

THE UNIVERSITY OF CHICAGO

# Sociology's Hesitancy:

Race, culture, and heterodox idea formation in early  
American sociological and anthropological theory

By

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## Introduction

At the turn of the 20<sup>th</sup> century, there was a curious divergence occurring between mainstream sociological and anthropological scholarship in America. In particular, while the country's most famous sociologists clung to Herbert Spencer's theories of social evolution, anthropologists began aligning with Franz Boas' skepticism of evolutionary theories of race and culture. The year 1902 marked the death of J.W. Powell, director of the Bureau of American Ethnography (BAE) and possibly the most famous anthropologist in the country. In the wake of his death, the singular influence of the BAE would begin to give way to alternate spaces of knowledge production, like museums and recently formed university departments.<sup>1</sup> While the BAE and Powell's evolutionary theory of culture had dominated American anthropology in the final two decades of the 19<sup>th</sup> century, the next few decades would see the center of gravity in American anthropology shift toward Boasian alternatives.

In a short period between 1902 and 1910, Franz Boas' vision of sociocultural anthropology, which emphasized the particular history and change of each society over time, would become an acceptable mainstream position in the country's most prestigious universities. These two competing scientific theories of human development—the evolutionary and the Boasian—also entailed diverging opinions on the normative 'value' of different societies. Evolutionary theory more often than not entailed a rejection of 'less evolved' peoples and their ways of life. On the other hand, Boasian sociocultural anthropology emphasized particularity and rejected a single path of development. The result was that Boas and his students felt compelled to

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<sup>1</sup> For an overview of the emergence of these alternatives, see Regina Darnell, "The Development of American Anthropology", 140.

argue against the hardest edges of the Progressive Era's scientific racism and eugenics. With former Spanish territories in Cuba, Guam, Puerto Rico, and the Philippines under American control, race and culture were topics on the tip of everyone's tongue.<sup>2</sup> Competing ideologies of race, and in particular *racial hierarchy* were the bedrock for debates about tutelary colonialism, the restriction of immigration from Asia, settler-indigenous relations, and the place of Black people in postbellum America. As the 20<sup>th</sup> century progressed, Boas' view would gradually supplant the theories of BAE ethnographers.

During the same period at the beginning of the 20<sup>th</sup> century, the country's most famous sociologists would not accept the Boasian arguments about culture. Particularly among the first presidents of the American Sociological Society and most of the members of the country's two most prominent sociology departments—the University of Chicago and Columbia University—evolutionary theory remained at the center of the debate. On the topics of race and culture, this led to predictable outcomes. As the sociologist James McKee wrote of the first two decades of the 20<sup>th</sup> century: “a new genetics invalidated any scientific case for racial inferiority, and the anthropologists provided a vigorous challenge to the claims of cultural inferiority. Sociologists accepted one of these arguments but not the other.”<sup>3</sup> As evolutionary explanations of the social world were losing their grip on many American thinkers, sociologists retained a thesis of inheritable inferiority amongst non-White populations. In some cases, evolutionary arguments were predicated on biological grounds, and in others, it was on behavioral or cultural. All of this

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<sup>2</sup> America's 'imperial culture', and in particular popular fixations on the defining of racial groups within the American empire, is discussed in-depth in Kaplan and Pease, *Cultures of United States Imperialism* (1993). For a related assessment of how this imperial culture bled into academic knowledge production, see Go, “Sociology's Imperial Unconscious”

<sup>3</sup> James McKee, *Sociology and the Race Problem: The Failure of a Perspective* (1993), 5-6.

occurred at a time when more and more anthropologists at elite universities were willing to reject cultural hierarchy altogether.

The divergence between the mainstreams of anthropological and sociological theory is the fundamental puzzle addressed in this thesis. In particular, it appears vital to investigate why one discipline, anthropology, was better prepared to discard evolutionary definitions of race and culture which are no longer seen as viable today. One way of addressing the problem would be as a matter of intellectual history—tracing the development of ideas over time and looking for what inspirations Boas looked to that the sociologists did not.

But this method of inquiry appears insufficient for the task at hand; sociologists, in particular, Black sociologists like W.E.B. Du Bois produced alternative theories about race and evolution. It wasn't that sociology as a mode of inquiry was unable to make use of non-hierarchical definitions of race and culture, it was that evolutionary theory held a Teflon-like capacity for shrugging off major criticisms. After considering the causes of divergence between mainstream sociological and anthropological notions of race and culture, I will return to the work of Du Bois as an example of how heterodox theories can form within scientific fields. Although Du Bois' work is merely that of an individual—and thus it is not able to be generalized to scientific fields in general—an examination of his methodological writings provides directions for future research on the topic of heterodox idea formation.

Throughout the paper, I treat the divergence between sociologists and anthropologists as a puzzle to be solved by the sociology of scientific knowledge.<sup>4</sup> I hold all factors, including

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<sup>4</sup> This theoretical orientation is best explained by David Bloor in *Knowledge and Social Imagery* (1976) Chapter 1 & 2. Bloor differentiated his approach as the 'strong program' of the sociology of science, which took knowledge

scientific practices, political imperatives, and the beliefs of individuals as equally plausible causes of the knowledge claims that these two groups of scientists held to. The strength of this approach is that it avoids two extremes. On the one hand, an overly “social” explanation which argues that the knowledge one creates is entirely determined by their position within society. The extreme of this position might say that Powell’s employment by the imperial American state was sufficient to make him a permanent supporter of racializing theories. On the other hand, it avoids a mode of explanation that ignores the contexts in which knowledge is created and instead focuses entirely upon who is doing ‘better’ science; the extreme of this version would consist of a hagiography of Boas or Du Bois as scientists par excellence. This version of the paper would inevitably reify a ‘true’ definition of race or culture, that the researcher simply has to go out and find scientifically.

Through an examination of the writings of Boas, Du Bois, Powell, and a veritable peanut gallery of early ASS presidents, this paper rejects one common explanation for the aforementioned divergence, which places fieldwork as the sole cause of innovation. Although fieldwork was an important component of Boasian and Du Boisian innovations, it was not sufficient on its own. Instead, I offer a speculative model for how Boas and his students generated conclusions that were at first heterodox, but gradually became dominant in the scientific field of anthropology. To this end, I demonstrate that although Boasians relied upon empirical data to make their claims, Boas’ theoretical divergences with Powell and the BAE stemmed primarily from new methods of interpretation rather than new data itself. Secondly, I show that innovations by a single individual were insufficient to generate a scientific

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claims themselves as explainable social phenomena. He contrasted this approach with a ‘weak program’ that his contemporaries practiced, that left scientific knowledge itself off the table, instead focusing on social practice around the science like funding streams and citation practices.

revolution—Boasian or otherwise. Instead, Boas' innovations were successful because they took place in a specific context—a period with a rapidly declining BAE and a rising university system that was not dominated by Powell. Thus, my explanation relies upon the confluence of three factors: (1) gathering of new data through fieldwork, (2) methodological innovations for interpreting that data in new ways, and finally (3) an institutional space for knowledge production that wasn't dominated by a competing theory. In the case of anthropology, all three were necessary before Boas' definition could become popular.

This three-part model is strengthened through a comparison of anthropological and sociological knowledge production. Sociologists adopted some Boasian principles while seemingly ignoring others; I argue that this was because some Boasian claims could be adapted to the Spencerian milieu of early American sociology but that others were anathema to it. However, because it was uncommon for sociologists to engage in fieldwork, it was difficult for anyone to amass the empirical case for rejecting the Spencerian paradigm in its entirety. In Kuhnian terms, the lack of data meant that there weren't enough anomalies in the Spencerian model to generate a paradigmatic crisis. Thus, even though sociologists didn't have the same incentives to hold to evolutionary theory as BAE ethnographers, they also didn't have any way of formulating a compelling alternative. What unified them was first and foremost their interest in the evolutionary models of Herbert Spencer, and without collecting their own data as most anthropologists did, there was no way to adjudicate between the Spencerian and Boasian paradigms of race and culture. By the first decade of the 20<sup>th</sup> century, when both groups were presented with alternatives to the evolutionary paradigm of race and culture, anthropologists were able to adopt it much more readily.

Although this model seems to explain Boasian anthropology and Spencerian sociology rather well, it is still only tentative. I have generated it largely through a close reading of the works of individuals whom I believe to be representative of greater trends in their respective fields—Boas, Powell, and ASS presidents. Of course, regardless of how representative they are, they are nonetheless individuals. Additional research that puts this model to the test across a larger survey of academic research might introduce new components to the model, clarify the conditions under which it is applicable, or suggest alternative explanations. Future projects that engage with the whole corpus of BAE reports or sociological publications like *AJS* are feasible places to start this process.

