Abstract: How Political Science Became Modern

This dissertation argues that changing ideas about race and engagement with race science were at the heart of a major transformation of political science in the 1920s, a transformation that I characterize as "becoming modern." This transformation was at once conceptual—visible in the basic categories and theoretical apparatus of the discipline—and institutional—affecting the daily practices and institutional setting of political science.

For the Gilded Age political scientists who built the first Ph.D. programs in the United States, historical development *was* racial development; political destiny *was* racial destiny. By the1930s, however, "the political" had come to appear largely autonomous. It was no longer a function of nature or unfolding historical essence, but rather a human creation and therefore subject to rational management. I show that political scientists effected this transformation in large part by engaging a parallel and roughly contemporaneous transformation in racial thought. Specifically, it was by thinking through the Boasian critique of evolutionary anthropology that political scientists produced a "modern" conception of politics, delinked from notions of racial development.

However, this is not a straightforward story of progress in which shedding prejudice leads to scientific advance. I show that these same interwar political scientists were deeply attentive to developments in "mental measurement" and eugenics. Of greatest interest to them were attempts to specify the capacities and limits of racial and other groups within the population, such as the World War I Army intelligence-testing program and successive attempts to create psychological and physiological tests that could measure capacities or predict responses. Animated by the possibility that citizens' capacities could be quantified and that this knowledge could be used to reform politics, influential political scientists worked to forge intellectual and institutional links with race science, including extreme figures within the eugenics and immigration restriction movements. This was particularly true in the early moments of institutional establishment of the discipline within a larger infrastructure for social science, as with the founding of the Social Science Research Council. As a result, I argue, this cohort did not so much abandon "race" as open space within modern political science for ascriptive hierarchy re-described in liberal terms.

What emerged was a vision of the political as an independent realm conditioned by the "facts" of citizens' natural capacities. In my view, this vision has been both productive and limiting for the discipline, suggesting research programs that we still pursue but at the same time closing off other areas of inquiry.

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How Political Science Became Modern:
Racial Thought and the Transformation of the Discipline, 1880-1930

by

Jessica Blatt

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Submitted to the New School for Social Research of the New School in partial fulfillment of the requirements for the degree of Doctor of Philosophy.

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Table of Contents

Key to Archival Sources v

1. Introduction: Modernism and The Political 1

2. "The Aryan Genius for Political Civilization:" John W. Burgess and the Founding of American Political Science 27

3. "To Bring Out The Best That Is In Their Blood:" *The Journal of Race Development* (1910-1919) 71

4. "To Lay These Specters To Rest:" Political Science Encounters the Boasian Critique of Racial Anthropology 100

5. "An Ounce of Eugenics:" Intelligence Testing and The Image of Democracy 130

6. "The Direction We Should Like To Go:" Race and the Institutional Establishment of Political Science 169

7. Postscript: Merriam's Legacy, "Race Relations," and Scientism in American Political Science 215

Works Cited 226

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Key to Archival Sources

CEM Papers:	Charles E. Merriam Papers, University of Chicago Library, Department of Special Collections, Chicago, Illinois
MVK Papers:	Mary Van Kleeck Papers, Sophia Smith Collection, Smith College, Northampton, Massachusetts
Oral History Project:	American Political Science Oral History Project, University of Kentucky, Special Collections and Archives
RAC-LSRM:	Rockefeller Archive Center, Laura Spelman Rockefeller Fund Collection, N. Tarrytown, New York
RAC-RF:	Rockefeller Archive Center, Rockefeller Foundation Collection, N. Tarrytown, New York
RAC-SSRC:	Rockefeller Archive Center, Social Science Research Council Archives, N. Tarrytown, New York
RMY Papers:	Robert M. Yerkes Papers, Manuscripts and Archives, Yale University Library, New Haven, Connecticut

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1. *Introduction: Modernism and The Political*

The interwar era was a heady time for American political scientists. Many thrilled to the sense that they were reinventing their discipline. A new rigor, new energy, and greater technical capacity were thought to be transforming the practice of political science and the knowledge it produced. It seemed that an endeavor freighted with tradition was giving way to objective, empirical modes of inquiry and, most important, a new recognition of the dynamism of the political world. Most tantalizing, many political scientists felt that they were on the verge of developing knowledge that would allow for rational "social control" amidst the apparent chaos of the early twentieth century.

The wildest hopes of these scholars for social control have come to seem quaint, but disciplinary lore has tended to agree, at least, that something significant was going on. Robert Dahl, in his memoir of a subsequent "revolution" in political science, identifies a move toward empiricism in the 1920s as a source and condition of possibility for the emergence of behavioralism decades later (1961: 763). For Gabriel Almond, the interwar years saw the "flowering" of a particularly "sophisticated" "social-psychological, quantitative approach" that had an enormous impact on political science as it was taken forward by his generation.[1] And in their seminal history of the discipline, Somit and Tanenhaus identify 1921 as the beginning of a crucial "middle period" in the development of American political science, characterized by a "momentous effort to move the profession toward a 'science of politics'" (1967: 87, cf. Ross 1991: 452).

[1] Transcript of September 20, 1978, interview by Richard Brodie, Oral History Project.

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Science, empiricism, methodological innovation: these are the buzzwords associated with interwar political science in the United States, particularly the remarkably ambitious and productive (particularly of subsequently famous political scientists and social science institutions) “Chicago School” led by Charles E. Merriam. Even John Gunnell, an emphatic dissenter to the reigning sense that the 1920s saw something akin to paradigm change in American political science, concedes that “hold[ing] Merriam's post-1920 work up against the arguments” of earlier American political scientists “always seems to evoke a sense of contrast," and credits interwar scholars with methodological if not major conceptual “refinement” (1992: 134).

But how should we understand this period? As Gunnell points out, an earlier generation had made similar claims to scientific rigor for itself. John W. Burgess, who founded America’s first doctoral program in political science at Columbia in 1880, had ambitions much like those of Merriam and his cohort: to forge a professional, scientific discipline that would bring rigorous methods to bear on the pressing political questions of the day. Also similar was a sense that momentous social and political change fueled the need for these new methods and approaches. For the “Chicago School,” industrialism, the growth of the state, immigration, and the war in Europe had revealed a world of interconnections and rapid change to which previous theory seemed inadequate. For the scholars of Burgess’s generation, it had been the destruction of the Civil War and the task of building a unified nation in its wake, coupled with the closing of the frontier and the possibility of imperial

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expansion, as well as an earlier wave of immigration, that called for new understandings of democratic legitimacy and the tasks of governing.

But where is the real difference then? Is it “only methodological,” a simple story of technical advances offering scholars new tools? Or were political scientists in the 1920s doing something qualitatively different?

I argue that the evidence points to qualitative novelty. Specifically, I argue that political scientists in the 1920s began to forge a professional language and a conception of the relationships between history, politics, and nature that were distinctively “modern.” Moreover, I argue that “race” was central to this transformation. That is, political scientists forged this new language and these new conceptions by rethinking earlier, hegemonic understandings of race in light of new developments in allied disciplines, particularly anthropology and psychology. They also sought institutional connections with these disciplines as they worked to increase the prestige of and resources available to professional political science.

Professional, formal study of political science was established in the United States in the wake of the Civil War with the founding of the School of Political Science at Columbia University. For the scholars who founded the discipline, political development and racial evolution were inseparable. Gunnell expresses a sense common among disciplinary historians when he identifies Columbia’s Burgess as “the figure who “[m]ore than anyone else…established the disciplinary, professional, and intellectual foundations of [American] political science” (2004: 73). In Burgess's elaborate framework of macrohistorical development, American political institutions were both the apogee of the development of civilization to date and the

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fruit of a "Teutonic germ" carried and developed by the young nation's Anglo-Saxon settlers. Understanding how those institutions should work meant discerning the patterns revealed by the history of Teutonic peoples; safeguarding them in their proper state and within their proper bounds required ensuring that Anglo-Saxons remained the "dominant" element in American society (1890, 1891).

By 1925, however, American politics had come to the president of the American Political Science Association (APSA) (himself a former Burgess student) to look like part of a "new world" (Merriam 1972 [1925]: 85)—cut off from organic or spiritual evolution, taking on a life of its own with its own dynamics. These dynamics could no longer be understood as a function of their origins and long-term historical development. Rather, rigorous empirical investigation of the present workings of government and the measurable characteristics and behaviors of populations would reveal the underlying forces governing modern political life. Nature and politics had become separate (though still interacting) spheres, each to be studied on their own terms and with their own methods. History, in turn, had lost much of its explanatory force; the present appeared much less the product of teleological or long-term evolutionary processes, and more as arising from "new tendencies" (idem).

In one sense, then, political science became modern in the 1920s in that political scientists shed much of their antiquarianism and started doing things and talking in ways more like they do today. They also, as we will see, started doing so within a new set of institutional arrangements. But I use the term "modern" to indicate that the new orientation guiding political science exhibited a number of

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specific characteristics that scholars have discussed in terms of "modernism" or a "modernist impulse" in the human sciences. Key to this change was a new orientation toward history and a transformed understanding of the relationship between nature and politics, as well as of how knowledge of each could be gained.

As David Hollinger pointed out in a classic 1987 essay, the term "modernism" takes one in a number of apparently contradictory directions. Aesthetic modernism has been identified with alienation, the celebration of subjectivity and uncertainty, and the embrace of artifice—not exactly an easy fit with the near-fetish of science, precision, and objectivity that we associate with the transformations of the human sciences in the early twentieth century. However, the "modernism" of the canonical literary and artistic modernists (or, in Hollinger's shorthand, "the Bloomsbury aesthetes") shares much with the philosophy of "scientistic" intellectuals of the early twentieth century who believed that exact knowledge, rationally deployed, could solve social conflicts and produce civic agreement. That is, both groups

> sometimes recognized that knowledge of the external world was not so easy to come by, that contradictions persisted in human experience that a real price was paid for the benefits of bureaucratic rationality, that a large measure of uncertainty was an enduring condition of life, that human beings had a propensity to act irrationally, that it was difficult to find an unchanging standard for moral judgments, that God might be dead, and that many sensitive individuals felt alienated from modern society (Hollinger 1994: 27-28).

For Hollinger, while "the aesthetes" responded to this predicament by deploying "strategies of artifice," many of their contemporaries turned to "strategies of reference." In this latter strategy, recognition of the subjectivity of human knowledge did not mean "giv[ing] up on the effort to organize culture around science" (28).

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Rather, it called for vigilance and techniques to guard against error. If the individual was subjective and fallible, perhaps finding new ways to define objects and ground knowledge could yield what Theodore Porter has called "mechanical objectivity" (1995). For Hollinger, this "cognitive modernism" became one of the dominant cultural ideals of the twentieth century, and formed the basis for the self-image of the social sciences as they developed as university-based enterprises (op. cit.: 29).

Dorothy Ross, too, identifies a "new understanding of the subjectivity of knowledge," as an essential component of what she calls the "modernist impulse," and agrees that where aesthetic modernism responded by devaluing rationality in favor of creativity, a complementary impulse, analogous to Hollinger's "strategies of reference," was to ground knowledge "in a universalistic method or logic that would preserve the privileged status of science" (1994: 2). Crucially, in both strategies, the past suffered a similar demotion. According to Ross, "Modernists ruptured the historicist continuity between past, present, and future, dissolving history into the transitional present and its subjective experience." No longer the source of truth and value, history gave way to a sense of "perpetual transition" (1994a: 172). For the emerging social sciences, this meant abandoning historicism in favor of rigorously controlled, "short-term study of the process of change" (1991: 318-319).

Bruno Latour also links the modernism of the social sciences, or in his terms "the modern critical stance" (1993:11), to a changed understanding of the significance of time and history. As he puts it,

> [M]oderns have a peculiar propensity for understanding time that passes as if it were really abolishing the past behind it They all take themselves for Attila, in whose footsteps no grass grows back. They do not feel that they are

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> removed from the Middle Ages by a certain number of centuries, but that they are separated by Copernican revolutions, epistemological breaks, epistemic ruptures so radical that nothing of that past survives in them—nothing of that past ought to survive in them (68).

That is, "moderns" for Latour are we who see ourselves as liberated from a past that does not matter. But "modernism" is not only a present-and-future orientation. Crucial to being modern is to understand others, people we think of as "premoderns," to be living in continuity with the past, and with nature.

For Latour, the essential move of the modern critical stance is one of "purification," in which "nature" and "culture" are separated, distilled into "pure forms" that can then be seen as acting upon one another. The mistake, for Latour, is that this obscures the "networks" that link the natural, social, and discursive worlds (including discourses about our past). To meaningfully understand these networks, we will need to recognize that "we have never been modern." In Latour's view, this is an ethical and ecological necessity—we must understand the ways society and nature are "doubly constructed" (6) if we are to ensure the survival of both. For my purposes, however, it is enough to note that Latour links modernism with a that sense we can tease apart the realms of "nature" and society or politics, measure the causal weight of each, and as a result gain a measure of control over both.

James Scott (1998) and George Stocking (1982 [1968]) have also linked modernism's abandonment of the past to the separation of nature and culture. Scott, like Latour, sees disaster produced by the modern vision of the world. In its extreme form, "high modernism," this is expressed as a drive to separate, quantify, standardize, and thereby transform the relations between nature and culture—an

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imperative that becomes a central tactic of state power and a source of deeply misguided policy in the twentieth century. In a less critical vein, Stocking identifies the early twentieth-century split between nature and culture as the condition of possibility for anthropology to become an autonomous intellectual endeavor, rather than a subset of biology or an exercise in the "pre-history" of a unitary "civilization."

This dissertation does not seek to join debates about the status or desirability of "modernity," but rather to examine the origins and effects of a particular set of conceptual and institutional changes in political science—changes that appear quite clearly to be a part of the larger trend in twentieth-century intellectual life that these analysts have discussed. (Though, for the record, I do argue that these developments seem to have both benefits and costs for the knowledge subsequently produced by the discipline.) One of the most crucial effects, in my view, is a new notion of the bounds and bases of "the political."

Stocking's observation that anthropology came into its own when it established its own, discrete object in the form of "culture," has parallels in other disciplines, as well. Most vividly, one can see an almost simultaneous development in economics, in which "the multitude of transactions designated (somewhat arbitrarily) as economic were abstracted from the rest of social life and reconstituted as an object, the economy, which behaves according to its own logic" (Breslau 2004: 379). I am making, in effect, a similar claim for political science—that is, that while earlier iterations of the discipline worked in a tradition that Ross (1991) has called "historico-politics," by the second decade of the twentieth century it is clearly possible to see political scientists working out a notion of the political as an

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autonomous sphere that can be studied independently of history, and that perhaps even more importantly is susceptible to reform through the application of expert knowledge. Moreover, I argue that this notion was worked out to a significant degree in a process of engagement with ideas—scientific and popular—about race.

Notably, this story is almost entirely absent from accounts of the discipline's history. A small but growing body of work on the history of international relations has begun to look at how racial thought infused political scientists' conceptions of America's relations to the rest of the world, and particularly how it helped legitimate American imperialism (Ng 1994; Schmidt 1998, 2008; Schmidt and Long 2003; Vitalis and Markovits 2002; Vitalis 2002, 2003, 2008, n.d., Blatt 2004; Henderson 2007), but these accounts are still exceptional enough to prove the rule.[2]

While this is changing to a degree, in general political science is not the most introspective of the social disciplines. In contrast to anthropology and sociology, the historiography of political science has only recently become a serious concern. Most of the major statements about the discipline's past that do exist, many of them generally excellent, either treat the racialism of its founders as a symptom of "the times" and therefore not of exceptional conceptual importance, or, as in some more recent accounts, note and usually deplore a "neglect" of race by students of politics for most of the twentieth century (Almond 1990, Crick 1959, Dahl 1961, Easton 1971, Farr et. al, 1990, Farr 2004, Gunnell 1993 and 2004, Katznelson and Milner

[2] Vitalis's work, in particular, has been an important source of inspiration for this dissertation, among other things first pointing me to the material that I discuss in chapter three, and that was the first original research I did on this topic.

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2002, Ricci 1984, Ross 1991, Seidelman and Harpham 1985, Somit and Tanenhaus 1967).

Expanding on this last theme, since the 1970s a number of (mostly African American) commentators have examined the "puzzle" of what I have come to think of as a kind of "political science exceptionalism"—the relative absence of attention to black and minority politics generally and of an impulse toward racial reform in political science as compared with other social sciences.[3] An early example of this critique is Donald R. Matthews's 1969 essay, "Political Science Research on Race Relations," which observes that "exactly six articles containing the word 'Negro' in their titles were published in the *American Political Science Review* between 1906 and 1963" (113). A more forceful statement of this point, published the same year as Matthews's essay, is Mack H. Jones and Alex Willingham's "The White Custodians of the Black Experience," which charged that "more often than not the black experience [has been] simply ignored" by social scientists, or at best dealt with outside the realm of "fundamental political questions about the nature of society" (1970: 31, 32). In 1983, Matthew Holden Jr. called the study of race "an academic graveyard" in political science. Hanes Walton chose *Invisible Politics* as the title for his 1985 book on black politics; even more explicitly, Ernest J. Wilson III titled an article appearing the same year, "Why Political Scientists Don't Study Black Politics, But Historians and Sociologists Do." And while they reported progress since the

[3] Economics is the only social science that most agree has done worse than political science on this score (Wilson and Frasure 2007).

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1970s,[4] Paula D. McClain and John A. Garcia's contribution to the 1993 volume of *Political Science: The State of the Discipline*, affirms that much of the discipline's history was characterized by a "tacit, if not expressed, agreement that some groups within the American political spectrum were not legitimate subjects for political scientists to study" (247), and quote as evidence statements to that effect by Ralph Bunche (from 1941) and Emmet E. Dorsey (from 1964). More recently, Wilson and Frasure (2007) document that "African American issues" are "still at the margins" of political science (as measured by representation in the discipline's flagship journals) and Walton and Smith (2007) show that sustained consideration of "the race variable" was absent from the major "state of the discipline" volumes published by the American Political Science Association until 1993.[5]

Explanations for this marginalization of black topics have focused on political and epistemological issues as well as methodological ones. Some of the earliest and most political of these explanations come from Mack H. Jones, Alex Willingham, and Martin Kilson. In their 1970 essay, Jones and Willingham presented the discipline's neglect of questions about the lives of black people in America as a consequence and expression of the larger system of racial oppression, and social scientists' inability or unwillingness to critically assess the what Hans Morgenthau called the "basic philosophical assumptions" of a "society based on racial discrimination" (in Jones and Willingham op. cit.: 31). Kilson reinforces this point in 1977 when he argues that political science has been characterized by a "norm gap," a failure to "adequately

[4] Fraga et. al. (2006) note that Latino politics were almost entirely ignored until the 1970s, but also see significant progress since that time.
[5] Another set of writings critically tracks trends in recruitment, employment, and disciplinary status of black political scientists (cf. Woodard and Preston 1985, Preston and Woodard 1984, Rich 2007).

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come to grasp with the *hypocrisy of power* in American society (163, italics original). He also argues that political science has been unable to grasp that "the Afro-American subsystem" is a "special sector" in American politics, a point that Jones amplifies in a 1991 essay where he states that "the irrelevance and disutility of U.S. political science for those concerned with black advancement are explained by the fact that in the United States black and white societies are adversaries" (26).

In his critique of the political quietism of contemporary scholarship Holden argues that American political scientists have not understood "black-white relationships in American society to raise *critical intellectual problems* for scholars, in contrast to raising 'social problems' for 'social activists'" (Holden op. cit.: 34, emphasis original). Walton and McCormick (1997) go farther, arguing that the study of black politics signals a "social danger," a controversial, combative stance that disciplinary convention mandates that "serious" scholars eschew.

Wilson attributes the lack of interest in "issues of black behavior in America" within the mainstream of the discipline to its preoccupation with the realm of elites and "decision-makers" (op. cit.: 600, 604). Tolleson-Rinehart and Caroll (2006) make a similar claim, arguing that the focus on state institutions and those with the capacity to influence them has led political scientists away from questions of inequality, including gender inequality. A number of scholars have also pointed to institutional and methodological segregation in the discipline, with a wide range of black scholars, finding the disciplinary mainstream hostile or indifferent to the questions they asked and the methods they used to answer them, leaving the APSA to work within the

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National Conference of Black Political Scientists (NCOBPS, founded 1969) (Dawson and Wilson 1991, Reed 2003, Rich 2007, Walton and Smith, op. cit.).[6]

Many of these methodologically focused explanations identify behavioralism's inordinate emphasis on the individual as unit of analysis as a hindrance to asking meaningful questions about racial stratification. As Dawson and Cohen write,

> A close examination of the literature dealing with race and politics especially that originating from political scientists…suggests that far from examining the social processes that racialize, categorize, and constrain the life opportunities of different groupings of people in this country, largely people of color, most of this work has focused on individual manifestations of political differences that correlate with visible and self-identified racial differences. Most of this literature takes racial categories as a given.... [and ignore] the historical and social contexts through which the complicated processes of racialization and categorization utilized in this country have developed and evolved (2002: 490).

Although framed differently, this critique in many ways affirms the perspective that commentators like Jones, Willingham and Kilson were putting forward in the 1970s, that disciplinary blinders were preventing political scientists from even perceiving the larger systemic forces at play in racial politics. (With the result that, as Charles V. Hamilton put it recently, that when black protest erupted in the postwar era, "the American political discipline was caught with its paradigms down" [2007: x].)

In general, these mostly complementary critiques and analyses offer a range of conceptual, methodological, and political/ideological reasons for the continuing marginalization of race in political science. But they do not, to my mind,

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[6] It is telling that a significant amount of the work on the status of black politics within the discipline cited above appeared either in the NCOBPS journal, the *National Political Science Review*, or in a recent volume entitled *African American Perspectives on Political Science* (Rich, ed. 2007).

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convincingly answer the question of why political science, particularly? History and sociology—which for all their problems have consistently paid more attention, at least, to racial stratification than has political science—operate within a similar political and ideological matrix, and sociology has embraced much of the individualizing methodology that has structured American political science. A focus on "elites" might go some way to explaining this gap, but give the prevalence of public opinion and survey research, and the explosion of political psychology, much of it focused on the behavior of "ordinary" people, in postwar political science, this seems questionable. So how, and why, exactly, is political science different?

Rogers Smith (2004) offers the intriguing beginning of a more satisfying explanation. For him, "race" drops out of the discipline about 1920. This is because most early-to-mid twentieth century social scientists—political scientists as well as their colleagues in other disciplines—"tended to think of racial identities as things generated at root by biology and/or economics and/or culture and/or history and/or often unconscious or at least informal social psychological process and social activities" (41). This put them outside the bailiwick of political science, which at most would see those identities as inputs into the political process rather than subjects for investigation. Basically, because "race" was understood to precede or be "fundamentally exogenous to politics" (or as Jones and Willingham put it more than three decades earlier, to be outside the realm of "fundamental political questions" [op. cit.]) it "belonged" to sociology, history, or anthropology.

As with much of the above, I endorse Smith's account in fundamental respects—in particular Smith's insight that race begins to look "exogenous" to

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politics in the period he specifies. However, my research indicates that his suggestive account misses the centrality of racial thought to the very conception of "the political" that he and others have perceptively identified as excluding questions of racial hierarchy. That is, this dissertation will show that the idea that *anything* could "precede" politics—indeed that politics represented a discrete, bounded sphere at all—was generated within academic political science *through* thinking about the status and meaning of racial difference.

I begin my account with Victorian political science. Chapter One establishes the centrality of race as a category in Victorian political science, and argues further that previous scholarship on the period misunderstands important aspects of the political theory of the time because it fails to take seriously its basis in racial ideology.

As noted above, political science emerged as an academic discipline in the United States in the 1880s. Shortly after the establishment of the first doctoral program at Columbia came another at Johns Hopkins. In the same decade, Columbia began publishing *Political Science Quarterly*, the discipline's first specialized academic journal. John W. Burgess was the driving force behind the Columbia department and *PSQ.* His counterpart at Johns Hopkins, Herbert Baxter Adams, was a similarly important figure. Both argued that American civilization was the fruit of a "Teutonic germ" carried and developed by its Anglo-Saxon settlers (Burgess 1890).

Teutonic germ theory, as it came to be known, rejected the idea that government was authorized by a social contract or formal mechanisms of democracy. Rather, the sources of sovereignty and legitimacy were to be found in an organic

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unity (termed “the state”) that preceded and was expressed in institutions of governance. “The state” was explicitly racial, a blend of the Hegelian “Idea” and late nineteenth-century racial anthropology and social evolutionism. Only the Teutonic (or Aryan) nations were destined to realize the highest form of the state (embodied in American principles of liberty and self-government). Latin and Greek civilizations had more limited political genius; Asia and Africa were home to only “unpolitical nations” upon whom Teutons were “authorized” “to force organization” “by any means necessary” (ibid: 46). This conceptual framework was not addressed exclusively or even primarily to questions of world organization or colonial policy, however. Rather, for Burgess and his generation of political scientists, a racial conception of “the state” was the source of accounts of American democratic legitimacy, the proper role of the United States in the world, and the role of government in regulating or effecting social change. Moreover, this sense that the proper bounds and course of political life could be discerned from the racial history of the world outlived the explicitly “Teutonic” framework, continuing to shape political scientists’ conception of their own enterprise through the Progressive Era.

The racism of these early cohorts has not gone unremarked by disciplinary historians, who tend to lump it in with a number of perceived theoretical shortcomings of the period, including overweening “legalism” and “formalism” and a measure of incoherence. While the purpose of this chapter is not to rehabilitate Victorian political science, I do argue that attending seriously to the role of racial thought in the period undermines both characterizations and particularly the latter. That is, rather than purely legalistic, Victorian political theory was intensely

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interested in geography and natural science, drawing its accounts of racial and historical development from these sources. More importantly, a number of the apparently conflicting commitments of Victorian political science—for and against colonialism in different contexts, and to both limited government and authoritarian politics—begin to make sense when one treats its conflation of race, history, and political life as intellectually important.

Chapter Three continues the focus on founding "moments," examining the first academic international relations journal published in the United States. The *Journal of Race Development* (*JRD*), published out of Clark University in Massachusetts between 1910 and 1919, was a creature of the Progressive Era, evincing a "reform" sensibility far removed from the Columbia department's Gilded Age focus on liberty and limited government. At the same time, its writers shared with that older cohort a basic commitment to the idea that historical development was fundamentally racial in character.

The *JRD* aimed, in its founder's words, "to present … the important facts which bear upon race progress, and the different theories as to the methods by which developed peoples may most effectively aid the progress of the undeveloped" (Blakeslee 1910: 1). Its premise was that scientific knowledge could harness racial evolution and turn it into "development." Through close reading of a wide range of articles from the *JRD* published during the first half of its existence, I show that the *JRD's* writers returned time and again to an organic notion of "civilization" in which race, culture, and political institutions together did not so much overlap as flow seamlessly into one another in a set of complex evolutionary processes.

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That is, even in a quite different political and intellectual setting, the identity of race and historical/political development that was at the core of Victorian political science continued to structure theoretical development in the discipline. This is particularly striking because, as I show, a number of scientific breakthroughs in the intervening years would seem to undermine such a view. Most significant among these was the abandonment by most of the scientific community of a belief in the Lamarckian heritability of acquired characteristics in favor of an understanding of genetic change based on the work of Gregor Mendel. For Lamarckians, the boundary between "nature" and "culture" (to use anachronistic terms) was insignificant—"habits" developed by living generations could become the inherited, innate characteristics of later ones. Mendelian genetics, on the other hand, sharpened these boundaries—inherent "traits" could be modified only by mutation, physical change to the "germ plasm." However, in what seems a testament to political scientists' commitment to the unity of race and history, in the pages of the *JRD* the new theory is turned to old purposes, and understood in ways that reinforce rather than undermine the basic conceptual apparatus inherited from Burgess's generation.

While Chapters Two and Three explore the functional identity of racial and historical development for significant currents of late nineteenth and early twentieth century American political science, the remainder of the dissertation examines a dramatic disruption of that link. The argument presented in this second section is that changing ideas about race are the source of the emergence in interwar political science of a modernist historical consciousness and of conceptions of "nature" and

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"politics" as fundamentally separate but interacting spheres that powerfully influenced the subsequent development of the discipline.

Specifically, two developments in the science of race and ascriptive hierarchy are key to an interwar-era transformation in the way political science understood its own task, and to the images of liberalism and democracy that it began to produce. These are the critique of evolutionary anthropology by Franz Boas and his students and the publicity attendant to the Army intelligence-testing program during World War I.

Chapter Four focuses on the first. Examining particularly the work of Charles Merriam and his influential cohort of colleagues and students, I argue that in this period you can see political scientists consciously engaging with the Boasian critique of Victorian anthropology as a way of thinking through and ultimately rejecting the overtly racialized (and often nakedly racist) framework that had been the common sense of their training. This was possible because Boasian theory provided the intellectual basis for de-linking modern political and social organization from evolutionary time. The result is an understanding of historical and political development that refers to medium-term processes rather than long-term evolutionary development (what I follow Dorothy Ross in calling a "modernist historical consciousness"). The concomitant modernist understanding of "nature" takes it as a separate sphere that is, for practical purposes, timeless and unchanging. Together, these suggest an understanding of "the political" as a more or less autonomous sphere conditioned and limited by the "facts" of nature. That is, it is through engagement

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with the Boasian rethinking of race that political science begins to understand history, nature, and politics as discrete categories in ways that are intelligible to us now.

As following two chapters will show, however, this did not lead political science entirely away from race, nor toward the relativism and emphasis on subjectivity that marked many "modernist" responses to the dissolution of historicism. One way in which "race" re-entered the picture had to do with political scientists' reception of the World War I Army intelligence-testing program, and of the boom in psychological testing that it engendered. This program, in which a newly developed set of intelligence tests were administered to all recruits, was the first large-scale application of psychological testing in the United States. It yielded a number of sensational findings, including that the average "mental age" of a white American recruit was 13 years, and that the intelligence of the American population was declining. It also "revealed" stark differences in intelligence between "native" whites, immigrants, and blacks. What is significant for present purposes is that leading political scientists in the period saw in such testing the possibility that psychological technology might be able to measure all sorts of human capacities, and particularly fundamental impulses governing political behavior.

That is, the tests, their findings, and the general research program of which they were a part, seemed to Merriam and his cohort to have revolutionary potential for new, more adequate representations (and possibly reconstruction) of democracy and its social and racial ordering. I argue that in their embrace of the model of human capacities implied by "differential psychology," we can see the basis for an image of democracy, and for a liberal political theory, that simultaneously rejects "race" as

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construed by Victorian political theory but leaves open the possibility that ascriptive hierarchy can be understood and described in new, more "objective" terms.

Chapter Six takes up these questions of continuity, change, and the role of race in the discipline from a slightly different angle. Still concerned with the 1920s, it focuses somewhat less on the products of intellectual labor and more on its contexts. That is, I put the work that political scientists produced partly to the side and examine the institutional "modernization" of the discipline, including the emergence of a new funding model, new modes of interdisciplinary collaboration, and new understandings of what constituted useful knowledge that could further the discipline.

This chapter shows that the images described in Chapters Four and Five, of politics and nature, of democracy, and of the role of political science in mediating those realms, retain their importance. But we will also see that more frankly racist versions of scientific thought, and the individuals and institutions promoting them, also retain their appeal for Merriam and his intellectual and institutional allies. That is, for an influential and respected cohort attempting to build the discipline both internally and in relation to other social institutions it is not the Boasian culture concept that seems most promising, but the "harder," more deterministic model offered by "social biology."

This chapter looks primarily at the founding of the Social Science Research Council (SSRC), its links to funding organizations (particularly Rockefeller philanthropy), and specifically at its very first intellectual activity, a collaboration with the National Research Council's Committee on the Scientific Problems of Human Migration. Founding the SSRC was part of Merriam's larger project of

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professionalizing and giving political relevance to political science and the social sciences generally. It brought him into close collaboration with three major figures in the development of the American academy: Beardsley Ruml, then head of the Laura Spelman Rockefeller Fund (LSRM); Robert M. Yerkes, the figure behind the Army tests and a major promoter of what he referred to as "psychobiology,"; and Merriam's brother, John C. Merriam, head of the Carnegie Institute of Washington and a major funder of psychological and eugenics research. This chapter looks at their joint project of disciplinary institutionalization, the first fruit of which was the Migration Committee.

In the words of a retrospective NRC publication, the migration studies were organized to "attack" "problems" relevant to "a study of racial groups in the United States" (National Research Council 1933: 39). Their main thrust was to study racial difference in a frankly biologized vein, and to develop the political and economic implications of the findings of such research.

Merriam's involvement with this is a case study in the messiness of racial ideology. Not personally committed to white supremacy or immigration restriction in any deep way, and intellectually and professionally entwined with figures actively resisting both, Merriam sees in the institutions and people promoting hardline race science opportunities for professional credibility and resources for his discipline. But he and his collaborators saw reason to hope that people's political responses, capacities, and limits could be specified by science and subjected to new forms of "social control," presumably under the beneficent direction of professional political scientists. These ambitions went largely unrealized, but their pursuit shaped the

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institutions and conceptual framework within which the discipline subsequently developed. In particular, it fostered the persistent notion that a truly “usable” science of politics would be one that could show how the timeless facts of nature (such as innate racial or other ascriptive differences), or at least the enduring facts of basic social and cultural impulses (such as deep-seated psychodynamics or entrenched prejudice), could illuminate the possibilities and limits of contemporary political life.

In my view, this evidence adds up to a significant challenge to what I have referred to as “political science exceptionalism,” in a couple of respects. That political science has been perceived to have evolved somewhat untouched by America’s racial dynamics is discussed earlier in this chapter. Another “exceptionalism” refers to the discipline’s relation to the social sciences more generally.

As noted above, while histories of the discipline do record a transformation in the early twentieth century, it generally appears as one in which new methods are brought to bear on political questions. In my account, however, while new methods do appear, they are linked inextricably to a larger rethinking of “the political” as a discrete object. In this light, political science begins to conform to a pattern within the larger transformation of the human and behavioral sciences in the twentieth century that Hollinger, Ross, and others have identified as the development of cognitive modernism.

That is, the human sciences in the early twentieth century began to differentiate and specialize in part because they created new intellectual objects, new ways of cutting up and examining the world: anthropologists invented the modern notion of culture and marked it off as their own; economists began to measure and

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plot the course of something called "the economy"; sociologists began to examine autonomous social processes, psychology both created new technologies for measuring things called "intelligence" and aptitudes (cf. Carson 2007) and, on a different register, claimed "the unconscious" as its special province. Likewise, political science, though it did not create a new word (as in "the economy," in the singular) or invest an old one with a very different significance (as anthropologists did with "culture" [cf. Stocking op. cit.]) effectively made a new object in this period, a political sphere with dynamics linked to but distinct from those of nature and history. It also set new tasks for itself—untangling the relationships between these spheres, and possibly revealing how the timeless truths of more "basic" realms of social life or the natural world could illuminate the effects, limits, or possibilities of political action.

The finding that this new object, and the tasks it implied, appeared largely as political scientists grappled with notions of race and ascriptive hierarchy also embeds the discipline within the story of race and American political development. Where a number of accounts portray "race" as a puzzling absence or gap in the discipline's past—a sin of omission—mine suggests that the notions of difference and hierarchy that are expressed in the language of race in America have been pivotal in shaping the professional study of politics. In a final postscript I turn briefly to the implications of this, suggesting that political science played a role in solidifying the "race relations paradigm" that began to take hold as the dominant framework for understanding racial oppression in America in the 1930s. That is, despite the fact that political scientists were largely absent from the group of social scientists that first elaborated

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this framework,[7] the discipline was deeply implicated in the larger move toward defining race as a "social" or "ethical" phenomenon (rather than one of political economy). Moreover, this move shaped the ways political scientists addressed themes of race in American politics when they finally did begin to attend to them seriously many years later. That is, by defining "race" out of "the political," in the 1920s, political scientists helped to set the terms of debate around racial oppression and social stratification more generally in ways that continue to have an impact on that crucial debate in the present.

I will close by pointing to some of the methodological commitments of this study. We have become accustomed to discussing "the role of race" in various aspects of political and social life. In this dissertation I have tried to be very cautious about the way I deploy such formulations, not least because they tend to presume the very separation between "race" and "politics" that I identify as a contingent product of the intellectual changes within the discipline in the interwar era. In consequence, "race," here does not necessarily refer to white supremacy, or racism, or "whiteness," or any such grand abstraction. Indeed, while commitments to white supremacy and racist attitudes are certainly rife in the discipline in the periods I discuss, most of the significant "action" in the account below does not feature political scientists' efforts to shore up white privilege. Rather, it pivots around the ways that popular and particularly scientific and social scientific ideas about racial difference shaped their ways of thinking about all sorts of things, including the most basic categories of politics and its relationship to what come to be conceived as distinct spheres of life

[7] It is worth noting that this group was convened in large part by the SSRC with Rockefeller money (Gordon 2009, Steinberg 2001).

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("nature," "society," "economy," etc.). That is, rather than focusing on the "role of race" broadly, I have tried to frame my investigation around how the ways the political scientists I study speak about hierarchy, difference, and proper social organization in this country, and how ideas about innate, ascriptive difference inflect those ways of speaking.

In practice, this translates into a heavy reliance on close readings of texts as well as extensive archival work. In this case, the archives of Charles Merriam (which include significant collections of papers belonging to Harold Gosnell and Harold Lasswell), of the Social Science Research Council, of the various Rockefeller philanthropies, and of a number of other individuals and institutions, yield a rich picture of institutional and intellectual change during this period, one that I hope begins to come to life in the pages that follow.

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2. *"The Aryan Genius for Political Civilization:" John W. Burgess and the Founding of American Political Science*

That American political science was characterized by pervasive racism in its early years is non-controversial. And in fact it would be surprising to find otherwise. While hardly a distinct entity until much later, the discipline began to emerge as a professional, university-based specialization in the1880s, the beginning of a period Rayford Logan famously described as the "nadir" of African American history (1954), as well as a period of intense interest in America's proper relation to the "darker" nations as the country extended its imperial reach outside continental borders. Moreover, the social scientists of this period—a group dominated by the sons of the "gentry class" that directed much of social and political life in the mid-nineteenth century—were "evolutionists almost to a man," and it was a truism of the evolutionary theory of the time that differences in social and political organization reflected more and less advanced levels of evolutionary progress (Ross 1991, Stocking 1982 [1968]: 112).

Most recent accounts of the origins of academic political science note that major figures such as the founder of Columbia's (and the nation's) first doctoral program in the discipline, John W. Burgess, were deeply racist (though the pervasiveness of that racism is sometimes understated). And the fundamentally racialized nature of American citizenship around the turn of the twentieth century has been widely noted, not least by Rogers Smith in his monumental study of citizenship

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jurisprudence from the founding through the Progressive Era (1997; cf. Gross 2008).[8] At the same time, I will argue, we have failed to take seriously the role of race in the conceptual "toolkit" of political science in its early institutional years.[9]

This chapter examines the role that ideas about race and racial difference played in the intellectual production of political scientists as they organized as a distinct academic discipline and profession in America in the late nineteenth and early twentieth centuries. By this I mean to do more, however, than just fill out the record by "putting race back in." I argue that giving fuller attention to the conceptual importance of race sheds light on the basic logic of much Victorian political science, and helps us to understand the place of race in political science a generation later, as the discipline is further consolidated with the founding of the American Political Science Association.

I begin with a discussion of the work of Burgess, by most accounts the founder of academic political science in the United States and the premiere theorist of what became known as the "Teutonic germ theory" of American political development—the idea, essentially, that American political and legal arrangements represented the product of a developing "genius for liberty" carried by Anglo-Saxon peoples. I go on to show that while not all political scientists relied on Burgess's

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[8] Smith (1997 and see especially 2004) specifically notes the pervasive racialism of Victorian political science.

[9] As noted in the previous chapter, important exceptions to this characterization have begun to emerge in the last decade or so, particularly in work on the early history of International Relations. See especially the work of Robert Vitalis (Vitalis and Markovits 2002, Vitalis 2002 , 2003, 2008, n.d.), which provided important inspiration for this dissertation. Note also that I refer here to the early "institutional" years of American political science. This is not to deny that important works of practical political analysis and political theory were produced in America before the 1880s, but to point to the fact that it was only beginning in that period that political science became a professional academic discipline.

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blend of Hegelian idealism with late nineteenth-century racial anthropology and social evolutionism, notions of citizenship, democratic legitimacy, and attitudes toward imperialism expressed by major figures such as Herbert Baxter Adams, James Bryce, Woodrow Wilson, William Archibald Dunning, and Richmond Mayo-Smith shared underlying assumptions associated with Teutonism and the basic understanding of the development of the American state that Burgess built around it. Moreover, I show that while Teutonism and much of the rest of Burgess's theoretical edifice begin to fade from view after the turn of the century, many of the assumptions that animated both of these remain visible in the discipline's treatment of citizenship rights, suffrage, immigration, imperialism, and the very possibility and meaning of rule of law.

Universities and Social Change in the Gilded Age

Before the Civil War, American higher education consisted largely of training for "gentlemen" in the classical tradition, and included a strong theological component. Beginning in 1824 with the founding of the Rensselaer Polytechnic Institute, this "old model" began to be supplemented with science and technical education, fields that grew rapidly around the middle of the nineteenth century. But it was not until the decades after the Civil War that anything like the modern, specialized, secular university appeared in the United States (Hofstadter and Hardy 1952).

