffer (Stouffer et. al. 1949*a*; Stouffer et. al. 1949*b*; Hovland et. al. 1949; Stouffer et. al. 1950). This foundation-driven work—much of it occurring in newly designed, laboratory-like spaces (see Isaac 2012 for a case study)—was absent in the earlier period.

<sup>4</sup> A partial exception to this second point is the literature on the Chicago School, some of which has noted that the theorization of space, particularly the city of Chicago, was a central concern of one institutional subset of early sociologists (Abbott 1999, pp. 193–222; Venkatesh 2001; Gross and Krohn 2005; Gieryn 2006; Owens 2012). Gieryn in particular makes something of the conceptual content of “laboratory,” noting a “rhetorical oscillation” between describing the city as “laboratory” and as “field site,” which served as a legitimation tactic for the Chicago sociologists.

<sup>5</sup> Thomas Gieryn and Bruno Latour are two exceptions. I address their interventions on this question below.

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<sup>6</sup> The method for map-making outlined in Ripley 1899 is said to be “easily within the powers of any student of statistics” (p. 326).

<sup>7</sup> The discipline of statistics was also a source for some of the conceptual ambiguities that accompanied ambitious formulations of laboratory thinking in sociology. The major innovators in nineteenth century probability and mathematical statistics, notably Karl Pearson, Francis Galton and Adolphe Quételet, provided building blocks from which many subtly different variants on the notions of cause, probability, prediction and control could be constructed, and the early American sociologists did not all follow the same recipe in combining their ideas. Notions of causality (Abbott 1998, pp. 150–159) and interpretations of probability that undergird understandings of predictive power and control were disputed (as they still are). Giddings’s dual reliance on Pearson and J.S. Mill’s logic of inference, for example (see Giddings 1914[1896], p. 400ff.; 1901, pp. 11, 57ff.), was just one possibility among many.

<sup>8</sup> The sites of the earliest psychological laboratories in the US were: Johns Hopkins (1883); Indiana University, University of Pennsylvania, University of Wisconsin (1888); Clark University, University of Nebraska (1889); Columbia University, University of Iowa, University of Michigan, University of Toronto (1890). The data used here come from a “native informant” surveying administrators and scientists when laboratory psychology was still a young science (Garvey 1929). Harvard had an informal psychological laboratory from the 1870s and a more formal institution from 1890 or 1891, both under the leadership of William James.

<sup>9</sup> James’s review of Wundt in *Science* is unsigned, but it is attributed to James at least two compiled volumes (James 1987; Bringmann and Tweney 1980).

<sup>10</sup> Cooley wrote that positivistic methodology was a “false ideal inconsiderately borrowed from the provinces of physical science” with no place in social science (1966[1918], p. 398; see also

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Hinkle 1980, p. 99). Florian Znaniecki is another interesting case. With W.I. Thomas, Znaniecki was the author of *The Polish Peasant in Europe and America*, one of the great early classics of humanistic sociology, which also included a long “Methodological Note” justifying the use of “personal documents” in sociological analysis. Znaniecki later published two papers in the *American Journal of Sociology* (1927; 1945) lamenting methodological confusion in sociology and the exclusion of “cultural facts” by a “logical system” that privileged the analysis of “natural facts” (1927, p. 535). Yet at the same time Znaniecki demonstrates a practical willingness to work within the methodological standards that the discipline at large had established.

<sup>11</sup> In a contemporary effort to give an explicit definition to “laboratory,” Karin Knorr Cetina goes some way towards combining in a single definition these two apparently contradictory concepts. She defines “laboratory” as a place where one works with objects that have been somehow “purified”: they need not appear in the laboratory *as, when* or *where* they appear in nature. Furthermore, objects and social order are not merely changed in the laboratory; they are “upgraded...in a sense,” rearranged by concerted effort to “derive epistemic effects from the new situation” (Knorr Cetina 1999, pp. 26–28).

An interesting feature of this definition is that its two central features, control over objects and heightened epistemological availability of objects, are somewhat in tension with one another. We only seek experimental controls to make inferences about things we are interested in but cannot observe (see Martin 2011, p. 57, fn.37). If laboratories were somehow able to make visible unobservable causal processes (whether in the form of mechanisms, functional relations, fields or something else), then researchers would have little interest in running experimental controls. Conversely, if the rearrangement of laboratory objects to “derive epistemic effects” was so simple that all possible worlds of interest could readily be played out at low cost, then there

would be little or no need to produce “purified” and simplified laboratory objects. This inner tension goes unobserved in the writings of the early American sociologists and among contemporary science studies scholars.

<sup>12</sup> For a review of ecological thinking and the role of “ecology” as a “trope” in Park’s writings, see Gaziano (1996).

<sup>13</sup> See also Park (1925). In this letter to McKenzie on the sessions and sub-themes to be represented at the meeting, Park did not suggest “the city as a social laboratory.”

<sup>14</sup> Although he has since become more closely associated with the discipline of anthropology, Redfield completed a dissertation in 1928 under Park’s guidance in Chicago’s then-joint Department of Sociology and Anthropology.

<sup>15</sup> Although some of the crucial documents in Redfield’s papers are undated, it seems clear that his engagement with the Santa Fe laboratory came after he had established his field sites in Mexico, in Tepoztlán and in the Yucatan peninsula for his dissertation work.

<sup>16</sup> Exceptions from the years studied are the issues: 1:1 (1895), 2:1 (1896), 5:1 (1899), 33:1 (1927), 34:4 (1929), 35:1 (1929), 35:5 (1930) and 36:1 (1930). The data points for these six years in the *AJS* trend line in Figure 1 thus may be underestimates.

<sup>17</sup> Attention to the word “laboratory” and the set of referents for which it stood in early American sociology shed a distinctive light on the foundational epistemological and methodological concerns of the discipline. However, to focus *too* narrowly on this single word—as a keyword out of context in simple text searches, for example—would be to lose sight of some phenomena of interest—both things-in-the-world and concepts—that are related went by other names. Other words—“museum,” “library,” “field site,” “experimental station,” “controlled experiment,” “observatory,” “study,” etc.—were sometimes invoked to convey some of the same conceptual

content as “laboratory” and to refer to similar working spaces relevant to the young discipline of sociology. While the two tables displayed here draw examples only from texts that use the word “laboratory,” the examples could be multiplied if we looked for the same conceptual senses when they are associated with other words. Those listed above (“museum,” etc.) can be found in use with similar conceptual associations and occasionally with the same referents as “laboratory.”