The rest of the paper unfolds in three sections. In the first section, I introduce the Bureau of American Ethnology (BAE) and its director John Wesley Powell, arguing that they held to evolutionary theory due to a conjuncture of individual causes—like Powell’s fascination with Louis Henry Morgan’s theory of culture—and structural ones—like the Bureau’s reliance on congressional funding. Boas’ personal decision to formulate an alternative theory can be explained through his methodological texts, but the popularity of sociocultural anthropology can only be explained with the additional context of changing institutions of knowledge production—the rise of university-based anthropology and the decline of the BAE.

In the second section, I compare the Boasian turn in cultural anthropology to the state of mainstream sociological scholarship in the first decade of the 20<sup>th</sup> century. At this time, sociologists were willing to accept Boas and Du Bois’ scholarship disproving *biological* hierarchy of peoples, but still relied on *cultural* hierarchy as a major component of their theories. While the BAE’s assumption of cultural hierarchy could be traced to its role in the appropriation

of indigenous land, sociologists' complicity was far less direct. To explain sociology and anthropology's differing definitions of culture, I argue that we must turn to the actual practices of scientific knowledge production within each field. Here, the methodological writings of Du Bois, in particular his critiques of mainstream sociology, provide a possible explanation as to why Spencerian theory remained dominant.

In the final section, I put the Du Boisian and Boasian projects into comparative perspective. Investigation of Du Bois' methodological writings and his relation to the ASS allows us to speculate why, like Boas, he was able to adopt a heterodox theory. Unlike Boas, Du Boisian sociology did not hit 'the mainstream' in the nation's most prestigious universities and journals. Given the time period, it is not at all ridiculous to attribute the marginalization of the Du Boisian paradigm to Spencerian social theory to racial bias. Although Du Boisian thought was barred from the best known and best funded spaces of knowledge production, this is not to say that there were no other practitioners of his thought. At the Atlanta Sociological Laboratory at Clark Atlanta University, Du Bois and his colleagues generated research which was both empirically informed and not inspired by evolutionary social theory.

An analysis of Du Bois' orientation toward science is dealt with briefly in this paper but would also be a fruitful topic for future research. In particular, a reading of Du Bois' methodological writings seems to show that it was his orientation toward scientific knowledge itself that led to some of his best-known theoretical innovations. This orientation toward scientific knowledge was a rejection of the natural scientific model of the European Enlightenment, in which the observer was wholly neutral. Instead, Du Bois approached scientific



knowledge production with political goals in mind, such as disproving common arguments for scientific racism.

Much like the speculative model generated in sections one and two, the causal power of Du Bois' orientation toward science in generating new theories deserves more attention. A promising avenue in this regard would be to expand this study to other early Black scholars who have received increased attention in recent years by sociologists, such as Ida B. Wells and Anna Julia Cooper. With a better understanding of how early Black social theorists viewed the relationship between science and political knowledge, perhaps a more general model would become apparent.

### **Section 1: Boas and the BAE**

The adoption of an evolutionary view of culture and race by the Bureau of American Ethnology was overdetermined. Both its leadership and its relationship to the American state encouraged researchers to approach indigenous peoples as relics of the past which could be catalogued and defined as such. While there was nothing novel about Americans defining their civilization in opposition to a savage Other, the BAE was formed during a nadir of settler-indigenous relations. In 1871, Congress passed the Indian Appropriations Act, creating the reservation system and federal boarding schools, and declaring “That hereafter no Indian nation or tribe within the territory of the United States shall be acknowledged or recognized as an independent nation, tribe, or power, with whom the United States may contract by treaty.”<sup>5</sup> When

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<sup>5</sup> Joel T. Helfrich et al., “No More Nations Within Nations: Indigenous Sovereignty After the End of Treaty-Making in 1871,” *The Journal of the Gilded Age and Progressive Era* 20, no. 2 (April 2021): 325, <https://doi.org/10.1017/s1537781421000141>.

the Bureau was officially formed in 1879, it assumed the role of teaching Congress about its new Indian “wards”. In the Bureau’s first annual report, director J.W. Powell summed up their ethnographic achievements:

In pursuing these ethnographic investigations it has been the endeavor as far as possible to produce results that would be of practical value in the administration of Indian affairs, and for this purpose especial attention has paid to vital statistics, to the discovery of linguistic affinities, the progress made by the Indians toward civilization, and the causes and remedies for the inevitable conflict that arises from the spread of civilization over a region previously inhabited by savages. I may be allowed to express hope that our labors in this direction will not be void of such useful results.<sup>6</sup>

Woodbury and Woodbury explained the relationship between BAE scholarship and the state quite succinctly: research from Powell and his students that painted indigenous peoples as uncivilized “demonstrated to Congress that something was being accomplished in return for its annual appropriations.”<sup>7</sup>

By understanding the BAE’s relationship to the state and to indigenous removal, one might conclude that BAE ethnographers were ‘handmaidens of colonialism’—in other words, that anthropological theory justified and shaped post-Civil War policies of Western expansion. This explanation would place too much weight on the influence of intellectuals like Powell, or his proteges W.J. McGee or Otis Tufton Mason. Indeed, before the BAE was founded, Congress

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<sup>6</sup> Powell, J.W. *Annual Report of the Bureau of Ethnology to the Secretary of the Smithsonian Institution*, (1881): XIV. <https://www.biodiversitylibrary.org/bibliography/38077>.

<sup>7</sup> Woodbury, Richard B., and Nathalie F. S. Woodbury. “The Rise and Fall of the Bureau of American Ethnology.” *Journal of the Southwest* 41, no. 3 (1999): 283–96. <http://www.jstor.org/stable/40170100>.

certainly didn't need the BAE or professional ethnographers to engage in policies of indigenous removal and displacement. On this point, it is sufficient to note that the Indian Removal Act was passed eight years prior to the act of Congress which made Powell head of the newly minted BAE. The BAE *emerged in the context of* Westward expansion, rather than serving as a prior ideological foundation for it. As scholars of empire and colonialism have argued about the relationship between scientific knowledge and empire, it is difficult to claim that knowledge production was a primary cause of imperial expansion. A more salient analysis will emphasize the ways in which imperial expansion shaped the sorts of knowledge claims that BAE ethnographers found compelling.<sup>8</sup>

Powell would remain an influential figure in American anthropology until his health faltered at the turn of the century. He had gained regard as a writer and thinker of anthropological theory on expeditions in the American Southwest, including the first US government-sponsored exploration of the Grand Canyon. Powell's expertise on the Southwest eventually got him the job of designing the reservation system for the Ute peoples in Utah and Nevada.<sup>9</sup> Powell came of age in the final generation where it was uncommon to become an intellectual without academic training. In the 1890s and the beginning of the 20<sup>th</sup> century, the rapid expansion of America's university system would produce the archetype of the professional social scientist. Powell did not hold a graduate degree, and conducted most of his research through government agencies, specifically the US Geological Survey and the BAE.

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<sup>8</sup> For example, see: Go, "Sociology's Imperial Unconscious", Vitalis, *White World Order, Black Power Politics*, Blatt, *Race and the Making of American Political Science*, Steinmetz, *The Colonial Origins of Modern Social Thought*

<sup>9</sup> Robert Vitalis, *White World Order, Black Power Politics: The Birth of American International Relations* (Ithaca: Cornell University Press, 2017), 46-47.

Intellectually, Powell's greatest inspiration was Lewis Henry Morgan's theory of cultural evolution. Morgan's impact on Powell was so great, that Morgan's book *Ancient Society* was required reading for all BAE employees. In *Ancient Society*, Morgan advocates for a unilinear path of development for human society that correlates to a universal trajectory of mental evolution.<sup>10</sup> The most primitive stage of Morgan's theory being savagery, followed by barbarism, and culminating in civilization. Civilization, as Boas would begin to point out in the 90s, was always defined as the civilization of the Western scientific observer. Morgan's stage-ist theory of culture did not systematically distinguish between biological race and cultural practice, instead relying on the Lamarckian assumption that behaviors and temperaments could be inherited biologically.<sup>11</sup> Before Boas and Du Bois, this muddling of biology, race, and culture was ubiquitous. Within a Lamarckian framework, both phenotypical traits, like brain size, and behavioral traits, like temperament, are linked to one's race and to one's biological lineage.