The details of the various causal stories proposed by scholars for the rise of the university are outside the scope of this chapter. But it is worth noting that a

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number of accounts link the new universities and the intellectual style associated with them to elite attempts to guide and manage the rapid social, economic, and demographic change of Gilded Age society. For both Thomas L. Haskell (2000) and Dorothy Ross (1991), the new universities are in significant ways products of "crisis." For Haskell, the rise of the ideal of the academic as a member of a specialized "community of the competent" was a response by Northeastern elites to a society that seemed increasingly disinclined to defer to its authority.[10] Ross's account follows Haskell's in basic outline. For her, the "crisis" in question unseated an elite consensus about the bases of knowledge and the course of American history, as science challenged theological authority and rapid change threatened comforting notions of America as an "exceptional" nation, immune from the vagaries of history (see esp. 53-64). Stephen Skowronek (1982) links the new universities to attempts to create and rationalize a modern administrative apparatus as part of the "rise of the new American state" around the turn of the twentieth century.

Daniel T. Rodgers, who focuses specifically on political science, situates the processes mentioned above as part of an elite anti-democratic mobilization provoked by anxieties about popular politics. For Rodgers, the professionalization of political science and law was a response by "the middle-class heirs to the old Whig-Protestant longings for unity" to the "terrifying…fragmentation" and "class and ethnic divisions" of the late nineteenth century (1987: 169). Rodgers views the

[10] Haskell wrote primarily about the American Social Science Association, which he saw as an important precursor to the model of academic legitimacy that was to define the modern university. Mary Funer has argued persuasively that Haskell overstates the extent to which the ASSA embraced nonactivist, neutral science as a value (Furner 1980). Nonetheless, Haskell's linkage of academic professionalism to class authority remains influential.

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establishment of political science as a university-based discipline as part of a "counterrevolution in political rhetoric" in which "a new coterie of professionals boldly and systematically under[took] the task of formalizing the amateur talk of politics" in the period following the Civil War. Confronted with economic depression, Populism and worker radicalism, a host of new regulatory measures, "the first serious stirrings of socialism" and feminism, and a host of other unsettling developments, the political scientists of the 1880s and 1890s saw their task as "wrest[ing] the language of political legitimacy away from the people" and putting a "new set of constitutional limits" around their powers (145-146).[11] Rodgers's description of professionalizing political science as particularly hostile to popular rule is borne out in the work of the figure generally identified as the founder (often "father") of academic political science in the United States, John W. Burgess of Columbia, who produced a deeply exclusionary account of democratic legitimacy that invoked "the people" in frankly ascriptive terms and even then limited their role to passive authorization of the dictates of an organically evolving legal order.

John W. Burgess and the "Teutonic Germ of Anglo-Saxon Liberty"

Burgess was born in 1844 to a Tennessee Whig family and came of age during the Civil War, briefly fighting on the side of the Union.[12] He was educated in the war's

[11] Rodgers does not comment on how the violent reaction against Reconstruction then underway might have figured into all of this.
[12] He notes in his 1934 memoir, however, that late in life he began a friendly correspondence with Jefferson Davis's widow, and from her gained a much more sympathetic appreciation for the "Southern point of view."

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immediate aftermath, studying with the Hegelian philosopher James Seelye at Amherst College. Prevented by poor health from continuing his studies under Francis Lieber at Columbia Law School, Burgess went on to apprentice with a former judge in Massachusetts before leaving in 1871 for two years of doctoral study in philosophy, political science, public law, and ethnology at the Universities of Göttingen, Liepzig, and Berlin.[13] He was eventually recruited by Columbia President Nicholas Murray Butler to take over the chair made available by Lieber's death and to establish a new school of political science on the European model at Columbia (Burgess 1934). The program, the first of its kind in the United States, was intended to train students for both university work and public service. Launched in 1880, it offered courses in history, constitutional law, diplomacy, public administration, political economy, and statistics, and was a key element in President Butler's successful attempt to transform Columbia into a university.[14] Burgess's example was soon followed at Johns Hopkins by Herbert Baxter Adams; between them, these two men created the departments that trained the first generation of American PhDs in political science.

However, it was Burgess who "[m]ore than anyone else…established the disciplinary, professional, and intellectual foundations of modern political science" (Gunnell 2004: 73), founding the American Academy of Political Science and the *Political Science Quarterly* (*PSQ*) (both originally vehicles for the work of Columbia's scholars), training a huge number of students, and producing, among

[13] He later also spent a year studying civil service training methods in Paris.
[14] Columbia's website notes that "Burgess was also chiefly responsible for keeping women out of Columbia during his tenure" (http://c250.columbia.edu/c250_celebrates/remarkable_columbians/john_burgess.html).

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other works, an influential two-volume treatise on the development of political institutions, *Political Science and Comparative Constitutional Law* (1890, 1891). According to Somit and Tanenhaus, “The Columbia School of Political Science was *the* formative institution in the development of the discipline, [the] program … that other universities consciously emulated or deliberately deviated from in setting up their own graduate work in political science. … In fine, when the School opened in the fall of 1880, American political science as a learned discipline was born” (1967: 21, emphasis added).

The notion that the English and American constitutional systems were traceable to “Teutonic” or “Aryan” roots was not original to Burgess by any measure. The notion of a “Teutonic chain” of descent culminating in English civilization was influentially elaborated by Sir Henry Maine in the mid-nineteenth century. Later, with Walter Bagehot, Maine traced this chain of descent back to “Aryan” ancestors. In America, Francis Lieber developed similar arguments (1853 [1877]), eventually adopting from the British scholars the labels “Teutonic” and “Aryan” for the history he described (cf. Ross op. cit.: 38-41).

Lieber was significant for what one commentator has called his "role in supplanting an earlier bloodless social-contract theorizing on the institutions of government " (Clinton 2005: 24). In Ross’s characterization, “Lieber discovered from his examination of Western history that political progress culminated in the conservative Whig principles of American constitutional government.” According to Lieber, what he at first called “Anglican” principles of liberty were “the leading subject of Western history and the characteristic stamp and feature of our race, our

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age, our own country and its calling" (op. cit.: 41, 22).[15] In practical political terms, this meant limited government (and especially free trade), opposition to imperialism on nationalist grounds, and a reliance on the "institutional" foundations of Anglo-American liberty, rather than abstract doctrines of rights such as had animated the French Revolution.

In Burgess's hands, Teutonism became the basis for a theory of "the state" that according to John Gunnell was to be "paradigmatic" for the emerging discipline (op. cit.: 84). It was distinctive in marrying Lieber's historicism, Hegelian metaphysics, and late nineteenth century ethnology and social evolutionism.

The crucial feature of "the state" was that it was *not* the government, or indeed any set of institutions. It was, rather, "the gradual and continuous development of human society… the gradual realization, in legal institutions, of the universal principles of human nature" (1890: 59). What we might now think of as the state, or the institutions of the state, were merely "that form in which the state vests the power of government in an organization or in organizations more or less distinct from its own organization" (1891: 2).

That is, the state is a parallel to the Idea in the Hegelian philosophy that Burgess absorbed first at Amherst and then during his graduate studies in Germany. It was the developing historical destiny of mankind; government was merely its instrument. However, the universalism in Burgess's framework was decidedly qualified. While the "American commonwealth" was "the ideal commonwealth for the world" (1895: 404), "states" were products of "nations," "organized politically."

[15] Though he was born and raised in Germany, Lieber's "our country" refers to the United States.

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Nations in turn were to be understood as emerging from the "coincidence" of "geographic and ethnic unities." "Primarily and properly," he wrote, "the word nation is a term of ethnology, and the concept expressed by it is an ethnologic concept." As he emphasizes on the first page of his magnum opus, the term "has reference…primarily to the relations of birth and race-kinship" (1890: 1).[16]

More concretely, in the American case, the state emerged when the English and North American nations, already separated by an ocean, were split definitively by the Revolution. Anglo-Americans once both geographically and politically isolated constituted a distinct unity that, like their parent nation, carried the "Teutonic genius for liberty" inherited from Saxon ancestors. Unlike the English, however, the Americans were unencumbered by the centuries of tradition that stalled political development in England. The revolutionary basis of the American republic meant that "[w]e Americans have seen the state organized" in its purest form, "outside of, and supreme over, the government" (70).

It was this self-organization of the state—not any compact of pre-existing commonwealths—that produced the Constitution; the Constitution in turn provided the basis for the legal doctrines and institutions that subsequently appeared as the state realized itself. Hence Burgess's framework functioned as a powerful argument for the Union as well as for the legitimacy of judicial relative to representative decision-making. If the states were created by a pre-existing, sovereign unity,

[16] Burgess studied statistics and ethnology under Johann Eduard Wappäus at Gottingen, an early pioneer in the professionalization of statistics in Germany (cf. Porter 1986: 178) and a prominent advocate of German colonial expansion (Fitzpatrick 2008: 172). Burgess found much of this work "dull" and "so voluminous that it was most difficult to form an interesting discourse out of it." However, he "reaped the greatest profit out of [Wappäus's] ethnological statistics" and claimed that he "could not have completed the chapters on 'the nation'" in *Political Science and Comparative Constitutional Law* without his notes from Wappäus's lectures (Burgess 1934: 103-104).

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secessionist demands based on the subnational states' independent sovereignty were illegitimate. And if the development of "the state" was to be seen in its laws and legal institutions, its judges and legal scholars—those Burgess admiringly called "the aristocracy of the robe"—would be better qualified to shepherd it on its course than any mass of representatives (1891: 365). This points, of course, to Burgess's notion of democratic legitimacy, which was grounded not in representation or any immediate manifestation of popular will or opinion but in the organic law produced by "the people" in their historical development. (It may be helpful to think of the State as equivalent to Rousseau's "general will" as opposed to the "particular will" expressed in legislative acts or other manifestations of popular politics.[17]) That is, American law and institutions were a product of the developing "germ" inherent to the Teutonic nation, and flowering unhindered in the new world, and the outcomes of popular or electoral politics were legitimate only when and insofar as they expressed the true spirit of those laws and institutions.

Burgess's mistrust of popular government included a clear antipathy to the emerging regulatory state, which he saw as an unwarranted intrusion of mass "whims" on the sphere of liberty guaranteed by the state. His emphasis on freedom and personal liberty did not extend to all, however. In an 1895 essay on "The Ideal of the American Commonwealth," Burgess wrote that the

> American commonwealth is already based upon ideal principles and has advanced many stages in an ideal development; … [W]e are compelled to regard those who should favor and advise … a revolution [of our system] as the enemies in principle of the American republic and of the political civilization of the world (425).

[17] Burgess does not himself use this terminology, but his near contemporary W. W. Willoughby does in much the same context (in Rodgers, op. cit.: 160).

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Such a "revolution" might threaten from three sources: forces of sectionalism, "pollut[ion by] non-Aryan elements," and "so called socialistic movements" (407, 410). Sectionalism, while largely defeated in the Civil War, still threatened in the form of Populism, making limits on mass government imperative. So, too, did the twin threats of non-Aryan pollution and socialism call for harsh and potentially authoritarian measures.

Lest there be any confusion, Burgess was careful to specify that "the prime mission of the ideal American commonwealth [was] the perfection of the Aryan genius for political civilization, upon the basis of a predominantly Teutonic nationality." This entailed "preserv[ing] our Aryan nationality in the state, and admitting to its membership only such non-Aryan race-elements as shall have become Aryanized in spirit and in genius by contact with it." Against objections that such exclusionary measures (which he implied might include actually expelling undesirable residents already present) might be anti-democratic, Burgess countered that, "the Aryan nationalities alone have created democratic states and that Aryan history is ever moving toward the realization of genuine democracy and its impartation to the rest of the world" (407). That is, the racial basis of democracy meant that its principles needed to be limited in order to reach their ultimate triumph in the future.

The threat of socialism revealed another justification for such exclusionary measures. Indeed this specter was not unrelated to the threat of racial pollution. In Burgess's view, "looking to government" was a "European habit" (1891: 412) and the

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appeal of socialism was due to “the immense immigration into our population of that very element of Europe's population to which such propositions appeal” (ibid: 411). That is, Burgess worried that such elements threatened American liberty with their ideas. But they also constituted a threat by their very natures—the disorder to which certain European populations were prone might be a justification for increased governmental capacity.

Burgess’s reference to race mixture and the possibility of assimilation (of groups becoming “Aryanized in spirit and in genius”) reflects the ambiguity of racial theory at this time. Burgess footnotes his discussion of the races of Europe and their distributions with a reference to the *Statesman's Yearbook*, an encyclopedia edited at that point by the Scottish geographer John Scott Keltie, but in his memoir attributes his knowledge of ethnology to J. E. Wappäus (see footnote above), whose lead he follows in identifying five major European “races” (Greek, Latin, Celtic, Teutonic, and Slavic). And while there was disagreement among major contemporary scholars on the identity and number of European races, it was widely accepted that the existing European races were the result of an earlier period of migration, conquest, and mixture, and that cultural development and assimilation were significant factors in European history. At the same time, Burgess did try to claim a specially unmixed status for Teutons (as a “simple” rather than an “amalgamated race”), whose predominance among otherwise mixed populations was in every case an “ethnical fact” that “had immense influence upon … political conditions” (1891: 18), and denied that historical experience could substitute for racial capacity (holding in a

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typical comment that "education [of nations] can only develop what already exists in seed and germ" [37]).

As noted above, the concept of the state was central to the identity of the emerging discipline of political science, and shared in its basic outlines by most major figures in the period. Daniel T. Rodgers characterizes it as "the core" of the language of professional political science, "the password by which one gained admission to the fraternity" of the discipline (op. cit.: 157, 160). While Burgess's disdain of actual governing institutions was shared by only some of his contemporaries and students, his basic model of historical development, and the racial basis of that model, *was* widely held, by scholars favoring a mix of policy preferences similar to Burgess's and others who opposed elements of this program.

Adams, Burgess's counterpart at Johns Hopkins, shared Burgess's Teutonism as well as his distrust of popular politics. As Ross puts it, both Adams and Burgess "wanted to protect established institutions from the demos by subordinating individual rights to history and the community" (op. cit.: 74). Like Burgess, Adams drew on a Hegelian framework (he, too, studied at Amherst with Seelye and then in Germany), and dedicated the bulk of his historical writing to documenting the Teutonic roots of early New England village organization. Principally, he was concerned to find links to Germanic traditions of "the sovereignty of the community over its individual or associate members" (Adams 1882: 37), a sovereignty that of course justified limiting immediate, popular demands.

But the basic understandings of political progress, rights, and legitimate political action that animated Burgess and Adams could be found among scholars of

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quite different political and theoretical leanings. Richmond Mayo-Smith is an example of someone whose politics differed substantially from those of the two senior figures, yet drew on very similar foundations to reach his conclusions.

Mayo-Smith was an economist and statistician at Columbia who published frequently in *PSQ*[18] and emerged as the "authoritative" voice on immigration restriction among the "progressive professionals opposing unfettered laissez-faire" around the American Economic Association, a group that included Richard T. Ely and E.R.A Seligman (Zolberg 2006: 210, 199). Also German educated, Mayo-Smith had been a junior colleague of Burgess's at Columbia (after Mayo-Smith's death in 1901 at the age of 47, Burgess dedicated a book to his "pupil, colleague, and lifelong friend" [Burgess 1902]).[19]

Mayo-Smith was particularly concerned with immigration. He published a series of long articles in *PSQ* in the 1880s (1888, 1888a, 1888b) that were the basis for an 1890 monograph, *Emigration and Immigration*, and a second series of articles a few years later (1894, 1894a). In these articles, Mayo-Smith paired a pro-labor concern for wages and union strength with a pro-imperialist, racialized understanding of national political development. The first two articles of his 1888 series on "Control of Immigration" focus, respectively, on defining the character of the "new" immigrant population (mainly from Southern and Eastern Europe, in contrast to the predominance of British, Irish, and Northern European immigrants of earlier decades)

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[18] Mayo-Smith's identification as an economist notwithstanding, if JSTOR is any guide the overwhelming majority of Mayo-Smith's publications in scholarly journals appeared in *PSQ*.

[19] Both Burgess and Mayo-Smith were mentors to E.R.A. Seligman, whose thesis Mayo-Smith supervised. Seligman, while not as radical as Richard Ely, became an important advocate of the progressive income tax, which Burgess vehemently opposed.

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and on determining its economic effects. It is in the context of the first question that he comments that the biggest increase in rates of immigration is among "directly the most foreign…of the foreign immigrants." For Mayo-Smith this is a function of both race and class – not only does the "new" immigration represent a "great infusion of foreign blood, some of it alien in every respect," it also brings the most "undesirable" representatives of foreign populations, some able to make the journey because of the cheapening of transport, others "dumped" by emigration societies in Europe (1888: 60-61). As to the second question, Mayo-Smith denies that immigrants bring economic benefit, arguing instead that, "this extraneous supply of unskilled labor…is bringing upon the laboring class a distressing competition, which threatens finally to lower the standard of living of the whole community" (1888a: 225).

Most interesting for present purposes, however, is the third of these articles, which concerns the "principles of political science" that bear on questions of immigration control. These "principles" embed economic organization, political rights, and national duties within a larger framework of the world-historical development of "the state" generally, and the American state in particular. Mayo-Smith makes short work of claims for universal rights such as freedom of movement, or for any cosmopolitan duty to all mankind; and he has little patience for the notion that as an immigrant nation America might in fact continue to benefit from an influx of population. In his view, which follows Burgess's, these ideas followed from a fundamental misunderstanding of the nature of political rights, the duties of civilized nations, and the process of historical development.

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As for the claim that universal, natural rights are at issue with respect to immigration, Mayo-Smith like Burgess makes clear that he sees the very notion of such rights as a misunderstanding, born of a narrowness of vision that mistakes the present state of things for eternal truth. In his view, rights and liberties are "merely historical…. The state that conferred the liberty may also withdraw it." Rights, that is, do not inhere in people—they develop historically in institutions, as part of the general development of the state. And against any ideas that a state that has developed such rights has the duty to extend the sphere of liberty to others, Mayo-Smith argues that it is in fact America's "duty to humanity" to exclude the "the depraved dregs of European civilization" and thereby see to it "that civilization progresses" (1888b: 410-411, 413).

Indeed, America's immigrant past could only properly be understood within this frame of progress. In its earlier, lower state of civilization, America needed foreign population to claim the continent's vast resources. The harshness of the early period of settlement mitigated the danger of welcoming that labor by exercising a salutary "disinfectant" effect. That is, the difficult conditions of early years fortunately "kill[ed] off a large number of those consigned" to them. "But as a country progresses it loses this capacity of absorbing the lower elements of other civilizations," and America was "getting to the limit set by nature" for the "work" of offering "opportunity to the poor and degraded of Europe" (ibid: 413). This did not represent a loss to humanity, however, because humanity's interest lay not in the fate of its lowest members, but in that of its highest: the "duty of every nation to humanity is to see to it that the higher civilization triumphs over the lower. It performs this duty

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best by preserving its own civilization against the disintegrating forces of barbarism" (413).

Therefore, as with the surge in immigration at the time, "when a contingent comes demanding admission, that is of a lower civilization rather than of the higher, its right to be admitted is not so plain. … Our duty to humanity is to maintain the high standard of living which a favorable combination of circumstances has enabled us to establish." For Mayo-Smith, Europe's "surplus population" was "threatening to drag us down to their level" and it was "not our place to submit" (idem), but rather to keep developing the American state on the basis, and on behalf, of a predominantly Teutonic nationality (411).

James Bryce is another figure whose work and theoretical temper would seem to present several contrasts with Burgess's. A British politician who traveled extensively in America, and who eventually served as British Ambassador to the United States from 1907 to 1915,[20] Bryce was no pro-labor radical, though he expressed some support for activist government, qualified by the certainty that "no people is shrewder than the American in coming to recognize the results of overbold legislation and modifying it when it is found to tell against the general prosperity" (1893 [1915]: 597). Nonetheless, his most significant differences with Burgess lie elsewhere: where Burgess disdained the workings of government in favor of its legal forms—the "spirit" that underlay and (to an extent) authorized government—Bryce employed a distinctly empiricist style, engaging in painstaking, almost ethnographic description of the institutions and dynamics of American government.

[20] He also served as APSA's first president during this time.

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Bryce's most important work, *The American Commonwealth*, has earned him comparisons to Tocqueville on the strength of his detailed foreigner's observation of American politics. *The American Commonwealth* first appeared in 1893, and went through many editions, despite consisting of almost 1000 pages of minute observation of the details and workings of American government, with chapters on all the branches and levels of government as well as on topics such as, "Why Great Men Are Not Chosen President."

Bryce, that is, evinced considerably more interest in the real than the ideal—in government itself rather than "the state." Nonetheless, for him political institutions and public opinion alike were products of racial characteristics, and history was to be understood as "a record of the progress toward civilization of races originally barbarous (1893 [1915]: 515) in accordance with their innate capacities.

Bryce was particularly concerned with the changing racial makeup of the American population, as well as the effects of climate on the political and social traits of populations in different areas of the county. For example, Bryce approvingly cites the fundamental conservatism of the American people as a bulwark against unwise revolutionary change, but is careful to note at the outset that his characterization refers only to "the native Americans" and is "not applicable to the recent immigrants from Europe, and, of course even less applicable to the Southern negroes," whose "unassimilable" presence in America he characterized as a "peculiar and menacing problem" (ibid: 285-297, 491).[21]

[21] He also notes the kindliness of Americans, as evidenced by the fact that "Cruelty to slaves was unusual while slavery lasted, the best proof of which is the quietness of the slaves during the war" (285).

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Indeed, Bryce situates American uniqueness primarily in race and climate, writing that there are "three points" that mark the American situation "new in the annals of the world." The first is "a huge people whose blood is becoming mixed in an unprecedented degree by the concurrent immigration of numerous European races." The second is the presence, "besides the predominant white nation, ten millions of men belonging to a dark race, thousands of years behind in its intellectual development, but legally equal in political and civil rights." The third is the vast and diverse territory, with its rage of climates possibly "modify[ing] the physical type of a race, and therewith even its intellectual type" (449, 466). For example, in the South, "although the winters are cool enough to be reinvigorative, and to enable a race drawn from Northern Europe to thrive and multiply, the summers are, in the lowest grounds, too hot for such a race" (458-459).

This predicament, to Bryce, seems to constitute and ample and sufficient explanation for the importation of slaves to the continent, leading him to the conclusion that climatic differences were at the root of the Civil War.[22] In this respect, Bryce was invoking what Robert Vitalis has called the "first law of international relations theory—as dominant in its day as the so-called democratic peace thesis is in the early twenty-first century—namely that differences in races made acclimation by whites to tropical environments impossible" (2008: 40). In general, however, Bryce was optimistic about the ability of the American Commonwealth to surmount the challenges presented by the climatic diversity of the

[22] "Physical differences--differences of climate, and of all those industrial and social conditions that were due to climate--were at the bottom of this strife" (Bryce 1893 [1915]: 458-459).

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continent and to assimilate European immigration, which in his estimation brought a useful range of race traits into the population:

> Nearly all the instreaming races are equal in intelligence to the present inhabitants. Of the acuteness of Jews and Greeks and Italians it is superfluous to speak. ... So too, the Poles and the Czechs are naturally gifted races, quite as apt to learn as are the Germans, even if less solid and persistent. Than the Armenians there is no abler race in the world. A blending of races has often in past times been followed by an increase in intellectual fertility. It is possible that from among the Jews and Poles with their musical faculty, or the Italians with their artistic faculty, there may rise those who...will carry the creative power of the country to a higher level of production (1893 [1915]: 483).

However, there were distinct limits to the potential for race mixture, and certainly to the potential for such mixture to be beneficial. For example, negroes were clearly excluded, and Bryce was careful to note that the proportions of Teutonic blood in the population were not being drastically reduced (perhaps, as he later speculated, because of a characteristic Teutonic disinclination to race mixture [1903: 19][23]); this combined with the "intellectual and moral atmosphere" that the "native Americans" had established reassured him that national character would not be substantially changed by the new elements (1893 [1915]: 922-923).[24]

William Archibald Dunning and Woodrow Wilson are two slightly younger writers who like Bryce resisted the Hegelian trappings of state theory to varying

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[23] Calling into some question Bryce's famed powers of observation is the fact that he cited a scarcity of children born to black mothers and white fathers in the American South as evidence of this aversion.

[24] Burgess too in this period credits the "race-proud Teutons" with an instinctive resistance to "amalgamation" (1895: 406). Bryce was less sanguine about the dangers of race mixture two decades after the original publication of *The American Commonwealth*, when in "The Relations of the Advanced and Backward Races" he wrote that "We cannot...predict what the result may be on the American people, after another half-century, of the great stream of non-English blood which is being poured into its veins. The [physical] type may remain, yet the national character may prove to have been affected. If however, one may venture on a generalization, it will be to the following effect. Where two races are physiologically near to one another, the result of intermixture is good. Where they are remote, it is less satisfactory, by which I mean not only that it is below the level of the higher stock, but that it is not generally and evidently better than the lower stock" (1903: 24).

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degrees yet in many respects reproduced the racialism and teleology characteristic of Burgess's work. Dunning was a student of Burgess's who became an important colleague on assuming the Lieber Chair in History and Political Philosophy in 1885. Along with his mentor, Dunning became a major figure in building the Columbia Political Science Department. Though he is generally remembered as the historian whose work cemented the characterization of Reconstruction as a colossal error into professional dogma for more than half a century (Foner 1982, 1988),[25] Dunning was proudest of his work in political theory, the main product of which was three volumes covering the history of political thought from Antiquity through Herbert Spencer (1902, 1905, 1922).[26] Wilson for his part had studied under Adams at Johns Hopkins, and like Dunning produced both works of history, notably the five volumes of his *History of the American People* (1902) and works of political theory and analysis (his 1885 *Congressional Government* appeared a year before he earned his PhD).

Like Bryce, Dunning professed to be more interested in the real than the ideal, and tried in his historical work to move toward what a mid-20th century analyst characterized as "a value-free empirical history" and a "neutral…mode of…analysis" (Leonard Krieger cited in Muller 1974: 328). Given that Dunning's work was full of sharp and scurrilous descriptions of Radical Republicans, "carpetbaggers," former slaves, and other figures promoting the Reconstruction project, Dunning clearly did not eschew historical judgment. However, what Krieger may be referring to is the fact that Dunning was famously reticent about his own political philosophy (cf. Merriam

[25] However also see Williams (1946) and Weisberger (1959) on earlier attempts to get out of the Dunning mold, including of course W.E.B. Du Bois's *Black Reconstruction in America* (1935).
[26] Somit and Tanenhaus agree that during the 1880s, Dunning "definitely regarded himself as a political scientist" (1982: 45, fn 5).

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1965: 137), and in his own work backed away from Burgess's tendency to view history through the lens of German idealism. For example, according to Muller (op. cit.), Dunning's first work on Reconstruction was his PhD thesis for Burgess, in which he represented the changed legal framework of the post-Civil War era as a positive accomplishment in the independent, spiritual development of American nationalism; the version published later as *Essays on the Civil War and Reconstruction* (1904 [1897]) omitted to make this interpretation explicit. And Dunning was critical of German idealism more directly in his later work (1913, 1913a,1922).

At the same time, there is no question that the question of racial capacity and fitness for self-government was at the heart of Dunning's appraisal of Reconstruction. This subject has been covered exhaustively elsewhere, and is sufficiently well known that I will omit most details here. Eric Foner's summary of the Reconstruction historiography of the "Dunning School" may suffice:

> In this view, vindictive Radical Republicans foisted black supremacy upon the defeated South, unleashing an orgy of corruption presided over by unscrupulous carpetbaggers, traitorous scalawags, and ignorant freedmen. Eventually, the white community of the South overthrew this misgovernment and restored…white supremacy…. (1982: 82).

What Foner does not emphasize is that while Dunning is certainly directly responsible for training the historians who produced the raft of studies of Reconstruction at the state level that solidified this interpretation, it was not original or limited to him and the students he trained. In the pages of *PSQ,* black people were

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the "alien (or lower) element" of the American population (Mayo-Smith 1888: 54),[27] or were represented by the "ignorant, turbulent, and offensive class" of "negro brute" speaking out for the franchise and determined to "outrage and murder" Southern whites' "young daughters" (Langdon 1891: 40-41). In other accounts, blacks were largely hapless, as when one reviewer of a series of books on Reconstruction commented that "the negro has been the spoil of the politician rather than a voter able to demand and command his rights" (Bancroft 1890: 689). In fact, both Burgess and Bryce's major works contained essentially similar accounts of Reconstruction, as did Woodrow Wilson's volume on the Civil War, *Division and Reunion* (1893), which offered vivid depictions of the horrors of black domination and a glorifying account of the rise of the Ku Klux Klan.

Certainly an understanding of the Civil War and Reconstruction in broad outline was shared by the four men: None saw secession as lawful, all saw the Union victory as a victory for a legitimate American nationality, and all disdained attempts to bestow political membership on the freedmen as a misguided application of what Dunning liked to refer to with contempt as "high ideals." Wilson in his later work emphasizes such apparently concrete questions as the workings of Congress and public administration, but in fact reproduces much of Burgess's mixture of metaphysics and organicism, describing the progress of constitutionalism as an awakening of "consciousness" while remarking that "government is not a machine, but a living thing. It falls, not under the theory of the universe, but under the theory of

[27] Interestingly, six years later Mayo-Smith seems to have a change of heart on this issue, rejecting the word "alien," and arguing that we should understand negroes as a thoroughly American but "unique" and "peculiar" (and still unassimilable) element of society (Mayo-Smith 1894: 429).

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organic life. It is accountable to Darwin, not to Newton" (1907: 56).[28] Neither does Dunning seem to have been able entirely to resist the temptation of departure from empiricism in favor of more general theoretical explanation. He ends his 1897 *Essays on the Civil War and Reconstruction* with some reflections on the lessons of Reconstruction for colonial expansion, commenting that,

> the ultimate root of the trouble in the South had been, not the institution of slavery, but the coexistence on one society of two races so distinct in characteristics as to render coalescence impossible; that slavery had been a *modus vivendi* through which social life was possible; and that after its disappearance, its place must be taken by some set of conditions which, if more humane and beneficent in accidents, must in essence express the same fact of racial inequality. The progress in the acceptance of this idea in the North has measured the progress in the South of the undoing of reconstruction. In view of the questions which have been raised by our lately established relations with other races, it seems most improbable that the historian will soon, or ever, have to record a reversal of the conditions which this process has established (384-385).

The above does not even begin to exhaust the ways that notions of racial development, racial hierarchy, and racial contact appear in the work of these scholars—that would require a separate dissertation. For present purposes it should suffice to note that not only does race appear as a basic category for understanding

[28] Gunnell sees Wilson's talk of "the state" and its Teutonic origins as "lip service" and points out that the idea of a sovereign entity that stood outside of and before government was beginning to fade away in Wilson's work (2004: 79-81). Given Wilson's practical political positions with regard to race, I am less inclined than Gunnell to discount his theoretical uses of it. And while Gunnell is certainly right that Wilson's commitment to state theory is less thoroughgoing than Burgess's, it seems to me to retain some force here. See for example Wilson's characterization of the Civil War as "a record of the triumph of the principle of national sovereignty." For Wilson, as for Burgess, "the war was inevitable, because that principle grew apace; and the war ended as it did, because that principle had become predominant" (1885 [1900] 32). Wilson also shared Burgess's interest in protecting the state against "the command of irresistible majorities," and saw the judiciary as well as the administrative apparatus as bulwarks against mass whims (ibid: 40). Likewise in 1907, Wilson echoed the Burgess/Adams line that a historically and racially constituted "community" coming into "consciousness" of itself lay behind the possibility of constitutional government: "A people not conscious of any unity, inorganic, unthoughtful, without concert of action, can manifestly neither form nor sustain a constitutional system, The lethargy of an unwakened consciousness is upon them, the helplessness of unformed purpose. They can form no common judgment; they can conceive no common end.... Nothing but a community can have a constitutional form of government" (1907: 8, 25-26).

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the course and proper future of American historical and political development in the work of all these men, it does so in fairly consistent ways, uniting theorists whose political and social leanings and theoretical styles otherwise divided them.

A Note On The Coherence of State Theory

It is fairly commonplace in the historiography of political science to denigrate the work of the discipline's founders as part of "a formalist science" particularly in contrast to what is seen as the more rigorous and "scientific" work that begins to appear in the 1920s (Seidelman and Harpham 1985: 25, cf. Sibley as cited in Worcester 2001: 181). (This characterization, based largely on the emphasis on comparative jurisprudence in Burgess's work, appears to have originated in the 1920s themselves, for example when the conveners of a series of conferences on the "Science of Politics" [see esp. Chapter Four] sought to distance their own enterprise from the work of their predecessors.) Rodgers, by contrast, sees Victorian political science as less overly formalist than fundamentally slipshod, arguing that Burgess's thought and state theory generally presented a "web of contradictions" (op. cit.: 164, 168).[29] As noted above, Rodgers situates the emergence of state talk in a late nineteenth-century attempt by elites to "elbow out the people, the men of elections and legislatures" from the exercise of power, largely as a response to "new demands, many of them from below" to use government power in various ways. The state was an abstract embodiment of sovereignty, "shimmering behind the conflict-ridden

[29] Crick (1959) also portrays Burgess as a figure whose Hegelianism fit uneasily with his deep distrust of the actually existing state.

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screens of everyday life" and ultimately serving to sever questions of democratic political legitimacy from such concrete manifestations of popular will as elections, legislation, or worse, labor and/or Populist militancy (ibid.: 147-149). It is in this aspect of state talk that Rodgers locates its reactionary core: "The political scientists' State was the logical consequence of their antirights talk. The State was the entity which held all right and powers, which in an extraordinary act of aggrandizement had swallowed them whole." Moreover, for Rodgers state talk was fundamentally ideological (though he doesn't use the term): "What sustained the word State, like the big words before it, were big and urgent uses" (161, 169), among which justifying *both* the Union *and* strong limits on its ability to regulate commercial activity was paramount.

Rodgers bases his view that the attractions of state theory were largely instrumental on the notion that it was otherwise a bit of a conceptual mess. The state is "the antonym of the people"—the justification for substituting judicial supremacy for popular politics—but at the same time identified with democratic legitimacy (146). The state is "sublime" but centralization of power by its agent, government, is deeply suspect (163). Finally, while much has been made of Burgess's assertions of the duty of "Teutonic" people to colonize the "unpolitical races," Burgess was also a fierce critic of American imperialism and particularly outspoken in his opposition to American annexation of the Philippines.

Rodgers is correct that each of these tensions can be found in Burgess's work. I argue, however, that the understanding of race in Burgess's work and in the work of other scholars influenced by his ideas about "the state" in fact brings coherence to

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what otherwise seem to be conflicting accounts of the sources of democratic legitimacy, the role of government in regulating or effecting social change, and the proper role of the United States in the world (imperialist or not)

The key to these apparent contradictions is that for Burgess the American state was still a work in progress. Once the ultimate form of the state had been reached—once America had "perfected its nationality," it would be safe for "the popular or democratic form" to "exert its greatest influence" (1890: 3). For now, however,

> "the ethnic character of the [American] population…is very cosmopolitan. It is, as to the greater part of it, a compound of many elements, mostly congenial and not difficult of amalgamation, having for its base the English branch of the Teutonic race; but it is conglomerated, so to speak, with other elements, numerically quite strong [such as 'Celts,' 'Mongols' and 'negroes'] with which it shows no tendency, or little tendency, to amalgamate (ibid: 20-21).

And because "as we shall see again and again in our further considerations" the "influence of this ethnical character upon the political and legal civilization of this population has been and is still very great," the state remained unperfected (idem).

That is, "national" (that is, racial) unity is in fact a precondition of the extension of a more clearly popular or democratic form of government. A truly "national" state "solves the problem of the relation of sovereignty to liberty" and "permits the participation of the governed in the government" because "[i]n a national state the population have a common language and common understanding of the principles of rights and the character of wrongs." As a result, the population will support only "the enactment and administration of laws…whose effect will be the realization of the truest liberty" (ibid: 39). In the Gilded Age, however, America was still "very cosmopolitan."

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The solution, it appeared, was judicial vigilance in preservation of the liberties already established against those cosmopolitan elements—the limitation, for the time being, of power by the legislatures and other direct instruments of popular will, in favor of the "aristocracy of the robe," at least until such time as the true national will could be realized. (At which time, of course, it would be identical to the edicts of the judges, who Burgess—reasonably, given the tendencies of the Supreme Court in the Gilded Age—saw as the protectors of the sphere of personal and economic liberty against excessive regulation.)

But there was an additional role for "policy" as well. In a chapter on the "conclusions of practical politics from the foregoing considerations," Burgess wrote that the "prime policy" of a modern constitutional state "should be to attain proper physical boundaries and to render its population ethnically homogenous. In other words, the policy in modern political organization should be to follow the indications of nature and aid the ethnical impulse to conscious development" (40). Indeed, such "policy" was not just advisable, but a "duty" which will allow a state to "contribute its just share to the civilization of the world." This "duty," moreover, could mean what might seem an awfully active role for government, such as "a policy which insists upon the use of a common language and upon the establishment of homogenous institutions and laws." Such "insistence" could include the "use of force," which would be when put to such ends "not only justifiable, but commendable; and not only commendable but morally obligatory." One might, for example, "righteously deport [any] ethnically hostile population," and ought to secure

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borders “against the deleterious influences of foreign immigration” (42-43) as well as correct for any “prodigality with the suffrage” (1895: 420).[30]

That is, constitutional democracy was the highest form of government, but democratic legitimacy inhered not on the procedures of elections or the actively articulated demands of the population, but in the expression of a nation’s soul. This could be glimpsed in America in the traditions and institutions evolved by its dominant, Teutonic element. But until this element was safeguarded and allowed to come fully into its own, the population itself was to be contained by precisely those institutions, with the judiciary highest among them. And while limited government was to be both a means toward and an outcome of achieving this consciousness, the sphere of liberty to be protected belonged to the dominant, Teutonic element, presumably embodied at least in part in the business class. Those forces demanding more regulation were identified with or corrupted by “foreign elements” and therefore fell *outside* that protected sphere. Thus Burgess could embrace “democracy” while disdaining representative institutions and popular politics, and could insist on both limited and authoritarian government.

The same dynamic holds when one examines the “puzzle” of why Burgess, who pronounced fulsomely on the duties of the Teutonic nations to have “a colonial policy” (e.g. 1890: 45) emerged as a fierce opponent of the Spanish-American war and the expansion of American empire overseas. In his memoir, Burgess described

[30] W.W. Willoughby, writing slightly later, explicitly endorses the American nation’s right to ethnic cleansing, as well, writing that "it lies within the legitimate province of an enlightened nation to compel--if compulsion be the only and the best means available--the less civilized races to enter into that better social and political life the advantage of which their own ignorance either prevents them from seeing, or securing if seen" (1900: 266)

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the war as "the first great shock" of his professional career, and his own efforts along with other Columbia faculty to use the pages of *PSQ* "against any steps being taken by our government which would lead to war with Spain" (1934: 317).

This opposition becomes understandable, once again, when we remember that at this point for Burgess although America was "already based upon ideal principles" and had "advanced many stages in an ideal development" it still needed to be "freed from some crudities and excrescences, and to pursue steadily the general course towards which its history points, in order to reach the perfection of its ideal…" (1895: 425). War in the service of acquiring overseas territory would steer away from that course on a number of fronts. Most importantly, it would add new, non-Teutonic and unassimilable elements to the population, and would expand the purview of government by expanding its functions, both in the preparation for and execution of war and in the administration of new colonies. The first of these would hinder the perfection of America's Teutonic nationality; the second would prematurely expand the government ahead of the sufficient development of the state.

Burgess's memoir is particularly interesting in documenting, however long after the fact, Burgess's "despondency and despair" at the American declaration of war and particularly at the eagerness of the business class to promote war "for the sake of profiteering by the vast increase of governmental expenditures." He laments that he was "brought to see that Americans were, after all, a warlike people, superficially informed, and easy to incite on Quixotic enterprises" (1934: 315-316). That is, the "best" elements of the American nation did not seem to be conforming to his expectations that they would advocate limited government and the further

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"Aryanization" of the American population as principled stand, irrespective of baser motives.

Clearly, the fact that Burgess's theoretical edifice could be shaken by the realization that the American business class might put profit over principle (even if the principle in question was a commitment to racial purity) points to a certain weakness in that edifice—this discussion is not meant to rehabilitate Burgess's reputation in any way. The point, rather, is that while Burgess was frequently absurd, wrong, and objectionable, he was anything but "inconsistent" or "contradictory." That is, Burgess's ideas only appear contradictory if the state is understood as universal. If one takes Burgess's race talk as seriously as his state talk, it becomes clear the "the state" represents both a racial soul and a racial project, the inner logic gluing together accounts of democratic legitimacy and the proper role of American government, domestically and abroad.

"The State" Fades Away; Its Racial Basis Remains

John Gunnell recognizes that state theory was much more ideologically flexible than Rodgers admits (Mayo-Smith, for example, argued from a basis in state theory for pro-labor regulation), but agrees that the concept was theoretically weak and unsustainable. Indeed, Gunnell blames the failure of state theory for a tendency to theoretical atrophy that in his view was to haunt the discipline for subsequent decades (op. cit.: 81). However, a look how race figured in political scientists' understandings of rights, the proper sphere of state action, and democratic legitimacy from the Gilded

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Age through the early decades of the twentieth century suggests that even as "the state" in the sense that Burgess articulated it eventually fades from view, the notion that race accounts for political capacities and underwrites legitimate variation in appropriate political and legal rights persists largely unaltered, and the lessons that these figures drew from America's racial past and present continued to seem instructive as political scientists increasingly turned their attention to America's relation to the "backward races" in the form of the Southern "negro problem" and America's new overseas possessions. A particularly notable continuity is the persistent link between some degree of racial uniformity and the possibility of non-authoritarian government and even of rule of law itself.

We have already seen that much of the racialism of Teutonic germ theory could be found in the work of contemporary scholars who did not embrace this framework in its entirety. For example, even as Woodrow Wilson sought to "demystify" the state and turn political scientists' attention to "the institution of government," he used much of the language of his older colleagues (Gunnell 2004: 22). This dynamic continues to be visible in the work of the first generation of American PhDs to achieve prominence in their own right. Gunnell has described the period after about 1900 as something of a "theoretical hiatus" for political science (ibid: 19.), and it is certainly true that much of the work that appeared in the early years of the twentieth century lacked the sweeping theoretical ambition of the work of a Burgess or an Adams. However, it continued to reflect aspects of the frameworks those figures had articulated, and which can be glimpsed in the work of people like Bryce, Dunning, and the others discussed above. Specifically, the post-Civil War

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settlement, imperialism, the "new immigration," industrialization, and labor unrest—all defining preoccupations of Burgess's generation—remain central in the new century, and are prominent in the early work of the American Political Science Association, and in ways that reflect the conviction, articulated by Burgess, that "[a]ll political civilization rests on human capacity" (1895: 408), a quality that varies by race.

The idea of an independent APSA, separate from the American Economic Association (AEA) and American Historical Association (AHA), was floated and approved in a series of meetings in 1903. Burgess was present for the very first of these meetings, but subsequently disappeared from any important role in the new association, which in many respects came to be led by Westel Woodbury (W.W.) Willoughy, then an assistant professor at Johns Hopkins. The first sessions of the new association were held at a joint meeting of the AHA and AEA in New Orleans in December of 1903, at which point an amicable separation between the associations was announced; APSA's first independent meeting was held in Chicago a year later, and its official organ, *the American Political Science Review (APSR)* began publication in 1907 (Gunnell 2006).

For Gunnell (idem), the founding of APSA represented a break from the first generation of political scientists primarily in that it made a conscious attempt to gain political purchase for the discipline by embracing a more detached, scientific outlook. This does not appear to have meant any major methodological shift; Burgess had articulated much the same goal, referring to his own work as "an attempt to apply the method, which has been found so productive in the domain of Natural Science, to

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Political Science and Jurisprudence" (1890: vi). Rather, Both Willoughby and Frank Goodnow (the latter notably in his first presidential address to the APSA [APSA 1905: 35-46]) hoped that the new association would work to excise from the rhetoric of the discipline the lofty tones of the political philosopher or "statesman" in favor of the sober persuasions of empirical observation.

In Vitalis's account of APSA's founding, conflicts over the propriety of American imperialism also played a role in determining the tone and leadership of the organization. By this point, of course, the question of whether or not to acquire overseas territory had been settled as a practical matter, and the new association was eager to make a contribution to the task of colonial administration, establishing for example a special section to consider the problems of "colonies and dependencies." But what APSA's younger organizers "saw as a bright new dawn for...the discipline, the Anglo-Saxon race, and thus civilization, others among the older and more eminent founders of the modern discipline…saw instead a dark and ignoble end of their own twenty-year-long effort to bring 'the searching light of reason to bear' upon problems of politics" (op. cit.: 2).[31]

It is likely in fact that both cleavages significantly shaped the new organization. It is suggestive, for instance, that while Burgess effectively dropped out of the leadership of the discipline in the years following the founding of APSA, Bryce, who like Burgess had opposed American expansion (chiefly on the grounds that it would bring undesirable race contact and oblige America to govern tropical territories unsuited to white habitation [cf. Love 2004: Chs 4-5]) but who eschewed

[31] It is further indicative of an intended break within the discipline that APSA's founders chose to inaugurate a new journal, rather than enter into a formal relationship with *PSQ* for example.

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Burgess's grand theorizing, was shortly to be elected president of APSA. It is also worth noting that many of the younger figures around the APSA were considerably more sympathetic to some level of activist government than their teachers had been, and evinced a typically Progressive zeal for improving the efficiency of precisely the government institutions Burgess had regarded with such suspicion.

Nonetheless, on many fronts the early proceedings of the APSA and early articles of the *APSR* sounded a lot like what could be found in *PSQ* a decade earlier. This is particularly true of treatments of the ongoing legal disfranchisement of the black population in the South. Discussions of "the negro" were not infrequent in the early years of the APSA and took place mainly with regard to this process, and in turn inevitably linked to retrospective evaluations of Reconstruction. It will not come as a surprise that opinion on these issues was near unanimous: the negro was inferior; Reconstruction, bad; disenfranchisement, at least acceptable and more often imperative.

Characterizations of black Americans in *The Proceedings of the American Political Science Association*, *APSR,* and *PSQ* by this cohort of political scientists did not depart significantly from those in Dunning's work—if anything they were more explicitly contemptuous. One writer referred to black people's "it seems, permanent, characteristics" such as "indolence and thriftlessnes" and tendency to steal (particularly chickens) (Stephenson 1906: 60); yet another to their "lack of initiative and inventive genius," mitigated only (as the same writer commented in an earlier article) by their good fortune in being "prepared to receive Christianity" by the tutelage of whites while in bondage (Fleming 1905: 279; 1904: 702).

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This last comment in fact reflects the more hopeful end of the spectrum of opinion as to the possible melioration of “negro inferiority” to be found in these journals. This perspective was informed by a vaguely Lamarckian, social evolutionary orientation toward racial difference, in which such “tutelage” might—in the very distant future—yield substantial improvements, perhaps even to the point of capacity to exercise political rights.

More frequently, black people are presented as embodying the limits to such an orientation. As William Chauncy Langdon had commented, “the negro is not an Anglo-Saxon, or a Celt, or Scandinavian—only undeveloped and with a black skin. … The African is on the contrary a wholly distinct race, and the obstacles to social equality and political co-efficiency between that race and our own are not factitious but anthropological” (op. cit.: 31). For J. A. Tillinghast, reviewing W.E.B. Du Bois’s 1902 edited volume *The Negro Artisan: A Social Study*, explaining black artisan’s relative social and economic status as a function of “lack of training” and “hostile race prejudice,” represented a failure to focus on the “extremely important factor” of the negro’s “inherited nature.” While Tillinghast acknowledged that “secur[ing] a really useful knowledge of negro hereditary endowment and of its divergences from that of the Caucasian is a profoundly difficult undertaking,” he was nonetheless certain that that “endowment” was “not equal to the task set for it under the conditions presented by the United States today.” Fortunately, however, specifying more surely the exact limits of black potential was “surely not…hopeless…. Some beginnings in that direction have already been made, and it is to be hoped that future studies of the negro in this country will devote increasing attention to it” (1904: 701).

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Tillinghast doesn't mention it by name, but those "beginnings" probably included Frederick Hoffman's *Race Traits and Tendencies of the American Negro*. That book, published to much acclaim less than a decade earlier, used actuarial data to conclude that "The conditions of life" were of "less importance than race and heredity" in determining the "tendencies" of a given group of people (1896: 51). For Hoffman, "the negro" embodied a host of unsavory "traits and tendencies," but the most significant of these were "excessive mortality" and its consequence, ultimate extinction. The *PSQ* review of that book wishes Hoffman had given slightly more attention to the "conditions of life," but ultimately endorses with evident relief his "convincing" thesis that "the race of negroes is on the downward grade" and hence unlikely to "menace our republican institutions" (Calkins 1896: 754).

The "fact" of likely extinction did not prevent most political scientists who addressed the topic from advocating measures to limit black participation in "republican institutions." Indeed, a JSTOR search of *Political Science Quarterly,* the *Proceedings of the American Political Science Association,* and the *American Political Science Review* before 1910,[32] yielded just one article joining a substantially sympathetic treatment of Reconstruction with skepticism toward efforts to strip black citizens of the right to vote.

[32] I did not include the *Annals of the American Academy of Political and Social Sciences*, which begins publication in 1890, here because of its looser connection to political science as a discipline; in fact the *AAAPSS* explicitly resisted the sort of disciplinary specialization and vocational distinctions (above all between the theorist and the reformer) that figures like Burgess and Adams were pursuing. If I had included it, I would have found greater sympathy for Reconstruction, not least in the writings of contributor W.E.B. Du Bois.

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This was a reprint of a 1905 address to the American Political Science Association by Albert Bushnell Hart entitled "The Realities of Negro Suffrage."[33] Hart pointed out that while "Negro suffrage" might have sounded radical and "modern" to many of his listeners, "the truth [was] that though there were plenty of restrictions on the suffrage from the earliest colonial times to the present day, those based on race came in late, slowly, and in some communities, not at all" (149). He went on to argue that Reconstruction governments had been unfairly maligned and that negro suffrage during Reconstruction had been incomplete, brief, and "suddenly" terminated in a "violent and irregular process" before "any complete proof of the capacity of the negro to exercise discretion in his vote" could be demonstrated (157). Hart concludes by listing four objections to the various subterfuges in place to restrict black voting: the deceptive nature of the laws themselves, the possibility that negroes will combine with the "small number of whites" also affected by such laws into a "proletariat of people who have no direct way of addressing their discontents"; the attempt through "grandfather clauses" to "set up a privileged class"; and the possibility that racial exclusion will extend to non-Anglo-Saxons, potentially scaring away needed foreign white labor from the South (164). These are summarized into two, more basic objections with which Hart ends his address:

> The principal grounds for criticism are two: first, that the system is really, although not openly, a discrimination between men on the ground, not of their character or their acquisitions, but of their color; secondly, that it means the permanent disfranchisement of the greater part of the negro race, and their

[33] Interestingly, years earlier it had been Hart who had commissioned Dunning to write his *Essays on Civil War and Reconstruction* (Muller 1974). In any event, Hart's relative racial liberalism appears to have been short lived, and he later fell into line on the question of disfranchisement, characterizing negroes as naturally inferior and lacking in civic "capacity" in a 1910 book (see Farr 2004: 39).

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> consequent relegation to a position in which one of the most effective springs of thrift and ambition is removed (165).

This final comment highlights the two registers on which Hart makes his critique. First, he is invoking the liberal tradition that "character and acquisitions" must differentiate men rather than ascriptive characteristics such as "color" (though presumably gender is an acceptable ascriptive characteristic on which to base civic exclusion). Second, he invokes the ("justified") small-r republican faith of "men like Sumner and Lincoln" who had seen that "the suffrage raised and dignified its possessor" (154). By this period, however, whether Hart intended it or not, this justification would also be inflected with Lamarckian notions of racial development, in which habits (such as self-restraint or self-government) could be transformed into racial traits. That is, Hart based his critique of race-based voting restrictions on major elements of the liberal and republican traditions in America and, at least partially, on a "practical" understanding of social and political development informed by contemporary scientific discourse.

What makes this article notable, however, is that it represented an extreme minority position. Indeed, when Hart delivered his paper to the APSA, he was paired with Baltimore Attorney General John Rose, whose talk on "Suffrage Conditions in the South: The Constitutional Point of View" was harshly critical of calls to enforce black voting rights (1905: 166); all three discussants at the panel took a similar stance.

Rose's address (without Hart's) was published in the inaugural issue of the *APSR* under the more direct title, "Negro Suffrage: The Constitutional Point of

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View."[34] Rose offered rhetorical agnosticism on the question of racial equality (radical supporters of black rights may have been "right or wrong"), but claimed that in any event efforts to hasten political equality in the South would only "postpone it" by putting Southern whites on the defensive (1906: 43).

Rose's sense that civil rights for blacks might be viable in principle, but only if achieved slowly and "organically" (and largely as a result of changes in the character of the black population) was matched by two of the three discussants at the 1905 APSA session. The first, John Martin, argued that "the ex-slave, unused to directing his own actions and incapable of coping with his old masters, could not retain possession of the weapon that the North had thrust into his hands; still less could he use it for his own advancement in civilization." On this basis, he asked "Of what use would it be, then, once more to confer upon him this gift at present?" The second respondent, S.C. Mitchell—having, he assured his listeners, "the good of the negro himself at heart"—affirmed that disfranchisement brought "relief" to "conditions that were no longer tolerable" and lamented that "the negro" had "bolted" for politics before "passing through" the "doors" of "thrift, … education, …. [and] religion" (Martin, Mitchell and Shepherd 1905: 166-167).

The third respondent, Henry E. Shepherd, eschewed any pretense at racial liberalism or professions of faith in blacks' eventual uplift, accusing Hart of speaking in "absolute ignorance" of the "demon of negro sovereignty." For Shepherd, Reconstruction constituted the "incubus of negro rule" and its end "the struggle of

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[34] The Stephenson article cited above appeared with Rose's in the inaugural issue of the *APSR*. Only two other full-length articles were included in this issue, meaning that half of the articles in the first issue of the *APSR* were devoted to explicating and ultimately justifying disfranchisement of southern blacks.

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civilization against barbarism—the white man against the savage" (ibid: 169). In fact most discussions of Reconstruction in the journals examined exhibited in the "reasonable" tones of Rose, Martin, and Mitchell. In all these accounts save Hart's, however, slavery was properly in the past but had in its time been at least somewhat beneficial; enfranchising the freed slaves had been folly; and disenfranchisement and legalized segregation ought to be met with understanding by whites outside the South.

Another thing that, however deplorable, ought to be met with understanding was lynching. Treatment of lynching in the *APSR* tended the echo Henry Cabot Lodge's position, articulated some years earlier in "Lynch Law and Unrestricted Immigration" (1891). In that article, which discussed the lynching of a group of Sicilian immigrants, Lodge (trained at Harvard as a political scientist), argued that the evil of lynching could be minimized by excluding the "classes of immigrants" that provoked such lawless behavior (Lodge 1891: 612). The same sentiments are expressed with regard to lynchings of blacks in the South by Shepherd in the discussion that followed the forum on disenfranchisement discussed above—specifically, that by forcing Southern whites to love with free blacks, the north had effectively made it impossible for whites to live under a system of rule of law (Martin, Mitchell, and Shepherd op. cit.: 169-170).[35]

Lynching aside, most of these writers were basically sanguine that a reasonable solution to the "troublesome question" of black presence in America had been reached with the end of Reconstruction (Bancroft op. cit.: 692). For political

[35] Interestingly, lynching is discussed in these terms more often in the *Annals of the American Academy of Political and Social Science*. That is, it is more often deplored, and more often blamed on the (lamentable) impossibility of governing disparate races under a uniform system of law.

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scientists around the turn of the twentieth century, it was not blacks but other racial groups that presented the true "menace to our republican institutions" in the form of the "new" immigration and of other forms of race contact, such as colonial governance (Morris 1906).

The pages of political science journals tended to join in the widespread sentiment around the turn of the century that as the quantity of immigration surged in the final decades of the nineteenth century, its "quality" decreased as a result of an increasing preponderance of southern European "races" among the immigrants. When the "new" immigration was cited favorably, it was almost always in counterpoint to discussions of the necessity of excluding from admission or full citizenship those perceived as even more radically racially different. In a discussion of "Immigration to the Southern States," Walter Lynwood Fleming links immigration to capitalist development, which will be furthered by "displac[ing]" the "unreliable" negro with other sources of menial labor, commenting that, "satisfactory dividends cannot be expected until the country is more thickly settled and is developed by the varied industries which the white immigrant and the northern capitalist will bring" (1905: 297, 284, 279).

For Paul Reinsch, soon to be an important force in the APSA section on Colonies and Dependencies and later president of the APSA (cf. Vitalis op. cit.),[36] "sentimental" efforts to integrate Filipinos into American society selectively recalled prior success in assimilating European immigrants, ignoring the "very different part of our national experience" that "has been supplied by the negro question and

[36] Before assuming the APSA presidency Reinsch also served a stint as Woodrow Wilson's Minister to China.

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Chinese immigration. The former has by this time taught us the lesson that deep racial differences cannot be bridged over by political institutions" (1904: 119). In a similar vein, Ernest Bruncken commented that the "the radical difference between Chinese immigrants and those from any European country—including even the elements most diverse from the original Germano-Celtic stock" (and "the true reason on which the exclusion of the Chinese can be justified") is that even if naturalized, Chinese "could not change in many centuries their fundamental attitude toward life, inherited from more than a hundred generations (1910: 145).

As Vitalis puts it "the papers, addresses, articles, and books of the social scientists that offered their expertise to the new imperial state...assumed the following: hierarchy was natural...it was biologically rooted, and it could be made sense of best by such concepts as higher and lower races, natural and historic races, savagery and civilization, and the like" (op. cit.: 38). And while they differed from Burgess on the extent to which the United States was ready to engage in imperialism, they followed his lead in imagining, again in Vitalis's words, "two fundamentally different logics and processes at work, and thus different rules that were to be applied, across the boundaries dividing Anglo-Saxons or Teutons and the inferior races" (ibid: 5).

Moreover, as both Bryce and Dunning had suggested, the new scientists of imperialism were at pains to take lessons from the experience of the South and its "negro problem." Reinsch, for his part, cautioned against destroying local customs in Africa by pointing to the moral degeneration that he perceived among former slaves in America and offered Booker T. Washington's model of industrial education as a

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possibility for that continent (cf. Vitalis op. cit.: 44-45). Indeed, for all his differences with Burgess, Reinsch in a late text written for the 1911 First Universal Races Conference echoed one of the older man's key contentions -- that "only the fully national" could meaningfully contribute to the civilization of the world (in ibid.: 52).

What becomes visible here that even as specific features of Teutonism faded away, race is still key to political development and to linked notions of rights and democratic legitimacy. And as we shall see once again in the next chapter, this basic set of assumptions was ideologically flexible. Just three years after the *APSR* was launched, a group of (mostly) progressive scholars around G. Stanley Hall at Clark University published the first issue of the *Journal of Race Development* (*JRD*). The *JRD*, too, embraced an expansionist role for America and sought to put social science expertise at the disposal of the state. But in this first International Relations journal published in the United States, the emphasis now lay on a sort of racial philanthropy and ideology of racial "uplift."

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3. *"To Bring Out The Best That Is In Their Blood*:" The Journal of Race Development (1910-1919)

Dorothy Ross (1991), following Bernard Crick (1959), saw the work of Burgess and his cohort as representing a synthesis of Darwin and Hegel. But in fact Darwin as such is very little present in Burgess's work. Burgess was attentive to natural science, but it was to ethnology more than any truly Darwinian notion of evolution that he anchored his Hegelian metaphysics. The triumph of Teutonic civilization may in some sense represent the "survival of the fittest," in Burgess's worldview, but this and similar terminology do not feature prominently in his major works. And while struggle is sometimes rhetorically invoked, in Burgess one finds a much more organic developmental process—the state unfolds rather than evolving through competition with others. That is, while for many Darwin showed that human society and history were grounded in nature and material processes, for Burgess it was more that the inverse was true—physical nature in a sense expressed developing metaphysical essences.

As we have seen, more properly "Darwinian" notions of heredity and hereditary nature did figure prominently in the work of the early figures around the APSA, who tended to see the limits of the politically possible in fixed, hereditary racial traits. Now we will turn to another (partly overlapping) group active in the Progressive era whose members grounded their work even more explicitly in notions of biological evolution. They did so, however, in a much more optimistic vein. This

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was the cohort of writers on international politics and American colonial policy around the *Journal of Race Development* (*JRD)*.

The *JRD* was the first International Relations journal published in the United States. While *PSQ*, the *AAAPSS*, and the *APSR* had all published many articles addressing questions of international law, diplomacy, great power conflict, and colonialism, no specialized publication existed before this time. It was launched in 1910 out of Clark University, by co-editors George Hubbard Blakeslee and G. Stanley Hall. Blakeslee was a China expert who would go on many decades later to advise the State Department on the post-World War II rebuilding of Japan. G. Stanley Hall was a pioneering experimental psychologist, trained by William James. He was a strong exponent of "recapitulationalism," the notion that the development of individuals in the womb and over the life cycle essentially reprised the stages of human evolution. From this he extrapolated a theory of "race children," in which "lower races" could consequently be understood as inhabiting the developmental level of children or adolescents ("something like…an arrested childhood" [Gossett 1997: 154]).

While the editors and contributors overlapped to an extent with the membership of the APSA, the *JRD* was a more self-consciously interdisciplinary affair, aiming to bring the insights of a number of fields to bear on what they saw as an increasingly interdependent and complex world. As such, it brought together leading Progressive Era academics and intellectuals with civil servants, missionaries, diplomats, and others. Over the years contributing editors included sociologists (such as W. E. B. Du Bois and Robert Park), political scientists (such as Blakeslee himself

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and David P. Barrows), historians (including Payson J. Treat) economists (Thorstein Veblen), anthropologists (Franz Boas, Alfred Kroeber, and the physical anthropologist William Curtis Farabee), and geographers (Ellsworth Huntington, later president of the American Eugenics Society), as well as significant numbers of non-academics; contributors ranged from Japanese colonial administrators to John Dewey.

In 1919 the journal changed its name to the *Journal of International Relations*. Three years later it moved to New York to become the house organ of the Council on Foreign Relations, where it is still published today as *Foreign Affairs.* This publishing history is fitting because the *JRD* represents a tradition that is in some ways lost and in others very much with us. The journal's pages show the influence of Lamarckian notions of heredity and crude, climate based evolutionary theory, long since discarded. And the confidence with which its writers speak of "civilizational" progress now seems quaint. However, in the *JRD* we can also see the forging of a language of development and a project of social change through elite scientific and political intervention that remains central to the social sciences, most visibly so perhaps in applications like Development Studies.

Robert Vitalis places the *JRD* within a complex of private foundations, academics, government and other associations in America around the turn of the twentieth century that constitute a "lost world of development theory in the United States" (Vitalis and Markovits 2002; cf. Vitalis 2002, 2008, n.d.). In Vitalis's accounts, these individuals and institutions organized around questions of "development of backward states and races (…) and what kinds of interventions if any are effective" in promoting it (Vitalis and Markovits 2002: 7). For him, it is in

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this "lost world," rather than in postwar politics and academia, that we should look for origins of the lines of inquiry and practice that later coalesced around "area studies" and the field of international relations. This chapter explores how the *JRD*'s writers and editors conceived their own enterprise, and the analytical tools they brought to bear on it.[137]

The most important such tool was an indeterminate boundary between biology and society. The *JRD* returns time and again to a notion of "civilization" in which, just as for Burgess and his Gilded Age cohort, race, culture, and political institutions together can be mapped onto evolutionary processes. Evolution itself appears as both natural and cultural, the embodied accretion of influences over time. It is this slippage between nature, culture, and society that made the *JRD*'s intellectual enterprise feasible. But, in contrast to Burgess and to some of the more racially conservative elements in the APSA, many in the group around the *JRD* found in the intertwining of nature, politics, and historical development the possibility for a far-reaching and potentially world-historical program of racial uplift.

As the title of this chapter suggests (it is taken from G. Stanley Hall's editorial in the premier issue), the *JRD* was dedicated to the proposition that the application of scientific knowledge could bring forth latent possibilities in the "blood" of peoples. That is, in common with most Progressive Era intellectuals, its contributors and editors largely understood "races" to be the basic units of history and "evolution" to be its motive force. What animated them was the hope that science

[37] This chapter concentrates particularly on the first few years that the Journal was published, partly for the sake of manageability, but also partly because the tone and focus of the journal changes somewhat with the outbreak of and American entrance into World War I.

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could harness evolutionary forces and put them to the service of "development." That is, if Burgess's Whig historico-politics saw the progress of civilization as the flowering of a racial telos, the idea of development elaborated in the *JRD* reimagined it as an active, politically and socially directed but still essentially racial project. This project, crucially, depended on the mutual permeability of nature and society, particularly the idea that just as nature shaped political and social institutions, changing political and social institutions could in turn leave their mark on an evolving nature.

While the *JRD's* personnel attests to the somewhat chaotic disciplinary matrix in which early twentieth-century academic work on international relations was situated, its editors clearly hoped that it would help to systematize international relations, the study of colonial administration, and "race development" as a field of study independent of the emphasis on international law that had dominated these areas in Burgess's time. And it met with some success: By the time it had begun publication as *Foreign Affairs* in 1922, courses specifically on "international relations" began to be offered by political science departments, and questions of international law and international relations increasingly to be understood as requiring separate sets of experts (Rogowski 1968: 400-401).

However, for present purposes the contrasts between the *JRD's* self-conception and that of the contemporary field of International Relations are more interesting than the continuities. In this early articulation of what was to become a subfield of political science so well established as to have its own acronym (IR), as for the more central founders of the discipline discussed in the last chapter, the

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biological and the political were inextricable, and the past and future of political institutions was to be sought in the register of evolutionary time. Significantly, however, for the writers associated with the *JRD*, political institutions not only sprang from nature, they were capable of directing nature's developmental course.

The JRD and Early-Twentieth-Century Racial Thought

The *JRD*'s high-powered cohort of writers generally expounded an expansive vision of America's role in the new century and the possibilities for worldwide progress and peaceful coexistence. For most of them, America was to lead the world in the uplift of the "backward" or "dependent races." This was to include colonial subjects (as in the Philippines), those of sovereign states (as China and Liberia), and "dependent" peoples within America's own borders ("the Indian" and "the Negro"). For some, this implied a sort of tutelary, temporary, paternalistic administrative/colonial endeavor; for others it meant something closer to what we would now understand as development aid.

It was almost always presented as a kind of reform. The idea was that even if they could not be erased, the meanings of racial differences could be changed for the better through education and political reform. While who really qualified as "backward," and how much so, were points of difference, it was an article of virtual consensus in the journal that backwardness itself could be accounted for by variations in developmental processes. This drew on conventional scientific and popular wisdom, as well as established doctrine in American social science, as we saw in the

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last chapter. All the same, unlike Burgess, Adams, and others who had worked in this tradition, the *JRD*'s editors understood themselves at least in some respects as advocates for those subject to imperial rule. Indeed, intellectually they shared much with Pan-Africanist ideology (associated of course with contributing editor Du Bois), both in seeing themselves as aligned with the aspirations of "the darker peoples" and as imagining those "darker peoples" to be in need of improvement (Stein 1989: 83; cf. also Stein 1986, ch 1; Reed 1996).

In the editorial that opens the first issue of the *JRD* in 1910, editor Blakeslee is critical of European colonialism and tries to differentiate his own project by explaining that the journal

> aims to present (…) the different theories as to the methods by which developed peoples may most effectively aid the progress of the undeveloped; (…) not how weaker races may best be exploited, but how they may best be helped to be stronger (1910: 1).

This endeavor was to be carried out on American soil as well as abroad—in the "struggle" that he called the "key to the past seventy-five years of American history": finding "some solution for the negro problem" (ibid: 4). Here Blakeslee offers a mild criticism of the American people. A few pages later, G. Stanley Hall is firmer, citing the "innumerable modes of extortion and misrepresentation that private greed is still allowed to practice upon…the Negro" in America (1910: 6-7).

So while the *JRD* generally affirmed the superiority of Anglo-Saxon civilization, it rejected some of the more vicious forms of white supremacist thought that were widely acceptable at the time. For many Progressives, as for most of those discussed in the previous chapter, Herbert Spencer and William Graham Sumner had

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"proved" that races were, in the words of one prominent historian at the time, "the fundamental division of mankind" with differences that, because they lay "in the very blood and physical constitution," could be altered only by the "slow progress of the centuries" (in Smith 1997: 416-17). Such views were commonplace; the Jim Crow system, unevenly but steadily taking root at this time, was often justified on the basis of 'science" which had disproved sentimental or religious notions of human kinship (cf. ibid: 417-18). They also figured in the discourse of anti-immigration societies and politicians, and in modified form in advocacy of U.S. military intervention to curb Japanese power in the Pacific.

The *JRD*, in contrast, mostly (if inconsistently) took the position that the mental and physical capacities of the races were not so deeply different, and occasionally argued for their basic identity. (*JRD* writers were particularly likely to laud the Japanese, whose role as a colonizer in Korea and China many saw as parallel to American efforts in the Philippines.) More generally, where others saw in evolutionary theory the scientific explanation of fundamental, permanent racial difference, they found a field of possibility for intervention and positive change. A small piece of evidence that they were understood to be advocates of non-white peoples is a 1914 review in *The American Journal of International Law* of a book edited by Blakeslee. The book in question is a collection of Japan essays from the *JRD*; the reviewer comments that, "As was to be expected, these lectures show decided pacific leanings [with] pronounced emphasis on the factors tending to draw the United States and Japan together, and the effect of the whole is to leave the feeling that the various contributors are too sanguine" (Krehbiel 1914: 180).

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The *JRD* certainly published its share of pro-colonialist rhetoric. To give just one example, in January of 1911, William S. Washburn, a former U.S. Civil Service Commissioner in the Philippines, contributed an obituary for an American military officer who had served in the Philippines. The title was, “A Worthy Example of the Influence of a Strong Man upon the Development of Racial Character,” and it described the deceased “a man who, by temperament, force of character, and training, was fitted to rule as a benevolent despot in a land where ignorance, treachery and tribal enmities bound the inhabitants to barbarism” (373). However, they also published articles critical of American attitudes and policies, with the occasional blistering denunciation. A 1912 article called “A Literary Legend: ‘The Oriental,’” by one Wm. Elliot Griffis is a striking example.[338] Griffis begins,

> Writers have created the “Oriental” of imagination, fancy, prejudice and bigotry, who has no counterpart in reality or has [sic] ever existed. It has become a “vested interest,” a staple and stock in trade, a permanent and ever-promising speculation to picture “the Oriental” as a being in human form whose nature is fundamentally different from the “Occidental.” Such a delineation and contrast has mercantile value. It pays in what the American loves so dearly—money (65).

It does so by enlarging

> the sale of tickets at the box office [and] the circulation of the newspapers. It delights the mob (…). The “Orientalism” which sells (…), which gets up periodical war scares and from nervous congressmen compels votes for big battleships, or which is set forth by politicians bidding for votes is not intrinsically different from that which was and is dearly loved in Europe. (…) Yet probably in no country more than in the United States of America, is our legacy of prejudice against “the Oriental” so worked in the interest of dollars and cents (67).

[38] This article was included in the book discussed by Krebhiel (op. cit.).

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Some writers went so far as to contest the idea that races can be ranked hierarchically (an October 1914 article by Wilson D. Wallis of the University of Pennsylvania targets Hall's "race children" theories on this score) or to advocate race-mixing. That same October 1914 issue contained a piece titled that concludes with the "hope the day may come again when the Pacific Ocean will become the intermediary for bringing together the innumerable racial globules that seem past amalgamation" (Brown: 159). However there were always tensions. Intermarriage was occasionally proposed as a solution to the "Indian problem," but this had less to do with racial harmony and integration than with obliterating Indians by gradually turning them into white people. Intermarriage between blacks and whites was never advocated.

Taken as a whole, the *JRD* presented complex and sometimes heterodox racial attitudes that were nonetheless anchored to common understandings of the workings of race in history (or perhaps more properly of the workings of history through race). In this sense it mirrored both the political science of the Gilded Age and the larger climate of Progressivism, in which myriad political projects emerged from Rogers Smith has called an "elite convergence" around "beliefs in empirical scientific expertise, experimentation, efficiency via rational organization, evolution, pragmatically defined values, and the fundamental reality of human interdependence" (Smith op. cit.: 412).

A period of significant industrial development, immigration, urbanization, and imperial expansion in America, the period between the turn of the twentieth century and the country's entrance into World War I is remembered on the intellectual front for an upsurge of optimistic reformism. Herbert Croly's *The Promise of American*

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Life, perhaps the paradigmatic statement of centrist Progressivism, argued that American prosperity, free political institutions, and the "worthier set of men" these would create offered "the highest hope for an excellent worldly life that mankind has yet ventured" (Croly 1909: 5). It was an era of great enthusiasm for organizations. Where earlier republican theory had held that America's promise and values could only be realized in a nation of independent small producers, and where for figures like Burgess and Adams had seen the expansion of government and imperial ambitions as a major threat to liberty, now for many it was modernity itself—big, efficient institutions, including government, corporations, and labor unions; expanded trade; and, for some, overseas territory—that would save American democracy from the twin scourges of economic depression and socialism/labor unrest (Ross op. cit.; Smith op. cit. ; LaFeber 1998). Fueled by support from industrialists interested in promoting scientific and technical progress, an explosion of professional societies, universities, and specialized journals generated opportunities and prestige for a newly self-conscious intellectual class (Ross op. cit.: 158-61).

It was also in this period that America became conscious of itself as a major player on the world stage, with new territories and newly consolidated spheres of influence in Asia, the Pacific, and the Western Hemisphere, the farthest extension on its soil a system of legally sanctioned racial segregation, and an increasingly aggressive colonial policy toward Native Americans that aimed at extinguishing tribal governments and freeing up collectively owned land for white settlement. According to Smith both major parties in the 1912 elections as well as Theodore Roosevelt's Progressive third party portrayed the United States as a "modern democratically and

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scientifically guided nation that was also culturally ordered, unified, and civilized due to the predominance of northern European elements in its populace and customs." These characteristics, they promised, fitted America not just to "cope with a rapidly changing world," but to "lead it" (op. cit.: 411).

Still, convergence around basic premises did not imply political agreement; left Progressives such as John Dewey called for democratization of both social life and industry, while for Croly centralization of both state and industrial power was the key to the future. On that spectrum, the *JRD* can, by reputation and personnel, be generally situated toward the left, along with many of the intellectuals that Smith and others see as furnishing much of the basis for the reform tradition in American social science (Smith op. cit.: 411, 419-424; cf also Ball 1995, Torgerson 1995).

The JRD's vision of America in the world

During the decades around the turn of the twentieth century, American foreign policy makers tended to see American possessions overseas as stepping stones to regional markets. A chief argument of antiannexationists in the late 1890s was that trade could be maintained without political control. For those associated with the *JRD*, these debates had missed the point—these possessions were not only means to the end of trade; they were flagships in a developmental project to which trade would be both a contributing factor and an outcome.

In a speech at a 1910 Clark University conference on the Far East, reprinted in the *JRD*, the Commissioner of the U.S. Civil Service in the Philippines makes the

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connection between domestic Progressivism and the *JRD*'s sense of its own mission: "The reform movement is dominant now in America, insisting not only on clean governmental operations, but also the enactment of laws for the betterment of the people, for their moral, mental, and physical elevation." It is "desirable and essential that these reform influences and the power of public opinion in the United States extend to the Philippines." This will "give the Filipino people every opportunity for development" (Washburn 1910: 40-41).

The *JRD* presented its agenda as an alternative to European-style colonialism. In his first and only editorial, cited above, Blakeslee explained that the *JRD* was to be devoted to the "general subject of the control of dependencies, a field in which there has already taken place a profound change of feeling and belief." The European idea of colonies as resources to be exploited was

> giving place to that recently introduced by the United States in the Philippine Islands—the policy of controlling a backward people only so long as it may be necessary to train them to carry on successfully an efficient government (op. cit.: 3).

Hall, too, urged the United States to take a path different from Europe's. For him, the great "possibilities for historic development along new lines" represented by the "ascendant" races were being stifled by modern colonial policy. In "The Point of View Toward Primitive Races," he wondered what would have happened if the Romans had exploited their European territories the way England was taxing India or Belgium exploiting the Congo, or if Japan had been partitioned in 1840. For Hall, America, which had "so lately become a competitor in the struggle (…) to parcel out among the leading nations all the remnants of the unappropriated territory of the

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world, ought to lead in this more humane and larger policy." He urged the establishment of an African Bureau in Washington, D.C., to exhibit the accomplishments of "the African."

> We should strive to make representative colored men self-respecting, (…), in a word, to bring out the best that is in their blood, and to mitigate surely, if ever so slowly, the handicap of race prejudice, for these things alone can give the black man true freedom (op. cit.: 6-7).

A similar policy was desirable for "the Indian," or "red man," by putting the government Bureau of Ethnology to greater use. It is interesting to note that here, "race prejudice," appears as failure to properly appreciate race *differences*. That is, rather than seeing black people as insufficiently rational, for example, whites and blacks themselves should appreciate blacks" unique traits, the cultivation of which will presumably eliminate race prejudice by eliminating its basis: blacks' retarded development.

The thrust of Hall's argument, however, is that while Europe had been exploitive, the United States should pursue a policy of "uplift" that would be both beneficent and informed by science. In a 1911 article entitled "Geographic Factors in South Africa," one W.M. Davis took an ominous tone about British administration in that country. What was lacking there, he implied, was "A trained understanding of anthropological problems, supported by a sympathetic interest in the well-being of native races." As it stood, the British risked "tempting the majority to violent revolution" (146).

For these writers, America was suited to the task of uplift by its national history and character. The whole of the July, 1914, issue was devoted to Latin

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America, particularly the development of political and economic institutions and new trade opportunities with the opening of the Panama Canal. Questions of trade balances, Latin American perceptions of American intentions, natural resources, and the like were discussed. But the developmental status of Latin American civilization is central to the discussion. The issue opens with something like a plea for racial indulgence from a Peruvian Envoy named Federico A. Pezet. In “Contrasts in the Development of Nationality in Anglo- and Latin-America,” Pezet argues that Latin America was disadvantaged relative to its northern neighbors in its prospects for developing republican institutions and prosperous economies. For Pezet, Anglo-America was colonized by homogenous Pilgrim families (reminiscent of Burgess and Bryce’s “race-proud Teutons” [see Chapter Two]) who confronted only relatively weak savages, leaving them plenty of virgin land. Latin America, by contrast, was settled by fortune-seekers from Iberia, who mixed with the stronger, more numerous native population, living off the latter’s wealth and labor rather than establishing homogenous, self-supporting colonies. Closer to Europe both geographically and climatically, North America got the “better” western European immigrants who were able to assimilate to the republican institutions inherited from the English.

Pezet summed up the Latin American disadvantage as follows: “Latin America, at the time of its inception into the family of nations, was a group of disassociated military nations, utterly unschooled in self-government, and inhabited in greater part by unfused races” who “from despotism and servitude (…) jumped into the most advanced form of government” (12-13); He concluded with a plea for greater understanding and aid in the “common quest for human uplift” (18).

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Here, the United States appeared as a potential partner with Latin-American elites in the cause of uplift. This is a twist on a more general theme in the *JRD*, in which the task seemed largely to consist in the *creation* of an elite, or rather of the right kind of elite. Evolution was never clearly defined in the journal—it seems to be less a definite concept than an interpretive grid through which to view the world. However, what is consistently clear is that it has something to do with progressive differentiation: from simple to complex organism; from simple to complex society. This question will be explored more fully below, but for the moment it should suffice to note that in "race development" this seems to translate into the move toward a capitalist division of labor and the appearance of class differentiation (to be distinguished from differentiation based on rank or "organic hierarchy," thought to be a characteristic of more "primitive" society). The developmental task then consists in establishing and maintaining such differences.

Articles on the Philippines in particular discussed techniques for creating "native" leadership. Training Filipinos to assume (gradually) higher positions in the civil service is the focus of Washburn's article in the very first number (also cited above). In it, Washburn writes that if Filipinos are "left to their own resources" they will fall into corrupt oligarchy (op. cit.: 46). Hence, "As stated by President Roosevelt in one of his messages to Congress, "It is important that this—the merit—system be observed at home, but it is more important that it be rigidly enforced in our insular possessions" (ibid: 53). That is, by rewarding honesty, education, and hard work, the U.S.–administered civil service can be the incubator for a class that will lead the Philippines and help to make them "in time—probably not in your day nor in mine—

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partially at least if not fully prepared for self-government" (ibid: 55). These sentiments appear repeatedly, as when contributing editor David P. Barrows's *A Decade of American Government in the Philippines, 1903-1913* is reviewed in 1915 by Payson Treat, who seconds Barrows's emphasis on the need for the education of a political class.

Articles devoted solely to American "Negroes" were relatively rare, but one such did appear in 1915. Howard Odum's "Standards of Measurement for Race Development," emphasized class differentiation. He endorsed the idea that race progress can be measured by "the degree to which [a race] tended to increase the proportion of its population above the lower classes…to the increase of the great middle class and especially the upper half" (378-9). And while articles were scarce, books on the subject were frequently reviewed, and generally commended to the extent that they identified unscientific prejudice as the chief factor condemning the large majority of black people to the lower classes and limiting the aspirations—and hence the salutary leadership—of the "better class" (a "Notes and Reviews" section in 1915 [Unsigned] is especially rich in this regard).

Sometimes the leadership to be established was by one non-white "civilization" over another. A 1910 article by W. Morgan Shuster on "Our Philippine Policies and their Results" advocated with solidifying the dominance of Christianized Filipinos over "Moros" particularly but also over "uncivilized" pagans. This was to include establishing a Jim Crow-type system of separate jurisdictions and administrations. Shuster thought that the Filipinos were "Christians and by nature

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peaceable," and the "pagans" could be reached, but that "a strong, quasi-military government is the only one suited to deal with the Moro problem" (61).

This view was contradicted in the lead article of the April, 1915, issue by John P. Finley, a Lieutenant Colonel and former Governer of a Moro province in the Philippines. In "The Mohammedan Problem in the Philippines," Finley defended the Moros against such charges, arguing to the contrary that they were highly civilized and should be afforded a much greater level of autonomy, away from the less civilized Filipinos.