By developing scientific proof for popular understandings that indigenous Americans were backward, childlike, and immature, Powell's BAE also developed scientific logics for paternalist settler policies like boarding schools. As a function of the scientific paradigm with which they were trained and of their relation to the American state, it is no surprise that BAE ethnologists were likely to adopt an orientalist understanding of their relationship to other societies. Their very ability to create knowledge about indigenous peoples was premised on their highly evolved social position.

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<sup>10</sup> Lewis Henry Morgan, *Ancient Society: Researches in the Lines of Human Progress from Savagery through Barbarism to Civilization* (Whitefish, MT: Kessinger, 2008).

<sup>11</sup> George W. Stocking, *Race, Culture, and Evolution: Essays in the History of Anthropology* (Chicago: Univ. of Chicago Press, 1997), 116-117.

In March of 1886, Powell delivered the keynote address at a meeting of the Anthropological Society of Washington (ASW). Prior to 20<sup>th</sup> century, the ASW was the most prestigious professional association of anthropologists in the country, in particular because it held close affiliation to the Smithsonian Institution in Washington, DC. At this meeting, Powell made explicit his vision of BAE research based on an evolutionary theory of culture. Because all peoples followed the same developmental trajectory from savage to civilized, the civilized ethnologist was granted special knowledge. With knowledge of their own civilization, the BAE ethnologist meeting with the savage or barbarian person was, as Charles King put it, “conversing with those who had yet to travel the same pathway he had once trod.”<sup>12</sup> King and Stocking concur with the assessment written by Donald Worster in his biography of Powell: that the BAE largely lived and died by Powell’s vision of anthropology. That vision was scaffolded if not wholly given shape by Morgan’s unilinear view of mental, cultural, and racial evolution.<sup>13</sup>

Powell and his backers in Congress also wanted the BAE to organize American anthropology more broadly. While only BAE anthropologists were under his direct supervision, Powell’s ultimate goal was to make the BAE the defining institution of anthropological research throughout the country. In the first Annual Report that he authored as director, he wrote that: “it is the purpose of the Bureau of Ethnology to organize anthropologic research in America”.<sup>14</sup> Before the turn of the century, Powell would be mostly successful in this endeavor. In large part, the reason for this success was because there was a lack of competitors.

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<sup>12</sup> Charles King, *Gods of the Upper Air: How a Circle of Renegade Anthropologists Reinvented Race, Sex, and Gender in the Twentieth Century* (New York: Doubleday, 2020), 46.

<sup>13</sup> In addition to aforementioned sections from King and Stocking on Powell and the BAE, see: Donald Worster,

<sup>14</sup> John Wessley Powell, “Report of the Director” in *Annual Report of the Bureau of Ethnology* :xi-xxxiii (1879-1880, xxxiii. Quoted in Woodbury and Woodbury, “The Rise and Fall of the Bureau of American Ethnology”, 287.

With the Smithsonian Museum being run by BAE employees and no university departments of anthropology, no other group could match the funding and coordination of Powell's network of scholars. The BAE, and therefore Morgan's evolutionary social theory, became the conceptual crux of American anthropology until the turn of the century.<sup>15</sup> Powell as a conduit for Morgan's writings was the natural choice for telling Congress what it already knew about its new indigenous 'wards'. Thus, throughout the final two decades of the 19<sup>th</sup> century, a new doxa for American anthropological scholarship was formed, one that had to be recognized and addressed directly before an alternative could be proposed.

On this backdrop, Franz Boas can be introduced. In 1899, Boas became a professor of anthropology at Columbia, and in 1902 he was indispensable in the founding of the American Anthropological Association (AAA). Although the Anthropological Society of Washington predated the AAA, the AAA was the first national rather than regional body for unifying those with an interest in Anthropology. This same year, J.W. Powell died, and the internal crisis of leadership that befell the BAE shifted the center of anthropological gravity toward university departments.<sup>16</sup> In the short time between 1902 and the 1910s, Boas' view of culture would become the dominant view in universities across the country.<sup>17</sup> Boas endorsed what we might call today a 'reflexive' approach to research. He was especially concerned about the tendency for the ethnographer to assume their own culture to be the "norm" or "standard" to be lived up to. Perhaps just as famous as his theory of culture was Boas' use of anthropometric data

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<sup>15</sup> Bruce G. Trigger, *A History of Archaeological Thought* (Cambridge England: Cambridge University Press, 2023), 125.

<sup>16</sup> In particular, Powell's protégé W.J. McGee had expected to be named director. Since 1894 he had served as de facto dire

<sup>17</sup> Baker, *From Savage to Negro*, 100; see also Stocking, *Race, Culture, and Evolution* and McKee, *Sociology and the Race Problem*.

to reject the foundations of biological racism—such as the presumption that brain size, head shape, and intelligence were stable across racial groups. At the crossroads of the cultural and the biological, Boas rejected the Lamarckian idea that learned behaviors could be transmitted from parent to child. The end result of these conclusions was a very different definition of “culture” than Morgan’s framework allowed for. For Boas, culture was (1) separate from biological race, (2) existed in many distinct forms based on one’s geographic location, and (3) was impossible to arrange hierarchically.

The speed at which Boas changed dominant notions of race and culture within professional anthropology is noteworthy.<sup>18</sup> Raewyn Connell has argued that close field work was a key difference between Boas and some other social theorists of his day like Herbert Spencer.<sup>19</sup> Perhaps Boas was a talented data collector, but so too were many of the BAE’s ethnographers. Clearly, this alone could not be the reason that Boasian thought broke the mould of his predecessors. Indeed, Boas’ work only entered the mainstream after the BAE’s influence had declined; after Powell’s death in the first decade of the 20<sup>th</sup> century, Morgan’s stage-ist theory of culture and race was quickly dislodged as the cornerstone of American anthropology. This suggests that close field work may have been instrumental in allowing Boas to generate his social theory, but that it cannot explain why Boasian theory became popular. Broader structural conditions regarding the where knowledge was produced and how knowledge producers were trained had changed by 1902. It was in this new context where Boas’ theoretical moves finally became convincing to more and more anthropologists.

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<sup>18</sup> Baker, *From Savage to Negro*, 100.

<sup>19</sup> Raewyn Connell, “Why Is Classical Theory Classical?” (1997), *American Journal of Sociology*, 1525.

Regarding these new structural conditions, the historian of anthropology Regina Darnell wrote:

“By 1902 many of the major anthropological institutions of the twentieth century had been founded. Museums and universities were able to provide employment and research funds for professionals who were increasingly likely to have had formal training in anthropology. For the new generation of anthropologists, the Bureau [of American Ethnology] was one of a number of institutional alternatives and one which offered more constraints on individual research than a museum or university position. The Bureau did not change in response to these new alternatives and thus found itself in the twentieth century outside the mainstream of a professional American anthropology which it had helped to create.”<sup>20</sup>

It was only once alternative spaces to the BAE were opened up in the form of university department that anthropologists were able to pursue the full scope of Boas' theoretical prescriptions. But how exactly did Boas arrive at his theoretical divergence from Morgan and Powell? Here, an adequate description of Boas' writings on ethnographic methods will prove clarifying.

In 1887, Franz Boas would initiate one of his first major debates with Powell and the BAE. Drawing on his fieldwork in Baffin Island and the Pacific Northwest, Boas published a critique of Otis Tufton Mason, the ethnological curator of the Smithsonian National Museum and

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<sup>20</sup> Regina Darnell, “The Development of American Anthropology 1979-1920: From the Bureau of American Ethnology to Franz Boas” (Philadelphia: University of Pennsylvania), 139.



an adherent of the Powell-Morgan program.<sup>21</sup> The article was published in the newly established magazine *Science*, where Boas had recently taken a position as a research assistant. In it, Boas critiqued the arrangement of the Smithsonian's exhibits, which were grouped haphazardly by broad culture grades. Beyond classification as a "savage", "barbarian", or "civilized" artifact, Boas noted that all items were abstracted from their particular geographies and histories.

Having conducted fieldwork in two very different places, Boas thought that it made much more sense to group artifacts by the locations that they came from, rather than the Morganite culture grade they could be sorted in to. The problem, thought Boas, was that "By regarding a single implement outside of its surroundings, outside of other inventions of the people to whom it belongs, and outside of other phenomena affecting that people and its productions, we cannot understand its meaning."<sup>22</sup> Unfortunately for Boas, the argument was ended when Powell authored a response of his own, also published in *Science*, defending his colleague Mason. He dismissed Boas' proposal as unworkable, believing that if artifacts were presented according to the peoples they belonged to rather than broad culture-grades, then the current curatorial layout of major museums would be upended.<sup>23</sup> As a junior employee at a new publication, Boas could not have hoped to win the war with Powell then and there. For the time being, Powell had the last word.