In Liberia, too, the problem appeared as establishing proper leadership and maintaining its control. Two articles on that country, one by the black scholar and diplomat George W. Ellis ("Dynamic Factors in the Liberian Situation") and the other by one Emmet J. Scott, both of whom had filled official U.S. government posts there, appeared in 1911. Scott's title posed the question, "Is Liberia Worth Saving?" Both articles answered in the affirmative, presenting the "Americanized" Liberians as a vanguard civilizing their race-fellows on the continent, and suggesting that this experiment was threatened by European power struggles in the region as well as by recalcitrant natives. The "Americanized Liberians" had "helped to uplift the natives—to no considerable degree, it is true, but nevertheless to an appreciable degree" (Scott 1911: 301). To fail to support the Liberian government against both threats would have been to sacrifice those gains and also to betray the trust of the Liberian elite, who deserved support in their efforts to establish control over the countryside.

Uplift did not have to be altruistic. Apart from short pieces by the vice-president of Miami University and the Director of the International Bureau of

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Students (arguing for the professionalization of Latin American universities, and increased exchange programs, respectively) the Latin America issue discussed above was uncharacteristically short on the rhetoric of benevolence. For example, "The Development of Our Latin-American Trade" by American businessman John Hays Hammond emphasized America's need for Latin American markets (1914: 44-8); and Hiram Bingham, of the Yale history department, speculating on "The Probable Effect of the Opening of the Panama Canal on Our Economic Relations with the People of the West Coast of South America" called for caution against over-exuberant investing, noting that the Indians of the region were "not ready for a boom" (1914: 64).

An article by W. D. Boyce, the publisher of *The Saturday Blade* and *The Chicago Ledger,* on the "Advantages of Making the Canal Zone a Free City and a Free Port" was more boosterish. But Boyce's stated motivations and his view of the disadvantages facing Latin America were unexceptional, bringing together the notions of developmental disadvantage (particularly its evolutionary and climatic origins) with recommendations for American businessmen and policymakers. He began with an overview of the history of human settlement of the Americas, noting that understanding South America's commercial development required that one "first analyze the original stock from which these people sprang" (1914: 68). In his view, the first settlers came from Asia across the Bering Strait, and hunted and fished their way southward. Thus employed, "[t]he Indian improved until he reached the warm country near the Rio Grande, and there in the hot climate, where life was easy he began to deteriorate" (ibid: 68-9). (Boyce did concede that some of this deterioration

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was offset in the temperate zones of the Andes, allowing for the development of Inca civilization.) His prescription was an energetic policy of free trade and of doing business "everlastingly on the square." As to the latter, he remarked that Latin Americans were "not used to it, but they will like it once they find it genuine" and would as a result come to prefer trade with the United States over Europe (ibid: 83).

Evolution, change, and heredity

As noted, turn-of-the-century social thought was deeply influenced by evolutionary concepts. Most of these long preceded Darwin, and conceived of evolution as a basically unilinear process—from lower to higher, less differentiated to more differentiated, savage to civilized—in which the social, cultural, and biological traits of a group developed in tandem. It was a framework in the popular scientific imagination as much or more for understanding human than animal differences, and its main application and evidence was the explanation of differences in appearance, customs, religion, and manual arts among groups of people in the world.

Among the most prominent early explanations for such biological change came from Auguste Lamarck, a French zoologist and botanist writing at the turn of the nineteenth century. Known as the doctrine of heritability of acquired characteristics, Lamarckianism held that adaptation to new conditions could lead to structural modification in adult individuals; new actions, "becoming habitual" could "occasion… the development of the organs which execute them" (in Stocking 1982 [1968]: 238-9). These new traits, or "organs" were then passed down to offspring.

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With the publication of Darwin's *Origin of the Species*, the subsequent rediscovery of Mendel's experiments on heredity, and a campaign by August Weismann against Lamarckian ideas, the processes and mechanism of evolution became the subject of heated and sometimes vicious debate. However, Lamarckianism was not wholesale discarded, and elements of Lamarckian thought were mixed with theories of natural selection and even, as we shall see, of mutation, both by "neo-Lamarckians," and by others who incorporated them less self-consciously (cf. Stocking op. cit., Ross op. cit., Reed op. cit.).

In his influential work on Lamarckianism in American Social Science, George Stocking argues that, "The idea that acquired characteristics might be inherited was stated or implied in the work of so many [turn-of-the-century] writers that it is impossible to avoid the conclusion that they were primarily reflecting a widespread popular scientific attitude whose roots lay deep in the western European cultural tradition" (Stocking op. cit.: 242). This may help explain why the logic if not the substance of Lamarckianism is so prevalent in the pages of the *Journal of Race Development*, despite the fact that in Stocking's view, the fight in the biological sciences had been by 1910 pretty much settled, with the heritability of acquired characteristics on the losing side.

The great majority of *JRD* articles focused on differences of education, customs, and living standards rather than biology. However, physical evolution was clearly understood as a related concern: many articles focused primarily on various aspects of biological evolution and, as we have seen, even topics that would seem quite far from questions of biology (like Boyce's article on eliminating tariffs in the

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Panama Canal Zone) were often prefaced by a brief overview of a few millennia of human migration and settlement. Articles on racial diversification in the Pacific region, the probable racial origins of the indigenous people of Latin America, the geographical origins of the peculiar racial characteristics of South Africa's "native races," white adaptation to tropical and subtropical climates, etc., were published without fuss; their inclusion required no justification, and no major discontinuity in perspective appears between these biologically oriented writers and their more socially minded colleagues.

This should not be surprising. As noted, most intellectuals at this time felt no need for clear-cut distinctions between nature and society. In Stocking's view, the lack of an autonomous subject matter doomed American social science in this period to be tentative and ineffectual. For him, the central problem for the social sciences in the early twentieth century "was not their domination by notions of biological or racial *determinism*, but rather their obfuscation by a vague sociobiological *indeterminism*, a "blind and bland shuttling" between race and civilization" (Stocking op. cit.: 265, emphasis original). It is clear this 'shuttling" was a crucial feature of what was going on in the *JRD*, but it hardly restricted the basic project. On the contrary it appears central to the uplift enterprise, as a way of simultaneously maintaining the boundaries between groups, establishing science and scientists as the authoritative source of practical knowledge about those boundaries, and giving science a clear entry point for directing change. The latter feature is what most clearly differentiates the *JRD*'s version of "race development" from Teutonism, for example. For Burgess, the role of political science was to identify the historical trajectory of

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civilization, in order to more or less get out of its way (as by preventing the "higher races" from being diverted from their course by too many dealings with the lower ones). The editors of the *JRD* saw the civilizing process as a much more dynamic one, in which knowledge could play an active, shaping role.

Both the indeterminate boundaries between what is natural and what is social and the confusion of Lamarckian, Darwinian, and Mendelian understandings of evolution are abundantly in evidence in a 1913 article by contributing editor Thorstein Veblen, entitled "The Mutation Theory and the Blond Race." In this article, Veblen explores what he describes as "two distinct but closely related captions: The Origin of the Blond Type, and the Derivation of the Blond Culture," in light of evolutionary theory in which change originates in mutation rather than through "usages" (492). His thesis is that there is one true "dolicho-blond" in Europe, "in the lands immediately about the narrow Scandinavian waters," and that the other "blond groups" are in fact "hybrid types" (ibid: 502). Veblen's interest in this topic, and his desire to isolate the natural germ of what he in other works characterized as the most progressive world culture, is noteworthy in itself, though clearly consonant with his emphasis on economic life as the result of the interaction of "instincts" (a concept that he, like G. Stanley Hall, took up from William James) with "institutions" (cf. Anderson 1993: 3). But in this context it is important to note what he makes of the new biological science of the day.

A central problem for the Darwinian theory of natural selection was the "origin of the fittest"; that is, where did variation come from in the first place? Veblen perceived correctly that Mendel's experiments would hold the key to that problem,

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though as it happens he was wrong as to how they would do so. What Veblen does, rather, is turn mutation theory into a theory of adaptation that mimics Lamarckianism in many important respects. That is, in his view, the "Mendelian postulate that the type is stable except for such a mutation as shall establish a new type" raised "at least the presumption that such a mutation will take place only under exceptional circumstances, that is to say, under circumstances so substantially different from what the type is best adapted to as to subject it to some degree of physiological strain" (ibid: 495). In this case, the "parent stock" entered Europe from Africa sometime in the late quaternary period, where it was "exposed to notably novel conditions of life, such as would be presumed…to tend to throw the stock into a specifically unstable (mutating) state" (idem). That is, external conditions would call forth appropriate mutations, in enough individuals that while they would inevitably have to mix with the parent stock, a new, "pure type" could in time arise. Moreover, culture itself could be among these environmental factors: "characteristic forms" arise "in adaptation to the peculiar circumstances of environment and culture under which each particular local population is required to live" (504).

In this scheme, geography and culture—the two main stimuli for change in neo-Lamarckian theory—retain their causal centrality. There is no room for the random, purposeless quality imputed to mutation by later understandings. Evolution thus remained something that could be directed—if culture could prompt mutation, changes in education, administration, and industrial organization could still write new characteristics into the bodies of populations. Which of course was central to a project

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of uplift in which racial distinctions were entirely "real" and important but at the same time remediable.

We have already seen that just as races can "develop," they can also decline, as in Boyce's treatment of the native populations in South and Central America. This of course had been at the heart of Bryce's concern about colonial expansion—the idea that America should not expand into climates where whites would not thrive. In the *JRD* the notion has two important implications that underlay much of the concern about development in general. Firstly, the opposite of "development" would not necessarily be stasis: it would more probably be exploitation by more developed peoples, decline, degeneration, and possible "race war." This anxiety was alluded to more often than directly stipulated, but it is nevertheless palpable, particularly in discussions of the situation in the Pacific and of what Davis in the article cited above referred to as "the advance of a civilized race into the land of an uncivilized race" (1911: 139) in Southern Africa and the Americas.

Also implied is that whites, too, faced possible collective decline. This was a pervasive anxiety in the context of industrialization and the "closing" of the frontier—the end of the republican image of America as a land of independent small producers, immune to European decadence. Ross argues that this anxiety produced a crisis among intellectuals concerned to replace this nineteenth-century vision of America's exceptional destiny with one appropriate to a new age. This crisis produced a range of responses, from the call for imperial expansion to replace the lost frontier, to a number of populist and/or antimodernist rejections of cities and

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industrial production, or alternatively to the embrace of modernity that characterized most Progressive intellectuals. For this last group,

> the realization of American liberal and republican ideals depended on the same forces that were creating liberal modernity in Europe, on the development of capitalism, democratic politics, and science. America's unique condition did not block the full effects of modernity on this continent, but rather supported it (Ross op. cit.: xv).

However, this did not mean that America's ideals would be realized automatically. Rather, science would determine how the course of modernity could best be directed. It was this spirit, in large part, that animated the academic reformers of the era; it was certainly this spirit that animated the group behind the *JRD*.

While the *JRD* did not generally advocate expansion by force of arms, or settler colonialism, it consistently presented America's future as bound up with that of the "non-Western" world, and advocated outward-looking policies. However, the new, internationalist world they envisioned carried inherent dangers. What would happen to whites as they ventured into the territories of "the dark races"? As we have seen, Latin America was sometimes presented as a cautionary tale on that score (by Latin Americans such as Pezet as well as by North Americans). The writer who attended most thoroughly to this question was contributing editor Ellsworth Huntington.

Ellsworth Huntington was a leading geographer who later became the head of the American Eugenics Society. He was author of a number of books as well as articles in nine major Geography journals and more than 40 other publications, ranging from the *American Historical Review* to *The Nation* (Visher 1948). In his first major book *The Pulse of Asia* (1907), he elaborated the "Huntington Theory," which

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suggested that significant and irregular climate changes, probably caused by changes in the sun, had profoundly influenced human culture. In his view, weather had stimulating (storms) and stultifying (unchanging heat) effects on the human constitution; the worldwide distribution of civilization could be explained by the distribution of temperate climates. His theories while not, as we have seen, original, were enormously influential, and he was responsible both for systematizing climate theory to a degree not seen before and further popularizing it among social scientists.

Huntington published four articles in the *JRD*: "Physical Environment as a Factor in Turkey" (1911), "Geographical Environment and Japanese Character" (1912), "A Neglected Factor in Race Development" (1917) and "The Adaptability of The White Man to Tropical America" (1915). The latter argued that the riches of tropical America and Africa will be developed only with the "help" of "people of European origin." The stultifying heat and evenness of the weather in the tropics, as well as the ease of life, are "conditions which for ages have acted as handicaps to every race whose lot has been cast in" the tropics (187). Such conditions make one "loathe to work" in general; prolonged exposure to them however, turns such lethargy into racial traits. He suggests that a few generations of habitation by whites in such regions could cause a like degeneration among them. (This would be compounded by the "fact" that "[e]xperience in all parts of the world shows that the presence of an inferior race in large numbers tends constantly to lower the standards of the dominant race" [ibid: 193].) The remedies, once again, were scientific and institutional: Huntington looked to advances in medical science to offset the corrupting effects of tropical disease and to (unspecified) advances in hygiene and institutional

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arrangements to prevent the debilitation of white generations by contact with their "inferiors."

There is a sense, then, in which the *JRD* represents a transitional moment. After World War I, the field of international relations will increasingly define itself, at least at the rhetorical level, by its rejection of the sort of sweeping, legalistic historicism that Burgess and others had elaborated—this small-p progressive view of the world seemed too clearly to have been disproved by the war. Instead, IR was to offer a set of "realist" interpretations of international realities as they stood. The *JRD* of course occupies something of a middle ground between these orientations. That is, while its general orientation was firmly of the social-evolutionary type, its emphasis on active policy intervention and ethnographic specificity in the study of other nations, as well as its rejection of the sense of a "natural" international order is clearly in line with the set of IR institutions, courses, etc. that emerged in the early interwar era (See Rogowski op. cit.: 403).

At the same time, the world the *JRD* describes, in which political institutions and human bodies are evolving together, and in which politics and culture have the potential to mould nature, or at least accelerate its processes, now appears deeply strange. Indeed, the persistence of Lamarkianism and its commingling with Mendelian genetics in the pages of the *JRD* seems a testament to political scientists' commitment to the unity of race and history, as the new theory is turned to old purposes, and understood in ways that reinforce rather than undermine the basic conceptual apparatus inherited from Burgess's generation.

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It is certainly radically different from the world that political science describes today. The next chapter will look at the dismantling of this world within political science, and its replacement with a more familiar, more "modern" one, in which nature is disentangled from both history and politics, or, in Bruno Latour's words, in which one can "distinguish clearly what belongs to atemporal nature and what comes from humans." For Latour, this is the distinguishing effect of "modern temporality" (1993:71). In the next chapter we will begin to see that political scientists arrived at a version of this temporality by rethinking the connections between race, history, and human capacities.

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4. *"To Lay These Specters To Rest:" Political Science Encounters the Boasian Critique of Racial Anthropology*

In earlier chapters I have discussed the functional identity of racial and historical development for significant currents of late nineteenth and early twentieth century American political theory. Now I will turn to a dramatic disruption of that link. In this chapter and the one that follows I argue that two developments in the science of race and ascriptive hierarchy are key sources of an interwar-era transformation in the way political science understood its own enterprise, and to the images of liberalism and democracy that it began to produce. These are the critique of evolutionary anthropology by Franz Boas and his students and the publicity attendant to the Army intelligence-testing program during World War I.

According to Dorothy Ross, it was in this period that Chicago political scientist and Social Science Research Council (SSRC) founder Charles Merriam's "scientific program began to transform the traditional concerns of the discipline." This transformation set his students—notably Harold Lasswell—on what Ross calls a course of "extension and elevation of scientism in the profession." For most historians of the discipline, this new course represents a significant modernization of the discipline, at least in the sense that it is when American political science begins to look recognizable to practitioners of the discipline today. More specifically, Ross locates interwar political science, particularly as practiced at the Chicago department led by Merriam, directly on the genealogical line of 1950s behavioralism (of which, for her, Merriam is "rightly the grandfather") (1991: 452, 457). Robert Dahl, a self-identified veteran of the behavioral revolution, concurs, naming Merriam's

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contribution to political science as one of the "specific…very powerful stimuli" to the subsequent "rapid flowering of the behavioral approach in the United States."

For Ross, Merriam's key contribution is a commitment to "scientism;" for Dahl, it is his fostering of a "new mood of scientific empiricism" (Dahl 1961: 763). However, as John Gunnell points out, these terms, like many used to characterize this disciplinary moment, are exceedingly vague. Moreover, commitments to something called "science" and to empirical evidence were hardly new in the discipline, and in fact constituted central claims to authority for the Teutonic theory of the founding generation. This leads Gunnell to characterize the interwar period as one more of continuity and "methodological refinement" than of real change in the discipline, though he concedes that "hold[ing] Merriam's post-1920 work up against the arguments of some of the most influential members of the Columbia school from which he emerged, always seems to evoke a sense of contrast" (1992: 134). This chapter, and the one that follows, claim that attention to the discipline's engagement with race and racial thought during this period can help to specify the content of that contrast.[39] That is, the methodological innovations of this period are connected to fundamental shifts in political scientists' understanding of the relationship between nature and politics, and of the relationship of both of these to historical and evolutionary time—shifts that are effected through engagement with the new sciences of race emerging from anthropology and psychology.

To make this argument, these chapters examine a subset of political theory literature in the 1920s, produced in large part by graduates of Burgess's Columbia

[39] Gunnell's claims receive further and fuller treatment in Chapter 5.

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Department of History and Political Science, that grapples specifically with the questions raised by the work of Boas and his students and by the new psychology of mental testing. Here I will focus on the first, arguing that engagement with the Boasian critique of Victorian anthropology led to a rejection by this cohort of the overtly racialized (and often nakedly racist) framework that had been the common sense of Burgess's generation. More significantly, it led them to sever the link between modern political and social organization and evolutionary time.

Integration and Differentiation of the Social Sciences in the Interwar Period

The years immediately following World War I were ones of significant ferment and self-examination within the rapidly expanding and professionalizing social sciences. In Ross's characterization, this period saw significant growth in the "strength of the professional structure of the disciplines and their consequent power to socialize recruits into professional norms" (Ross op. cit.: 392). This was certainly true of political science. As we have seen in earlier chapters, the field in its early decades was closely allied not only to political economy, statistics, and sociology but to education reform, journalism, civil and military service, law, and missionary work as well. It was not unusual, in fact, for many of these fields to intersect in a single biography. Just to give one example, David P. Barrows, a contributing editor of the *JRD* who would go on to become President of the University of California and chair of its political science department, received his Ph. D. in anthropology in 1897, and did an extended stint as a Taft appointee running the school system first in Manila

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and then in the Philippines as a whole before returning to the academy in 1910. His academic career, moreover, ran concurrently with an illustrious military one, which included intelligence and other service in the Philippines and elsewhere and culminated in his 1926 commission by President Coolidge as major general of the United States army.[40] According to Albert Somit and Joseph Tanenhaus's portrait of the early years of institutional political science, Barrows' career may have been unusually distinguished, but it was not atypical in the way it mixed disciplines and scholarly and non-scholarly activity. In their periodization, it is not until the discipline's "middle period"–roughly 1921-1945–that the field begins to emerge as a clearly defined, specialized, university-based academic discipline. It is also a period in which, as newer departments consolidated, and the students of Burgess and Adams fanned out across the county, Columbia and Johns Hopkins ceded their place of leadership in the discipline, significantly to a rising University of Chicago under Merriam (1967: Ch 1, 55-57, 110).

Institutional development was accompanied by efforts at new intellectual coherence and reach in the social sciences as a whole. The establishment of the Social Science Research Council (SSRC), discussed in detail in Chapter Six, was one of the most tangible of the efforts to bring the social sciences to bear in a coordinated way on social and intellectual problems. But it was far from the only one. Both during the 1920s and since much was made of the "blossoming" of a newly self-conscious "science-oriented social science" (Ross op. cit: 311) in the aftermath of the first

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[40] University of California History Digital Archives, http://sunsite.berkeley.edu/~ucalhist/general_history/overview/presidents/index.html#barrows.

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World War. One touchstone for this development in political science is Merriam's mid-1920s book, *New Aspects of Politics* (1972 [1925]).

Merriam, like Barrows, was someone whose careers spanned various fields. While he spent almost his entire career in the political science department of the University of Chicago, which he chaired from 1923 until his retirement in 1940, he also held a number of prominent roles in public life. Indeed his biographer has claimed that "Merriam was an American activist of his generation before he was a political scientist," a characterization which is undoubtedly accurate, though it may overstate the extent to which the role of "pure" political scientist was available before Merriam and others helped bring about the modern organization of the social sciences (Karl 1974: x).

Born in Iowa in 1874, Merriam was part of an extended family that included a number of important figures in state Republican politics. After Studies at Lenox College and the State University at Iowa City, where he focused on law, he moved to New York City to study at Burgess's Columbia department in 1896. Three years later he left for a year in Germany, where he finished his doctoral thesis on *The History of The Theory of Sovereignty Since Rousseau*; by 1900 he had moved to a job at the University of Chicago, his home base for the rest of his life. But for the first 20 years of his professional career, he was better known as figure in Chicago politics than as a political theorist, successfully running for alderman in 1909 on a reform platform, and gaining the 1911 Republican mayoral nomination with the help of a group of wealthy, reform-minded backers (including Julius Rosenwald; Harold Ickes managed his

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campaign), and only narrowly losing in the general election.[41] He served in the military in World War I, including, significantly, as a propaganda officer in Rome,[42] and remained active in state and city politics, as alderman (he lost his seat in 1917) and in state Progressive and "fusion" politics from 1912 until about 1920, when after a primary defeat in his second run for mayor he dropped out of professional politics in considerable disillusion. From that point he devoted his considerable political skills to academic institution building, transforming the Chicago Political Science Department into a major force in the discipline, helping to coordinate research within and bring foundation funds to the university (particularly through his role in establishing the Local Community Research Council [LCRC]), effectively founding a series of institutions including the SSRC and the Public Administration Clearing House. During this time he served on Hoover's Research Committee on Social Trends; later he was to serve on Franklin Roosevelt's National Resources Planning Board (NRPB) (ibid.).

But in fact he was but one of a number of ambitious institution builders eager to redefine their disciplines and the social sciences in general at this juncture. One result of this more general enthusiasm was the publication of a number of thick, edited, programmatic volumes with titles like *The History and Prospects of the Social Sciences* (Barnes et. al., 1925) and *Recent Developments in The Social Sciences* (Ellwood, Wissler, Gault, et. al, 1927), as well as more specialized collections like T.

[41] Merriam attributed his loss to fraudulent vote-counting. Karl (1974: 71-72) agrees this may have been the case, but also cites the popularity of Merriam's Democratic opponent with Chicago's immigrant communities.
[42] This experience certainly fostered his longstanding interest in what has come to be known as "political communication."

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V. Smith and Leonard D. White's *Chicago: An Experiment in Social Science Research* (1929). In 1930 this synthetic ambition culminated in the publication of the first volume of the pathbreaking *Encyclopedia of the Social Sciences*.

By their very inclusiveness, all of these works instantiate an interest in integrating the various spheres of human knowledge, and their editors' comments generally present this task as an urgent response to a perceived increase in the complexity and interrelation of modern, industrial civilization (to say nothing of the complex interrelations demonstrated by the far-reaching devastation of the war). It is interesting to note, however, that in so doing the books also effect a significant amount of "boundary work," defining the increasingly professionalized disciplines. This desire for both integration and professional differentiation is clearly visible in the introduction to *The History and Prospects of the Social Sciences*. In it, Harry Elmer Barnes comments that

> If better and saner types of conduct are to be achieved, this must be brought about by giving the individual a better set of … guiding criteria for conduct. What these … shall be can only be determined by the most earnest and prolonged *collaboration* of natural and social scientists, *each a specialist*, and all dominated by the aim of social betterment (op. cit.: xv, emphasis added)[43].

This somewhat paradoxical set of purposes is striking in the very structure of *The Social Sciences and Their Interrelations* (1927). In this volume, editors Alexander Goldenweiser (a prominent anthropologist then based at the New School for Social Research) and William Fielding Ogburn (of the University of Chicago's

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[43] Of course, this quote, and much of the rhetoric surrounding all of these efforts at defining the social sciences and their relationship to one another, also instantiates at least a possible tension between notions of "scientific detachment" and "political relevance," though as has often been commented about the social scientists of this period, they mainly seemed to believe that the results of "dispassionate" science would more or less automatically be social progress. In any event, that question has been analyzed elsewhere (e.g. Seidelman and Harpham 1985) and is not central to the argument of this chapter.

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sociology department) call for "constant cooperation" between social scientists, with "an all but complete disregard of academic and classificatory distinctions" (7) while at the same time making an almost comically extensive use of such distinctions: The table of contents lists 34 short chapters; the first set consider the relationship of anthropology to, respectively, economics, ethics, history, law, political science, psychology, religion, sociology, and statistics. Then follow chapters on the relationship of economics to, again respectively, ethics, law, political science, psychology, and statistics. The pattern then repeats for history, political science, and sociology, with a final four chapters on "The Social Sciences and Biology," "The Social Sciences and Education," "The Social Sciences and the Natural Sciences," and "The Social Sciences and Philosophy."

The ambiguous, if not to say self-contradictory, effect of this structure was noted by contemporaries. E. B. Reuter in the *American Journal of Sociology* describes the volume as primarily an attempt to "define the various social sciences" (1928: 998) and political scientist C. E. G. Catlin, reviewing the book for the *Philosophical Review* saw it as "a significant indication [of] the importance for method…of delimiting the fields of the respective social disciplines in other than a merely popular fashion" (1929: 497).

This simultaneous impulses toward interdisciplinarity and the carving out of distinct institutional and intellectual spaces for the various fields is similarly in evidence a relatively early "state of the field" effort for political science, co-edited by Merriam and, once again, Harry Barnes, a particularly prolific sociologist, historian, and political theorist then teaching at Clark University. In the early 1920s the two

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began planning a Festschrift for their shared mentor at Columbia, William Archibald Dunning. The volume, eventually published as *A History of Political Theories, Recent Times: Essays on Contemporary Developments in Political Theory* (1924; hereafter *HPTRT*) was meant both as a tribute to Dunning and as a sort of sequel to and update of his three-volume *History of Political Theories* (1902, 1905, 1922), which had surveyed the history of Western political thought from ancient Greece through the theories of Herbert Spencer. (Merriam's doctoral thesis, and his early books [1903, 1920], were very much in the Dunning mold). Commissioned essays by Dunning's former students were meant to "present and interpret" the "many interesting and significant developments in political thought" since the late nineteenth century, when Dunning's final volume left off (Merriam in *HPTRT*: vii). Primarily intended as a textbook for political science courses,[44] the book contained contributions not only from teachers in that discipline but from people holding positions in departments of sociology, anthropology, law, and philosophy, among others.[45] This of course reflected the diverse careers of Dunning's many students, but also the purpose of the editors. Describing their vision of the field, co-editor Barnes wrote that the subject of "political rights" needed to be "rejuvenate[d]" and "divest[ed]" of "metaphysical

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[44] Correspondence between the editors on the planning, execution, and eventual fate of the volume can be found in CEM papers Series 2, Subseries 3, Box 25, folder 15. In particular, in letters on January 20 and May 16 of 1924, Barnes reports to Merriam that the book is getting wide use in introductory courses, as hoped, and is set to "pulverize" a competing text then planned by Francis Coker. (Coker, who contributed a chapter on sovereignty to the Merriam/Barnes volume, may have agreed: His *Recent Political Thought*, Barnes's apparent referent in the letter, did not in fact appear for another decade.)

[45] It also represents the incompleteness of professional specialization by this time. Most of "non political scientists" in *HPTRT* published articles in *APSR* and *PSQ*; the political scientists, for their part, wrote for a wide range of journals, including those identified with sociology, history, philosophy, economics, law, and statistics.

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origins and implications" (387).[46] Merriam amplified this point in his in his contribution, characteristically titled "Recent Tendencies in Political Thought," in which Merriam applauded the beginnings of a revival and "restudy" of the "Aristotelean doctrine" of man's political nature "more and more upon the ground of fundamental impulses, ethnic, economic, or psychological" (23). In this vision, echoed throughout the volume, political science, in order to claim its own place and achieve relevance in the modern world, needed new premises; paradoxically, these premises were to be sought in "fundamental" knowledge about human behavior drawn at least initially from other disciplines.

Another tension was more specific to the nature of the Festschrift form. As the editors' correspondence on the matter sometimes gently alluded, the book was intended at once to celebrate Dunning and also to stake out the differences in their own generation's political science, if not from Dunning's own, then from that of Dunning's generation.[47] For the most part, this task is handled quite delicately. One of the most striking areas of difference, containing by far the most strongly worded criticism of the scholarship of the group's teachers, and in particular Burgess, had to do with the treatment of race and racial difference.

Rejecting Teutonism in Political Science

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[46] An earlier version of this essay had been given as an invited address to the APSA, and was reprinted in the *APSR* in1921. The earlier version opened by commenting that the "fact that a sociologist has been asked to appear on the program of the American Political Science Association … is an admission that some political scientists have at last come to consider sociology of sufficient significance to students of political science" (Barnes 1921: 487).
[47] Dunning died while the book was in its editorial phase, turning the volume into a sort of memorial.

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The volume as published contained thirteen chapters covering general currents in political thought, theories of democracy, pluralism and state sovereignty, international law, jurisprudence, socialism in theory and practice, pragmatism, the "contribution of sociology to modern political theory," social psychology, anthropological theories of the state, anthropogeography, and "race as a factor in political theory," among other topics. The book was reviewed in all the main political and social science journals (e.g. Curtis 1925, Elliot 1925, Fenwick 1925, Gettell 1925, Grierson 1925, Smith 1925, unsigned in *AAPSS*: 1925). The vast majority of the reviews were highly respectful; almost all noted the prominence and distinction already achieved by the majority of the contributors. The reviews also overwhelmingly (and generally positively) noted that the contributors, all products of the Columbia Department of Political Science and History, were both reflecting and contributing to a political theory significantly different from that of their teachers. To the reviewer for *The American Journal of International Law*, *HPTRT* represented a "treasure-house" of all that was "current" in political theory (Fenwick op. cit.: 242). In *The International Journal of Ethics,* T. V. Smith of the University of Chicago noted that, "political theory has become socialized," commenting that the book was characterized by a "more concrete orientation than Professor Dunning himself was able to achieve"; a change, according to Smith, "for the better" (op. cit. 312). Likewise, for Raymond Gettell in *The American Historical Review,* "The volume under review is strongest where Dunning was weakest," that is, in recognizing the "change in point of view resulting from the contributions of anthropology, sociology, and social psychology" (op. cit. 575).

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Two exceptions to the positive value given to this shift were the reviews in the *American Political Science Review* and the *Philosophical Review*. W. Y. Elliot of the University of California qualified an otherwise laudatory review by characterizing the book's "tone" as "too largely sociological" (op. cit.: 178). The book's lone mainly negative notice appeared in *The Philosophical Review*, where Matoon M. Curtis of Western Reserve University commented that, "Insofar as politics has leaned on sociology, it has become weak and incoherent, forgetting logic, ethics, and aesthetics alike" (op. cit.: 499).

Even Curtis, however, was favorable toward the book's treatment of race, praising a highly critical discussion by Franklin Hamilton Hankins of Teutonism and its variations as well as contributions by Alexander Goldenweiser and Franklin Thomas, both critical of teleological "stage" theories of political evolution and anthropogeography (ibid: 498-499). Other notices echoed the sentiments, as when John Grierson of the University of Chicago, writing in the *American Journal of Sociology*, commented that, "The greatest single pleasure of the book--if style and wit still count for anything in political science--is Professor Hankins' chapter on 'Race as a Factor in Political Theory.' Professor Hankins' refutal of the Aryan and Teutonic myths has a gusto which ought to lay these specters to rest for good" (104, cf. also Smith 1925: 313).

As should be clear from the foregoing, many of the contributions were sharply critical of the Teutonism of Burgess—who as the founding chair of Dunning's

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department had also taught many if not most of the contributors—and Adams.[48] (These figures are invoked both by name and by allusion). The break with Teutonism is generally presented as a move to incorporate the more "modern" scientific findings available to a new generation of scholars.[49] Moreover, other varieties of race theory came in for frequently devastating dismissal. Hankins's contribution addressed this topic most directly. Hankins was a statistician and sociologist who had headed the Department of Political and Social Science at Clark University until he left to teach economics and sociology at Smith College, where he remained at the time of writing. Author of a study of Adolphe Quételet's statistical work (his doctoral dissertation at Columbia [Hankins 1908]); former contributing editor of the *JRD;* frequent contributor to sociological and statistical journals, *Political Science Quarterly,* and the *Annals of the American Academy of Political and Social Science*, Hankins was a prominent Progressive, reformer, and outspoken pacifist then at work on what would become *The Racial Basis of Civilization: A Critique of the Nordic Doctrine* (1926).

Hankins' essay for *HPTRT*, eventually entitled "Race as a Factor in Political Theory" (earlier working titles had included "The Contributions of Social Biology to Political Theory" and "The Contributions of Social Biology and Statistics to Political Theory"[50]), minced no words. It unhesitatingly reduced Teutonism to a symptom of "prejudice" and "pride," writing that,

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[48] Merriam for example had taken at least two courses from Burgess, including his class on Comparative Constitutional Law of the Commonwealths of the United States and Comparative Constitutional Law. His notes from these classes can be found in CEM Papers, Series 1, Subseries 3, Box 6, Folders 3-6. Moreover, Burgess mentored Merriam, for example helping him as he began his career (see correspondence in Series 1, Subseries 3, Box 1, folder 17).

[49] Though Hankins in fact claims that even the science available to Burgess in the late nineteenth century should have been enough to undermine his faith in the "Teutonic germ" (533; see below).

[50] See correspondence between Merriam and Barnes, op. cit.; also correspondence between Merriam and Hankins, CEM Papers, Series 2, Subseries 3, Box 31, Folder 9.

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> [C]ertain definite doctrines of racial purity, racial superiority, and racial capacity for political organization and cultural achievement … have figured largely in the writings, speeches, and calculations of statesmen, publicists and scholars during the last half-century. By all odds the most important of all such doctrines in recent times is that known as Aryanism… the ancestor of a variety of descendant doctrines which still exert a powerful influence over popular emotions and on the thought of scholars and litterateurs. For it is an historical fact that Aryanism differentiated into Teutonism, Celtism, Anglo-Saxonism, and Nordicism--depending on the particular form of race prejudice and pride which the particular circumstances of Germany, France, England and America seemed to require (511).

Not only serving local chauvinism, Teutonism was scientifically suspect, based on "the naïve and popular" conceptions of race "of an age preceding the discoveries of modern anthropology and ethnology" (514). "[D]octrines of the mythical potency of the Teuton for political and cultural ascendancy (524) were "idealized" (530), "gross exaggeration" (535), and, worst of all, "metaphysical or idealistic" (538).[51]Burgess specifically was not only wrong but unduly impressed by European conceits: "While acting in harmony with that type of pseudo-science which was considered good political policy in certain German university circles," by embracing Teutonism he "committed a serious error from which an intimate knowledge of existing anthropological knowledge would have saved him" (533).

Hankins begins his essay with a wide-ranging critique of the "doctrine[s] of the inherent supremacy of an imaginary Aryan race" (514) elaborated by figures from

[51] James W. Ceasar is one of many who have noted the "intellectual campaign" of American Progressives against concepts they labeled as "metaphysical" but that had, by previous generations, been understood as "science"; this is clearly a salvo in that campaign, if a late one. Ceasar also points out that the rejection of Teutonism in the interwar era is related to a more general disenchantment with "the German forests" so romanticized by Teutonic mythologizing, which had, as a result of the war and the sentiments kindled by it in the United States, "lost much of their luster, as well as their foliage" (2007: 23, 19). Oren (2003: 10) explicitly links the rejection of Teutonism to disillusionment with Germany linked to the war, and also to the wartime propaganda work of many political scientists, including Merriam who worked in Italy.

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Arthur de Gobineau to Houston Stewart Chamberlain and given credence by "social selectionists" such as Paul Broca, Francis Galton, Georges Vacher de Lapouge, and others. On Gobineau and Chamberlain Hankins is scathing, at one point despairing that "elaborate criticism of all [Gobineau's] generalizations is not possible here' (514) and that Chamberlain's work among other things constitutes "a complete rejection of all the approved methods of modern physical anthropology and their replacement by a method of intuitive discernment of spiritual affinity" (522). To the "social selectionists" he is gentler, conceding that they had made a substantial contribution to the understanding of heredity but insisting nevertheless that they had "neglected" the diversity within racial groups, constructing "ideal" types that were not useful to a truly "scientific" understanding of race (530).

Burgess himself is taken to task for combining "the race mysticism of Gobineau" with Fichte and Hegel's "even more mystical philosophy of state" (535). Hankins criticizes Burgess's work for similar scientific faults to those discussed above, including confusion about the category of race itself and a misunderstanding of its relationship to culture, as when he conflates nationality and race or alternates between using language as a proxy for race and distinguishing between racial groups who speak the same language (531-536). Hankins goes on to link Teutonism to "the most systematic exposition" of the "present form of Anglo-Saxonism…contained in Madison Grant's *The Passing of the Great Race, or the Racial Basis of European History*," which he describes as a "veritable fountainhead from which has poured an avalanche of Nordic mythologizing, race mysticism, and sociological dogmatizing" in which "contradictions and inconsistencies are overlooked, while preference is given

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to those doctrines which strike a deeply responsive chord in popular tradition and race egotism" (540-541).

That is, Burgess's work was not only wrong, but also politically objectionable. Hankins goes on to amplify this point, accusing Burgess's work of "aid[ing] in perpetuating a point of view" characterized by an "ego-centric and ethnocentric theological interpretation of historical processes." This "point of view" implied "[t]he Teutonic nations also *must* have a colonial policy […in which…] there are no supposed rights of barbaric peoples which need to be respected," and ultimately held that "might makes right in the political sphere" (537-538). As such, for Hankins, it was "one of the psychological features leading to the Great War… (534).[52]

Hankins is by far the most forthrightly hostile critic of Teutonism and race theory generally, and the most forthrightly political, in the Merriam and Barnes volume, but his position does not appear to have been idiosyncratic. In his own contribution, Barnes wrote of "the essential illiteracy and scientific bankruptcy which is self-confessed on the part of any writer who would attempt a racial explanation of the political development of any European state, ancient or modern" and of "how extremely tenuous is all evidence for the doctrine of racial superiority[,] the Aryan myth and all allied vestiges of racial arrogance which have perverted history and politics from the days of Artistotle and St. Peter" to the present (371). Similar sentiments appear in a number of other essays. Charles Elmer Gehlke, in his discussion of "Social Psychology and Political Theory" characterizes the notion of

[52] As noted above, Hankins had spoken out against American involvement in World War I. Indeed, a number of the contributors to this volume were outspoken opponents of America's entry into the war, including co-editor Barnes, Alexander Goldenweiser, and E.M. Borchard, who contributed a chapter on International Law.

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"an 'instinctive leaning' toward parliamentarism in the Anglo-Saxon, or toward despotism in the Slav" as "the extreme of absurdity" (420). Even the generally diplomatic Merriam, in his introductory chapter, noted that political and social theory of the previous generation was "often overlaid with race prejudice, or with national influence or propaganda of an absurdly transparent type," with much theorizing on the topic of race taking "casual and superficial traits and characters…for the temperament and attitude of the group, often with the most astounding disregard for the primary elements of scientific method" (19-20). In total, seven of the thirteen chapters specifically address questions of race and racial difference, and in not one of these is a racial theory of history advanced or even partially endorsed. In this sense the volume as a whole represents a resounding rejection of the category of race as a basis for political theory, and as such a significant break with the work of the founding generation. The sense of generational change is amplified when you consider that, by definition, all contributors to *HPTRT* had studied in Burgess's Department of Political Science and many had studied with Burgess directly.

While perhaps rejecting Burgess and Adams-style racialism more explicitly than most, in this the contributors to *HPTRT* seem to have been taking what had become a more or less mainstream position within the discipline. The early decades of the twentieth century were of course a high point for "Nordicism" in America, with Lothrop Stoddard's *The Rising Tide of Color Against White World-Supremacy* (1920) and *The Revolt Against Civilization* (1922) just two of the many white supremacist screeds following up on the success of Madison Grant's 1916 *The Passing of the Great Race*, a bestseller that was reissued and reprinted multiple times into the 1920s

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(Tucker 1996: 93). And of course the immediate postwar era was generally one of nativist and racial hysteria. However, responses to Nordicism and its many proponents in the review pages of two major political science journals, *The American Political Science Review* (*APSR*) and *Political Science Quarterly* (*PSQ*) in the first half of the 1920s in general ranged from qualified endorsement to, more commonly, mild condescension or outright dismissal. A new edition of *The Passing of the Great Race* was reviewed in an unsigned, brief "book note" in *PSQ* in 1920 as little more than "an attempt…to glorify the 'Nordic' race" (*PSQ* 1920: 697). In 1921, *The Group Mind* (1920) by William McDougall, a leading "race psychologist" and "spokesman for the inequality of races" (Stocking 1982 [1968]: 216-217), was described in *PSQ* as "mystical," "metaphysical," linguistically sloppy, and lacking "a critical attitude toward alleged facts" (McBain 1921: 123). Four years later, McDougall's *Ethics and Some Modern World Problems* (1924) received nearly as critical a review in the same journal from Frank Knight. Knight eventually conceded that McDougall's work had some scholarly value, but he began his review by characterizing the book as primarily "propaganda" and "special pleading" in contrast to "science" (1925: 140, 138).