Boas did not remain quite for long, however. In 1896, he published another searing critique, this time of mainstream anthropological methods of data analysis. Once more in *Science*, Boas published a critique of the comparative method of anthropology; this method was

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<sup>21</sup> Charles King, *Gods of the Upper Air*, 53.

<sup>22</sup> Franz Boas quoted in King, *Gods of the Upper Air*, 54.

<sup>23</sup> King, *Gods of the Upper Air*, 57.

popular amongst many of his peers, and was the method that Powell and Morgan had adopted for analyzing ethnographic data from the field. The article “The Limitations of the Comparative Method of Anthropology”, was published in the magazine’s fall edition. In it, Boas attacks the assumption that each and every human society develops along the same trajectory. The argument that he was attacking goes like this: if all human societies develop similarly due to “the uniform working of the human mind”, then one can compare currently existing primitive societies with civilized societies to get a picture of that line of evolution.<sup>24</sup> In practice, this often meant the comparison of white Western civilization with that of a flattened caricature of Africa or Asia. In the US, it also frequently meant comparing Black people unfavorably with their more ‘evolved’ white peers. Among its practitioners, Boas calls out by name E.B. Tylor, Herbert Spencer, Adolf Bastian, and others. In summing up its accomplishments, Boas contends that the comparative method “has been remarkably barren of definite results”.<sup>25</sup>

The way forward, he claimed, was in renouncing the failed attempt to construct one single theoretical model for how cultures evolve. This was a shot across the bow at many of the reports published by Powell and the BAE. The Boasian dictum that “all is individuality” in ethnography was fundamentally irreconcilable with a notion of “the uniform working of the human mind” that undergirded unidirectional models of cultural and racial evolution.

As noted previously, Boas would also defend methodological reflexivity as a prerequisite to engaging in anthropological theorizing. He cautioned against a tendency that he noticed in the work of his peers, where the researcher’s own civilization was held up as the standard to which

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<sup>24</sup> Franz Boas. “The Limitations of the Comparative Method of Anthropology.” *Science* 4, no. 103 (1896): 901–8. <http://www.jstor.org/stable/1623004>, 901.

<sup>25</sup> Boas, *The Limitations of the Comparative Method of Anthropology*, 908.

all others are held. He warns that: “It is but natural that, in the study of the history of culture, our own civilization should become the standard, that the achievements of other times and other races should be measured by our own achievements.”<sup>26</sup> Doing so leads to faulty conclusions based on a “grand system of the evolution of culture”, which he believed to be “losing much of its plausibility.”<sup>27</sup>

These early debates give a glimpse into why Boas came to different conclusions than Powell and the BAE. Boas’ quibbles were not necessarily with the artifacts and observations collected by his peers—by their data. In search of a social theory that more accurately described the empirical world around him, Boas instead outlined a new way of interpreting and presenting ethnographic data. In combination with his own data collection and the changing institutions of anthropological research, Boas’ innovations in data analysis paved the way for many American anthropologists to change the way that they conducted research.

These were the two dominant approaches to anthropological research in the 1890s. BAE ethnographers, led by Powell, adhered to the comparative method. As Connell argues, the comparative method lent itself well to researchers who assumed that they came to field work from a position of cultural superiority. For example, a white settler studying indigenous ways of life on a newly established reservation in Utah. If the researcher begins with the assumption that their own culture is ‘civilized’ and that the culture of others is ‘savage’, then they are led not just to making comparisons, but to a very particular form of comparative data analysis. Adopting the Boasian alternative—the historical model—quickly complicates this assumed notion of cultural

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<sup>26</sup> Franz Boas. "The History of Anthropology." in *Congress of Arts and Science: Universal Exposition, St. Louis, 1904*. Boston; New York: Houghton, Mifflin and Company, 473-474.

<sup>27</sup> Boas “The History of Anthropology”, 479.

hierarchy. If one assumes that the differences between cultures are explained by historical particularity, rather than successful advancement along an evolutionary spectrum, then a hierarchical assessment of different cultures, peoples, or races becomes nearly impossible.

It should be clarified here that comparison is not in and of itself imperial or orientalist. Boas conducted comparisons between cultures all the time, but he did so for the purpose of highlighting their individuality without assessing their value in comparison to one another. For example, in chapter 10 of *The Mind of Primitive Man* (1911) he uses comparison to demonstrate that indigenous Americans in the Pacific Northwest and Arizona cannot neatly be sorted into the same “savage” or “barbarian” bin.<sup>28</sup> Cultural practices between different groups did indeed vary, but this was not grounds upon which each group could be valued. Boas’ critique was instead levied at a particular assumption made when his peers were conducting their comparison—the assumption that cultures were all developed along a singular path and could be given value and meaning in a similar hierarchical manner.

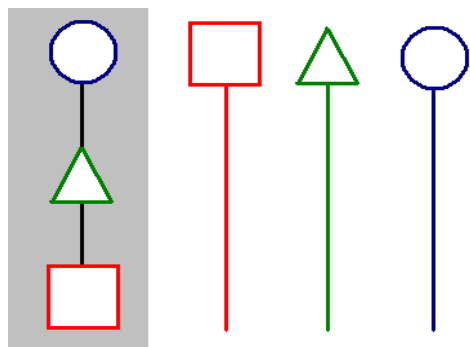
Boas’ primary methodological innovations were thus not about the importance of close fieldwork as Connell seems to argue. Both Boasian sociocultural anthropologists and BAE ethnographers were responsible for physically going out into the world, observing real people, and taking notes. Instead, the moves that Boas made to differentiate himself were at a prior epistemological level. While he may have not used these terms, Boas advocated for anthropological researchers to provincialize their findings and to reflexively examine their own social location. The historical model of anthropological research requires the researcher to think long and hard about *when* and *where* their findings are relevant, rather than assuming all

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<sup>28</sup> Franz Boas, *The Mind of Primitive Man* (London: Forgotten Books, 2015), 175-185.

observations to be evidence of a universal whole. Such an argument is almost indistinguishable from some postcolonial theorists' calls to 'provincialize' European social theory.<sup>29</sup>

Recounting Boas' story does not merely echo current calls to action, however. It also brings something new to the table. The particular order of events in the Boasian turn in anthropology raises very important questions for how and when reflexivity and provincialization can be helpful. Boas had developed his peculiar approach to ethnography long before his ideas became popular. In his initial spar with Powell and Mason, he was on the fringes of the discipline. Against the most important anthropologist in the country, it did not matter if his ideas were intrinsically "more scientific" or "unbiased". The relative fame of each thinker made it unlikely that Boas would be able to bring his view of culture into the mainstream. It was only with his hiring at Columbia, his training of graduate students, the revival of the AAA, and the decline of the BAE that Boasian notions of culture became widespread. This sequence of events suggests a rather simple but nonetheless crucial conclusion: although any individual scholar can generate a heterodox theory, the existing political economy of knowledge production must have an available space for that heterodox theory to flourish. Otherwise, no matter how powerful its observations are, there is no guarantee of the theory's popularity.



This diagram is a helpful illustration of the difference between Boas' historical method and the comparative method. If the square, triangle, and circle are taken to be three different societies, the historical ethnographer would view them all on unique trajectories (right) while the

<sup>29</sup> See, in particular, Dipesh Chakrabarty, *Provincializing Europe: Postcolonial Thought and Historical Difference* (Princeton: Princeton University Press, 2008).

comparative ethnographer would assume them all to be at different stages on the same evolutionary path (left).

## Section 2: Sociology and the Congress

The bulk of this section is an analysis of papers presented by leading American sociologists—Lester Ward, Franklin Giddings, Edward A. Ross, and W.I. Thomas—at the International Congress of Arts and Sciences in 1904. The goal is to sketch out a broader sociological imagination at play in the halls of the earliest sociology departments. This raises two questions: firstly, why are these authors worth analyzing, and secondly, why is the Congress the proper setting to analyze their writings? With regard to the first question, it is true that the sociological imagination of these thinkers—which revolved around the writing of Herbert Spencer—was not the *only* sociological imagination at play in the Progressive Era. Social evolutionary thinking was popular, but it was not without alternative. Section three of this paper will introduce the work of W.E.B. Du Bois as one such alternative vision of the social.<sup>30</sup>

However, despite the existence of alternatives, it is undeniable that the work of Spencer had immense influence over early American sociology.<sup>31</sup> The four sociologists analyzed in this section each accepted certain premises of Spencerian sociology, even though each of them had distinct sociological projects of their own. They also each served as president of the American Sociological Society—Ward being the inaugural one—were widely published in the *American Journal of Sociology* and left their marks on top university departments. Given this, the debates

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<sup>30</sup> Young and Deskins in their article “Early Traditions of African-American Sociological Thought” provide a succinct but rich overview of the key differences between Du Boisian sociology and that of his peers in Cooper and Wells.

<sup>31</sup> Daniel Breslau, “The American Spencerians” in *Sociology in America: A History* (2007), 46.

in which they were engaged, the presuppositions that they adopted, and the conclusions that they drew from their scholarship cannot be ignored.

But why is the Congress the proper place to analyze their writings? For one, it is an incredibly convenient place to do so. The event attracted these four thinkers, but it also attracted two other thinkers analyzed in this paper, Franz Boas and W.E.B. Du Bois. Although Du Bois did not present at the Congress, he did write a direct response to the papers that will be analyzed here. The presence of these three parties provides the opportunity for two synchronic comparisons in 1904. First is a comparison between their work and that of Franz Boas. This comparison highlights the differences between anthropological and sociological theory that existed at this time. Boas had achieved a prominent place within American anthropology while his anti-hierarchical theories of race and culture were known. No similar thinker can be identified amongst the mainstream American sociologists. Given this fact, it is reasonable to conclude that something about sociological knowledge production was more amenable to evolutionary theory than was the case in anthropology. The second comparison is between the American Spencerians and Du Bois, which will be visited in the third and final section of this paper.

In addition, the Congress, which was held in conjunction with the St. Louis World's Fair of 1904 invited scholars to present not on specific findings from their own work, but on two broad themes: (1) the scholarly view of mankind's progress in the previous century and (2) recent developments in scientific thinking.<sup>32</sup> In his introductory address, Officer of the Congress Simon Newcomb said that the organizing committee had aimed "at nothing less than a survey of

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<sup>32</sup> A.W. Coats, "American Scholarship Comes of Age: The Louisiana Purchase Exposition 1904" (1961), 404.

the realm of knowledge, as comprehensive as is permitted by the limitations of time and space.”<sup>33</sup>

Before diving into a comparison between the sociologists and Boas, it is worth pointing out a crucial difference between the sociologists and that of the BAE. While both groups adhered to an evolutionary social theory, sociologists’ belief in an evolutionary hierarchy of cultures came about for different reasons. While it is true that many sociologists were boosters of the imperial cause overseas, their relationship to empire was far less direct than BAE ethnographers.<sup>34</sup> While the BAE received funding from Congress that was contingent on its investigation of indigenous peoples, sociologists in university departments had no such incentive. In an era before the rise of philanthropic foundations, university research—particularly at private universities—enjoyed relative autonomy of topic selection and theoretical inspiration.<sup>35</sup>

Despite this autonomy, the movers and shakers of early American sociology held on to the Lamarckian thesis that adapted traits could be inherited biologically.<sup>36</sup> Lamarckian biology offered the final obstacle to disentangling culture from biology, and subsequently separating racial hierarchy from biology. One example of Lamarckian thinking in sociological theory can be found in Frankling Giddings’ *Principles of Sociology* (1896). He writes about human diversity:

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<sup>33</sup> Simon Newcomb, “The Evolution of the Scientific Investigator” (1904), 135.

<sup>34</sup> For example, Franklin Giddings published the headline article in a 1898 issue of *Political Science Quarterly* titled: “Imperialism?”. In it he makes the case for the necessity of American occupation of the formerly Spanish territories. Giddings published this in his capacity as a social scientist and private citizen, not as a representative of a government body.

<sup>35</sup> While the Rockefeller and Carnegie foundations exercised a great degree of control over university funding for most of the 20<sup>th</sup> century, this phenomenon increased dramatically after WWI see works by David Nugent: “Orientalism in war and peace: the politics of academic scholarship during the long twentieth century” (2021) and “Knowledge and Empire: The Social Sciences and United States Imperial Expansion” (2010).

<sup>36</sup> Stocking, *Race, Culture and Evolution*, Chapter 10: “Lamarckianism in American Social Science”.



“From among the thousands of variations that must have been produced by the combined action of association, crossing, mental activity, and environment, natural selection very early picked out certain characteristics that were to become permanent elements in race differentia.”<sup>37</sup>

For Lamarckians, it was not only physical traits, but also mental activity like learned behaviors which could be inherited, and thus variable according to one’s race or biological ‘stock’. This belief allowed sociologists to engage in a “shuttling” between racial and cultural causes of inferiority.<sup>38</sup> Subsequently, this shuttling allowed sociologists to explain racial hierarchy in a way that was buttressed by their mimicry of natural scientific language. In the process of establishing the new social scientific disciplines as sufficiently ‘scientific’, appeals to biological evidence of social phenomena were a powerful tool that lent objectivity to many studies.<sup>39</sup>

While the Boasian paradigm would not become the dominant anthropological approach to culture until the 1910s, Boas had still carved out a space for his project in the mainstream of his discipline by the Congress in 1904. For one thing, he had helped to reformulate the American Anthropological Association in 1902 and was one of its vice-presidents; his heterodox views on cultural relativity and racial equality were known, and indeed the basis of his scholarly fame. If these views were wholly intolerable to other anthropologists, Boas simply would not have been able to hold positions of leadership. Even as early as 1895, E.B. Tylor, one of the most famous theorists of the evolutionary model of culture had acknowledged that Boas’ critiques of the

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<sup>37</sup> Giddings, *The Principles of Sociology*, 230.

<sup>38</sup> George Stocking, *Race, Culture and Evolution*, pg. 260; Here, Stocking is quoting Boas’ student Alfred Kroeber, who used the term shuttling to explain the oscillation between factors that today we would call “cultural” and “biological”, but that were interchangeable at the turn of the 20<sup>th</sup> century.

<sup>39</sup> Breslau, “The American Spencerians”, 50

comparative method had contributed to “a most necessary reformation” in anthropological theory.<sup>40</sup> Tylor, of course, retained his evolutionary views in spite of these critiques, but he viewed the Boasian paradigm of culture as a worthy intellectual opponent; in sum, Boas’ views were not reconcilable with those of his peers, but they understood each other’s arguments nonetheless. The mainstream sociological community did not produce its own Boas. Spencerian social theory was the air that they breathed, and no major contender existed within the *AJS* or the *ASS*. Even as Boas’ views became a known alternative to the evolutionary view of culture in anthropology, mainstream sociologists produced no such alternative framework for understanding culture and race.

The rest of this section first summarizes the state of Boas’ thought at the Congress, and then introduces the four sociologists in turn. When necessary, I provide biographical background and references to additional works by each thinker, in order to place the year 1904 into a broader intellectual context.

Boas presented a paper titled “The History of Anthropology” at the Congress. While he claims to be presenting a neutral account of his discipline’s trajectory, this paper is actually a rather straightforward critique of the stagiest model of human development. For Boas, the argument for the evolutionary model had “been made from both the cultural and the biological point of view” but he believed that neither held water.<sup>41</sup> In a similar vein, Talcott Parsons declared that Herbert Spencer—and subsequently the evolutionary model of sociology—was dead in 1937 in the first edition of *The Structure of Social Action*.<sup>42</sup> Parsons made this argument

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<sup>40</sup> E.B. Tylor quoted in Stocking, *Race, Culture, and Evolution*, 211.

<sup>41</sup> Boas, “The History of Anthropology”, 474.

<sup>42</sup> Talcott Parsons, *The Structure of Social Action* (New York: Free Press, 1968), 1.

about sociology's intellectual history one of the foundations of his discipline-building program in the 30s and 40s. Thirty three years prior to Parson's declaration, Boas made a similar declaration about anthropology. Presenting at the Congress, Boas said:

“the grand system of the evolution of culture, that is valid for all of humanity, is losing much of its plausibility. In place of a simple line of evolution there appears a multiplicity of converging and diverging lines which it is difficult to bring under one system. Instead of uniformity, the striking feature seems to be diversity.”<sup>43</sup>

Boas did not cite Powell or Morgan by name here, but their theories were certainly the ones on the chopping block.