The *APSR* review of Roland B. Dixon's white supremacist tract, *The Racial History of Man* (1923), referred to the book as a "polygenetic outburst" and commented that, "The days of slavery called forth a whole series of polygenistic treatises, and now, after the great war, we may expect a blossoming of [similar] theories of diverse human stocks" (Starr 1923: 676-77)[53]. A few numbers later in the

[53] It is worth noting that the author of this review, Frederick Starr, was a biologically-minded anthropologist from the University of Chicago generally favorably disposed to racial determinism (cf. Stocking 1982 [1968]: 184, 281).

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same journal, international relations scholar Raymond Leslie Buell reviewed three books interrogating the relationship between race and civilization. Buell's review, itself rather complacently paternalistic in its attitude toward "backward" peoples, took a jab at "Nordic idolators," who were, Buell believed, likely to be "disappointed" by the fact that none of the books "harp[ed] upon" white supremacy (1923: 496). Quincy Wright took much the same tone with a book by Vice-Admiral G.A. Ballard considering the comparative maritime talents of Nordics, Latins, and Anglo-Saxons; for Wright, Ballard's discussion of Anglo superiority in this regard, while marking him as "a good Englishman" also indicated his "lack of meticulous scholarship and careful objectivity" (1924: 413-414). Similarly, Hankins was asked to review John Grant's *The Problem of War and Its Solution* for *PSQ;* the review faulted the book for using old-fashioned classifications of the European races (1924: 522). Another review in the same issue made disparaging reference to the "Nordic propaganda" associated (by name) with William McDougall, Lothrop Stoddard, and Madison Grant (Hayes 1924: 503). The following year, Stuart Rice urged "pro-Nordics" to a "dispassionate" reading of Herbert Adolphus Miller's book, *Races, Nations, and Classes*, a book diagnosing the urge for racial domination as a psychological disorder (Rice 1925: 294).

This is not to say that white supremacy itself was categorically denied or even seriously challenged in these pages with any regularity; Buell's review, cited above, noted with an approval typical of the *APSR*'s general tone on the subject that one of the authors "does say that the white man is better than the black, because 'he has behind him a tradition of collective growth in power and knowledge for now nearly

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three thousand years, in which each individual has a share'" and this meant "the West should use this accumulated power, as a trustee, 'to bring up the whole body of their fellow men to the level reached by the most advanced'" (op. cit.). As will be explored more fully below, the rejection was not of racism per se, but primarily of race as a proxy and catchall explanation for "civilizational" development, of the associated "historical" method, and for making distinctions between European groups (on the latter, cf. also GilFillan 1924).

The mounting rejection of the Burgess/Adams brand of historicism can be seen in the review in the *APSR* of Burgess's late work, *Recent Changes in American Constitutional History* (1923) (a denunciation of what Burgess saw as the menacing expansion of the regulatory powers of the state and the looming threat of a world state). Here, the senior scholar receives only barely respectful treatment. While the reviewer, Harvard's H. A. Yeomans, begins by saying that the book merits "careful and widespread attention," he goes on to make clear that it belongs to a previous generation, noting that it is marred by "pervading exaggeration" and "gloominess" to the extent that "few except students and pessimists will care to read" it and going on to contrast it with another work, which though also "historical" in method is nonetheless "keen, sane, and up-to-date" (1924: 398-399).

Of course, both Burgess-style historicism and more explicit "Nordicism." "Aryanism," and etc., retained credence among some political scientists. William McDougall was still called upon in 1923 by the Williamstown Institute of Politics (an organization founded three years earlier to study and disseminate information about international affairs) as an authority on "Race as a Factor in World Politics," for

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example (see Williams 1923: 648). And Charles C. Josey's *Race and National Solidarity*, a white supremacist tract so extreme that the *Journal of Philosophy* speculated that it might be a "hoax" (the reviewer called it "the most unblushing and brutal appeal for the cultivation and extension of the white race we have seen," and noted that it "outdoes Madison Grant and Lothrop Stoddard" [Wolfe 1924: 444]) received favorable mention in the *APSR* the same year. (The review opined that Josey "writes in good temper, is actuated by no unreasoning prejudices, and makes what many readers will regard as a strong case" [Unsigned in *APSR* 1924: 209].) And passing reference to, for example, the advantages of a high ratio of "Teutonic racial elements" (Sims 1920:69), the "handicap" of "racial diversity" (Cox 1921: 244), the "well recognized sociological principle" of racial segregation (Ross 1925: 639), or the "unequal capacities" of the races (Commons 1922: 147), as well an understanding of colonialism as at least potentially a project of "uplift" (e.g. Buell op. cit.) remained relatively unexceptional in the pages of both journals. Nonetheless, if the review pages of the major journals are any indication of theoretical trends, which it seems reasonable to expect they are, sweeping syntheses of race, history, and political development were increasingly viewed as passé, if only methodologically.

This shift in attitude is captured well by the "Report on the Second National Conference on the Science of Politics," published in the *APSR* in February 1925. That document contained a section on "Pre-Scientific Studies" that largely dismissed the theoretical bases of the work of a previous generation of political scientists. It did not, however, dismiss the possibility of a relationship between race and politics. Rather, it

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called for a new understanding of this relationship as a question to be tested, rather than a premise from which to depart:

> Before quantitative work can be begun in the social sciences it is perhaps characteristic to find a period of speculation and historical inquiry into the subject. Out of much study come they hypotheses that can, at a later time, be subjected to quantitative scrutiny. It is frequently possible to discuss these hypotheses gleaned from historical or other informal evidence without seeing the immediate possibility of experimentally verifying them. Such theories, doctrines, and hypotheses break up gradually into groups of more specific questions that may be studied with the quantitative controls of scientific method. This will probably be the case with such questions as the influence of Nordic or Mediterranean nationalities on American civilization (Hall et. al., 1925: 113).

That is, Teutonism, Nordicism, etc., belonged to an earlier "period of speculation," a perhaps necessary phase in the development of a science presumably coming into its own, or beginning to do so, in the 1920s. These theories then were to be rejected less for their emphasis on racial inequality than for their speculative, "historical," and "non-quantitative" character.

While in the context of political science the main targets of this faint praise would clearly be Burgess and Adams, this "pre-scientific" understanding of political evolution was also associated with what Barnes called "the imposing but treacherous edifice of Morganian genetic sociology" (*HPTRT*: 367). That is while Burgess and Adams were interested in the particular determinants and genealogy of Anglo-Saxon civilization, they were working very much in a tradition with Lewis Henry Morgan and other Victorian social evolutionists associated with the "comparative method," who painted a broader, if clearly similar, picture. Morgan's foundational work, *Ancient Society* (1877 [1964]), for example elaborated a sequence of seven "stages" of social evolution that was "historically true of the entire human family, up to the

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status attained by each branch respectively" (3-4). It was against this notion of a predetermined, teleological racial development, and what they characterized as the synthetic, deductive methodological style associated with it, that the contributors to *HPTRT* aimed their attack.

The Impact of Franz Boas

Just as the Gilded Age political scientists had borrowed heavily from early anthropology to construct their schema of political evolution, in making this shift their students (by the interwar era prominent scholars in their own right) were responding to important shifts taking place in anthropology. Each of the seven *HPTRT* contributions to directly address race makes reference to, and many substantially discuss, what Barnes refers to as "the work of a group of American anthropologists led by Professor Franz Boas [and using] a truly inductive method" (367). Gehlke's discussion of social psychology acknowledges the competing framework of "another group of social scientists": "the cultural anthropologists, Boas, Lowie, Goldenweiser, Wissler, Kroeber, and others," including the sociologist William Fielding Ogburn (417). Throughout the texts there are references to the "critical anthropologists" (e.g. Hankins 544; Thomas, 459) and "the American group of anthropologists, under Franz Boas" (Willey 58), as well as to concepts worked out within the emerging Boasian framework,[54] such as Ogburn's "cultural lag" (e.g.

[54] There has been some debate over the idea of a Boasian "school" or "paradigm" (cf. Castañeda 2003 for a discussion of this issue, also see Stocking 1982 [1968] and Stocking 1992, esp pp. 123-126). For present purposes, however, the question of whether cultural anthropology by this point or any other

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Merriam 11)[55].

The foundations of the Boasian critique of social evolutionism around the turn of the twentieth century were laid in Boas's work in the Pacific Northwest in the 1890s, during which he began to understand cultural change in that context in historical terms—as the product of cultural "diffusion"—rather than developmental/evolutionary ones. This formed the basis for the elements of Boasian anthropology that are best remembered outside the profession: the elaboration of a new concept of culture, and a limited rejection of racial determinism. Eighteenth-century humanistic and evolutionary concepts of culture generally understood it as the "progressive accumulation of the characteristic manifestations of human creativity art, science, knowledge, refinement---those things that freed man from control by nature, by environment, by instinct, by habit, or by custom"—that is, as the distinguishing characteristic of the "superior races." Around the turn of the century however, Boas and those associated with him began to elaborate a concept of culture "as weighted, … limiting, … homeostatic," "a determinant of behavior" (Stocking op. cit.: 201-202). No longer the progressive elaboration of universal rationality, culture

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had achieved the status of "normal science," or more generally of the appropriateness of such Kuhnian terminology for the social sciences (or at all), is not at issue. Suffice to say that the contributors to *A History of Political Theories, Recent Times*, clearly understood Boas and certain of his students to represent a particular, relatively coherent, and new, theoretical approach.

[55] Ogburn defined "cultural lag" as follows: "The thesis is . . . that the source of most modern social changes today is the material culture. The material-culture changes force changes in other parts of culture such as social organization and customs, but these latter parts of culture do not change as quickly. They lag behind the material-culture changes, hence we are living in a period of maladjustment." (Ogburn 1922: 196). As such, for Ogburn it was mainly intended to characterize problems within a given culture. As the concept moved into general usage it could also characterize a disconnect between a particular people's cultural level and that of the surrounding society and economy. In this usage it could characterize peasants moving into an industrial economy or urban setting (American blacks in the north were often analyzed in this frame), or immigrants confronting modern American conditions, for example.. (The novelty of this formulation should be emphasized. According to Stocking, it was only around 1910 that the plural "cultures" begins to appear in the writing of Boas's students.

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became a burden of tradition shared by distinct groups. As such, it was simultaneously universal—something that applied to both "modern" and "primitive man" –and plural; one could now speak of distinct culture*s.* This move opened up two radical possibilities, none of them immediately taken up in their entirety even by Boas himself: that race could be disentangled from culture, and that cultures could be equal. More immediately embraced were two slightly less radical (at least politically if not intellectually) possibilities: that social and political organization should be studied as historical phenomena, and that the constraints of *both* culture *and* nature might weigh as heavily on members of the "superior" races as on the inferior.

The prominent member of Boas's circle associated with *HPTRT* focused on the former. Alexander Goldenweiser, along with Alfred Kroeber, Robert Lowie, and Edward Sapir, had been among of the first of Boas's students to achieve independent stature in the discipline. A "brilliant Boasian maverick," the time of writing Goldenweiser was among the refugees from Columbia's wartime nationalism teaching at the New School for Social Research and doing what Margaret Mead was later to characterize as the first work on cultures as "wholes" (an idea that was to become an important tenet of the 1930s culture and personality tradition in particular) (Stocking 1992: 295). His essay for the Merriam and Barnes volume, "Anthropological Theories of Political Origins" stated flatly that, "the idea of the uniformity and universality of such succession [as Lewis H. Morgan and other social evolutionists described] can no longer be entertained" (445). This was in the course of an attack on the "undue recourse to hypothetical argument" in extant "theories of social and political evolution" (433). The thrust of his critique was to show that the

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burgeoning ethnographic record showed few examples of the successions (matriarchal to patriarchal marriage forms, etc.) predicted by the theory, and many counterexamples. On the subject of "primitive political organization," Goldenweiser argues that while politics, or rather the "integrating" tendency of political "consciousness," is "universal and as old as society itself",[56] the "modern state" is something else altogether, characterized by a coordination in one institution or set of institutions of the "legal, religious, economic and other cultural functions" that are generally dispersed in "primitive society" among various non-state "constituent units of the social aggregate" (such as religious societies or clans) (454, 446, 455). As a result, the emergence of the modern state is not a radical change in the order of society along the lines of an evolutionary leap, but a new organization of "constituent units" of society, prompted in turn by historical circumstance, such as war, territorial expansion, and economic change. The essay concludes,

> It is for this reason that the study of the problems presented by the historic state tends to develop into a special discipline. This is as it should be. If only it is remembered that political organization is of the essence of human society, that one or another form of political life is omnipresent, then the separation of the study of the modern historic state as a distinct branch of socio-historic inquiry becomes not only justifiable but imperative (454-455).

That is, the grand evolutionary system-builders of previous generations, anthropologists, sociologists, and political theorists alike, had missed the point. The state, or politics more generally, was a modern, *historical* phenomenon that needed to be studied on its own terms. This was certain to be welcome news for political scientists interested in carving out distinct institutional and intellectual territory. The

[56] As against Victorian social evolutionism, which saw politics as emerging "upon the ruins of kinship organization" (455).

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historical specificity of the modern state is emphasized again in the next two essays in the volume, in which, respectively, Franklin Thomas gives extended consideration to Robert Lowie's critique of geographical determinism and Hankins attacks Nordicism (together these three essays conclude the book), again emphasizing the dangers of assuming uniform causes for similar historical phenomena.

The combined effect of these emphases and other critiques of Nordicism sketched above is to suggest a new orientation for modern political research, in which political institutions are no longer intimately intertwined with a developing human nature, and, it therefore follows, no longer necessarily studied in terms of grand, macrohistorical schemes. The centrality of this shift to the field, and of race to this shift, is underscored by the fact that of all the essays in the volume, those cited above as addressing "race" specifically are with one exception the only essays in the volume to claim to offer methodological or foundational claims for the field as a whole.[57]

For Barnes, for example, "the time ha[d] arrived when the old lion, political science, [might] lie down in peace with the young lamb, sociology" (358). This

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[57] The exception is "Political Implications of Recent Philosophical Movements," essentially a primer on pragmatism by Dewey student Herbert W. Schneider. The remaining essays are defense of the notion of state sovereignty against "pluralist" critiques by Francis Coker, a call by Borchard for greater realism in the study of international law, a similar critique by "Dunning School" historian Caleb Perry Patterson of the study of jurisprudence, an overview of socialist theory by labor economist Paul Douglas, and an essay on the demise of internationalism among German Social Democrats up to the war. Moreover, I should qualify my classification. In reality, some of these essays do touch on racial questions. Borchard, for example, notes that existing "idealist" theories of international law do not account for de facto asymmetries of power, as in colonial relationships. They simply do not make the category of race, or racial theories of politics, central to their argument. Significantly, Coker's essay, while mainly negative (outlining the shortcomings of pluralism, which he likens to anarchism) qualifies his attack by granting that, "Pluralists have, however, made clearer than has been made before the superiority of society to law" that thus that "to consider political society solely in its legal aspect is now indeed an inadequate approach to political theory. A complete philosophy of the state must give elaborate attention to the social origins of the state and the social and psychological materials out of which the state fashions its laws. It seems probable, however, that the more effective corrective of the older, formal approach has come rather from the sociologists" such as Malcolm Willey and Barnes, whom he references, and who do make race a central problematic.

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meant, primarily, a move toward a process-oriented, psychologically informed study of the extra-political and extra-institutional factors shaping modern politics. Echoing Goldenweiser, Barnes wrote, “the state, of modern political terminology, is a very late and recent product of social evolution, and is, thus, by its very origin and genesis, as well as by analysis of its present state and functions, demonstrated to be a product, creation, and creature of society.” Political science can only escape metaphysics by “accepting as indispensable prolegomena the sociological generalizations with respect to the underlying social foundations of law and political institutions” (365, 361).

For Barnes, moreover, this represented a return to a “true” political science that had been “interrupted and obstructed for a half century by the influence of the lawyers [such as Burgess] upon political theory and practice”:

> What modern sociology has done for political science is not to originate the synthetic approach to politics, but rather to put the lawyers of the metaphysical and 'mechanical' schools to rout, and to restore the viewpoint of Ferguson, Hall, Madison and Calhoun. Indeed, it has done more than to restore this general viewpoint; it has strengthened it and modernized it through an infusion of Darwinian and Neo-Darwinian biology and functional and behavioristic psychology (401).

Merriam makes much the same observation in his introduction, citing as the major methodological advance in political science in recent decades "restudy" of the "Aristotelean doctrine" of man's political nature "more and more upon the ground of fundamental impulses, ethnic, economic, or psychological" (op. cit.), and insisting that these must be studied in “relation to time or place-given conditions" (33). Contributions by Gehlke, Schneider, and Hankins similarly link what Barnes characterized as the Boasian “"destruction of… Morganian...sociology” (367) to a

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new, foreshortened time horizon for political study.[58]

This should not be at all surprising; in fact, it puts political science clearly in line with currents in the social sciences more broadly. The momentous impact of Franz Boas, his students, and his sympathetic colleagues on the social sciences has been widely explored; according to Stocking, the influence of the Boasians and others working on parallel lines was such that by the 1930s, the culture concept was "paradigmatic" for the social sciences (1982 [1968] and 1992; similar claims are made by Barkan 1992, Cravens 1988, Purcell 1973, and Tucker op. cit.). More specifically, Dorothy Ross characterizes the 1910's and 1920s as a period in which the "study of natural process," encouraged by the Boasian turn, yielded "new models of American liberal change" within a "modernist conception of historical time." In her summary of changes affecting the social sciences as a whole,

> Historicism had been intertwined with evolutionism during the nineteenth century; now the work of Franz Boas and others challenged the fixed, unilinear model of evolutionary development. Aided as well by the disproof of the inheritance of acquired characteristics, one of the supports of the older theory, the fixed structure of evolution was dismantled. Attention turned to the particular interacting factors that produced human variety and progress, increasingly to the study of culture. The evolutionary depiction of long-term change over time began to be replaced by short-term study of the process of change (op. cit.: 318-319).

The conformity of political science to this broader trend is noteworthy, however, in light of what I call the "exceptionalist" account of political science with regard to race (see Chapter One). That is, Merriam, the contributors to *HPTRT,* and likeminded

[58] Thomas hope for a new approach to the environmental influences on human behavior exemplifies a similar shift, in what now seems a slightly comical way: such a new approach, unlike the traditional anthropogeography discussed in most of the essay, would not use climate to account for culture and political organization, but rather look at the temporary effects of weather on "the metabolism of life" (503).

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scholars staked their claim to a more "modern," "scientific," political science on a new orientations toward nature and history that were worked out, much as occurred in other disciplines, *through* rethinking the status of race and its relationship to history.

Specifically, by disentangling nature and history, such that nature now appeared much more as a source of relatively static laws and properties underlying and limiting historical and political development than as the realm and principle of development itself, political science set itself two new and related tasks. The first was to study historical and political dynamics in the medium term. That is, since, in Goldenweiser's terms, "origins" could no longer be understood as determining the character of "the modern state" such determinants, to the extent they were to be found in history at all, were to be found in recent political, social, and institutional history—not evolutionary time. The second was to determine how nature—as we shall see, particularly the natural characteristics and tendencies of individuals—limited or provided possibilities for politics. Nature, once in a sense the medium or essence of historical development, now appeared as its substratum, a source of laws and truths that would show what was—and was not—politically possible or likely.

This indicates, of course, that the rejection of teleological, racial theories of history within the discipline by no means represented a wholesale turn to "culture" at the expense of "nature," or the ascriptive characteristics of populations. In the next two chapters, we will see that in fact nature and more specifically ascriptive difference were not set aside but rather retained their importance even as they shifted in meaning.

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5. *"An Ounce of Eugenics:" Intelligence Testing and The Image of Democracy*

We saw in the preceding chapter that a significant group of prominent practitioners of political science, trained in the deeply racialist Victorian tradition of political theory, sought to find new bases for political research in the interwar era. This involved moving away from history (particularly history understood as an evolutionary process and/or search for origins) and toward other social and biological sciences.[59] In a representative statement in his *HPRT* contribution, Harry Elmer Barnes argued that such sciences would offer "indispensable prolegomena" to political study. Tellingly, however, Barnes offered only one concrete example of such foundational work: "The extensive data which have been brought forward by the recent intelligence tests administered by the United States army" (361).

In this chapter I examine political scientists' reception of this testing program, and of the boom in psychological testing that the Army tests engendered. I argue that an influential group of political scientists saw in such psychological testing the possibility of a set of premises for the study of the "fundamental impulses" governing political behavior even more promising than the "new" anthropology that had largely informed their rejection of Victorian political evolution. In their embrace of the model of human capacities implied by "differential psychology," we can see the basis for an image of democracy, and for a liberal political theory, that simultaneously

[59] Note that in this period anthropology was as much a biological as a social science; Boas himself worked extensively in what we would now term biological anthropology, as in his study of immigrant head-forms.

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rejects "race" as construed by Victorian political theory and re-describes ascriptive hierarchy in new, more "objective" language.

The Army Tests

The massive mobilization accompanying America's entry into World War I swelled the United States's armed forces to unprecedented levels. It also brought in a set of recruits much more diverse in ethnic and linguistic terms than any previous. This appeared to present distinct and new organizational and personnel challenges for the armed forces at a crucial moment—challenges that might translate into opportunities for the increasingly organized and self-conscious intellectual professions emerging from the Progressive Era. It was a group of American psychologists who were most successful in turning this situation to their advantage, both in terms of furthering their own research agendas and of demonstrating the potential importance of their profession to the organization of national life.

The vehicle was the colossally scaled program of intelligence testing administered to recruits to the United States Army after 1918. As a number of scholars have noted, the Army did not seek out the help of the scientific community (cf. especially Kevles 1968 and Carson 1993, 2003, 2007). Rather, led by Robert M. Yerkes, committees of the American Psychological Association and the Psychology section of the National Research Council mounted a campaign to persuade military officials that intelligence testing could sort recruits and rationalize personnel practices within the armed forces. Yerkes and his allies met considerable initial resistance and

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were ultimately turned down by the Navy. They did succeed, however, in convincing the Army brass to consider first an individualized test meant to eliminate "defectives," and, ultimately, to adopt a massive group testing program for both literate and illiterate or non-English-speaking recruits. This phase of the program was meant to provide information on such attributes as suitability for officer training or specialized duty. (The test for literate English speakers was known as the Alpha test; illiterates and non-English speakers, along with many who scored poorly on Alpha, took a primarily visual test called Beta.) The program was less than completely successful in transforming military practices—the Army, while it eventually routinized some mental testing, never fully embraced Yerkes's vision nor made testing a central or decisive factor in its placement decisions after the war (Carson 1993: 209-305)—but it resulted in a quantum leap in the visibility and prestige both of intelligence testing and of the psychologists who developed and executed the Army program.

Information about the results and methodology of the Army tests was made available in 1920 in Robert M. Yerkes and Clarence Stone Yoakum's brief *Army Mental Tests,* and more comprehensively the following year in an 890-page National Academy of Sciences report edited by Yerkes. These statements certainly generated interest from both lay and scientific observers, but the testing program reached a much wider audience—and generated new controversy—with the 1922 appearance of *A Study of American Intelligence* by Yerkes's junior colleague, Carl C. Brigham.

This interpretation and popularization of the tests' aggregate findings "became a scientific standard" (Black 2003: 84), generated significant coverage in the popular

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press, and attracted attention from policymakers (who discussed the data in the 1922 hearings before the House Committee on Immigration and Naturalization that led to the enactment of severe and clearly racialist immigration restriction two years later). From the first lines of the introduction by Yerkes (who was an enthusiastic publicist for the book), it becomes clear that the testers had broader ambitions than contributing to the war effort. As Yerkes put it, "For our purposes in this country, the army mental tests give us an opportunity for a national inventory of our own mental capacity, and the mental capacity of those we have invited to live with us" (xx). This primarily involved, as the first line of Yerkes's introduction made clear, addressing the "question of the differences that may exist between the various races of man, or between various sub-species of the same race…" (xix).

For Yerkes, as for Brigham, the relevant "races" were "negroes," as well as those understood to predominate in three subsequent "waves" of immigration to the United States—Nordics (predominant until 1840), semi-Nordics (Irish and German, 1840-1890), and finally, the "Alpine Slav and Southern European Mediterraneans" then still arriving in great numbers (xix). The tests generated data on 116,000 individuals, of whom roughly 12,000 were foreign-born, 81,000 were "native-born Americans," and 23,000 "negroes" [sic] (xx), and their results were reported in class and racial terms. That is, results showed "native whites" to have the highest intelligence, negroes the lowest, and the foreign-born somewhere in between. Brigham also reported a comparison between "white officers" (though the racial designation in this context was probably redundant), "white draft" (including many "native" whites and the vast majority of the foreign-born) and "negro draft." It was

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this comparison that yielded the much-discussed "fact" that the average (white) draftee to the U.S. Army had a "mental age" of 13 (actually 13.54)[60]. This was in contrast to 18.84 for the white officers and 10.41for the "negro draft" (80, cf. Table 1 and Figure 31, reproduced below).

Breaking down the scores of the foreign-born by test-takers' national origin, Brigham showed that average scores of immigrants from England, Scotland, and Holland exceeded the average for native-born American whites; those from a host of other "source countries" fell below that figure (with Germany and Denmark closest to the "native" American average, and Italy and Poland at the bottom of the distribution) (119). A parallel comparison showed the negro draft's average below that of all the European-born, though only marginally lower than the average scores of Poles and Italians (150).

These results conformed to the expectations of the testers, and to prevailing scientific and lay beliefs about the hierarchy of European "races" and between whites and blacks. Two findings, however, produced difficulties. The first was that blacks drafted from southern states scored below those from northern states. This prompted a rare (and very partial) concession to an "environmental" explanation of intelligence differentials from Brigham, who attributed part of the "superior intelligence measurements of the northern negro" to "the greater amount of educational opportunity, which does affect, to some extent, scores on our present intelligence

[60] "Mental age," a concept developed in France by Alfred Binet and Théodore Simon around 1904 to indicate the level of performance achieved by half of test-takers of a certain age, was the form in which the Army intelligence results were reported; Terman had already developed the concept of IQ, which represented a ratio of "mental age" to chronological age, but only the earlier formulation was used for the Army tests.

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tests."[61] However, he continued to maintain that racial and innate factors were determinative, explaining the remainder of the differential by: " the greater amount of admixture of white blood[62]; and…the operation of economic and social forces, such as higher wages…and a less complete social ostracism, tending to draw the more intelligent negro to the North" (192).

A second problematic finding was that "[w]ith increase in the time of residence, the differences between the native born and the foreign born [whites] become increasingly less [sic] significant" (93). Brigham spends more time on this difficulty, but concludes that the difference between "older" and "newer" immigrants represented a "real difference of intelligence" rather than "an artifact of the method of examination"—a question the earlier National Academy of Sciences report had left open (93). For Brigham, that is, this unexpected finding does not indicate a problem with the tests, nor does it challenge the notion that intelligence is inborn and fixed. Rather, "[i]nstead of considering that our curve…indicates a growth of intelligence with increasing length of residence, we are forced to take the reverse of the picture and accept the hypothesis that the curve indicates gradual deterioration in the class of immigrants examined in the army, who came to this country in each succeeding five year period since 1902," a deterioration attributable to "changes in the source of supply" of immigrants (111, 116). Specifically, each of these periods shows "a very marked decrease in the proportion of the immigration from England and Germany, and a substantial decrease in the proportion of immigration from Scotland, Sweden,

[61] This statement is significant in that it substantially contradicts Brigham's position in the rest of the book. For example, earlier he had explained a correlation between years of schooling and intelligence scores by arguing that higher intelligence caused longer schooling (Brigham 1922: 63).
[62] No evidence of this "greater amount of admixture" is cited.

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and Ireland. On the other hand, the proportion of immigrants coming from Austria, Russia, and Italy showed a marked increase at this time" (113).

However, Brigham concludes that the "race factor," while real, does not explain the totality of the difference between groups of immigrants. Rather, class also plays a role: "the decline in intelligence" of more recent compared to earlier immigrants "is due to two factors, the change in the races migrating to this country and to the additional factor of the sending of lower and lower representatives of each race" (178). In support of this hypothesis Brigham cites "the observation, repeatedly confirmed by experiment [by Lewis Terman and others], that children from the professional, semi-professional, and higher business classes have, on the whole, an hereditary endowment superior to that of children from the semi-skilled and unskilled laboring classes" (188).

Brigham concludes his study, unsurprisingly, with warnings against miscegenation[63] and calls for selective immigration restriction and further, unspecified "legal steps …which would ensure a continuously progressive upward evolution" (210). In this Brigham was entirely in line with a host of racial doomsayers and eugenicists enjoying great prestige at the time, including Madison Grant, Charles W. Gould, and Georges Vacher de Lapouge (all of whom Brigham cites).[64] All the same, the book and the publicity around the tests generally provoked a fair amount of controversy, much of it focusing on the anomalous results

[63] "We … face a possibility of racial admixture here that is infinitely worse than that faced by any European country today, for we are incorporating the negro into our racial stock, while all of Europe is comparatively free from this taint" (209).
[64] Yerkes in fact characterizes *A Study of American Intelligence* as a "companion volume" to Gould's *America, A Family Matter.*

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concerning northern and southern blacks and older and newer immigrants, but also on the problematic character of the concepts of "intelligence" and "mental age." Walter Lippmann, for example, published an influential series of articles highly critical of many of the conclusions drawn from the Army tests' results (though not of the Army's use of the testing program itself) in the *New Republic* in 1922-1923. However, even Lippmann conceded that the tests had value. In the sixth article of the series, "A Future for The Tests," Lippmann wrote:

> Once the pretensions of this new science are thoroughly defeated by the realization that these are not "intelligence tests" at all nor "measurements of intelligence," but simply a somewhat more abstract kind of examination, their real usefulness can be established and developed. As examinations they can be adapted to the purposes in view, whether it be to indicate the feeble-minded for segregation, or to classify children in school, or to select recruits from the army for officers' training camps, or to pick bank clerks. Once the notion is abandoned that the tests reveal pure intelligence, specific tests for specific purposes can be worked out (1923: 11).

As we shall see, political scientists, including and particularly Charles Merriam, were highly attentive to the efforts—organizational and intellectual—of the psychologists, as well as to the controversy the Army tests provoked. Their reception of all these things indicates both the importance of racialized science to shifts in the practice and conceptualization of political science and the ambivalent relation of political scientists to that science.

"Intelligence" and the Resinscription of Ascriptive Hierarchy in HPTRT

Many of the figures around *HPTRT* steered away from the "hardest" racial interpretations of the tests. However, few expressed skepticism in print that what the

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tests were measuring was in fact, "intelligence," or at least some relatively fixed and stable set of capacities, or that these tests provided socially useful data. For Harry Barnes, for example, psychological testing was to "do much to aid in [the] all-important problem of arriving at a scientific estimate of variations in mental capacity in the population with all the implications which this carries for political questions." For Barnes, the Army tests "have demonstrated that the innate mental differences in the social population present even more serious and stubborn problems for democracy than the prevailing levels of economic and social power and capacity" (378).

In his introduction to *HPTRT*, Merriam presents a more tempered but similar view, commenting that while much of the political philosophy of the earlier generation had been "deeply colored with the obvious interests of race, class and nation--in short with the defensive and aggressive propaganda of various groupings," it had opened many "significant questions…regarding the nature of races and nations," and that these questions remained open. Prefiguring the position of the "Report on the Second National Conference on the Science of Politics" (Hall et. al., 1925, see Chapter 4), to come out the following year, Merriam listed these "open questions" as, among others,

> What are the specifically innate characteristics of the various races or groups or nationalities, by whatever name known? What are the specifically acquired characteristics, the social heritage as it came to be known, of these groups? How far may they be determined and defined? What is the difference between the biological heritage and the social heritage of the English and the Chinese, or the Italian and the Russian?

While the answers to these questions up to Merriam's time had been largely "vague and in no sense definitive," Merriam expressed confidence that "there was shortly

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emerging an objective and scientific attitude" that would be enable political scientists more adequately to address such questions, and the related ones of "the political nature, psychology, or behavior of man; … the constituent elements in the process of political control; and…the possibilities of their training and adaptation by human intelligence" (19-21). Characteristically, Merriam does not specify exactly what work is manifesting this "objective and scientific attitude," but he ends the essay by commenting that "the advances toward technical knowledge of the political process" are to be sought in "[a]ccumulation of historical data, broader observation of political prudence, some advance in the statistical measurement of political phenomena, the beginnings but only the faint beginnings of political psychology, [and] adumbrations of social psychology not yet achieved" (45).

This exhortation to "new" methods or approaches is echoed throughout the book and, as noted in the previous chapter, in most of the favorable reactions to it in the scholarly journals. In general, however, the specific findings or even form of such new scholarship remains somewhat underspecified.[65] One exception of course is the work of the Boasians in undermining Victorian social evolutionism, but from the point of view of political science, at least, this seemed to take a mainly negative form, discrediting old frameworks rather than suggesting new avenues of research or particular methodologies for political scientists themselves. Apart from Schneider's pragmatic orientation toward "experiment," or the related "realism" proposed by Borchard and Patterson, contributors offered only two more-or-less defined areas in

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[65] In his chapter on "Political Implications of Recent Philosophical Movements," Herbert Schneider gives up even the attempt, noting that "This chapter must...not be expected to throw light on the future, nor even to clarify the present, but merely to give a general account of how and why philosophical thought has cut loose from its old moorings" (314).

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which to look for possible new bases for political theory. These were social psychology and what was variously called “differential psychology” (Willey 57, 60, 62, 78; Merriam 27; Barnes 372, 378), “differential biology” (Willey 57; Barnes 371), “social biology” (as in Hankins’s proposed title), or, most specifically, the study of “the variability of mental capacity” (Gehlke 424).

Like Merriam, many contributors gestured toward the possibilities of social psychology while offering little about its specific findings or their implications. Gehlke’s contribution on the topic is a survey of this historical development in the field, focusing mainly on challenges to “instinct” theory by Freudian and cultural approaches to the group/individual relationship, both of which appear to Gehlke to suggest the need for “a more dynamic and pragmatic conception of 'human nature'" (412). The second area receives much more specific consideration. Merriam, in his mild way, noted that recently some psychologists had drawn “conclusions adverse to democracy and equality from [the] results" of research into the “differentials in the fundamental intelligence of human beings” (27). Barnes is more vigorous, enthusing that “[i]t is probable that this differential psychology, when once adequately developed, will provide the most valuable information which has yet been placed at the disposal of political science by any psychic or social science” (372). Even Gehlke sees the future of social psychology in the very individualizing field of intelligence research and its methods, posing the meaning of differential intelligence within and among groups as an “unsolved problem of social psychology, which is of prime importance for political thinking” and suggesting that this research program be

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expanded to include new "methods of measuring the emotional and impulsive functions of the mind" (424).[66]

It is not surprising that "intelligence" should have been on the minds of political scientists in this period. (A rough indicator that this was a genuine, and genuinely new, preoccupation within political science as a whole can be found in a JSTOR search of political science journals in the first three decades of the twentieth century. Before 1924, terms like "intelligence," "mental test," and "differential psychology" rarely appear in any political science journal. When they do, it is exclusively in the context of industrial relations research, generally in articles by the testers themselves, and mainly in the less-specialized *Annals of the American Academy of Political and Social Research*. Beginning about 1924, such references, as well as more allusive invocations of intelligence research, begin to appear in the *APSR* and *PSQ*.) A number of historical accounts document the "explosive growth" in intelligence testing in America in the interwar period (Carson 2003: 308; see also Carson 1993 and 2007; Cravens 1988: Ch 7; Tucker 1996: Ch 3; Kevles 1968, Sokal 1990: esp. Chs 4-5).[67] Indeed it is hard to overstate the dramatic growth in the prestige and cultural impact of mental testing in America in the WWI and early interwar period. From a specialized and relatively controversial practice among psychologists at the turn of the century, mental testing had become by the mid-1920s a large-scale, routinized practice, had moved the to absolute center of the

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[66] In this he may have been typical of the direction of social psychology as a whole in the United States. Greenwood (2000) suggests that the ironically "a-social" character of much of American social psychology can be traced the mid-1920s, when, led in large part by Floyd Allport, the field largely abandoned relatively expansive "conception[s] of the social" in favor of an "individualistic experimental program" supported by "a restrictive for of methodological individualism and…a particular form of moral (and political) individualism" (443, 453).
[67] The account below draws on all these sources, but particularly on Carson 2007.

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psychological profession, and established a new understanding of "intelligence" firmly within the mainstream of American culture; moreover, this new understanding was one that held special appeal for social-control oriented Progressive intellectuals.[68]

As the previous chapters have emphasized, the notion that individuals and groups differed in their natural endowments, particularly mental and characterological ones, was of course nothing new in American culture or American political science. Indeed, according to Carson, American political theorists have consistently "linked conceptions of the sociopolitical order with the findings of mental philosophy" in ways that allowed them to "argue for both equality *and* hierarchy" (2007: 17, 13; emphasis added). However, beginning in the Civil War era and continuing through World War I, a number of developments in anthropology and psychology radically transformed the ontological status and practical entailments of the notion of natural mental difference.

Long before Burgess made the Teutonic racial heritage the basis of his systematic, academic political theory, the "Gothic thesis" that constitutional liberties were particularly Nordic in origin enjoyed wide credence among educated Americans, appearing prominently in the rhetoric of Revolutionary war pamphleteers, for example (Ceaser 2006: 18-38). And of course it is now widely acknowledged that the American Founders' insistence that "all men are created equal" was tempered by belief in the inherent inferiority of black people, women, and other groups (cf. e.g.

[68] As Carson (2007, cf. esp. 245-270) shows, even critics like Walter Lippmann and John Dewey were loathe to dismiss the potential of intelligence tests entirely. Rather, they tended to focus on details of technical execution or interpretation. For Carson, "[t]he shift in focus of the cultural conversation about intelligence from its implications for democracy to the technicalities of testing itself is one indication of the testers' success" (269). Their success was also indicated in the readiness with which many critics of racial exclusion and segregation seized upon the possibilities of mental testing for promoting the advancement of "exceptional" individuals regardless of race.

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Jordan 1977: Chapter 12; Smith 1997: Chapters 4-6; Boulton 1995). In the early nineteenth century, however, the dominant strain of "mental philosophy" in America was derived from Scottish "Common Sense" Realism, which stressed the variety of human "talents" and saw their development as largely a function of effort and character rather than inborn nature (O'Donnell 1995: 6-8). This general orientation crossed ideological lines, appealing to Whig educational reformers and Jacksonian Democrats alike—just as for Whigs it offered resources for the defense and perpetuation of respectable, Christian republican order, Jacksonians could find in it justification for their own conceptions of meritocratic openness (Carson op. cit.: Ch 2). What is significant is that while both interpretations certainly left open the possibility of natural (i.e. racial or gendered) hierarchy, this was generally justified on bases of propriety, custom, and convenience to a functioning social order rather than unalterable underlying conditions, leaving it "never fully naturalized, and thus…open to debate and alteration" (ibid: 60).

However, as the nineteenth century progressed, American concerns about group comparisons, particularly racial ones, provided much of the impetus for a reduction of the multifarious "talents" of Enlightenment political theorists and mental philosophers to the singular "intelligence" of the mental testing programs of the early twentieth century. In particular as the debate over slavery mounted during the years leading up to the civil war, many American social and political analysts found Common Sense unsuitable to their needs. As abolitionists sought to ground their appeals in the language of Christian universalism and equality, defenders of slavery began increasingly to turn to the new sciences of race that eschewed the human

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malleability emphasized by Common Sense in favor of “the classificatory sciences, where single, decisive differences demarcating groups were critical and interest in hierarchies was strong” (ibid: 83).

Ethnology applied the Aristotelean notion of a hierarchical “Great Chain of Being,” which linked species in an unequal ladder from the lowest organism to God, to the variety among humans. Relative intelligence, already understood in this zoological, comparative framework as essentially linear and graded, was a key differentiator in this context. In seeking to apply it to human variation, ethnologists in the United States and Europe increasingly focused on attempts to quantify and compare the attributes, particularly intellectual ones, of human groups. This was manifest particularly in an intense interest in craniometry, the comparative analysis of skull size and facial angles that attempted to provide data for a definitive placement of human groups in terms of their evolutionary history and relative mental capacities. In Europe, despite interest in the implications of this research for questions of colonial expansion, this generally remained a relatively specialized discourse. In America, however, it achieved much broader cultural impact by appearing to speak directly to controversies and anxieties about race that were widespread among the population.[69] (Samuel Morton’s 1839 comparison of the skulls of the different “racial

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[69] Carson (2007) compares the status and reception of intelligence research and testing in France and the United States from the Revolutionary period through the mid-twentieth century, concluding that the notion of a singular, linear, measurable intelligence, while gaining some purchase in both countries, has been embraced with much more enthusiasm in America as a practical way to make determinations of the relative merit of citizens. To simplify his complex argument (hopefully not beyond recognition), Carson identifies three broad factors as contributing to this outcome: 1) American suspicion of governing elites (whom the French were more likely to trust with making proper determinations of relative merit, unsupported by the “objective” data provided by the tests); 2) American resistance to centralization (which for example made competitive national university entrance examinations, such as France has long employed, comparatively problematic); and 3) the political salience of race in

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groups" residing in America, *Crania Americana,* was widely understood in scholarly circles as having provided "definitive empirical evidence … for black inferiority," presold its entire first printing, and became a staple of American proslavery argument, for example [Tucker op. cit.:18; see also Gould 1981]).

By the end of the century, craniometry had been largely abandoned by serious scholars (as had the related science of phrenology and the polygenetic theories of human evolution in support of which craniometric evidence had frequently been marshaled). However, the research program of which it was a part—in Carson's gloss, the ambition for "precise quantitative measurement" of intelligence as "unitary...native intellectual endowment that varied by degrees"—was carried forward by psychologists. Only now the attempt was to measure the "mind rather than the body" by giving "concrete embodiment" to intelligence "in its own technology, the mental test" (2007: 109).

While serious efforts to measure a singular, underlying trait called intelligence through mental tests began in the 1880s with the work of James McKeen Cattell, they failed to gain widespread support among psychologists and met apparently definitive criticism around the turn of the century. But the impulse toward a physiological understanding of mind was firmly established as the vanguard of the discipline, with major figures like William James and G. Stanley Hall pushing for laboratory-based, experimental research psychology. This made for a favorable climate when in 1910 Henry H. Goddard of the Vineland, New Jersey Training School for Feebleminded

America, which lent particular import to possible group-level comparisons within the citizenry. Indeed, as Carson emphasizes both people arguing for racial hierarchy and opponents of segregation and racially exclusionary policies saw great promise and possible advantage in "intelligence" evidence.

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Boys and Girls introduced the Binet-Simon test to the American psychological community. The Binet-Simon had been developed in France a few years earlier, intended for use in evaluating schoolchildren. It was revised for use on American subjects in 1916 by Lewis Terman. Terman published his Americanized version as the Stanford-Binet test and coined the term "Intelligence Quotient," or "IQ" for what it measured. The American version of the test attracted wide interest (of course with various reservations) from the mainstream of the psychological community. For Carson, this marked "a fundamental divide in the American history of intelligence" (ibid.: 183). That is, in the minds of many American psychologists, "intelligence" was no longer a *quality* (or series of qualities) linked first to character and then only amorphously to the body, but rather a measurable, essentially fixed, biological *quantity*.

However, it does not seem to have attracted much immediate professional interest from political scientists. It would take "intelligence and its tests" a few more years to capture their imaginations, and when they did it was because of the same thing that brought these things to the attention of a much wider public of intellectuals, business people, educators, reformers, and others: the publicity surrounding the Army testing program.

In some ways Robert Means Yerkes was not an obvious candidate to bring the new understanding of intelligence, and with it the discipline of applied psychology, to the forefront of American culture. He was an early critic of Terman's version of the tests (on technical grounds—he had developed his own, rival intelligence scale), and by all accounts seems to have been a combative, ungracious character more suited to

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his research with animals (he specialized in chimpanzee research) than to professional leadership. Nonetheless, as noted above, by convincing Army leadership to support a massive program of intelligence testing of new recruits during World War I, he contributed to solidifying the place of intelligence testing within the psychological community, raising the profile of that community within the larger culture as well as contributing to its institutional development, and demonstrating in the minds of many that a mass society could and should more efficiently and justly order itself with benefit of technologies that could measure the innate attributes of citizens. His efforts and public statements also lent credibility to the small but influential eugenics movement, and, as we have seen, encouraged a racial interpretation of the results of intelligence testing.

Despite its limited (however real) impact on the Army, the testing program had an enormous impact on psychology as a discipline and on cultural understandings of intelligence and ascriptive hierarchy among the larger public but also among academics. First off, it gave large numbers of American men direct experience with the tests. According to Kevles, by late spring of 1918, 200,000 recruits were undergoing tests each month. Second, with the establishment of a school of military psychology at Fort Oglethorpe In Georgia, it trained large numbers of testers and brought prestige and, crucially, previously unheard-of institutional backing to social-control oriented scientific research. (Yerkes himself was inundated with requests for the tests, and given $25,000 of Rockefeller money to establish a school-based testing program.[70]) Finally, its findings attracted wide enough public comment to make the

[70] See Chapter 6 for further discussion of this point.

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existence of the tests and the idea of IQ or mental age common knowledge and to stir considerable controversy, particularly directed at the alarming figures about the "white draft" and, to a lesser extent, black Americans.

We have already seen that the contributors to *HPTRT* rejected the "hard" racist interpretation of the tests as propounded by Brigham and Yerkes and that to some extent this reflected the prevalent attitude toward this brand of scientific racism in the main political science journals at the time. However, as briefly indicated above, the tests, their findings, and the general research program of which they were a part, seemed to the *HPTRT* group to have revolutionary potential for new, more adequate representations (and possibly reconstruction) of democracy and its racial ordering.

As we have seen, the least cautious promoter of this outlook in *HPTRT* is Harry Elmer Barnes. For him, the Army tests represent a first step in the "all-important" task of assessing the range of mental capacity of the polity, "with all the implications which this carries for political questions." However preliminary this step, Barnes leaves little doubt as to what those "implications" were likely to be:

> Already it [differential psychology] has revealed the fact that there is no greater illusion possible than the Jacksonian thesis of the actual equality of men in political or other form of human activity. It has given scientific confirmation to the old Aristotelean dogma that some men are born to rule and others to serve and makes it clear that we can have no efficient and progressive social system unless we recognize the real value of leadership and make it possible for the actual intellectual aristocracy to control society (372-373).

Others, including Merriam himself (cited above) are more understated but echo Barnes's insistence on the importance of the tests and the brand of psychological investigation they represent. Merriam's more judicious attitude toward the tests is

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echoed by Malcolm Willey of Dartmouth University, in his *HPTRT* contribution, “Some Recent Critics and Exponents of the Theory of Democracy” (an article that Merriam found "excellent" and among the most fundamental to “political theory in the narrower sense” in the book)[71] Willey casts his lot with neither of the groups named in his title (exemplified by Madison Grant on the one side and George Bancroft on the other), but rather a third that sees flaws in democracy but prefers it to the alternatives and is “hopeful for its improvement” (49). The focus of the article is revealing. Almost no consideration is given to the institutions of democracy, the workings of constitutions, or the history and development of democracy. The problems for democracy posed by “the great economic groups” is noted but not long explored (65). Rather, Willey makes it clear that the key question is the natural (“psycho-biological”) characteristics of the population, writing that “no sound appraisal of the theory of democracy can be undertaken” without “an appreciation” of “1) …inherent differences, physical and mental, between races of mankind, [and] 2) the inherent differences, physical and mental, between individuals of the same race” (57). Cautiously rejecting the racial interpretation of the Army tests, he notes that the “entire position” of inherent racial inequality “has been attacked penetratingly” by Boas and his students for whom “historico-cultural factors are basic in the seeming inequality of different groups of man” (58).[72] But individual difference is another story, one that “utterly blasts the hopes of the older equality theorists” (60).

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[71] Merriam to Barnes, November 23, 1923 and June 4, 1924. CEM Papers, Series 2, Subseries 3, Box 25, Folder 15.
[72] Willey himself was in some measure associated with “the Boas group,” having recently co-authored a survey article on the culture concept with Melville Herskovits for the *Journal of Sociology* (Herskovits and Willey 1923).

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Like Barnes, Willey sees the hopeful alternative in "a democratic-aristocracy that will make for social welfare" (61) and in eugenics. Where Barnes had alluded to "conscious control of evolution by the human mind" (390) (to be accompanied and enabled by a new definition of "natural rights" and more expansive understanding of the proper sphere of state activity [387-391]), Willey is more straightforward, commenting that, "[t]o the extent that the democratic theory of equality interferes with putting into operation a program of intelligent mating, it is to be opposed" (59). This perspective recurs throughout the volume—most emphatically, strikingly enough, in Franklin Hankins's attack on Nordic and Teutonic theory.

Hankins's discussion of, "Race as a Factor in Political Theory," treated extensively above, is particularly noteworthy in this context for a startling (to a modern reader) turn it takes at the end. While the bulk of the article consists of blistering denunciation of theories of inherent racial inequality, the essay (and with it the volume as a whole) ends by putting inherent, natural inequality the basis of America's "future political security," which for Hankins may best be guaranteed by an "ounce of eugenics" (548).[73] Hankins' strenuously argued attacks on Burgess, Madison Grant, Lothrop Stoddard, Carl Brigham, William McDougall, and other proponents of varying types of racial determinism are described above, and list objections that will be very familiar to modern critics of race theory. More discordant to the modern reader are the final, and apparently to the author most damning, set of criticisms he offers. For example, Carl Brigham's "demonstration" in *A Study of American Intelligence* that recent immigrants were less intelligent than earlier ones

[73] The full quote, and final sentence of *HPTRT,* is, "An ounce of eugenics is worth a pound of race dogmatism so far as the future political security of country is concerned."

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could have represented "a real service" to the study of immigration, "doubtless one of the very greatest problems before the American people" (546) if it had focused more exclusively on the "cheapening of the cost of transportation and similar factors [that] have resulted in bringing us larger proportions from those who were unsuccessful in their own countries" (545).

That is, Brigham's mistake was to "muddle up the whole issue" (546) with race, when it was really one of class, and of the inferior biological material represented by lower-class immigrants. Hankins levels a similar charge at Clinton Stoddard Burr, whose 1922 book, *America's Race Heritage*, had linked the declining biological quality of the American population with race mixing. Hankins does not contest that the "warped brains" that Burr sees "menacing our domestic political life" exist; rather, he simply lists a number of eugenic screeds, including the 1917 book *The Jukes* (an interpretation of Richard Dugdale's 1877 study by Arthur H. Estabrook of the Cold Springs Harbor Eugenics Record Office) and Henry H. Goddard's 1912 *The Kallikak Family: A Study in The Heritability of Mental Retardation,* as evidence that "degeneracy" can also proliferate in the lower orders of "pure native American stock" (546).

Moreover, despite all preceding argument, race is not to be discounted entirely. Also gravely in error are "certain of the American anthropologists" who expound the "dreadful and deluding modification of eighteenth century egalitarianism, that the races are all equal" (idem). Hankins goes on to explain that it is the *European* races that cannot be usefully differentiated as such (because so largely mixed among themselves). However, where "the white and negro" or

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contemporary North and South European immigrants to America are concerned, for Hankins, after the Army tests "there can, on the other hand, be no longer doubt of differences in average mental capacity" (547).

The "average" in the preceding sentence is important, and points to one of the key features of the brand of liberal late-Progressivism that *HPTRT* largely embodies and the direction in which it hopes to move the discipline of political science. For Hankins political scientists needed to consider *both* "average" inequalities *and* "the wide variation of abilities in each group" as well as the "overlapping" that occurs "throughout most of the range of variation" (547-548). This echoes a number of urgent calls in the book to attend to "individual" variation (e.g. Willey 47, Barnes 347, Gehlke 424) and indeed more generally to replace "traditional and authoritarian" measures of populations ("races and nations") with "material," "measurable," and "comparable" ones (Merriam 20). In practice, this means replacing both the "mystical" and "legalistic" (because categorical) formulations of race theory that had influenced earlier generations of political scientists *and* the (also "mystical" and categorical) "egalitarianism" attributed (presumably) to members of Boas's circle.[74] The technology of intelligence tests and the statistical apparatus within which they were embedded (and within which they could place populations) were immensely appealing in that context, possibly allowing political scientists to fit the "abstraction" of democracy to its "actual life conditions" and give them "a firmer basis for their

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[74] In fact, the Boasian rejection of racial hierarchy was incomplete, particularly in its earlier iterations, and some students identified as "Boasians" occupied ambivalent and intermediate positions in relation to what George Stocking has called "the scientific reaction against cultural anthropology" in the interwar era (Stocking 1982 [1968], Ch 11). Clark Wissler, Boas student and member of the eugenicist (and deeply white supremacist) Galton Society is an example Wissler becomes a member the SSRC committee on Scientific Aspects of Human Migration (on which more in Chapter 6) in the mid-1920s.

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inductions…upon the solid ground of objective fact" (Willey 47, 61). This “solid ground” was nature, no longer the dynamic field of teleological or evolutionary change from which politics and political change emerged, but now an array of “facts” about individuals’ and groups’ capacities, accessible by technical means.

As we have seen, Merriam’s endorsement of differential psychology as exemplified by the army tests was qualified. And elsewhere (both in published work and private correspondence), he voiced sharper criticism. In a 1924 *APSR* article Merriam wrote that “most of the dogmatic assertions regarding the bearing of differential psychology on democracy have been made by those who were neither students of government nor of psychology” and that for his part, Merriam had been “unable to discover thus far any conflict between differential psychology and democracy" (Merriam 1924: 476). In the same set of passages, Merriam also questions the extent to which “intelligence” as measured by the tests is entirely hereditary and/or directly related to political competence. And in an April 16, 1922 letter to William C. Bagley, one of the most prominent contemporary critics of intelligence tests and what he saw as the “educational determinism” they fostered, Merriam wrote,

> I think what you are saying needs to be said on a good many occasions, and rather forcibly. I do not think that democracy is as much imperiled [by] psychology as by the lines of reasoning followed by some of the psychological group. They are likely to get on the wrong side of the democratic movement and jeopardize the advance of their inquiries. Of course, whatever is true will be developed in the long run, but the danger is that half-truths or unverified hypotheses will be used for class purposes.[75]

At the same time, Merriam retained close ties and sought ever-closer

[75] CEM Papers, Series 2, Subseries 3, Box 25, Folder 13.

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professional ties with the major figures associated with the tests (this is discussed in detail in Chapter 6), and in fact sought to find or help develop new forms of assessment that might be more helpful to political scientists. We have already seen that some *HPTRT* contributors hoped that the model of intelligence testing could be expanded to other, more specifically political attributes, as when Gehlke suggested the possibility of assessing citizens for all sorts of capacities and tendencies that might affect political behavior. Merriam's correspondence shows that for him, this was not a far-off hope but seemed to represent a very real and relatively imminent possibility. He wrote repeatedly to a number of prominent psychologists, including Yerkes,[76]Harold C. Bingham,[77] and L. L. Thurstone,[78] about the possibility of developing tests for "leadership," aggressiveness, and other political traits, and frequently reminded them to keep him abreast of any promising developments in that area. His student, Harold F. Gosnell, also expressed similar hopes, writing in a 1923 *APSR* article on "Some Practical Applications of Psychology in Government"[79] that ""[t]he perfection of tests of emotional, volitional, and moral traits would be a great aid to the public administration" as would the development by psychologists of possible "qualification tests" for electors and others charged with public duties" (735).

Merriam did get some encouragement in this endeavor, particularly from Yerkes and Thurstone. Yerkes for example wrote to Merriam in January of 1923

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[76] CEM Papers, Series 2, Subseries 3, Box 43, Folder 16.
[77] CEM Papers, Series 2, Subseries 3, Box 26, Folder 2; in one case Merriam hoped Bingham could help him specifically get information on "leadership" tests being developed by Sidney L. Pressey at Ohio University.
[78] CEM Papers, Series 2, Subseries 3, Box 41, Folder 11.
[79] The article reprised a 1922 APSA presentation by Gosnell.

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about impending “important developments” with respect to work sponsored by the Institute for Government Research on “character rating or measurement” and also mentioning National Research Council plans for the “development of methods of measuring human characteristics" (op. cit); Merriam subsequently became intimately involved with the latter initiative (the story of which is told in Chapter 6). But in terms of concrete findings or actual testing programs, little came of it. Both Merriam and Gosnell made some attempt to apply the psychological “attitude” if not the actual technologies (because non-existent) in their books of the early 1920s, but given the technical and practical constraints were forced to content themselves with making gestures in the direction of “hard,” psychological assessment to accompany their rigorous institutional analyses. (One only slightly disappointed *APSR* reviewer wrote of both Merriam’s *The American Party System* [1923] and Gosnell’s *Boss Platt and His New York Machine* [1924] that despite the authors’ pretensions to the contrary, the books’ conclusions depended “primarily on exhaustive analysis of political forces and political organization rather than upon 'psycho-biological' analysis" [Brooks 1924: 629].)

On one very crude level, political scientists’ eagerness to follow the example set by psychology during the war is easily understandable. The Army testing program had raised the profile of psychology enormously, with considerable benefits for researchers in that discipline, not only in terms of prestige and visibility, but in the concrete forms of foundation funding and commercial opportunities: Intelligence tests became a valuable commodity, packaged and sold by consulting firms staffed by psychological experts (Carson 2007: 253). It also brought psychologists, however

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briefly, closer to the center of state decision making than political scientists had yet been (with the dramatic, but very limited, exception of Woodrow Wilson). In that context, a certain amount of envy and perhaps even indignation on the part of political scientists seems inevitable. This in fact comes through rather plainly in Gosnell's 1923 article, in which he points to some of the shortcomings of the intelligence tests for measuring specific capacities, and comments that the "psychologists cannot hope to become specialists in the various other fields where they have been called in to do consulting work. The political scientist must formulate the problems in his field and then endeavor to secure the co-operation of the psychologists, the educators, the psychopathologists, and the personnel experts" (op. cit.: 743).

The complicated and incompletely successful story of Charles Merriam's attempts to "secure" just such "co-operation" comes next. For now it will suffice to mention that the dream of establishing a "psycho-biological political science" speaks to a number of issues. One is the general appeal of objective, statistically presented, seemingly empirical data. As has been widely remarked, statistical language was immensely appealing in the context of the paramount Progressive values of "continuity and regularity, functionality and rationality, administration and management" (Wiebe 1967). Moreover, with historicism largely discredited, the need for adequate, quantitative data on the present and recent past was a consistent and urgent theme for political scientists. Lack of uniformity, coverage, and professionalism in government statistical reporting was frequently lamented in the pages of the *APSR*, and various schemes for generating alternative and

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complementary sources of data were consistently proposed, and many instituted, during this period. This was clearly an effect of the new, "modernist historical consciousness" discussed in the previous chapter. It was also, however, linked to more prosaic disciplinary anxieties and ambitions.

In his investigation of "the quantitative technologies used to investigate social and economic life" Theodore Porter notes that they are rarely adopted first by experts in positions of power (1995: 43). Rather, "the drive to supplant personal judgment by quantitative rules" such as cost-benefit analysis or inferential statistics "reflects weakness and vulnerability." It is "a response to conditions of distrust attending the absence of a secure and autonomous community" and "must be understood partly as an adaptation to institutional disunity and permeable disciplinary boundaries" (ibid: xi). In an analysis of the Army Corps of Engineers in the early to mid-twentieth century, for example, Porter argues that cost-benefit analysis is a "form of quantification [that] grew up not as the natural language of a technical elite, but as an attempt to create a basis for mutual accommodation in a context of suspicion and disagreement" (149). By referring political decisions (about allocating funds for infrastructure, for example) to "an ideal of mechanical objectivity," cost-benefit analysis allowed Army engineers to overcome barriers of bureaucratic conflict and public doubt (189). That is, "the regime of calculation was imposed not by all-powerful experts, but by relatively weak and divided ones" (149). Similarly interested in providing useful knowledge to state agencies, and solidifying their standing as a profession, political scientists were understandably attracted to the possibility of generating quantifiable, objective data about the political capacities of citizens—data

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that was capable of compelling agreement because embedded in a technology at least arguably endowed with “mechanical objectivity.”

Again, this interest crossed barriers familiar from the study of scientific racism, for example. Walter Lippmann is generally remembered as a fierce critic of the Army tests and their interpretation by Terman and Brigham.[80] But he, too, was intrigued by what he referred to in a 1923 letter to Charles Merriam, “an examination of the relation between political science and the sciences which are now attempting to supply us with our premises." In the same letter Lippmann commented, " Why [Yerkes] should think I am opposed to mental measurements is more than I can imagine. Is it necessary to confuse criticism of an interpretation with opposition to a method?"[81]

This “method” of ascertaining the capacities of citizens appealed to an ideal of knowledge as technology, in which ascertaining the workings of nature, in this case political nature, would allow political scientists to craft appropriate technical responses. As Merriam wrote in his 1922 article summing up the state and prospects of “Political Research,” “We may ask what are the requisites of citizenship? ... What are the obstacles to 'efficient' citizenship? Are they physical, psychical, social, or economic? Can these obstructions be located and diagnosed, and can they be measurably trained and controlled?" (Merriam 1922: 320). The promise of tests that could offer access to the facts of political nature was that it might position political scientists to propose the reshaping of institutions—and possibly even values—in

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[80] The public debate between Lippmann and Terman is partly reproduced in Block and Dworkin (1976: 1-45).

[81] Walter Lippmann to Charles E. Merriam, February 17, 1923. CEM Papers, Series 2, Subseries 3, Box 34, Folder 12.

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conformity with that “nature.”

They also offered a way out of the “mysticism” and “metaphysics” of historicism that did not share the radicalism—or provoke the violent opposition—associated with the Boas group (cf. Stocking 1982 [1968]: Ch 11). Much more appealing were the languages of ranges and averages which left the population neither starkly divided as if by fiat (Merriam’s “authoritarian” measures) nor undifferentiated and unrankable. The great virtue of intelligence tests and the related tests that Merriam, Gosnell and others hoped to develop was that they offered access to the “natural” sources of hierarchy and by extension to a vision of the proper, liberal social order. And they did so in the languages of quantification and objectivity rather than those of historicism and racial essences.

That is, for social scientists, political scientists included, intelligence data provided an exciting new model for abstracting and making useful knowledge out of human capacities—turning them into data. For a generation impressed with the power of statistical analysis to generate lawlike regularities and determined to ground their research in empirical findings, this would have been very attractive on its own. Perhaps even more importantly, however, the use of intelligence technologies by the Army (and increasingly by private companies offering what we would now call management consulting) showed how these human capacities could be linked into networks that were useful to the state and other large organizations, thereby providing a model for social scientists eager to generate findings that would allow them to take a social and political role more akin to engineering than to philosophy.

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It also shows that while Merriam and a number of other major figures in the field may have rejected the more illiberal racial interpretations of the implications of the army tests, they did not reject the basic premises underlying those interpretations, in which the right kind of technology could offer access to the actual substance of merit. In this respect what we are seeing is the emergence of a new understanding of how ascriptive hierarchy fits into liberal discourse at the level of formal, elite theory. As discussed above, the idea that "unnatural" hierarchy must be resisted so that a *properly* hierarchical social order can emerge has long been a prominent feature of liberal discourse in which objectivity, fairness, and individualism (however variously defined over time) appear as the proper form of equality. In its languages of overlapping ranges, intelligence discourse left room for a stratified system to be understood as liberal and merit-based. The double move effected in *HPRT* and in related political science literature in this period—of rejecting a racial interpretation of history and embracing new measures of ascriptive hierarchy—creates separation between racial ideology qua white supremacy and more neutral, "unprejudiced" notions of hierarchy that might be accommodated to a liberal, elite-guided democratic system.

Conclusion, and a Note on Continuity and Change within the Discipline

Charles Merriam is one of only a handful of early twentieth-century political scientists to be consistently remembered by a discipline that does so little to examine or celebrate its institutional past. To both celebrants and detractors, Merriam

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generally appears as a "creator of modern political science" (Katznelson and Milner 2002: 25) and the driving force behind the emergence of "a research program…exemplifying…empirical research, quantification, and social psychological interpretation" that set the stage for the postwar "behavioral movement" in political science (Almond 1990: 27). The place of Merriam and his "Chicago School" in disciplinary memory is well summarized by Heaney and Hansen in a recent *APSR* article:

> The "Chicago School of Political Science," while not the only voice for a science of politics, was still for its day the most cohesive, productive, and influential contributor to the development of political science on a natural scientific model….[Its members] were among the first to use advanced empirical methods in political science, including survey experiments..., content analysis..., field experiments..., and correlation, regression, and factor analysis.... [G]raduates of the Chicago School were the vanguard of the behavioral revolution that fundamentally reshaped political science. Together they created the science of politics that became the mainstream of the discipline during the 1950s and '60s--and remains with us still" (2006: 589).

The "Chicago" model influenced not only research but perhaps as significantly the teaching of political science. The typical political science curriculum in the Chicago department in the late '20s and early '30s was to be the typical political science curriculum at Harvard, Yale, and Johns Hopkins only a decade later. For example, in 1917 Merriam dropped his course on Political Theory (or, as he referred to it, "Staatslehre") for one on "The Scope and Method of Political Science," which urged students "away from the tradition imported from Germany in the 80s and 90s" toward a scientific, empirical, and psychologically informed study of politics, and which was to become a core segment of the graduate program for more than 20 years.[82] Other

[82] Charles Merriam to Beardsley Ruml, June 5 1923. RAC-LSRM, Series 3, Subseries 6, Box 64, Folder 682.

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courses that began to displace more traditional offerings by the mid-1920s included Merriam's "Research in Politics and Citizenship" and "Systemic Politics"; "Introduction to Political Research," taught variously by Merriam and two of his most distinguished students-turned colleagues, Leonard D. White and Harold F. Gosnell; Gosnell's "The Electorate" and "Comparative Political Parties." The emerging field of political psychology was represented by Merriam's other star mentee, Harold D. Laswell, who taught "Non-rational types of Political Action," and "Public Opinion and Propaganda." Similar courses began to appear at Chicago's peer institutions much later in the decade, and not consistently until the mid-'30s (Heaney and Hansen op. cit.: 591-592).

No doubt this curricular influence can be accounted for in part by the diffusion of Chicago-trained scholars to other departments in those years—by its own account, the University of Chicago was by the mid-20s the site of some 80% of graduate work in political science in the country (in Bulmer 1980: 99). While this figure seems exaggerated, Chicago likely produced a disproportionate share of political science Ph.D.s in this decade, many of whom were part of what Heaney and Hansen characterize as a dense "dissertation advising network" centered around Merriam, White, and Gosnell, with only slightly fewer students studying under the more junior Lasswell (op. cit.: 592).

More generally, the work of this cohort appears in the literature as "a clear response and reaction to the failures of an earlier generation" (Seidelman and Harpham 1985: 112) and as marking the moment in which a discipline dominated by historical and legalistic approaches to politics began fundamentally to reorient itself

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toward the empirical study of contemporary mechanisms and institutions of politics (cf. Ross 1991: 450-473). I argue that the engagement with racial thought outlined above helps to explain why we see a disciplinary transformation when we do, and why it went in the direction it is understood to have taken.

It should give us pause, however, that one of the most perceptive of our discipline's historians, political theorist John Gunnell, has argued strenuously against the existence of any such shift. He focuses on the figure of Merriam himself, but his argument is not difficult to extend to other significant contemporaneous figures in the discipline. For Gunnell, "There is...probably no more prevalent and persistent, but misleading, conventional wisdom about the history of political science than the assumption that Merriam's work represented a fundamental break in either the theory or the practice of the discipline" (Gunnell 1992: 133).[83] Gunnell insists that Merriam's thinking in the interwar period was significantly continuous with the earlier, historical work of the "Columbia School," particularly his principal teachers, Burgess and Dunning. Among these continuities were: intense interest in the state and state action, particularly executive action; emphasis on the civic educational role of political science; and the ideal of "scientific" inquiry itself. (As Gunnell correctly points out, "the commitment to historical analysis in the late nineteenth century was itself a form of scientism" [134].[84]) Dunning's insistence on the importance of ideas

[83] This article is reprised and expanded in Gunnell 1993, Chapter 4.

[84] See also Ross 1991, especially pages 259-266, on the identification of Rankean historical method and scientific objectivity for the practitioners of "historico-politics." Moreover, Merriam's own vision of science was quite broad, as indicated by this typical invocation of "the typically scientific process" as entailing "painstaking detail and … closely related conclusion[s]" or, again, "efficient spirit and objective method which reached for the truth without regard to struggles of interests for power, or without respect to authority or convenience rooted in the past." While he does not explicitly mention Burgess, for example, he seems to be invoking his teacher in the same essay when he praises the

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in politics, and Burgess's understanding of political theory as a source of political order also reappear strongly in Merriam's thought. Gunnell concludes that "Merriam was dedicated less to a critique of contemporary political science and political theory than a defense of its progress and possibilities" (141), coupled with a degree of "methodological refinement" (134).

In this, as has also often been commented of him, Merriam was very much a product and a voice of the Progressive era. The category of "social control" and the key role of experts in bringing about rational, non-radical democratic reform were both at the very center of Merriam's thought and at the very center of intellectual production for much of the period spanning Merriam's education and political and academic careers – certainly long before the appearance of his most famous programmatic statements, his 1921 "The Present State of the Study of Politics," the 1925 book *New Aspects of Politics,* and his presidential address to the American Political Science Association the following year.

So if indeed the theoretical stance and vision of American politics that we identify with Merriam, and have understood as representing something new within the discipline, shared a significant set of characteristics with that of the preceding generation of political scientists, we are still left with a question: Why the perception of a radical break or transition in the first place?

One possibility implied by Gunnell is that subsequent scholars could be simply reproducing statements about what political scientists wished to achieve, and

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scientific rigor of turn of the century writers on "the state" that sought its origins in "various historical, anthropological and ethnological inquiries [rather than] the fictitious state of nature, looming so large in the natural law philosophy" (Merriam in *HPTRT:* 39, 13, 21).

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confusing these with actual achievements. Merriam himself consistently spoke in utopian, almost prophetic terms, about "new" currents in social science and society alike, and it is possible that the recurrence of the "break" trope may be traceable more to repetitions of his own claims than to analysis of his work. Moreover, is also true that Merriam's intellectual production was considerably less empirical and systematic than the model of political science he elaborated in his programmatic statements, and that his students went on to practice more of what he had preached than he himself ever did. And of course the narrative of a break with historical theory in favor of a scientific approach in political science has been useful for presentist purposes, as a sort of ancestral totem for the behavioral movement and its intellectual descendents, or alternatively a target for others who see "scientism" as hostile to theory, political engagement, or both.

Obviously, however, I do not find this completely convincing. While Gunnell correctly points to ways in which interwar political science shared some basic political orientations and self-understandings with a preceding generation, the preceding analysis suggests that commentators such as Ross, Katznelson, and others, are correct to perceive something significantly "new" at play in the political science of the 1920s. That is, I am arguing that the "methodological refinement," that Gunnell identifies, even if harnessed to "old" questions and familiar preoccupations, was linked to an even more important set of epistemological and ontological shifts. These new understandings of the perceived relationship between politics, history, and nature opened the tantalizing possibility that in the place of a "mystical" (or what amounted to the same thing, only worse, "German") teleology or even the

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philosophical anthropology of the Anglo-American political theory tradition, American political scientists could ground their work in an account of the regularities of behavior of a whole range of groups—“pre-political” “givens” useful in tandem with political and medium-term historical developments for explaining varying political outcomes.[85]

As noted above, Smith (2004) has singled out the early 1920s as when "race" in a sense falls off the disciplinary radar. And in this he is following a number of observers who have remarked on the absence of any racial reform impulse in political science (see Chapter One). Smith writes that, like social scientists in other disciplines, most early-to-mid twentieth century political scientists "tended to think of racial identities as things generated at root by biology and/or economics and/or culture and/or history and/or often unconscious or at least informal social psychological process and social activities" and therefore as outside the purview of political science, which at most would see those identities as inputs into the political process rather than subjects for investigation. Essentially, for Smith, because "race" was understood to precede or be "fundamentally exogenous to politics" it was not an important subject for political scientists (ibid: 41).

However the foregoing analysis suggests that precisely at this moment when race was supposed to be dropping from the discipline, particularly the 1920s, the

[85] As noted above, the identification of Teutonism and the brand of historicism it represented with Germany was a distinct liability in post-WWI America, where nativist and particularly anti-German feeling ran high. Its rejection here must be seen in that context. It is worth noting that the identification of the work of the Gilded Age political scientists with a somewhat misguided “German” detour within an essentially liberal American political theory tradition encouraged by this interwar rejection has made it into the modern historiography of the discipline (cf. Fries 1973). However, as discussed in Chapter Two and briefly above, while Burgess was indeed trained in Germany, Teutonism itself has a robust history in American and English political thought, predating the American Revolution.

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Chicago political scientists were moving *toward* the "pre-political," including how politically significant ascriptive traits might be unevenly distributed among populations, a notion that is basic to racial thought in America. The move of rejecting Teutonic and Nordic theory while at the same time embracing the “natural” (and racialized) hierarchies of differential psychology at once carefully establishes distance from anti-liberal racial nationalism *and* embraces the possibility of ascriptive, probably hereditary, hierarchy. By extending the ascriptive framework outside the limits of race and gender and into the heart of the democratic polity itself, it makes the pre-political the very basis of “modern” political analysis. The story of the “disappearance” of race from political science thus here appears as the story of the separation of nature and politics and the redescription of ascriptive difference in liberal terms.

Merriam and his cohort were defining a new image of the possible relationship between human nature and politics, and a new time horizon onto which that image could be projected, both of which would prove enormously influential and intellectually productive for following generations of political scientists, and which I would argue, are still visible, if often unarticulated, in current political analysis. And the translation of Enlightenment ideas of equal rights and “natural aristocracy” into the psychological languages of intelligence and capacities in which they participated remains uniquely potent in the American context.

However, the story of 1920s political science’s engagement with racial thought does not end there, nor as neatly as all that. The following chapter will take up these questions of continuity, change, and the role of race in the discipline from a

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slightly different angle. Continuing to focus on the 1920s, it will focus somewhat less on the products of intellectual labor to examine its contexts. That is, I will put the books and journals partly to the side and examine institutional "modernization" of the discipline, including the emergence of a new understanding of the necessary physical infrastructure for the discipline, a new funding model, and a new institutional structure for interdisciplinary collaboration. In the process we will see that the images described above of politics and nature, of democracy, and of the role of political science in mediating those realms, retain their importance. But we will also see that more frankly racist versions of scientific thought, and the individuals and institutions promoting them also retain their appeal for Merriam and his intellectual and institutional allies. That is, for an influential and respected cohort attempting to build the discipline both internally and in relation to other social institutions it is not the Boasian culture concept that seems immediately to promise intellectual purchase for an autonomous and "relevant" discipline, but the "harder," more deterministic model offered by "social biology."

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6. *"The Direction We Should Like To Go:" Race and the Institutional Establishment of Political Science*

In 1945, Harold D. Lasswell gave a talk on "The Future of the Social Sciences as Policy Sciences," outlining what he saw as the mission of the social sciences in the postwar era.[86] Later, Lasswell would define "policy science" as "concerned with knowledge *of* and *in* the decision processes of the public and civic order" (Lasswell 1971: 1, emphasis original). In the brief 1945 statement, however, he left the concept hazier, perhaps acknowledging that it was still at least in part an aspiration. Nonetheless, the talk communicated a clear sense of accomplishment and new potential, as well as a certain urgency: the "release of atomic energy" had "created" an "emergency" that required "all the knowledge, imagination, and influence at our command." But Lasswell was optimistic that social scientific "thought patterns" as they had evolved could now "point toward the potentialities of the future and generate creative ways of achieving our values in action." In addition to what he saw as promising emphases on systematic logic and definitional rigor in the social sciences, these new and promising "thought patterns" for Lasswell included a "common perspective" centered around the "concept of 'culture'…joined with the concept of 'personality'… [and] the emphasis put on trend…analysis [over]… the quest for universal and immutable propositions."[87]

That is, Lasswell identified precisely the innovations outlined in earlier chapters—the mining of anthropological and psychological perspectives to form a

[86] Harold D. Lasswell, unpublished memorandum from 1945, "The Future of the Social Sciences as Policy Sciences," CEM Papers, Series 4, Subseries 1, Box 120, Folder 6.
[87] It's intriguing to note that Lasswell credits "[Clark] Wissler [on whom see more below] and his colleagues"—not Franz Boas—with the formulation of the culture concept.

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concept of a socio-political realm analytically separable from “nature,” and a new focus on medium-term historical dynamics—as the conditions of possibility for a new kind of social and political science, one appropriate to guide democracy into the atomic age. This is hardly a coincidence, given that Lasswell was one of the most prominent early products of Charles Merriam’s “Chicago School” of political science, which had been very much in the business of promoting this outlook. While Lasswell does not mention his mentor by name in this talk, the vision of social science he outlines is clearly consonant with the hopes and ambitions Merriam repeatedly outlined more than two decades earlier (e.g. Merriam 1921, Merriam 1974 [1925]). And Lasswell does credit one of Merriam’s very close associates, noting the “open secret that the postwar [that is, interwar] expansion of the social sciences owes a great deal to the vigor with which B. [Beardsley] Ruml and his associates in the disposition of Rockefeller money irrigated the field."

This chapter explores the “fields” that Lasswell and his teacher and colleagues labored in, and dense network of relationships they cultivated in their effort to promote a political science that could lay claim to both scientific rigor and practical utility. While the new conceptions of politics and its relation to social life they forged and promoted over the course of the decade were a powerful contributing factor to the measure of success they enjoyed in this regard, the professional organizations and resources they secured and the networks they entered into were also, as Lasswell acknowledged, important contributing factors to the spread and eventual dominance of what came to be known as the “behavioral approach” and the project of “policy science” that Lasswell advocated.

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As it happens, ideas about race and the people and institutions promoting and conducting race research figured centrally in what Martin Bulmer (1980) has called "the early institutional establishment of social science research" in Chicago in the interwar period—a project in which political scientists first organized through the APSA were prime movers—and the networks of ideas, influence, money, and relationships that made it possible and were in turn strengthened by it. Political scientists working to promote both their own discipline and social science generally in fact latched on to race science, eugenics, and other forms of biological determinism then circulating, as potential intellectual resources and as avenues of institutional connection between the emerging social sciences and the natural sciences, which by then had a real advantage in terms of prestige, financial support, and legitimacy with state agencies. (One important factor in and indicator of this advantage was the inclusion of the natural sciences in the Congressionally chartered, foundation-underwritten National Research Council [NRC][88]). An episode that illustrates this dynamic in interesting ways concerns the very first organized intellectual activity of the Social Science Research Council (SSRC), begun before the Council's official founding. This was an effort, spearheaded by Charles Merriam, to include members of the fledgling organization in the work of the Committee on Scientific Aspects of Human Migration (CSPHM) an NRC-run, mainly Rockefeller-funded program of research on what participants often referred to as "race contact."

That is, just as political scientists' interest in the individual- and group-level

[88] The NRC was organized by the National Academy of Sciences at the request of Woodrow Wilson in 1916, as a war preparedness measure. (The Academy itself had been organized as part of the Civil War effort.) In 1918, Wilson promulgated an executive order mandating the organization to continue functioning in peacetime.

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differences "revealed" by intelligence research complicated the story of their Boasian-influenced rejection of Teutonism in the 1920s, the story of institutional "modernization" in the discipline in that same period reveals that political scientists certainly did not set out to "abandon race" as they went about redefining their discipline. Indeed, for some of the most significant political scientists in this period, creating links between political science and race research (as well as allied forms of research on human difference) appeared a promising route both to real insight about the workings and possibilities of American democracy and to increased scientific and political legitimacy for their profession.

"Institutional Establishment" and the Chicago School of Political Science

Two key innovations in the organization of social science at Chicago in the 1920s were the Local Community Research Committee (LCRC), based within the University, and the SSRC, which though officially independent of Chicago, drew much of its early momentum, and directed a substantial share of its early resources, to Chicago scholars (Heaney and Hanson 2006: 593, Bulmer and Bulmer 1981: 385). Each got the lion's share of its early funding from the Rockefeller philanthropies and was shaped in consequential ways by the vision of Charles Merriam and his collaboration with Ruml and others. And each sponsored attempts to craft social analysis from biological, and particularly "psychobiological" "facts," including accounts of racial difference.

As noted above, there is some controversy over the extent to which a

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"Chicago School" of political science can be said to exist as what Merriam's biographer has grandly called, "a genuine school of thought" (Karl 1974: viii).[89] However, it is indisputable that the Chicago political science department under Merriam's chairmanship (1920-1940) was a force in the discipline, and was for much of the 1920s and 1930s viewed by contemporaries as, if not the leading department in the country, certainly the most innovative or forward-looking. For Heaney and Hansen much of the success of the Chicago department in creating a model for emulation (see further discussion in Chapter 5) derived from a "confluence of disciplinary receptiveness, institutional opportunity, and entrepreneurial talent" (2006: 589)—that is, the Chicago political scientists produced attractive ideas, had a supportive administration (which for example allowed Merriam to hire his own students on to the faculty, in defiance of academic norms), and were relatively unified in outlook and aggressive in advocating their approach, for example through such initiatives as the National Conferences on the Science of Politics that Merriam was central to organizing.

Bulmer emphasizes further institutional factors in his explanation of why the social sciences at Chicago, particularly sociology and political science, were able to turn social research "in a new and different direction" in the 1920s, achieving a "pre-eminent position" by the beginning of the following decade (Bulmer 1980: 109-110). Drawing on Edward Shils's (1980) concept of "institutional establishment" for academic disciplines, Bulmer shows that during the 1920s the social sciences at Chicago achieved a "relatively dense" "social arrangement" that allowed for the

[89] Gunnell (1992) has pressed this case against the distinctiveness of the "Chicago School" most forcefully.

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development of institutional "authority," "the acquisition of resources," and "provision for the diffusion of … results" in such a way as to multiply their impact among both scholarly and lay publics" (Shils quoted in Bulmer op. cit.).