Keeping in mind Boas' bold claims about the death of evolutionary anthropology, let us now examine the state of the sociological field. Franklin Giddings presented at the Congress and was one of the era's most explicitly evolutionary thinkers. Giddings was inspired by what Dorothy Ross called the ‘positivist Darwinism’ of the British mathematician Karl Pearson.<sup>44</sup> At the Congress, he presented a paper entitled “The Concepts and Methods of Sociology”. Much to Du Bois' chagrin, Giddings advocated a vision of sociological inquiry that started with abstractions rather than observations. An account of what is most central to sociology must “remorselessly exclude those concrete particulars” and instead “be restricted to conceptions that are elemental, general, and in a degree abstract.”<sup>45</sup> He considered the three essential abstractions of sociology to be (1) a definition of society, (2) the different classifications of social facts, and

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<sup>43</sup> Boas, “The History of Anthropology”, 479.

<sup>44</sup> Dorothy Ross, *Origins of American Social Science*, pg. 227-229; for a more in-depth view on Giddings' program at Columbia, see also and the book *Sociology and Scientism* by Robert C. Bannister.

<sup>45</sup> Giddings, “The Concepts and Methods of Sociology”, 787.

(3) the processes of social evolution. While Giddings' views on the separation between biology and culture are not explicitly stated, his descriptions of classification and social evolution make it clear that he still held on to a linear notion of cultural development.

The first social fact to be determined for Giddings was “types or kinds of societies”.<sup>46</sup> The fundamental divisions being made based on the balance of instinct and reason that exist in associations of people. The list is arranged hierarchically, with a grouping of blood-relatives being the least sophisticated and the most sophisticated being that of an “Idealistic” society. Lower grades of societal development, especially “tribal organization characteristic of savage and barbarian life” have to be “outgrown” before reaching modernity.<sup>47</sup> Giddings describes a ‘unilinear’ account of civilizational development that is at home amongst both Spencerian and Morganite theories. The language of savage and barbarian were commonly understood in the terms put forth by Morgan and Tylor.

On the third abstraction of social evolution, Giddings writes: “We accept the evolutionist point of view, and regard all the transformations that occur within any social group as a phase of that ceaseless equilibration of energy taking place throughout the universe.”<sup>48</sup> The mechanisms through which equilibration occurs on society are migration, the formation of natural inequalities in society due to aptitude, and the relationship of society to its ecological surroundings. His vision of the most evolved society is decidedly liberal—“The goal of social evolution is a complex, flexible, liberal organization, permitting the utmost liberty and mobility to the individual, without impairing efficiency of organization as a whole”.<sup>49</sup> This is contraposed with

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<sup>46</sup> Giddings, “The Concepts and Methods of Sociology”, 791.

<sup>47</sup> Giddings, “The Concepts and Methods of Sociology”, 795.

<sup>48</sup> Giddings, “The Concepts and Methods of Sociology”, 796.

<sup>49</sup> Giddings, “The Concepts and Methods of Sociology”, 797.

‘low types of society’ in which the individual belongs to their social organization for life. The language of higher and lower civilizations, the mechanism of evolution, and the assumed endpoint of society being a liberal individualist democracy all make it obvious that Giddings’ work repudiates the basic Boasian premises and embraces the stagiest model of human evolution.

The problem with Giddings’ theory is not the mere belief that some societies are modern, and others are not; the ontological status of ‘modernity’ is a topic for a different paper. At a more fundamental level, Giddings’ social theory was hierarchical because it was predicated on the notion that his own society was the Platonic ideal of such modernity. This supposedly scientific view came with a moralizing sentiment baked into it—that other societies must become like his own if they are to ‘progress’, and that his society must not become like others if it wants to remain modern. In his famous defense of US expansion, an article titled “Imperialism?”, Giddings made these views explicit. He argued that the inevitable “experiment” of imperial management would broaden both “opportunities and responsibilities” for the American people.<sup>50</sup> Adding later on that it is the paternal duty of the American people to evolve the democratic politics of the Philippines before the autocrats of Russia or China could do the same. The end and the beginning of the social theory is that societies—or cultures, races, civilizations—can be distinguished from one another based on observable scientific criteria, and that based on these criteria, they can be evaluated as normatively preferable or distasteful.

A second future ASS president, W.I. Thomas, presented on “The Province of Social Psychology” at the Congress. Thomas is perhaps best known for the study co-authored with Florian Znaniecki *The Polish Peasant in Europe and America* (1917). He believed that one of the

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<sup>50</sup> Franklin Giddings, “Imperialism?” (1898), 603.

primary objectives of the subfield of social psychology was to determine to what degree differences in temperament, character, and genius were due to biology and to what degree they were due to the cultural practices of different races: “what have sometimes been regarded as biological differences separating social groups are not really so, and that characteristic expressions of mind are dependent on social environment.”<sup>51</sup> For Thomas, the sources of cultural variation were rather up for debate. Did racial groups behave and believe differently because of their innate biological qualities? Or did they do so because of how they were socialized. While Thomas and Giddings both endorsed what Giddings called the “psychological view” of society over the “organic view”, Giddings’ research project looked for the essential qualities of each race which led to differences in psychological traits.<sup>52</sup> Thomas, on the other hand, was still wrestling with the question of where psychological traits came from. Giddings’ preference for examining natural ability also leads to a more distinctly evolutionary view of how different peoples can change. For, example, in determining what state policies were necessary to hasten social development.

The difference between Thomas’ and Giddings’ views on the sources of behavior in different racial groups opens up the opportunity to talk about a major difference between sociology and anthropology that was apparent by 1904. More specifically, it is worth noting that the research process of Thomas was very different from that of Ward, Ross, or Giddings in 1904. One of the reasons that Thomas’ *The Polish Peasant in Europe and America* is read more often than Giddings’ *The Principles of Sociology* is the amount of empirical data contained in each. As Martin Bulmer observed in his history of the Chicago School of sociology, Thomas and

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<sup>51</sup> W.I. Thomas, “Province of Social Psychology”, 865.

<sup>52</sup> Giddings, “The Concepts and Methods of Sociology”, 788-791.

Znaniecki's book marked a "shift in sociology away from abstract theory and library research toward a more intimate acquaintance with the empirical world"<sup>53</sup>. Bulmer's study seems to ignore the work of both Du Bois and other Black sociologists, as well as researchers who were involved in research at Hull House like Jane Addams. Nevertheless, Bulmer's point rings true for these four theorists—at a point in time where most sociological research took place in libraries and at the theoretical level, Thomas left his office and collected his own data. Thomas' novel experience of data collection—the same process that was a necessary condition for Boas used to draw his own conclusions about race and culture—is hard to dismiss as insignificant. While Thomas still maintained a vaguely naturalist and evolutionary theory of society, it was a noticeably softer version than Giddings' more hardline view of racial aptitude.

Lester Ward, who would become the first president of the American Sociological Society, presented a paper titled "Evolution of Social Structures". Lester Ward's work in the first decade of the new century is indicative of an attempt to demarcate a *social* rather than biologically evolutionary view. In his reading of Ward's 1906 book *Applied Sociology*, James McKee observes that Ward retained the evolutionary view of society but did not justify this argument on biological grounds.<sup>54</sup> In *Applied Sociology*, Ward had taken up the argument made by biological anthropologists: that there was no proof that races were intellectually unequal. For Ward, the upshot of intellectual equality was that any person regardless of race or class could be assimilated into more advanced cultures. However, as Breslau notes, Ward was still an American Spencerian at heart. He did not believe in biological inferiority of Black Americans, but he

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<sup>53</sup> Martin Bulmer, *The Chicago School of Sociology: Institutionalization, Diversity, and the Rise of Sociological Research* (1986), 45.

<sup>54</sup> McKee, *Sociology and the Race Problem*, 29; See also Ward, *Applied Sociology: A Treatise on the Conscious Improvement of Society*, 110.

tacitly endorsed the Lamarckian thesis that one's temperament, behavior, and culture could be inherited through blood. The upshot of this argument is that different races—who were the central movers of Ward's theory—could be analyzed in terms of their behavioral and cultural value. Indeed, Ward still saw the social world in terms of 'higher' and 'lower' peoples.<sup>55</sup>

At the Congress, Ward's paper "Evolution of Social Structures" shows similar thinking. He tracks the evolution of society from barbarian tribes to modern states, arguing that this occurs in large part due to a series of "race wars". The first step in development of complex society is "the conquest of one race by another."<sup>56</sup> Conflict between racial groups continues, leading to escalating stages of war and hostility. This conflict produces subjugation, caste distinctions, and the institution of slavery. At this point people are taught to labor and industry takes off, the rationalization of legal apparatuses begins, and states form. All of this occurs with a socially dominant 'higher race' at the helm. Eventually, however, Ward believes that the higher races will have to cede ground to their subordinated counterparts— "As time goes on, the situation is accepted by all, and race-prejudices give way."<sup>57</sup> Assimilation eventually occurs first institutionally and then biologically through intermarriage. This assimilation is only possible because racial difference arises not from natural biological difference, but by the long arc of social evolution. Ward thought that some races were 'higher' or 'lower' than others, but that this difference in culture was caused by previous historical developments in conquest, politics, and production, rather than biological predispositions. Paradoxically, however, Ward's Lamarckian

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<sup>55</sup> Breslau, "The American Spencerians", 39.

<sup>56</sup> Ward, "Evolution of Social Structures", 846.

<sup>57</sup> Ward, "Evolution of Social Structures", 849.



presuppositions did mean that difference in culture was tied to biology and race, even if it was not *caused* by it.

Edward A. Ross was yet another presenter in the sociology section of the Congress who would become president of the ASS. Ross was one of the period's most active scholars on race, race relations, and eugenics. In 1901, at the annual meeting of the American Academy of Political and Social Science, Ross gave a keynote address entitled "The Causes of Race Superiority". He starts by arguing that races aren't necessarily different due to biological factors. Ross instead privileges "condition" and "surroundings" as the reasons that some races have developed physical, mental, and cultural superiority. He takes Darwinian thought on directly, which he thinks "exaggerates the race factor and regards the actual differences of peoples as hereditary and fixed." The causes of race superiority that he outlined in his 1901 address are a vague intermixture of physiological and psychological ones. He notes that "climactic adaptability" varies between, for example, the Spanish and the French.<sup>58</sup> The former being more resistant to heat and tropical diseases than the latter. Alongside this physiological factor, he includes temperamental ones like "energy", "self-reliance", and "foresight". This becomes clearer when Ross discusses attitudinal differences—for example that the Arab race is likely to ruminate on the past while the Jewish race is likely to look to the future opportunistically.

Ross' view of both natural capabilities—like supposed resistance to heat and disease—alongside what we would today call 'cultural' differences—like attitudes on the past or future—were both anchored to a notion of heredity and blood. As Robert Vitalis points out, the conclusions that Ross had drawn from this theory of racial superiority were used to justify a

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<sup>58</sup> Vitalis, *White World Order Black Power Politics*, 69-75.

eugenic program.<sup>59</sup> Ross was especially concerned with restricting immigration of races he saw as inferior. Especially concerned with immigration from Asia, he believed that letting in too many non-Anglo-American people would lead to “race suicide”, or the full dilution of the gene pool that had made America such a vigorous, inventive, and progressive nation. In his “Causes of Race Superiority” speech from 1901, he argued that Americans in the South had already felt the early effects of race suicide; the cause was the introduction of Black slaves to the gene pool.

At the Congress, Ross would present “The Present Problems of Social Psychology”. By 1904, he thought the factors that made up national characteristics were up for debate: “One of our first tasks is to settle whether national characteristics should be dealt with by social psychology or handed over to ethnology. This depends on whether differences in national traits are due primarily to race-endowment or to situation and history.”<sup>60</sup> As discussed above, it is unclear if Ross himself distinguished between phenotypical and psychological factors when talking about “race-endowment”. What is clear, however, is that he had a firm notion of cultural hierarchy in mind. As Roscoe Hinkle observed of Ross’ book *Foundations of Sociology* (1905), Ross did not believe in a stage-ist view of civilization.<sup>61</sup> It is worth noting, however, that the nod to ‘situation and history’ infers that Ross may have been familiar with the work of Franz Boas. As discussed in the previous section, situation and history were the distinguishing factors of Boas’ approach to analyzing data from his field work.

While Giddings and Ward retained something similar to Morgan’s notion of a unilinear development of society, Ross actually sided more with Thomas by believing in the existence of

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<sup>59</sup> Vitalis, *White World Order Black Power Politics*, 30-32.

<sup>60</sup> Ross, “The Present Problems of Social Psychology”, 879.

<sup>61</sup> Roscoe C. Hinkle, *Founding Theory of American Sociology, 1881-1915* (London: Routledge, 2016), 219.

many paths of societal development. The primary difference in their thinking was that Thomas would work himself out of the premise of biological hierarchy earlier on, while Ross would become a leading voice in the American eugenics movement.<sup>62</sup>

The presenters at the Congress can be seen as representatives of a larger trend in sociology. They all went on to serve as president of the ASS. Giddings' leadership at Columbia, Ward's role in the early ASS, Thomas' long—albeit controversial—career, and Ross' writings both in public and academic forums influenced early sociology in a profound way. Their agreements and disagreements with Spencer, as well as their visions for sociology would be institutionalized in a way that other visions would not.

So far, this paper has analyzed sociology and anthropology's differing definitions of race and culture. Boas' success in popularizing an anti-hierarchical model of anthropology in the first decade of the 20<sup>th</sup> century raises the question of why sociologists were dragging their feet. In particular, when some arguments against genetic or biological hierarchy were beginning to take hold why did mainstream sociologists still find Spencerian evolutionary social theory so compelling? This all occurred even without some sort of top-down financial or political incentive for sociologists to adhere to a hierarchical vision of culture and race. In the following section, this question will be explored through the methodological writings of W.E.B. Du Bois. Although Du Bois was merely an individual observer, and thus it is difficult to make general conclusions based on his thought, the methodological and theoretical contrasts between Du Boisian and

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<sup>62</sup> For more on the relationship between social scientists and the eugenics movement, see: Thomas Leonard, *Illiberal Reformers: Race, Eugenics, and American Economics in the Progressive Era*.

Spencerian sociology provide a few possible causes for sociology's hesitation in adopting an anti-evolutionary paradigm for researching race and culture.

### Section 3: Du Bois and the American Spencerians

With the work of Giddings, Ward, Thomas, and Ross as a backdrop, Du Bois should now be introduced fully. Much recent scholarship has noted both the empirical and methodological innovations that Du Bois brought to early American sociology. The most well-known among these works is likely Aldon Morris' book *The Scholar Denied: W.E.B. Du Bois and the Birth of Modern Sociology* (2015).<sup>63</sup> Other work, particularly by postcolonial sociologists, has turned to Du Bois' writing as a useful counterweight to classical theory which they believe to be orientalist or imperial.<sup>64</sup> One example of Du Bois' generative theoretical reversals is contained in one of his most memorable passages, in which he flips the 'Negro Problem' of post-bellum America on its head; instead of asking how Black culture, biology, or circumstance create a problem for White American society, he asks: "How does it feel to be a problem?"<sup>65</sup> Starting from this standpoint allows Du Bois to generate a theory that would have been unavailable to a thinker like Giddings, who always approached Blackness as a problem to be investigated from the outside by the disembodied rational observer. This passage underscores the novel orientation that Du Bois took toward knowledge production. As opposed to the American Spencerians who saw themselves as objective observers, Du Bois came to scientific study from a place of political action and a

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<sup>63</sup> Aldon Morris' book, *The Scholar Denied: W.E.B. Du Bois and the Birth of Modern Sociology* (2015) attempts to argue that Du Bois was a (or the) founder of sociology in the United States. Another example is Jose Itzigsohn and Karida Brown's book *The Sociology of W.E.B. Du Bois: Racialized Modernity and the Global Color Line* (2020), which attempts to provide a primer for early-career scholars and undergraduates on Du Boisian thought. Brown and Itzigsohn also place a special emphasis on synthesizing and articulating what a 'DuBoisian' sociology looks like.

<sup>64</sup> One such example is *Postcolonial Thought and Social Theory* by Julian Go (2016), which is also where I draw the example of the 'Negro Problem' from analyzed later in this paragraph.

<sup>65</sup> W.E.B. Du Bois, *The Souls of Black Folk* (Oxford: Oxford University Press, 2008), 7.

distaste for widespread falsehoods that were spread about the Black community. This approach to science will be investigated in further detail later on in this section.

In addition to his underlying theory of science, Du Bois also differentiated himself from the American Spencerians in terms of the relationship between empirical and theoretical realities. Along with Boas, Connell contrasts Du Bois' methods with the grand ethnography of the classical theorists.<sup>66</sup> For Connell, one of Du Bois' main contributions to sociological research was a more "modern" approach to ethnography, that entailed "detailed description of a whole way of life" rather than sweeping generalizations of complex cultures for the sake of comparison.<sup>67</sup> The *Philadelphia Negro* is compared with a paradigmatic example of grand ethnography: William Graham Sumner's *Folkways* (1906). In *Folkways*, Sumner attempts to put together "all that we have learned from anthropology and ethnography about primitive men and primitive society". The result are single paragraphs in which Sumner jumps from Madagascar to the Arctic Circle and society to society, drawing on field notes from explorers and missionaries. Connell summarized the level of empirical detail that one can achieve using Sumner's 'grand ethnography' best: "Few cases delayed the author for more than a few sentences."<sup>68</sup>

The central Du Boisian concepts of the veil, two-ness, and the global color line were developed *through* empirical studies of race in *The Philadelphia Negro* (1899), the reports of the Atlanta School, *The Souls of Black Folk* (1903), and more. In contrast to much grand ethnographic writing like Sumner's *Folkways*, which plugged a large number of birds-eye view observations into a broadly Spencerian framework, Du Bois generated new theory out of data

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<sup>66</sup> Connell, "Why is Classical Theory Classical?", 1525.