A key piece of this was the LCRC, "an embryonic and loosely integrated framework for empirical research" that "represented a significant departure from the model of the individual scholar" (Bulmer op. cit.: 54-55), by acting as a vetting agency, planning body, and conduit of funds for collaborative research projects by University of Chicago social science faculty. The LCRC began its work in 1923, and the SSRC, conceived and shepherded to reality by Charles Merriam essentially simultaneously, was in many ways an extension of the model of the LCRC, drawing on similar personal and institutional networks for the resources that sustained it.[90]

These two developments, and particularly the founding of the SSRC, are generally remembered as a turning point in the history of the social sciences in America. As we have seen, the 1920s were a decade of specialization and professionalization for the social sciences. By this point, all the major national disciplinary associations now extant had been founded, and while the boundaries between disciplines were still relatively permeable, major intellectual and institutional efforts were aimed at demarcating the various branches of scholarly work. But even as this fragmentation into distinct fields was being laboriously accomplished, disciplinary leaders began to worry that something was being lost, and to search for ways to integrate and coordinate knowledge production. That is, while more

[90] Gabriel Almond remembers the SSRC as emerging from the LCRC, for example. Transcript of September 20, 1978 interview by Richard Brodie, American Political Science Oral History Project, University of Kentucky, Special Collections and Archives, p. 99.

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concentrated, specialized and rigorous fields appeared the best way to lift the social sciences out of the realm of "speculation," this was still a cohort that had come of age in the Progressive Era and been shocked by the conflagration of World War I, and a widespread sense of the interconnectedness of the modern world as well as of the potential of integrated knowledge for achieving some measure of mastery over that world is visible in their institutionalizing work.

These were the twin impulses behind the publication of volumes like Harry Elmer Barnes's *The History and Prospects of the Social* Sciences (1925), *The Social Sciences and Their Interrelations* (Ogburn and Goldenweiser 1927) and the 1930 *Encyclopedia of the Social Sciences*, discussed above (see esp. Chapter Four). More consequentially, it was the impulse behind the creation of both the LCRC and its more famous national counterpart, the SSRC. The latter was, and remains, a research-sponsoring and coordinating body made up of representatives of the major social scientific professional associations.[91] According to a 1924 report published in the *APSR*, the Council was meant to work as "a powerful factor in the more adequate organization of social research and in the development of scientific methods in the social sciences" (Crane et. al.: 594). To this end, it established committees meant to identify, advise, and channel funding toward promising research; a program of

[91] The American Political Science Association and American Sociological Association first committed to the project in late 1922, joined shortly by the American Economic Association and then the American Statistical Association. They were soon joined by The American Historical Association, The American Psychological Association, and the American Anthropological Association. The "Social Research Council" had its first meeting in February 1923. (Crane et. al. 1924: 594; Cf. also correspondence between Charles Merriam and Mary Van Kleeck, MVK Papers, Box 84, Folder 10, for a more nuanced timeline than that provided by published accounts). The APA was originally reluctant to join, citing their inclusion in the NRC, but relented within a few months (John E. Anderson to Charles Merriam, January 31, 1924. CEM Papers, Series 2, Subseries 3, Box 25, Folder 9; John E. Anderson to Robert M. Yerkes, January16, 1925. RMY Papers, Series 2, Box 88, Folder 1695).

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fellowships and conferences; and a set of publication programs aimed at making basic information more accessible (such as indices of state laws and social science abstracts). Crucially, it would work across fields, supporting appropriate experts and promising students in collaborative or at least coordinated work on questions that the prominent scholars sitting on its committees saw as important and feasible.

This was a consequential innovation. Dorothy Ross sees it as "a major catalyst for the focus of social science on scientific method" (1991: 401); for Bulmer, the advent of the Council's activities was among the most important of a set of "fundamental changes in social science research [that] marked the beginning of a modern phase in its organization" (2006: 283). These changes could be seen in the conceptualization of research itself, the relationship between academic research and foundations (and the viability of both, with funds flowing to the former and credibility to the latter[92]), the balance of power within and between various academic centers (to the great benefit of the University of Chicago, for example), and, somewhat later, the relationship between social scientists and the state (when SSRC-affiliated scholars staffed Hoover's Committee on Social Trends and then FDR's National Resources Planning Board [NRPB], for example). That is, the SSRC (like, for a moment, the LCRC) eventually worked to dramatically raise the visibility and

[92] The SSRC was also useful to foundations that had been burned by political controversy surrounding their work. Rockefeller philanthropy had proved especially vulnerable to this given the publicity around the brutal labor practices of the foundation's benefactors. For example, in the wake of the 1914 Ludlow Massacre, Rockefeller attempts to fund "industrial relations" research aroused considerable suspicion. But turning funds over to independent experts provided a measure of political cover. Carnegie had met with similar skepticism in its early efforts to fund social science research, and proved reluctant to re-enter the field through much of the 1920s (Bulmer and Bulmer 1981, Fisher 1993, Fosdick 1952). These concerns are addressed specifically in early letters announcing approved funding requests by the LSRM, which generally contained language requesting that the recipient be discreet about the original source of funds.

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prestige of the social sciences—and therefore of the notion of "the social" as a legitimate object of inquiry—and was a major element in the social and intellectual configuration "that led one political scientist to grandly describe the twentieth century as 'the century of the social sciences'" (Worcester 2001:145-146).

However, I argue here that for political science, at least, the immediate goal when these organizations were conceived was not autonomy for social science. A significant percentage of the work done by political scientists through the LCRC in its early years consisted of attempts to bring a "psychobiological" or physiological approach to bear on questions of citizenship and leadership. And Merriam and his colleagues in the early years of the SSRC in fact hoped to incorporate that organization into the NRC. This aspiration represented a bid for legitimacy and for the security of inclusion in an already-established institution with a good record of securing funding. But it also reflected a hope for closer integration with the natural sciences, which, as noted above, Walter Lippmann hoped might soon "supply us [political scientists] with our premises."[93]

The very first scholarly activity of the SSRC, in fact predating its official launch, reflects both the intellectual ambition signaled in Lippmann's comment, and the very savvy organizational entrepreneurship that Merriam is famous for. This was the contribution of a "conference group" to the NRC's Committee on Scientific Problems of Human Migration (CSPHM). The SSRC's first full-fledged committee was a parallel body referred to variously by the same name as its counterpart or as the Committee on Scientific Aspects of Human Migration (CSAHM). In the few

[93] February 17, 1923. CEM Papers, Series 2, Subseries 3, Box 34, Folder 12.

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scholarly accounts of this collaboration, it is depicted as a terrain of struggle, in which figures from the NRC try to impose a set of racial-deterministic frames, and representatives of the social sciences insist on the primacy of social and economic forces. My analysis, based on extensive archival research, shows that while there is an element of truth in that account, the story was in fact more complicated. In particular for the political scientists involved, the fairly rapid demise of both committees and the brief move of the SSRC's Migration Committee toward questioning the eugenics/racial anthropology line in psychology and biology represented a disappointment, and indeed at least coincided with the moment when political scientists ceased to be active in this work.

The Committee on Scientific Problems of Human Migration, NRC

The CSPHM had its origins in the push for immigration restriction after World War I, and while it attempted a certain measure of impartiality in its formal statements, was unmistakably engineered to support a restrictionist argument. The two figures most active in its organization were Robert M. Yerkes, former head of the Army intelligence testing program, who in the 'teens and '20s held a number of positions of responsibility within the NRC; and Charles Merriam's older brother, John C. Merriam, a former University of California paleontologist who had chaired the NRC for a year in 1919, and resumed the chairmanship in 1921, when he also began a decades-long run as President of the Carnegie Institute of Washington.

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Yerkes's views on immigration and race have been discussed above.[94] The elder Merriam brother was a quieter but if anything more significant promoter of restrictionism, eugenics, and race science broadly. He was among the closest friends and collaborators of *Passing of the Great* Race author Madison Grant. The two essentially co-founded the Save-the-Redwoods League, an organization that linked the fate of the noble trees of the American northwest to that of the nation's imperiled Nordic race (cf. Spiro 2009: Chapter 11). One of Franz Boas's "long-standing political enemies," Merriam played a key role in forcing the senior anthropologist's resignation from the NRC in 1919 (Castañeda 2003: 239-242; cf. also Stocking 1982 [1968]: Chapter 11). He was also among a select few invited by Grant and Charles B. Davenport (the founder of the Eugenics Record Office at Cold Spring Harbor, a collaborator with Yerkes on the Army testing program and many other initiatives, and "the most prominent racist among American scientists" [Barkan 1992: 69]) to become charter members of the Galton Society. In a 1918 letter outlining his plans to the President of the American Museum of Natural History (AMNH), which eventually

[94] In addition to his own research and writing, Yerkes was active in a number of eugenics organizations. Along with Madison Grant, Charles B. Davenport, and the American Museum of Natural History's president, Henry Fairfield Osborn, and others, he signed on during this period as a "charter member" and financial supporter of the Eugenics Society of the United States of America. This group was founded in 1923, in recognition of the fact that "the time is ripe for a strong public movement to stem the tide of threatened racial degeneracy following in the wake of the War. America in particular needs to protect herself against indiscriminate immigration, criminal degenerates, and the race suicide deplored by President Roosevelt" (Irving Fisher to Robert M. Yerkes, January 15, 1923. RMY Papers Series 2, Box 79, Folder 1517). It was later renamed the Eugenics Committee of the United States of America and affiliated with the International Commission on Eugenics, with Yerkes on the advisory council and Davenport and Grant on the small Executive Board (Irving Fisher to Robert M. Yerkes and reply, November 21 and December 3, 1924. RMY Papers, Series 2, Box 79, Folder 1517). Yerkes was also on the Carnegie-funded Eugenics Research Committee along with former Carnegie President (and Ruml mentor) James Angell as well as Davenport, Clark Wissler, and others (Meeting of the Advisory Committee on Human Behavior at the Offices of the Carnegie Foundation for the Advancement of Teaching November 26, 1924. RMY Papers, Series 2, Box 60, Folder 1144).

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hosted the group, Davenport explained that the Galton Society was meant to be "an anthropological society…self-selected and…very limited in numbers, …and also confined to native Americans, who are anthropologically, socially and politically sound, no Bolsheveki need apply" (in ibid: 68).

More concretely, the group was meant as a place for proponents of "racial anthropology" to organize in response to the recent takeover by Boasians (presumably the "Bolsheveki" Davenport had in mind) of the intensely factional American Anthropological Association (cf. ibid: Chapter 2; Stocking op. cit.; Kevles 1985: Chapter 5; Tucker 1996: Chapter 4; Spiro op. cit.: Chapter 12). The Galton Society as such produced little, but the patrician, right-Progressive, Republican social network it represented was a powerful one. As Jonathan Spiro puts it, "when…Galtonians like John C. Merriam and Robert M. Yerkes were put in charge of the nation's science establishment in the 1920s, it was upon their fellow hereditarians in the Galton Society that they bestowed government positions and federal grants" (op. cit.: 308).

Spiro's reference to "federal grants" here, while not inaccurate, is a bit misleading. The funds these figures "bestowed," while often funneled through the quasi-governmental NRC, in fact did not come from Congressional appropriations but from foundations, including John Merriam's own Carnegie Institute (which financed most of Davenport's work at the ERO, for example) and the Laura Spelman Rockefeller Fund (LSRM), a short-lived but very influential unit of Rockefeller philanthropy, under Beardsley Ruml's direction.

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While it was the LSRM that essentially underwrote the CSPHM, it was in fact to the Russell Sage Foundation (RSF) that Merriam and Yerkes directed their first request in 1922 for funds for what at that stage they still called "immigration" research. After a series of informal discussions, in November of that year Yerkes submitted a proposal for seed money for a conference and some exploratory studies to Mary Van Kleeck, Director of the Department of Industrial Research at the RSF. The proposal noted that it seemed "evident" that "America's policy of restriction of immigration" was to "be reconsidered in the near future" and stated the belief of the NRC's Division of Anthropology and Psychology that in this context it had "a contribution to make," particularly in "the further perfecting of psychological tests which would yield information never before adequately collected on racial characteristics" and "enabl[e] the United States to know through facts and not through mere impressions what effect upon a nation will result from migration…."[95]

That is, for Yerkes at least this research suggested an opportunity not only to fund his fellows in the eugenics movement and make a contribution to the push for immigration restriction, but also to keep resources flowing to his work on mental testing.[96] That the application would be approved must have been a foregone

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[95] Memorandum Regarding Request of National Research Council through its Division of Anthropology and Psychology for a Grant and for Other Cooperation from the Russell Sage Foundation in a Study of Problems of Human Migration, November 1, 1922. MVK Papers, Box 84, Folder 10.

[96] Yerkes's real dream and main preoccupation was not perfecting mental testing on human subjects but pursuing primate research, and after years of lobbying for adequate funding eventually left psychometrics behind to work with chimpanzees. Nonetheless, he promoted the extension of testing research throughout the '20s, arguing that while intelligence tests were valuable, they did not yield nearly detailed enough information about a range of human capacities, and needed to be supplemented by occupational, emotional, and characterological measures. Moreover, he viewed his work with human and primate subjects as fundamentally of a piece, arguing that chimpanzees represented a more easily controllable population that would yield results applicable to the same sorts of social control questions he hoped to solve through mental testing (Haraway 1989: Chapter 4; Yerkes 1930).

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conclusion, because just eighteen days after it was submitted, the NRC held a "Conference on Human Migration," financed by part of a $5000 grant from the RSF. The meeting was by invitation only, and the Galton Society and eugenics community were heavily represented among the chosen, who in addition to John Merriam (who chaired the proceedings), Yerkes, and Van Kleeck, included H.H. Laughlin (Davenport's deputy at the ERO), William McDougall (race psychologist and author of *The Group Mind*), Carl C. Brigham (Yerkes's deputy in the Army mental tests and author of *A Study of American Intelligence*), Raymond Dodge (then-chair of the NRC Division of Anthropology and Psychology, and a member of the Galton Society), Clark Wissler (an anthropologist and AMNH curator who had briefly studied with Boas but who allied with hereditarian race psychology; also a charter member of the Galton Society[97]). Other participants included several representatives of the Surgeon General's Office and War Department.

Yerkes's opening remarks made the purpose of the meeting, and the research agenda it meant to establish, very clear, and also indicated that participants were "among friends" and could speak freely:

> Our Division…for a considerable time has been interested in the scientific problems which grow out of immigration, or putting it more generally, of the migrations of diverse ethnic groups. We have had a committee for some time on Race Characteristics. The committee found it difficult to finance work under that title and the Division of Anthropology and Psychology a few months ago authorized a committee on Problems of Immigration, emphasis

[97] In fact Wissler was a psychologist interested in intelligence differences before he was an anthropologist, earning a doctorate in psychology in 1901 for his work under James McKeen Cattell at Columbia, and continuing to work on "individual mental differences" under Cattell for two years thereafter. It was during his doctoral work in psychology that Wissler first encountered Boas, and the two were on the Columbia anthropology faculty together for many years. But the bulk of Wissler's training in anthropology happened at the American Museum of Natural History, whose staff he joined in 1902 and with which he was closely associated for the rest of his career, even after joining Yerkes at the newly established Yale Institute of Psychology in 1924 (Murdock 1948).

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> being laid on scientific problems as contrasted with practical. … There is no thought of publishing a report of this conference… [and] no reason why any of us should not say what we think with the assurance that it will not go beyond this room.[98]

Presentations at the conference included Laughlin's " The Measure of Specific Degeneracies in Immigrant and Native Populations in the United States," Assistant Surgeon General J.W. Kerr's "The Detection of Physical and Mental Defects Among Arriving Aliens," and Carl C. Brigham's "Intellectual Status and Race," and similar offerings. The one discordant note was an address by Kate H. Claghorn of the New York School of Social Work (with Van Kleeck one of two women present), who questioned the efficacy and findings of mental tests and argued for research on immigrant life histories that could offer insight into how to improve the conditions greeting new immigrants.

Claghorn's talk was met with near-complete silence. The others, however, excited extensive and congenial discussion, from which emerged a fairly clear consensus that immigration was fundamentally an aspect of, in John Merriam's words, "the race problem in its broadest sense."[99] As Laughlin put it, the immigration debate had been played out until then largely in terms of the interests of capital and labor. "But there is a more farsighted and patriotic view" to be worked out via the "scientific method."[100] Yerkes concurred in his final remarks outlining the task that faced the NRC as he saw it:

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[98] Proceedings, Conference on Human Migration, Arranged by the Committee on Scientific Problems of Immigration, Division of Anthropology and Psychology, National Research Council, Washington D.C., November 18, 1922. RMY Papers, Series 2, Box 73, Folder 1398, page 2.
[99] Proceedings, Conference on Human Migration, op. cit., p 3.
[100] Ibid p. 10. Laughlin's contribution to this included "Relative Social Inadequacy" rankings of national and race groups that he had prepared for the House Committee on Immigration.

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> I have not been convinced by the facts presented that any country can safely undertake the stupendous task of trying to work out an economic program which will fit all races of man—not to mention educational and other aspects of a social system. … The great opportunity for our committee is to help place the whole matter on a more secure, objective basis.[101]

The Committee to be thus charged was formulated three days later, in a meeting between Yerkes, Merriam, and Raymond Dodge. The only conference participant not considered for committee membership was Claghorn; in the end, invitations to join the three NRC officers on the new committee were extended to Van Kleeck, Wissler, and the chair of the NRC Biology and Agriculture Committee, the University of Chicago zoologist Frank Lillie.[102]

The full committee had its first meeting in January of the following year. In his advance memo, Yerkes suggested a mix of possible projects "in connection with interracial associations and relations…racial cooperation and antagonism, mixture or supplantation, deterioration or extermination," including "preparation of better methods of measuring human traits," studies of "differential birth-rate and of race mixture," of immigrant individuals and communities, and of the "relation of environmental factors to racial traits."[103] At the meeting itself, these topics were approved and subcommittees authorized to examine each one.[104]

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[101] Ibid. p. 43.
[102] Minutes, First Meeting of Committee on Immigration, November 21, 1921. RMY Papers, Series 2, Box 73, Folder 1398. The membership of the committee changed over time, and numerous subcommittees and "conference groups" were added. Yerkes served as chair through the crucial early period; he was succeeded briefly by GM Stratton and finally by Clark Wissler. For details see Wissler (1929).
[103] Suggestions for Committee on Scientific Problems of Human Migration, Submitted by the Chairman of the Committee for consideration at a meeting called for January 25, 1923. RMY Papers, Series 2, Box 73, Folder 1398.
[104] Minutes of Meeting of Committee on Scientific Problems of Human Migration, Washington DC, Thursday, January 25 1923, RMY Papers, Series 2, Box 73, Folder 1398.

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In the end, the Committee operated for six years with a total budget of around $140,000; the great majority of those funds were provided by the LSRM. Projects it financed included Carl C. Brigham's "Extending the Applicability of Mental Measurement" (Or "Internationalizing Mental Measurement"), which sought to reduce the influence of language barriers on intelligence test results; "Study of Primitive Human Response" (an attempt under Raymond Dodge to identify group and individual differences in neurological and physiological responses to stimuli); "Analysis of Human Personality" (attempts to devise tests for non-intellectual traits, under the direction of industrial psychologists Walter Van Dyke Bingham and C. S. Yoakum); "Analysis and Measurement of Mechanical Ability"; "The Influence of Race Upon Pathology" (Raymond Pearl's Johns Hopkins-based research on the differential "vigor" and mortality of racial groups); attempts to link race, "handedness," and temperament by June E. Downey of the University of Wyoming; "Comparative Study of Mental Traits of Negroes and Whites" (under Joseph Paterson of the George Peabody College for Teachers in Nashville working with Wissler); and a number of other projects, including studies of the anatomical effects of "race crossing" in Hawaii by Wissler, and the development of an "automatic correlation computing machine" by Clark Hull of the University of Wisconsin (cf. Hull 1925). The most notable projects to come out of a "sociological conference group," chaired by Van Kleeck and staffed largely by people nominated by the proto-SSRC, were studies undertaken under the auspices of the National Bureau of Economic Research (NBER) under Wesley Mitchell and Harry Jerome, of the relationship of migration

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patterns to the business cycle, the labor supply, and trends in industrial mechanization.

In the Committee's (few) public reports and (numerous) funding requests and internal memos, as well as in correspondence between its members and discussion at its meetings, these projects were generally presented as fundamental methodological work that would provide techniques for gathering more nuanced and authoritative data about racial traits and the consequences of race mixing than had hitherto been possible.[105] But it never lived up to the grandiose expectations of its planners, and in the end was shut down somewhat quietly. Already in 1926, Ruml commented to Wissler, who had recently taken over the chairmanship of the committee from Yerkes, that "it would be a mistake to print a public report from the Committee" because its work "could not help being misunderstood, and the lack of cohesiveness as between the various projects would, I feel, be subject to a good deal of unfavorable comment by persons who were not informed more intimately than any report could be counted upon to make them…"[106] (The Committee began to wind down its work at this point, effectively shutting down by early 1928.) Yerkes himself consistently referred to his experience on the CSPHM as a great disappointment, though he remained committed to its basic premises throughout his career (Yerkes 1930).[107]

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[105] E.g. Report and Recommendations of Committee on Scientific Problems of Human Migration, Presented to the Division of Anthropology and Psychology [of the NRC], April 8, 1925. MVK Papers, Box 84, Folder 11.
[106] Beardsley Ruml to Robert M. Yerkes, April 16, 1926. RAC-LSRM, Series 3, Subseries 6, Box 59, Folder 632.
[107] Cf. also correspondence between Yerkes and Van Kleeck in 1925 (MVK Papers, Box 84, Folder 7). For example, in a letter dated July 10 of that year, Yerkes remarked that "I am deeply disappointed by the failure of our plans…."

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This is not to say, however, that the CSPHM was insignificant. Indeed, for Elazar Barkan, its main function was as a gatekeeper, "blocking major funding sources from scientists who were not associated with its work" and thus "delaying" for much of the 1920s "the oncoming dominance of the cultural school" of understanding racial difference (op. cit.: 112-113). It is also for present purposes interesting as a site in which relations between the natural and social sciences were being negotiated by a number of major players in academia, the science establishment, and the foundation world. As was so often the case in such circles in the interwar period, Charles Merriam was central to the action in this respect, and though his immediate goals with respect to the CSPHM went largely unfulfilled, his dealings with the committee allowed him to intensify and further a set of relationships that would prove crucial to the successful launch of the SSRC and several other major undertakings.

The SSRC and "Migration Research"

It was at the very first full CSPHM meeting in January 1923 that Van Kleeck and Yerkes suggested involving social scientists in the Committee's work, specifically "the political science and historical group" being organized by "Dr. Merriam's brother."[108] This suggestion hardly came out of the blue, however. Charles Merriam had for a period of time been closely attentive to the work of the NRC, and had

[108] Comments by Mary Van Kleeck and Robert M. Yerkes, respectively. Minutes of Meeting of Committee on Scientific Problems of Human Migration, Washington DC, Thursday, January 25, 1923, op. cit.

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considerable contact with it through his brother, to whom he was very close and who repeatedly expressed interest in the possibilities of the younger Merriam's work for "bring[ing] into relation to political studies the evidence available from the related fields which have heretofore been considered a legitimate part of scientific research."[109] Charles Merriam had also been in correspondence with Yerkes for a period, repeatedly requesting information on psychometric tests for political traits (see Chapter 5).[110] Moreover, by this time planning had already begun for both the LCRC and what was still at that time referred to as the "Social Research Council" (a moniker certainly meant to evoke a parallel with the NRC); Merriam was hopeful that the LSRM under Ruml would prove as sympathetic to these new initiatives as Merriam's sources in the NRC seemed to indicate Ruml was to the natural science body.[111]

A University of Chicago-trained psychology Ph.D., Ruml was asked to join the Laura Spelman Rockefeller Fund (LSRM) as Director in 1921 at the age of 26.[112]

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[109] John C. Merriam to Charles Merriam, March 10, 1922. CEM Papers, Series 2, Subseries 3, Box 35, Folder 20. On involvement with NRC, Charles Merriam had for example been involved in the Council's efforts to reorganize itself as a peacetime body, advising the annual meeting of the NRC Division of States Relations in May 1921 on "Reorganization of state government in relation to research as a state function." Annual Meeting of the Division of States Relations Agenda, NRC, May 27, 1921. CEM Papers, Series 2, Subseries 3, Box 35, Folder 20.

[110] A typical, somewhat later letter in this chain of correspondence reads, "Some time ago we discussed the possibility of a test for efficient citizenship. I remember your saying that the psychological test could be provided if you were informed what traits or qualities were to be tested. I worked on this subject through the summer and fall with one of my classes, but thus far have not come to anything like a definite conclusion. I hope, however, after a little more fumbling around to be able to set up a frame-work of it, based on the common judgment of traits of good, bad, and indifferent citizens." Charles Merriam to Robert M. Yerkes, January 19, 1923. CEM Papers, Series 2, Subseries 3, Box 43, Folder 16.

[111] Shortly after arriving at the LSRM, Ruml began advocating for that organization to begin directly supporting the NRC, which to then had been largely underwritten by Carnegie money. See Vernon Kellogg to Beardsley Ruml, June 28, 1922. RAC-LSRM, Series 3, Subseries 6, Box 57, folder 617.

[112] The LSRM was the fourth of a number of funds established with Rockefeller money, including at that time the Rockefeller Foundation and the General Education Board. It was dissolved in 1929 as part of a general reorganization of Rockefeller philanthropy, with some functions devolving on a new

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The LSRM had been founded in 1918 by John D. Rockefeller Sr., in memory of his late wife, with an endowment of nearly $74 million. For the first years of its existence, the Fund operated on a social service model by then familiar, making grants to children's, social welfare, and missionary organizations of the type supported by its namesake during her lifetime. However, with the appointment of Ruml to the newly created post of Director, the Fund was to rethink, if not its basic ends, at least its means and the role of academic research in achieving them.

Ruml, for all his youth, had by this time worked with most of the luminaries in the field of applied psychology, and had experience and high-level contacts in academia, the military, and business. He trained in applied psychology first at Dartmouth under the industrial psychologist Walter Van Dyke Bingham and later for his doctorate at Chicago with James Angell. Subsequently he worked briefly for the Carnegie Institute of Technology before joining the War Department, where he was involved in World War I mental testing (with Walter Dill Scott as co-director of the Division of Trade Tests; this was a somewhat rival initiative to Yerkes's, emphasizing occupational rather than intelligence testing). After the war, Ruml moved with Scott to Philadelphia, briefly helping to found the nation's first industrial psychology consulting firm, The Scott Company, then returning to Carnegie as assistant to Angell, who by then was president of the Corporation.[113] During this time Ruml managed to produce a fair amount of scholarly research, including original

"Spelman Fund of New York" and others being absorbed by the Rockefeller Foundation (cf. Bulmer 2006, Bulmer and Bulmer 1981, Biehn 2008, Fosdick 1952: Chapter 11).

[113] Angell, who recommended Ruml to the Rockefellers, would soon leave Carnegie to assume the presidency of Yale. He also served as chairman of the NRC, and had been an advisor to the Army mental testing program.

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work in statistical correlation (Ruml 1916; Kornhauser, Franklyn and Ruml 1919), a co-authored book on mental testing of school children (Dewey, Child, and Ruml 1920) and journal articles on occupational and intelligence testing (e.g. Ruml 1916a, 1919, 1920, and 1921).

Ruml is often portrayed as taking Rockefeller philanthropy in a radically new direction. As Bulmer puts it, the "evolution from charity to knowledge-based social engineering may be shown by considering briefly the history of the LSRM…and its successor after 1929, the Social Science Division of the Rockefeller Foundation" (Bulmer 2006: 282; cf. Bulmer and Bulmer 1981). Indeed, Ruml's chief innovation was to redirect funds from more a more ethically-oriented, reform-minded (and heavily female) model of social welfare work toward one that embraced "objective" knowledge as a means to achieving social change (cf. O'Connor 2002: Chapter 1; Ross op. cit.: 393-396). In practical terms, this meant vast new sums not only for universities themselves (which were already getting significant resources from foundations and their benefactors)[114] but now for specific research programs and individual researchers who were certified by new, ostensibly more rigorous disciplinary norms, rather than for direct service programs.

But there are also significant continuities in the LSRM's work. As O'Connor points out, as much as the "large-scale, corporately organized private philanthropy" that began to appear in the first decade of the twentieth century had links to Progressive social reform and charity work, from the beginning these organizations

[114] Rockefeller money had in fact underwritten the founding of the University of Chicago itself, though John D. Rockefeller Sr., had subsequently proved reluctant to keep the funds flowing (Karl 1974: Chapter 7).

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had their own take on this agenda. Even Russell Sage, of the big new foundations the most identified with a progressive social agenda, was less interested in supporting "bottom-up" community work than in making social survey efforts more rigorous and scientific (idem). So the ideal of objectivity, and the interest in rigorous, academic social research, was an aspect of foundation ideology from the beginning. Moreover, the mandate of Rockefeller philanthropy had always been to contribute to the "welfare of mankind," and the LSRM was conceived as something quite conventional. A 1919 memo outlining plans for the new foundation suggested that it "Emphasize work in New York City, having special reference to the direct relief of suffering… Emphasize the human touch."[115] Even under Ruml, the LSRM's giving was consistently framed (both by foundation officers and grant seekers) as having fairly straightforward social welfare implications. As Kersten J. Biehn explains, social welfare and academic research were conceived by the officials charged with dispensing Rockefeller funds, Ruml included, as fitting together into an "informal, loosely defined human engineering effort" (2008: 21).

So when Ruml took the LSRM into a newly large-scale program of funding institutions to direct and undertake socially relevant research in the social and natural sciences, it was not so much a departure as it was finding new and potentially more sustainable ways to achieve longstanding objectives, with clear roots in a Progressive-era social vision. As Ruml put it in a memo to his new employers, the would-be "social engineer" found the social science available in 1922 "abstract and remote, of

[115] "The Laura Spelman Rockefeller Fund: Suggestions for Further Study," July 31, 1919, RAC-RF, Record Group 2, Series 0, Box 53, Folder 549.

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little help to him in the solution of his problems." But this should turn foundations toward, and not away from, the university:

> An examination of the operations of organizations in the field of social welfare shows as a primary need the development of the social sciences and the production of a body of substantiated and widely accepted generalizations as to human capacities and motives and as to the behavior of human beings as individuals and groups.

Ruml continued by specifying that "[u]nder the term 'social sciences' we may include sociology, ethnology, anthropology and psychology, and certain aspects of economics, history, political science and biology."[116] This unconventional definition of the social sciences—anthropology and psychology, at that point, were still seen primarily as natural sciences[117]—links to a longstanding Rockefeller interest in the promotion of "social hygiene," a term that in much of Rockefeller philanthropy was essentially substitutable for "human welfare," and which was centrally concerned with "controlling reproduction and reducing the incidence of venereal disease" in order to achieve general improvements in "mental and social fitness" (Biehn op. cit.).

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[116] Ruml, General Memorandum by the Director, LSRM, October 1922, RAC-LSRM, Series II, Box 2, Folder 31, pp. 9-11.

It is also interesting to note that the idea of directing LSRM funds to academics in fact predates Ruml. In a November 21, 1919, letter to Starr J. Murphy, John D. Rockefeller Jr. wonders "if we desired to do so, could we not give from the Laura Spelman Rockefeller Memorial funds for the professors and teachers in colleges and schools?" He continues: "I could wish that the education which some professors furnish was more conducive to the most sane and practical and possible views of life rather than drifting, as it does, in cases, toward socialism and some forms of Bolshevism. Is it not true that many of these professors would not have had their opportunities for education but for the funds contributed gratuitously by the people whom they seem so ready to assail? It seems to me that some influence ought to be brought to bear…." Starr replies four days later, defending academic freedom and advising against direct pressure on academics Ruml's eventual turn to academic professionalization seems tailor-made to appeal to the concerns of both Rockefeller and his more politically astute advisor, though when Ruml's name was put forth to lead the LSRM, the mandate of the organization was still to concentrate on women and children, particularly child nutrition and infant and pre-natal care. See Raymond B. Fosdick to John D. Rockefeller Jr., December 3, 1921. All this correspondence in RAC-RF, Record Group 2, Series 0, Box 53, Folder 549.

[117] Both were included in the NRC, for example.

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Ruml was obviously a figure of real importance for anyone aiming to foster large-scale, capital-intensive research in areas of social and political relevance, and his connections to Yerkes, John Merriam, and others would have put him on Charles Merriam's radar early. Add to this temperamental and intellectual affinities—the two men clearly shared the Progressive vision of better living through technocratic management, and both were big, charismatic personalities who liked to make big plans[118]—and their association seems almost inevitable.

Nonetheless, Merriam's first overtures when Ruml took over the LSRM were met coolly. Ruml's correspondence at the end of 1922 and the beginning of 1923 shows him to be somewhat skeptical about the junior Merriam brother and the various plans emanating from the University of Chicago social sciences—early funding for the LCRC was approved by the LSRM in January of 1923, but only on the strength of L.C. Marshall's involvement (Marshall taught political economy at Chicago and was endorsed to Ruml by the latter's former mentor and boss at Carnegie, James Angell), and Merriam's initial requests for funding for research on citizenship were referred back to the new Chicago committee.[119] Ruml was also initially a bit disparaging in internal foundation memos about Merriam's plans for a "Social Research Council," writing in July 1923,

> I have not talked personally with any of the people who propose the formation of this council, but the feeling is that this group was somewhat disappointed in the inability of the American Council of Learned Societies to raise money and decided to form a council of their own for the same purpose. Just why they felt they could raise money when the Council of Learned Societies could not, I

[118] Ruml's size was apparently literal—he was famously obese, a bon vivant and gourmand whose intellectual and institutional ambitions were just two among a whole set of prodigious appetites.
[119] James Angell to Beardsley Ruml, January 16, 1923, and Ruml to Raymond Fosdick, December 21, 1923. RAC-LSRM, Series 3, Sub-series 6, Box 71, Folder 756.

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don't know. Certainly the group of people back of the present movement is a much less distinguished group than that which sponsored… [the ACLS]…and in general it seems to me a less promising movement….[120]

It is hard to fault Ruml for his hesitance. For all the excited talk coming out of the social sciences in the early 1920s about useful knowledge, it does seem that a would-be social engineer would find little could be the basis for really new directions in social policy or programs. Political scientists—the group at the forefront of the "SRC" initiative, which in fact came out of discussions at an APSA meeting—in particular seemed to be somewhat stymied in the early 1920s. A remark by Robert T. Crane of the University of Michigan, one of the early organizers with Merriam of the SSRC, is perhaps unusually candid but probably not atypical in its frustrated confusion:

> Political science has done very little. … We have been simply studying government. We are interested in the field of politics, and when we try to approach that field I think all of us are entirely at a loss at the present time. The most hopeful prospect at the present moment seems to be along the psychological line and still there are few of us who have any idea what that really means. How we can approach the question, unless it is through the help of the psychologist, we have no idea.[121]

However, within a year the tide had clearly turned: by early 1924, LSRM funds were flowing quite generously to both the LCRC and the SSRC, with Ruml and Merriam beginning a close professional and personal association that would span the rest of the older man's career. In the end, Ruml and Rockefeller philanthropy more generally, were to become the principal patrons of Merriam's institutionalizing work and his individual research; he would even assume the directorship of a small

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[120] Ruml to Fosdick, July 16 1923, RAC-LSRM, Series 2, Box 3, Folder 39.
[121] Minutes of the Meeting of the Sociological Conference Group, Committee on Scientific Problems of Human Migration, Offices of the National Research Council, March 29, 1923. MVK Papers, Box 84, Folder 9, p. 23.

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Rockefeller foundation in the 1930s.[122] But the relationship was not a one-way street. In a sense, Ruml's LSRM, the LCRC, the SSRC, and the CSPHM were all born together: Of the first LSRM grants for basic research (under the funding regime that began in 1923[123]), the largest—$60,000—went to the NRC for the CSPHM, The next largest recipient (getting $36,000) was the University of Chicago. In 1924, the SSRC received $445,000, a take matched only by a similar grant to the Brookings Institution, and well more than any other institution could boast (Bulmer and Bulmer 1981: 383-387). The SSRC provided valuable service to the LSRM, in acting as a vetting agency for its funding programs and providing political cover—studies were not "Rockefeller funded," but sponsored by a body of independent experts. And it was often difficult to tell where precisely programs originated. Just to give a few examples: Ruml and Merriam essentially planned the SSRC fellowship program together (cf. Karl 1974: Chapter 7), and the second major SSRC initiative, launched after the collaboration with the CSPHM, was a committee on International Communication that Ruml had proposed.[124] On a smaller scale, a study by Manuel Gamio of the "antecedents" of Mexican migration to the United States, sponsored by the SSRC's version of the Migration Committee, was essentially planned and so enthusiastically promoted by one LSRM official (with active support from both John

[122] The Spelman Foundation of New York, established in 1929 to continue on a smaller scale the work of the disbanded LSRM.

[123] Grants for "scientific research and investigation" for the first four years of the LSRM's existence totaled $51,000 (Bulmer and Bulmer 1981: 352).

[124] The "Agenda for the Meeting of the Social Science Research Council," held on February 16, 1924, notes that the proposed International Communication study was "referred to us for consideration … on the suggestion of Dr. Ruml of the Spelman Foundation" (RAC-LSRM, Series 3, Sub-series 6, Box 64, Folder 682)

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Merriam and Ruml) before its adoption that some embarrassment and a flurry of apologetic letters ensued (though the project still went through as envisioned).[125]

It is not entirely clear what turned Ruml in Merriam's favor, though John Merriam was probably a factor behind the scenes. What is clear is that Charles Merriam had enthusiastic supporters in Yerkes and Van Kleeck, who in connection with their work on the CSPHM brought Merriam to Ruml's attention many times, and advocated long and enthusiastically for the inclusion of the "social science group" in the work of the NRC committee, and eventually (though unsuccessfully) the NRC itself.[126] Their first overtures lead to the formation of a "sociological conference group" to the CSPHM. This had the significant side effect of putting the fledgling "SRC" for the first time in front of the Rockefeller board, when the original CSPHM funding request was considered and approved at the board's April 1923 meeting.[127]

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[125] The project in question was what became Manuel Gamio's *Mexican Immigration to the United States* (1984 [1931]), and both the idea for a Mexican study and of attaching Gamio to it came from Leonard K. Frank of the LSRM, probably in consultation with Gamio's friend, John Merriam. After a long series of memos and letters boosting the project, in late 1925 Frank wrote to Charles Merriam to say that, "[i]n talking over the project Dr. Ruml has pointed out what I neglected to note, namely that I had put myself in the position of seeming to urge this project upon you and the Council," and to urge him "consider the situation as one between the Council and Dr. Gamio." Merriam's reply noted that the Gamio proposal was with the Council "and in all probability will be approved…. I do not consider that either you or my brother was urging the matter on us, but thought you were merely showing us an excellent opportunity. My brother was especially emphatic in calling attention to the advantages of the proposed plan, especially as it involved a certain amount of cooperation on the part of the Carnegie Institution." Just over a month later, Frank wrote to Gamio himself to say that he was happy to hear that the proposal was progressing and admonishing him that "if you are asked to undertake this work it will be for the Council and not for the Memorial and I believe it would be wiser if no mention of the Memorial's interest or participation were made by you." Leonard K. Frank to Charles Merriam, December 14, 1925, and reply December 16, 1925; Frank to Manuel Gamio, January 29, 1926. RAC-LSRM, Series 3, Subseries 6, Box 56, Folder 603.

[126] Cf. Mary Van Kleeck to Robert M. Yerkes, February 2, 1923. MVK Papers, Box 84, Folder 10; Yerkes to Charles Merriam, February 2, 1923. CEM Papers, Series 2, Subseries 3, Box 43, Folder 16; Van Kleeck to C. Merriam, February 7, 1923, and reply February 13, 1923. MVK Papers, Box 84, Folder 4.

[127] The minutes of the CSPHM Biological conference group are appended to the main funding request. In that meeting, apropos of proposed studies of immigrant groups, Yerkes comments that "[t]his is, of course, largely, if not, primarily, a sociological matter and had been referred to a conference group, of which Miss Van Kleeck is in charge. That group met last week for preliminary discussion… The group

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The social science conference group was put into motion after informal discussions in New York on February 2 of that year with Van Kleeck, Yerkes, and a few representatives of the major social science associations, including Henry P. Fairchild of the Economics Association and Howard L. McBain of APSA. Just a few days after the meeting, Van Kleeck wrote to Charles Merriam to say that she "was much interested in the suggestion made at the Chicago meetings of the various social science groups for a national research committee of the social sciences." She continued,

> If progress has now been made in carrying out these plans, it would greatly facilitate co-operation with the National Research Council in the problem in which its committee on human migration is now interested. It would seem as though no project could be undertaken which would bring together more directly the social scientists and the group affiliated with [the NRC]. ... Large as is the subject of human migration, we may regard this study of it as a means to a still larger one—the development of more adequate plans for research in social and political science.

Merriam wrote back promptly, expressing strong interest in the project, and hoping that the incomplete organization of the social science council "will not make any material difference in the practical workings of the enterprise."[128] And indeed the organization of the conference group went ahead despite the non-existence of a formal body to sanction it. Yerkes and Merriam met in March of that year in Chicago with Robert Park, the social psychologist Ellsworth Faris, Edith Abbott of the School

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represents sociology, economics, government. They recognize their dependence upon other interests, on the several biological sciences—they want help from anthropology, and psychology. … It happens that two or three organizations have recently made a move looking toward the organization of a National Research Council for the social sciences. Professor Charles Merriam of the University of Chicago is, I think, the prime mover." Appendix 1, Biological Conference Group to the CSPHM, Submitted in support of the NRC's funding request to the LSRM. RAC-LSRM Series 3, Subseries 6, Box 59, Folder 634.

[128] Van Kleeck to C. Merriam, February 7, 1923, and replies from Merriam on February 13 and February 27. MVK Papers, Box 84, Folder 4. (Quote is from the latter.)

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of Social Service Admininstration, law professor Ernst Freund, and the political scientist Robert Crane, and agreed to send a delegation to the next CSPHM meeting at the NRC.[129]

When the CSPHM funding request got to the LSRM in April, it included formal proposals from the social science conference group for "group and individual study" of immigrants, as well as a number of projects for "consideration."[130] In the end, the "group and individual study of immigrants," (essentially the ongoing work of the Chicago sociologists under Park) went unfunded, and only two of the originally proposed projects bore any fruit. (These are the NBER studies mentioned above; they were eventually supplemented by a survey by Fairchild of European sources of information on migration from that continent [cf. Wissler 1929].)

The political scientists involved, despite repeated expressions of enthusiasm, never got farther than suggesting vague plans for a "study of the relation of human migration to the successful functioning of democratic government, and the influence of various groups upon the forms and administration of government."[131] But in fact, the interest of Crane and Merriam, the political scientists most intimately involved with the conference group, were not so much in carrying out migration research as in

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[129] In his letter to Van Kleeck about the meeting, Merriam alludes to the fact that "special facilities" will be needed to facilitate the conference group's work. "I take it, however, that what you wish to outline first of all is a series of projects or method of approach to the subject, which would be, of course, a prerequisite to obtaining any facilities." March 9, 1923. MVK Papers Box 84, Folder 4. Merriam could not make the D.C. meeting, but wrote to say that "Personally I am very much interested in the aspect of the problem that Dr. Yerkes proposes to study, since many of the problems are involved in our systems of government and political control. I am confident, however, that a general study of the type indicated will be of immense value in all of its departments and believe it should be pushed forward as energetically as possible. C. Merriam to Van Kleeck, March 20, 1923 MVK Papers Box 84, Folder 4.

[130] Report of Conference Group Regarding Subjects for Research on Economic, Social and Political Aspects of Human Migration, April 2, 1923. RAC-LSRM, Series 3, Subseries 6, Box 59, folder 639.

[131] Report of Conference Group Regarding Subjects for Research on Economic, Social and Political Aspects of Human Migration, op. cit.

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benefiting from it. The expected benefits were to be both intellectual and material.

The intellectual benefits, again, were related to the "premises" that Lippmann saw lacking in political science. Indeed, Crane's remark about hoping for the "help of the psychologist" was a response to a query from Van Kleeck about the potential contribution of political science at the very first conference group meeting. And despite his many confident pronouncements about the "beginnings of the psychological treatment of politics" (Merriam 1923a: 286), Merriam himself freely admitted that when it came to "how we are going to bring the contributions of psychology directly to bear upon our problems in political science," he and his colleagues were still "feeling our way along."[132] At the same time, he was hopeful, joking half-seriously in the summer of 1925 at the first of several SSRC-run (and LSRM-funded) social science retreats at Dartmouth (generally known as the "Hanover Conferences"),

> We [politicians] shall be very happy if [industrial psychologist] Dr. [Elton] Mayo is able to show us that by improving on the spasticity of our colons we can carry the precinct a little more strongly, or that altering the salt balance will help us carry the ward…. If our other friends, the anthropologists, can throw any light upon a national convention by discussing an Indian war dance, why shouldn't we learn that? ... If any of our psychological friends can discover all the differentials in the human faculties, they will be very helpful to us.[133]

But Merriam did think he and his colleagues had something to offer. Indeed, on the subject of "differentials in the human faculties," he continued, "[w]e will guarantee to

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[132] C.D. Allin to Charles Merriam, January 15, 1925, and reply January 19, 1925. CEM Papers Series 2, Subseries 3, Box 25, Folder 1.

[133] Report, Conference of Psychologists, Called by the Laura Spelman Rockefeller Fund, August 26 – September 3, 1925, Hanover, NH. RAC- LSRM, Series 3, Subseries 6, Box 53, Folder 568, pp. 506-507.

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make a more practical use of them than the psychologists."[134] And in both public and private during this period, Merriam was openly critical of aspects of the methodology of intelligence testing and of "the dogmatic assertions regarding the bearing of differential psychology on democracy." He hoped, however, that in discussion with students of politics, psychologists might develop more sensitive and relevant tests for "the kind of capacity that is essential for the purposes of political cooperation and organization in governmental association," whether "predetermin[ed]" by heredity or not (1924a: 476).[135] For this reason he was particularly interested in the aspects of the CSPHM's work that strove to go beyond intelligence testing and toward occupational and character diagnostics.[136]

But while Merriam was certainly enthusiastic about certain of the CSPHM's specific initiatives, his principal interest in the project seems to have been a broader, if clearly related, one: the possibility that collaboration with the CSPHM could be a stepping stone for inclusion of the SSRC into the NRC. A series of resolutions at planning meetings for the SSRC in 1923 indicated that the "general opinion was that it would be highly desirable to enter the National Research Council," as opposed to joining the American Council of Learned Societies (ACLS) or remaining

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[134] Ibid. p. 507.

[135] Merriam was consistently agnostic on this point. While he admitted in the 1924 article that "The role of inheritance in predetermining social and political traits, as far as our knowledge goes, seems less significant than the part played by social training and environment" he held open the possibility that "if it be shown that political ability follows any such fixed laws, it will then be possible to ascertain what these laws are, determine the conditions under which ability or the lack of it arises, and shape the course of the race accordingly. Eugenics is racing along as fast as mental measurement, and may keep pace with it" (Merriam 1924: 477). Remarkably, in this article he is also agnostic about Lamarckian inheritance, commenting that "Nor are we yet informed as to the transmission of acquired characteristics" (476).

[136] This stance did not put him at all at odds with Yerkes. While Yerkes's public statements were strongly supportive of the validity of the intelligence tests and the racial interpretation of the Army tests, in private and in funding requests he frequently noted the inadequacy of those tests, arguing for precisely the sort of extension and refinement that Merriam called for.

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independent, and correspondence between Ruml and Merriam makes it clear that this was a goal the two men shared with the elder Merriam brother as well as with Yerkes and Van Kleeck. (Charles Merriam commented to Ruml that he was "greatly taken with the idea," later noting that Yerkes was taking their proposal to various figures in the NRC's governing body, the National Academy of Sciences, and "I sincerely hope that Dr. Yerkes has been able to make some progress in the direction we should like to go.")[137]

It was clear from an early stage that the National Academy of the Sciences was reluctant to take on involvement in the social sciences, however, so Merriam and Ruml and their allies in the NRC tried a number of tacks. One was to create a "joint committee on methods" that would bring together representatives of the new council with the NRC.[138] Another was to keep pushing for formal joint representation of the two Councils on the CSPHM, something they repeatedly proposed until it became clear in 1924 that this would not go through. Failing that, the SSRC settled on a strategy of setting up parallel and cooperating committees, that might demonstrate the value of such cooperation. As Merriam put it in an SSRC report to the LSRM in 1926,

> [A] significant problem of the Council is that of obtaining still more intimate cooperation with the students the field of natural science. We have taken steps in this direction through the establishment of overlapping committees on Scientific Aspects of Human Migration, on Pioneer Belts [white settler colonies], and especially by inviting into our Council two organizations now represented in the National Research Council [the APA and the AAA]… As

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[137] Merriam to Ruml, November 1 and November 20, 1923, CEM Papers Series 2, Subseries 3, Box 39, Folder 10. See also November 16 letter, same folder, as well as Charles Merriam to Yerkes (Nov 20 1923, RMY Papers, Series 1, Box 34, Folder 644), for a description of the SSRC resolutions.
[138] Yerkes to Van Kleeck, November 13 and December 14, 1923. MVK Papers Box 84, Folder 6.

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> the methods of social research become increasingly scientific and the social implications of certain of the natural sciences become clearer, it is inevitable that there should be narrowing of the dangerous gap that now lies between the natural and the unnatural sciences…. I express my own opinion only when I say that far more intimate relationships between natural and unnatural science are imminent in a future startlingly near, and that the scope ad method of the study of human behavior are likely to fall upon days, so revolutionary, that ordinary political and economic revolutions will appear as mild and ineffective murmurs.[139]

The Migration Committee established by the SSRC for this purpose proved frustrating. Chaired by Edith Abbott (by then Dean of the School of Social Service Admininstration), what comes to be known as the Committee on Scientific Aspects of Human Migration in fact seems to work at cross-purposes with the CSPHM, leading both Yerkes and Van Kleeck, who had been invited on to the second committee as well, to resign in considerable frustration. However, while the SSRC committee was eventually far more open to an "environmental" interpretation of racial difference than its NRC counterpart had been (going so far as to propose funding Boas's research),[140] the problem does not seem to have been that the social science committee planned anything like a direct assault on racial determinism. Wissler remained on the Committee, and the main anthropological study that it sponsored was Gamio's work on Mexico and Mexican immigration, a project originally suggested by Gamio's friend and ally, John Merriam (cf. Castañeda op. cit.). Rather, Abbbott in particular seems to have been resistant to the focus on "basic methodology" so dear particularly to Yerkes, finding even the Chicago Sociology studies (which had been

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[139] SSRC Report, RAC-LSRM, Series 3, Subseries 6, Box 64, Folder 687, pp. 19-20. The SSRC also set up parallels to NRC committees on Sex Research and on "The Negro."

[140] Report of the Committee on Scientific Aspects of Human Migration, December 18, 1926, and Report to the Chairman of the Social Science Research Council from the Committee on Scientific Aspects of Human Migration, March 31, 1927. Both documents in MVK Papers, Box 84, Folder 11.

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rejected by the social science conference group of the CSPHM as insufficiently productive of new methodology) "too theoretical and vague," and commenting that psychiatric social workers were "putting back" the field of social work "by 25 years."[141]

Indeed, most of the projects put forward by the SSRC version of the committee under Abbott reflect the engaged scholarship of the social survey movement more than anything else. The first round of suggestions, submitted by Abbott in 1924, included a number of fact-gathering projects, such as an index of societies engaged in welfare work for migrants in the United States and Europe; studies of the extent and pattern of migration among "negroes" as well as in Latin America, Canada, and Europe; a study of "emigration conditions in Europe"; investigations of the labor conditions facing immigrants in the United States; and "domestic immigration problems" (such as "exploitation").

In the end, only six projects were funded, most of them sharing some characteristics with the survey-research approach to social science. These including a statistical study of population movements in the United States and Europe over 100 years, the "negro migration" study, Gamio's work on Mexico (probably the most scientistic of the projects) and another study by the University of California economist Paul Taylor on "The Mexican Labor Problem in California," a study of the "Causes of Swedish Emigration" by Florence Janson of Rockford College, and the

[141] Lawrence K. Frank memo of interview with Edith Abbott, March 9, 1925. RAC-LSRM, Series 3, Subseries 6, Box 64, Folder 683.

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continuation of one of the NBER projects originally sponsored under NRC auspices, on immigrants and mechanization of industry.[142]

Toward the end of the Committee's existence, it does seem to have aimed more aggressively at the agenda of racial anthropology put forward by the CSPHM, proposing among other things critical reconsideration of H.H. Laughlin's work on the "Relative Social Inadequacy" of immigrant groups, and of the "Question of Racial Abilities"; and examinations of the "Causes and Conditions of Race Prejudice," the effect of geographical context on "Comparative Rates of Assimilation," and, finally, a study by Boas on the "Effect of Environment on Racial Types."[143] But these projects were never funded through the Migration Committee (some appear to have received support from other SSRC committees later), and indeed the committee itself ceased new work in 1927, the year after these proposals were floated.

While Abbott's general orientation may have been uncongenial to Ruml, Yerkes, and Van Kleeck, it is hard to pin the declining fortunes of the SSRC committee on this late turn toward critical engagement with the race science promoted by the NRC committee. Indeed, it was roughly in this same period that the NRC's committee began to lose support from its sponsors as insufficiently productive of coherent, useful results.[144]

Rather, the record suggests that the demise of both migration committees toward the end of the 1920s appears attributable to their shared failure to make good

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[142] Report of the Committee on Scientific Aspects of Human Migration," December 18, 1926. MVK Papers, Box 84, Folder 11

[143] Idem.

[144] In the 1930s, the NRC does back away from the most extreme eugenics work; for example defunding the ERO. But there is no clear evidence that the demise of the CSPHM is connected to this later shift.

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on the promise of integrated, useful knowledge that would yield clear and indisputable public policy implications. Connected to this, and possibly even more important, is the failure of the general project of linking the NRC and the SSRC. In any event, it is clear that Charles Merriam's enthusiasm waned as soon the possibility of closer integration of the two committees was foreclosed—while he remained on the SSRC committee in name, minutes show him absent from committee meetings after 1925, and mention of the committee's work disappears from his correspondence with his brother and with Yerkes at this time; his further mentions of it to Ruml are only to transmit Abbott's funding requests in his function as chair of the SSRC. Again, this does not appear to have been the result of any particular hostility to the project as it unfolded under Abbott—indeed, Abbott and Merriam were to work together in many capacities for years to come. Rather, Merriam simply seems to have turned his attention elsewhere.

Other Sites in the Search for "Premises"

The failure of Merriam's hopes of including the SSRC in the NRC did not dampen his ambitions for the scientific status of his discipline and of the social sciences generally, or for finding ways to bring exact knowledge about the "pre-political" factors shaping human behavior—Lippmann's "premises"—to bear on political questions. Indeed, the relationships forged and strengthened in the attempt provided key resources as he continued to push both agendas.

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The National Conferences on the Science of Politics, which took place for three years in the early 1920s before they were absorbed into the APSA meetings, were one way for Merriam to pursue both aims. As organizer of the popular section on politics and psychology, Merriam brought in figures related to the CSPHM and found a visible pulpit for promoting his conviction that present defects aside, particularly in the fields of mental and occupational testing, "genuine progress in the study of politics is likely to be made" from a "continuation" of the "significant advances [being] made toward more scientific study of traits of human nature underlying political action, and of the processes that in reality constitute government" (1924b: 125). The yearly LSRM-funded Hanover conferences in the mid-1920s were another place where Merriam could pursue these connections and encourage the sort of research he favored—at the 1925 conference, for example, there was much discussion of "Professor Merriam's [proposal for] study of differential and social traits of races or other groups."[145]

And Merriam was successful in directing LSRM funds to significant research into the non-or pre-political determinants of political behavior, including much the research that was to set up two of Merriam's earliest and most influential protégés, Harold Lasswell and Harold Gosnell, as independent researchers. In the case of Lasswell, Merriam was able to point his young colleague on a course of investigation of the psychological and physiological determinants of behavior that was to profoundly shape the course of the younger man's career.

[145] Hanover Conference, Social Scientists' Report, 1925. RAC-LSRM, Series 3, Subseries 6, Box 52, Folder 563, p. 17.

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In 1925, Merriam received authorization for a direct appropriation from the LSRM for $18,000 to commission and supervise a series of investigations of civic education in “modern states,” including the United States, Germany, Great Britain, Switzerland, France, and Russia, among others. (The project eventually received a total of $58,000, including $10,000 to subsidize publication of the resulting volumes).[146] As part of this project, Merriam arranged for Lasswell to work with George Elton Mayo, an Australian psychologist “known to Merriam as a psychopathologist who could state clearly the relation between physiological fatigue, mental health, and social and political behavior" (Trahiar 1981: 185). In fact, Merriam had met Mayo through the NRC’s permanent secretary, Vernon Kellogg; together the two commended Mayo to Ruml, who was to become his close friend and patron, securing funding for a temporary position for Mayo at the University of Pennsylvania’s Wharton School of Business and later helping him to find a permanent job as Professor of Industrial Research at the Harvard Business School (ibid.).[147] LSRM funds subsequently made it possible for Lasswell to spend six months with Mayo at Harvard, and several months in Germany, doing “preliminary work on tests …designed to develop if possible a physiological or psychopathological basis of varying types of citizens.…”[148] This work was meant, in Merriam’s gloss, to determine whether the relation of “different types of personality responses … with reference to civic affairs” to “temperament, physical makeup, personal experience, and kindred factors” might be “directly discoverable by the application of more

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[146] See correspondence in RAC-LSRM, Series 3, Subseries 6, Box 70, Folder 747.
[147] Merriam had in fact tried unsuccessfully to bring Mayo to Chicago (Ross 1991: 456-457).
[148] Report to LSRM on Comparative Civil [sic] Training, July 1st 1925-December 1st 1925. RAC-LSRM, Series 3, Subseries 6, Box 70, Folder 747.

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modern scientific methods to the problem."[149] Lasswell's own expectations were even more expansive—he wrote to Merriam in 1926 that "An exposure of the biological factors which condition response [to political stimuli] will … open a whole new field of reference points for research and restatement of politics."[150] This effort was complemented by smaller scale efforts at the psychobiological study of "leadership" by political scientists through the LCRC.[151]

The final result of the project was somewhat less dramatic than all that—a planned Mayo/Lasswell volume on civic personalities was never written, and a 1927 "Memorandum on Comparative Civic Training" commented that "[i]t was not expected that Mr. Lasswell would discover specific relationships between civic types and physiological and psychiatric traits but significant progress was made in the development of a method for this purpose."[152] However, the project not only stimulated Lasswell's interest in psychopathology and politics, it also gave him his first experience conducting psychoanalytic interviews and tests, which he was allowed to do at the Boston Psychopathic Hospital during his sojourn with Mayo (Trahiar op. cit. 180-181). This experience contributed directly to Lasswell's landmark *Psychopathology and Politics* (1930).

On a quite different register, Gosnell's LCRC-sponsored work (some of it with Merriam) on non-voting and non-naturalization used survey research and statistical method in new ways. It was also significant in that it brought empirical data

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[149] Funding Proposal, Comparative Civic Training, 1925. Ibid.
[150] Lasswell to Merriam, November 1, 1926. CEM Papers Series 2, Subseries 3, Box 34. Folder 4.
[151] Local Community Research Committee Annual Report 1928-1929 to the Laura Spelman Rockefeller Memorial, undated. RAC-LSRM, Series 3, Subseries 6, Box 71, Folder 753.
[152] Merriam to Ruml, July 20 1927. RAC-LSRM, Series 3, Subseries 6, Box 70, Folder 748.

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about the social, cultural, and individual traits of citizens to bear on the question of political behavior. In 1928, the LCRC approved funds for Gosnell's research on "the negro in politics,"[153] work that result in the first book-length treatment of black politics by a political scientist, Gosnell's *Negro Politicians* ([1967 [1935]).

The LCRC was also the vehicle for achieving one of Merriam's longstanding institutional goals. While he was not able to persuade the university administration or the Rockefellers to fund an independent Institute of Politics (or Institute of Government Research, as it was called in some proposals),[154] he was key to the eventual success of the LCRC group in getting the LSRM to fund the construction of the Social Science Building at the University of Chicago, a project championed by Ruml. (At his speech at the building's dedication in 1929, Merriam commented, "When I look at the Building, I see 'B' [for Beardsley] written upon it from every angle" [in Bulmer 1980: 81]; Ruml actually moved to Chicago in 1931 for a brief period to serve as Dean of the Social Sciences Divison). The LCRC was also given the authority to decide how the building would be used, and Merriam's vision of interdisciplinarity—and of the need for collaboration between political scientists and investigators of the "pre-political"—is clearly visible in the plans for the building. Facilities included a number of card-sorting machine and statistical data rooms, and a "pyschological-psychiatric laboratory." Moreover, faculty members were not housed by discipline, but rather in clusters related to subject matter—Howard Gosnell was

[153] Local Community Research Committee Annual Report 1928-1929 to the Laura Spelman Rockefeller Memorial. RAC-LSRM, Series 3, Subseries 6, Box 71, Folder 753.
[154] See Charles Merriam, "An Institute of Politics for the University of Chicago," undated memo (clearly from the mid-1920s). CEM Papers, Series 4, Subseries 5, Box 122, Folder 3. A Public Administration Clearing House, under Louis Brownlow, came later.

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paired with the sociologist William Ogburn, and Harold Lasswell's office adjoined that of the pioneering attitude researcher L.L. Thurstone.

The above does not begin to exhaust the dense intellectual, personal, and institutional web to which Merriam was central and which so profoundly shaped the social sciences in this period. As I see it, however, this slice of that history provides a vivid picture of the way political science was reconceiving its own task at the time. We have seen that, once no longer aimed at discovering deep developmental principles, systematic political science was becoming a study of autonomous political dynamics upon which "inputs" from other, distinct and more basic and primary realms of human life—specifically "nature" and "society"—could be seen to act. This slice of institutional history offers a more nuanced sense of how these figures understood the relationship between political science and what Smith has called the "pre-political," and how they wished to position themselves with regard to it.

Much of what they hoped for didn't happen. Merriam was rebuffed in his efforts at collaboration or merger with the NRC, and both versions of the Migration Committee disbanded within less than a decade having achieved what many participants agreed were paltry and disappointing results. While the LCRC was the site of a local and temporary triumph for the more "scientistic" mode of social science within the University of Chicago[155] and bore significant fruit in the form of things

[155] Charles Merriam, Leonard White, and Leonard Marshall were in important respects able to shift the resources of the LCRC toward political science and economics, and away from sociology, history, philosophy, and social work, all of which were seen to be pursuing more "descriptive' and less "scientific" methodological agendas. However, by the time the LCRC had realized its greatest triumph—the inauguration of the Social Science Research Building in 1930—the university's academic leadership was changing—with for example the elevation of Robert Maynard Hutchins to the presidency—and becoming much less sympathetic to the program of Merriam and his colleagues, with the somewhat ironic result that by the postwar period, when the Chicago School's model began to see

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like the construction of a multi-million dollar Social Research Building on the campus, the personality and citizenship research that political scientists hoped it would further turned out to be more or less dead ends.

But the story of these partial failures is highly illuminating of the ambition of this group of political scientists and fellow-travelers. It is particularly interesting from the point of view of how they imagined that "pre-political" factors—such as those presumably about to be illuminated by race science and other forms of differential biology and psychology—might at some point yield insight that could be integrated into a more efficient and fair reconstruction of politics and American democracy.

I have occasionally been asked to clarify whether I am offering an "internalist" or "externalist" account of change within discipline of political science. I take the former to signify a focus on the geneaologies of ideas, and the reciprocal influence of political scientists (and to a lesser degree other scholars); the latter seems to be largely a term of reproach, signifying an "old-fashioned" (read Marxist), mechanistic reading of the discipline in which political scientists are generally the mouthpieces of various (usually but not always dominant) political groups, forces, interests, etc., and disciplinary discourse changes with the balance of forces. So in the interest of professional self-defense, I usually opt to be an "internalist." But in fact when I follow the story of political science in the decades around the turn of the twentieth century, I find that I am not necessarily staying within (or even able to locate) the lines between "inside" and "outside."

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wide acceptance in political science as a whole, it was rapidly losing ground in its home institution (cf. Bulmer 1980, also Hartz 1943).

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The problem with this dichotomy is that what counts as "internal" changes over time, largely as a result of a complex mix of "external" events and decisions that are "internal" to the extent that they are taken by political scientists and university administrators. To give just one example from the present project, the interwar boom in social science funding is linked to the establishment of the NRC, meant to bring the resources of science to bear on questions of war-preparedness and national security in the context of World War I. Merriam both capitalized on and helped to catalyze this boom, which in turn created a new set of interlocutors for political scientists. Foundations and institutions such as the NRC constituted important audiences with their own criteria of relevance and rigor. They also helped to shape interdisciplinary contact and collaboration, as is especially clear in the case of the alliance of political science and applied psychology encouraged by such influential figures as Ruml. These new arrangements also inspired renewed hope that government itself would patronize political science and absorb a significant number of political science graduates as expert advisers and administrators. That political scientists should entertain this hope at all flowed from political scientists' (read "internal") ideas about proper social organization—by this period political science was saturated with Progressivism, at least to the extent that many political scientists saw some form of activist government as desirable and thought that scientific experts were the appropriate figures to guide government in the task of administering a social world that they saw as unprecedentedly complex. But it also flowed from their more prosaic concerns about disciplinary (and personal) prestige and viability (including questions about funding, jobs for graduates, etc.).

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These aspirations of course were only partially realized in the short term, but nonetheless they helped to shape research programs and the distribution of resources and of professional approbation within universities and institutions like the SSRC. So does the turn by political scientists toward empirical, medium-term research—and toward a modernized, liberal understanding of racial hierarchy—result from changes in their ideas (recognition of the implications of Boasian anthropology, intellectual excitement about psychological testing) or changes in their context (particularly new funding structures created in response to war and shaped by wartime nationalism and xenophobia)? I can't separate these, much less privilege one over the other. And if I were to do so I think I would miss much of what is interesting to me about this moment—how, for example, among anthropologists the "Boasian turn" clearly pointed toward culture (if not entirely away from hierarchy), while for another group of scholars (my tribe—political scientists, particularly the group around Merriam) it led in significant ways (if only temporarily) *away* from culture, toward a more biologistic, psychological model; or how someone like Merriam, who rejected both racialized social evolution and extreme forms of biological determinism, should have consistently sought to move political science toward research programs with what he characterized as "eugenic" implications.

That is, this chapter also points to the difficulty of pinning down the roles of racial ideology and white supremacy in social science research. Merriam, for all his criticism of theory that was "overlaid with prejudice or with national influence or propaganda of an absurdly transparent type" (*HPTRT:* 19), was at the very least sympathetic to the idea that race could function as useful proxy for all sorts of

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differences in capacities, tendencies, etc., and his intellectual and institutional ambitions led him to seek alliances and links with some of the more extreme exponents of eugenics, immigration restriction, and other forms of scientific racism. This ambivalence, I think, helps to understand how "race" comes to figure in the subsequent development of political science largely as an "independent variable," something that explains rather than requiring explanation, something outside of politics, and in consequence, as so many have pointed out, something of an absence.

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7. *Postscript: Merriam's Legacy, "Race Relations," and Scientism in American Political Science*

In 1945, the same year that Lasswell prophesied the coming of the policy sciences, Congress revisited the question of whether the social sciences should be included in a unified national science agency, holding hearings on the composition and mission of the proposed National Science Foundation (NSF). The inclusion of the social sciences received enthusiastic support from liberal New Dealers in Congress, and vigorous advocacy from Social Science Research Council (SSRC) representatives such as Wesley Mitchell and Robert Yerkes. Their principal obstacle was a sense that social science was contaminated by ideology (as evidenced by its association with New Deal planning, among other things). This stirred opposition among natural scientists eager to shield themselves from potential controversy; they made common cause with conservative politicians who had only recently managed to cut off funding for the social science-heavy National Resources Planning Board (NRPB).[156]

At least in part as a strategic response, the SSRC representatives stressed in their testimony a definition of "the enterprise of American social science [as] nonthreatening, with an emphasis on technical, nonpartisan, and value-neutral professional expertise." Despite these protestations, suspicion of the social sciences remained strong in Congress, and support from the Truman White House proved lukewarm: in the end the social sciences were not meaningfully supported by the NSF

[156] The NRPB "had developed an extensive philosophy of federal social insurance and called for cradle-to-grave welfare programs, what Charles Merriam and others called 'A New Bill of Rights,' including the 'right' to decent work and fair pay; to adequate food, clothing, shelter, medical care, education, and security; to a system of free enterprise; to come and go freely; to equality before the law; and to rest, recreation, and adventure." After a series of attacks from a "coalition of outraged conservatives" in the early '40s, funding ran dry in 1943 (Solovey 2004: 397; cf. Merriam 1944).

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until the 1960s (Solovey 2004: 416). Though this effort was unsuccessful, it signposts a reassertion by the nation's pre-eminent social science body in the postwar era of both the scientistic agenda and the technocratic faith in science that animated many of the principal figures in American social science in the 1920s but had come under sustained critique from figures like Karl Mannheim during the Depression and the run-up to World War II.

In its postwar iteration, this debate not only pitted social scientists and liberal politicians against natural scientists and conservatives—it also provoked considerable controversy within the top echelons of American social science. Senior social scientists like Charles Beard and Robert M. Lynd argued that failing to address the value orientation of research actually hindered its usefulness by driving such questions "underground." Particularly vocal critics of the SSRC's "official" line on social science objectivity included Louis Wirth and Gunnar Myrdal, both of whom were prominent scholars of what had come to be known as "race relations" and harsh critics of what Wirth characterized as the "unfinished" nature of the American democratic project (ibid.; cf. also Myrdal 1944, Wirth 1946).

It is unsurprising that we should find these scholars wary of excessive dependence on the natural sciences, as well as of defining social science as entirely separate from ethics and values. This was of course the aftermath of World War II, when the complicity of race science in Nazi genocide was beginning to enter popular consciousness. But it was also the first real flowering of the "race-relations paradigm," the notion, first given definite form by Robert Park and his colleagues in Chicago sociology in the 1930s, explored by Rockefeller-funded SSRC committees in

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the 1930s (Gordon 2009), and brought to national attention with greater force with the publication of Myrdal's *American Dilemma* in 1944, that the subordinate position of blacks in America was fundamentally a problem of racism—of unethical or irrational group attitudes. The "cure" in most of these accounts was a large-scale national commitment to ethical renewal, a "therapeutic" intervention aimed at "disabusing whites of the distorted beliefs that they harbor about blacks" (Steinberg 2001).[157] That is, for these scholars "race" was by now quite firmly both a "social" and an "ethical" question—to sever these two domains, as the SSRC representatives at the Congressional hearings seemed to be trying to do, could effectively undercut the basis of their political and intellectual project.

That is, lines that had been quite indistinct in the early interwar era had become much sharper. In the story of the NRC Migration Committee and related initiatives, and the light they shed on Merriam and his colleagues' institutionalizing efforts in the 1920s, we see that despite the adoption from anthropology of the culture concept, and the rejection of Teutonism and Nordicism as the basis of history, the "race relations paradigm" had not quite yet emerged. While "attitudes" and "race prejudice" were beginning to appear significant and measurable, for the major institution builders and intellectual innovators in political science in the 1920s, these new objects, like race itself, still seemed, however fuzzily, to be part of the domain of natural science. As such they might be a source to be mined for possible insight about basic human differences and their relation to political processes.

But of course the "hard" interpretation of biological difference—racial or

[157] Wirth's 1946 article appeared, typically, in a special issue of the AAAPSS devoted to "controlling group prejudice."

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otherwise—was not fundamental to this overall conception of a political realm conditioned by more basic external factors. “Culture,” “society,” and “attitudes” might all be conceived in ways that fit nicely with this project. These two possibilities were in fact pursued in different measures by some of Merriam’s most prominent students. Dorothy Ross has commented that "[i]f Lasswell picked up on the hard side of Merriam's progressive message, Gosnell developed the soft side" (1991: 457). The same could be said for the visions of the “pre-political” that the two men pursued: Where Lasswell moved toward psychopathology, and a vision of political behavior that referred back, however remotely at points, to a vision of individual bio-psychological difference, Gosnell’s work pointed to group cultural and sociological difference as the source for relevant determinants of political behavior.

Gosnell’s work in the interwar period can’t be said to operate fully within a “race relations” frame—indeed, as James Q. Wilson points out in his 1967 introduction to a reissue of *Negro Politicians*, that book “lacks any explicit theoretical orientation and offers few large generalizations”—in its focus on leadership style, the importance of political oratory, the role of religion, and the consequences of white prejudice, it is clearly the lineal antecedent of that frame within political science research on voting and legislative behavior as well as public opinion. Just to take up the latter, for example, in a recent survey Melissa Harris-Lacewell comments that “political scientists have spent the last several decades developing the field of black public opinion into an important contribution to our understanding of black American politics,” demonstrating “a wide and persistent gap between the political attitudes of white and black Americans” and mapping “the

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unique contributions of black cultural practices, psychological processes, and political traditions in shaping this distinctive constellation of public opinion" (2007: 107). That is, while political science largely failed to pay sustained attention to black political behavior until postwar civil rights protest (and the related emergence of a small group of black political scientists) essentially forced the issue, much of the template for how it was to do so—the focus on culture, social psychology, and group dynamics construed as relatively autonomous forces outside politics—was forged in this interwar moment. Later products of the "Chicago School" including Gabriel Almond and V.O. Key were to work in related directions, with Almond contributing to the development of the notion of "political culture" and Key doing pioneering analysis of the role of white prejudice in American political development (Almond and Verba 1963, Key 1984 [1949])

However, as Ross points out, the "hard" side of the legacy was also significant. In Merriam's interest in mental testing and the work of the Committee on Scientific Aspects of Human Migration, we can see that while political scientists no longer viewed racial development as the key to history, racial characteristics, reconceived as variables that could determine political behavior or at least indicate likely tendencies, still appeared to present exciting possibilities for policy-relevant findings. This aspect of "Merriam's technological imagination" can be seen Lasswell's work, which, while less focused on race, picks up on this sense that different kinds of bodies produce different kinds of political responses (Ross op. cit.: 455). This is particularly clear in his 1930 book, *Psychopathology and Politics*, which offers a psychoanalytic interpretation of the appearance in society of "agitators,"

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"administrators" and "theorists," arguing in effect that the early experiences of people of each of these distinct types in effect shape them so profoundly as to determine the course of their later engagement with politics. And throughout his career he consistently sought what Gunnell has called the "psychological reality behind politics and political ideology" that would "make society manageable" (1993: 123).

While a number of critics have lamented that Lasswell's insights have been "neglected" by subsequent investigators in political psychology (Eulau 1999, Hirschfelder-Ascher 2005), there can be no question that his work effectively inaugurates the field of political psychology. In a less direct way, we may see in the "hard side" of Merriam's legacy, and particularly Laswell's early work with Mayo, an antecedent of the recent development of a mini-subfield in American political science concerned with "genetics and politics."

Work in this area is linked to a longer and more diffuse set of efforts in what some practitioners have called "biopolitics" (cf. Blank and Hines 2001)—the attempt, institutionalized in the 1980s with the APSA-affiliated Association for Politics and the Life Sciences and its journal, *Politics and the Life Sciences*—to "realign political theory so that it is based on a scientific conception of human nature" (Strate 2002: 798). But this larger field has only begun to have a presence in the main disciplinary journals in the past few years, with the appearance of work that purports, in the words of one major statement, to "incorporate genetic influences…into models of political identity formation" (Alford, Funk, and Hibbing 2005: 153).

So for example Alford, Funk, and Hibbing argue for a "substantial heritable component" to political attitudes and behaviors (50%, to be exact), using the

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performance on political "attitude tests" of monozygotic (MZ, or identical) as compared to dizygotic (DZ, or fraternal) twin pairs (idem.). And while this subfield remains small (at least in part, no doubt, because most political scientists don't have the competence in basic molecular biology to engage with it, never mind actively pursue such research), it has excited considerable interest. The article cited above appeared as the lead in the May 2005 edition of the *APSR*, prefaced by a cover illustration depicting a strand of DNA, and an editorial that commended "intriguing evidence that genetic predispositions play a more prominent role than political scientists have recognized" (APSR 2005: iii). The following year, the *APSR's* editor noted that the article "set a new standard for political science in terms of the media attention and public discussion that its publication…provoked" and ranked it as possibly "one of the most important that the *APSR* has ever published" (Seligman 2006: 172). Since, APSA meetings and journals have featured a small but regular sampling of work of this type.

This is not the place for an extended discussion of these efforts, which in any event have been effectively critiqued by both political scientists and molecular biologists (cf. Charney 2008, 2008a; Beckwith and Morris 2008).[158] For present purposes it should suffice to note that the excitement generated by them speaks to the enduring power of the image of a set of non-political factors that, if discovered, could significantly explain political behavior. This is particularly notable given that political scientists' attempts to quantify the extent to which political attitudes (for example) can be attributed to "nature" rather than "nurture" come just as this dichotomy itself is

[158] Though not, it is interesting to note, in the flagship *APSR*, but rather in *Perspectives on Politics*, a much less highly ranked APSA publication.

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being seriously challenged within biology itself, in favor of models of "continuing process[es] of development that emerge from the constant dialogue between genes and environment" (cited in Charney 2008: 302; cf. Fausto-Sterling 1992, 2000). Indeed, while I don't want to push the parallel too far, there is something of a resonance here with the embrace of Lamarckian notions of inheritance in the pages of the *Journal of Race Development*, even as they were finally abandoned within biology and anthropology, and the vogue among political scientists for opposing "biological" with "environmental" or "social "determinism" (Alford, Funk, and Hibbing 2008: 321; Hennegan and Hatemi 2008: 333) even as molecular biology moves away from this set of questions.

However, putting aside the merits of any particular research agenda, the story of political science's "becoming modern," while it hardly fits in to a clean narrative of scientific progress, clearly shows that it was "good" for the discipline in a number of clear senses. The first is that it provided the basis for the emergence of behavioralism, the closest thing to a unifying paradigm the discipline can be said to have had in the postwar era. It also provided the intellectual basis for a number of research programs that we still pursue—quite apart from the boomlet in genetics and politics, political psychology is a major subfield, and while racial politics per se remains "at the margins" of the discipline (Wilson and Frasure 2007), very little of the massive output of research on voting, partisanship, and public opinion within the discipline neglects to at least control for what Walton and Smith call "the race variable" (2007). That is, the identification of an autonomous sphere of politics and the concomitant search for the outside factors that shape that sphere has to a

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significant extent shaped the intellectual practice of the discipline for the better part of its existence, and given a measure of coherence to at least a significant range of that practice.

But I will close by suggesting that it also worked to close off other avenues of inquiry. In particular, the vision of an autonomous political sphere conditioned by outside "inputs" has made it hard to see how politics and indeed political knowledge itself (such as our own discipline's output) may actually generate a lot of the "variables" that we are taking as exogenous. That is, it makes it difficult to see and understand how the social, psychological, and cultural characteristics that appear to shape political reality may in fact be artifacts of political processes and social science discourses.

This indeed is the critique advanced by Smith in his article identifying the 1920s as the moment in which race drops out of political science. As he puts it, understanding race as "pre-political" may contribute to political scientists' failure "to explore fully the role of politics in creating racial identities and racial conflicts" and by extension the role of racial politics in shaping many political patterns identities, institutions, and developments that do not appear to have much to do with race" (2004: 41). Efforts to correct for this failure—to uncover "how the ideological and material elements of race are produced, negotiated, and altered in and through politics" and how "race has structured and been structured by political institutions, political discourse, and public policy" (Lowndes, Novkov, and Warren 2008: 3)—are numerous and ongoing. So for example we are beginning to learn how everything from patterns of residential and labor market segregation to attitudes about the

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welfare state and affirmative action can be linked directly to New Deal-era policy decisions (e.g. Brown 1999; Katznelson 2006, Quadagno 1994). Or how urban development policies of the last several decades have been shaped by social scientific ideas about race, and have in turn shaped local racial political alignments (e.g. Reed 1999). Or how ethnic and racial categories change over time and are negotiated through political and bureaucratic practices (e.g. Nobles 2000, Hattam 2007). But as Taeku Lee points out, a "yawning gap" still remains between much of this new work on what we might call the "co-production"[159] of identity categories and politics and the bulk of work aiming to illuminate the workings of such categories within the mainstream of the discipline (2004: 1). My research suggests that the conception of distinct "political" and "pre-political" realms, effected by the reimagining of the relationship between race and politics by political scientists in the 1920s, is at least a significant part of the genealogy of this "gap."

Political scientists in the early twentieth century did not abandon race – they thought hard about it, reconceptualized it, and in the process reconceptualized politics, their own discipline, and the relationship of both to history and nature. Moreover, at least in the interwar period, they did not so much drop "race" as relegate it to the status of "variable," tempted by the possibilities for social control suggested by race research. And this had profound consequences for the subsequent development of our discipline. Why haven't we noticed this before?

[159] This is a term borrowed from Science and Technology Studies, particularly the work of Sheila Jasanoff (2004), who uses it to highlight the connections between understandings of the natural world and practices related to it.

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Part of the answer certainly lies in the relative lack of historical work on the discipline. But this is not as simple an answer as it appears to be. The fact that political scientists came late to historical self-reflection relative to other American social sciences is, in my view, connected in turn with the discipline's self-image and sense of its place in American history. Put crudely, American political science has long been entangled in two "progress" narratives. The first is the progress of science, in which past theory represents, essentially, discarded error. In this framework, disciplinary history takes the form of stories of struggle and discovery or memoirs of great figures, both forms that are unlikely to take seriously those features of past thought that do not seem to have contributed to the insights of the present. The second is the progress of liberalism, in which American liberal ideals have worked themselves out and been realized in greater degree over time. In this view, racial hierarchy and racial prejudice are aberrations—excrescences to be shed, not essential components of political, social, or intellectual systems.

Another part of the answer points lies in the fact that, despite much important work on the topic, political scientists have only partially changed how we think about race – it's still something to add on, still signals attending to a world separate from the one we studied before. My hope for this project is that it will contribute to an emerging body of literature within political science and elsewhere that does not accept that distinction but rather examines how the intellectual, political, and economic codifications of difference, distance, and hierarchy that are subsumed under the idea of "race" in America have shaped a range of institutions and practices.

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