<sup>67</sup> Connell, "Why is Classical Theory Classical?", 1524.

<sup>68</sup> Connell, "Why is Classical Theory Classical?", 1525.

collected from close field work. The fact that Du Bois' conceptual tools arose *from* empirical study, rather than theoretical tracts from Spencer, perhaps explains why we continue to find Du Bois' concepts useful today, but Talcott Parsons was convincingly able to declare the death of Spencer in the mid-30s. Du Bois' seminal contribution was to take a concept that was constantly used with imprecision by other sociologists—race—and to approach it systematically, beginning with evidence from the empirical world. The remainder of this section will look for the methodological and theoretical moves that allowed Du Bois to formulate a heterodox program of research, and in particular, how he differentiated this project from the ASS camp of sociologists.

Although he was not invited to present, W.E.B. Du Bois attended the Congress as an audience member; his scholarship represented an alternative to evolutionary thinking from within sociology. He wrote in dialogue with the same thinkers—notably critiquing Spencer and Comte—as the sociologists that were discussed in the last section. Yet, he did so from the other side of the 'veil'. In *Dusk of Dawn* (1940), Du Bois reminisces about his graduate study at Harvard and in Germany, where he noticed a peculiar shift in scientific explanations of racial difference. In the face of new biological evidence disproving biological hierarchy—much of which was being collected by Franz Boas—Du Bois observed that “race became a matter of culture and cultural history”.<sup>69</sup> His peers who resided on the white side of the veil held onto an underlying assumption of Black inferiority, even as the old evidence—brain weight, the cephalic index—failed to line up with empirical reality. To Du Bois, this shift from biology to culture taught a profound lesson. Regardless of their claims to objectivity regarding empirical reality,

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<sup>69</sup> W.E.B. Du Bois, *Dusk of Dawn: An Essay Toward an Autobiography of a Race Concept* (New Brunswick, NJ: Transaction Publishers, 1997), 49-50.

scientists were still susceptible to holding theoretical beliefs that didn't align with the data they collected.

It is symbolically fitting that Du Bois was allowed into the room to spectate at the Congress, but not allowed to present. This is because his work represented what Aldon Morris called the path not taken in early American sociology.<sup>70</sup> Du Bois' program was a potential approach to sociological investigation that flourished on the margins but was rejected by mainstream scholars in the ASS. Starting in the 1890s and into the first decade of the 20<sup>th</sup> century, much like how Boas critiqued Powell and the BAE, Du Bois began publishing critiques of the theory and method of his fields' biggest names. The two scholars both made the argument that common conceptions of race in their respective fields were confused, and that the way out of that confusion was through a reorientation of the relationship between empirical data and theory.

One crucial difference between the BAE and mainstream sociologists, however, was that the BAE adopted an evolutionary framework as a matter of a unified institutional mission. As discussed in section one, their charge to investigate the causes of difference between white civilization and the indigenous civilizations of North America was backed by federal funding. As Powell put it, the BAE was to study the races that the United States had already defeated.<sup>71</sup>

In contrast to anthropology's *institutional* alignment under the BAE, mainstream sociology was instead characterized by alignment in modes of *thinking*—Comtean and Spencerian. Moreover, the sociologists of the ASS did not abide by a clearly defined *goal* like

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<sup>70</sup> Aldon Morris, "Sociology of Race and W.E.B. Du Bois: The Path Not Taken," essay, in *Sociology in America: A History* (Chicago, Illinois: University of Chicago Press, 2007). 503; Zine Magubane makes a similar argument regarding Du Bois' focus on 'the deeds of men' in "Following 'the Deeds of Men,'" in *Global Historical Sociology*, ed. George Lawson and Julian Go (Cambridge, United Kingdom: Cambridge University Press, 2017), 122.

<sup>71</sup> Vitalis, *White World Order, Black Power Politics*, 46.

BAE ethnographers did. Even after the founding of the ASS in 1905, sociology's main institution for professional collaboration, its members belonged to different university departments. In the *absence* of an obvious political, financial, or otherwise "non-scientific" cause for sociology's adherence to evolutionary theory, we must instead turn to its very practices of knowledge production; I will do so using some of Du Bois' writing on theory and method.

Du Bois' unpublished memo "Sociology Hesitant" (1905) is a critique of the Comtean and Spencerian streams of scholarship that he witnessed at the Congress. Although they are not named directly, it is likely he was referring to the leading men of the American Sociological Society discussed in this paper: W.I. Thomas, Franklin Giddings, E.A. Ross, and Lester Ward, among others. In particular, Du Bois was upset about sociology's confused relationship between empirical observation and theory. He writes: "Comte and his followers noted the grouping of men, the changing of government, the agreement in thought and then instead of a minute study of men grouping, changing and thinking proposed to study the Group, the Change, and the Thought and call this created Thing, Society."<sup>72</sup> The reason that sociology was 'hesitating', believed Du Bois, was because his peers held *a priori* definitions of concepts like "society" without questioning them empirically.

Moreover, he argued that the very data his peers relied upon was incapable of disproving these *a priori* abstractions. Their work was: "Limited because their data were imperfect-woefully imperfect: depending on hearsay, rumor and tradition, vague speculations, traveller's tales, legends and imperfect documents, the memory of memories and historic error."<sup>73</sup> Even if

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<sup>72</sup> Du Bois, *W.E.B., 1868-1963. "Sociology Hesitant", 1905. W. E. B. Du Bois Papers (MS 312). Special Collections and University Archives, University of Massachusetts Amherst Libraries, <http://credo.library.umass.edu/view/full/mums312-b212-i003>, 2.*

<sup>73</sup> Du Bois, "Sociology Hesitant", 4.



sociologists held a proclivity toward empathetic treatment of their racial or cultural Others, the data they were working with was unlikely to bring them scientific proof for their empathy.

It is worth posing the counterfactual in the context of anthropology. If an anthropologist in 1904 believed that a hierarchical ordering of cultures and races was questionable, they were likely to take Boas' theory into the field to test it. In the process of collecting new data, they may have verified that their own idiosyncratic views on hierarchy were correct. In other words, ethnographic methods offered an easier way for a researcher to *incidentally* stumble upon a new way of thinking. On the other hand, if one remained a "car window" scientist, observing societies from a distance and incorporating "traveller's tales" and "legends" into their comparisons, there is no chance for them to confirm heterodox theories or beliefs. Amongst sociologists, car window methods may have been one reason that Spencerian theory attained its surprising tenacity.<sup>74</sup>

Du Bois' observation certainly seems valid when applied to the most popular scholarship of the day. Like in BAE anthropology, many sociological theorists used the comparative method in order to make theoretical statements about empirical reality.<sup>75</sup> This also partially validates Connell's argument regarding the stifling influence of 'grand ethnography' on heterodox idea formation. However, the marked difference between sociological and anthropological work was that sociologists were often an additional step removed from the process. While anthropologists were responsible for both collecting data on foreign peoples and then interpreting it through comparison, sociologists were not. In an 1895 article on the "Relation of Sociology to Anthropology" Lester Ward actually praised this division of labor. For Ward, who was also a

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<sup>74</sup> The phrase 'car window' sociologists is from Du Bois quoted in Morris, "Du Bois at the center: from science, civil rights movement, to Black Lives Matter", 3-4.

<sup>75</sup> Charles Camic and Yu Xie, "The Statistical Turn in American Social Science: Columbia University, 1890 to 1915," *American Sociological Review* 59, no. 5 (October 1994), <https://doi.org/10.2307/2096447>, 782.

member of the Washington Anthropological Association, anthropology was a “concrete” science, responsible for obtaining artifacts and observations of other cultures through ethnographic expeditions. On the other hand, the newer science of sociology was an essentially “abstract” one which used the data collected by anthropologists to generate theories of human development.<sup>76</sup> Ward published this argument in the *American Anthropologist*, with the intent of convincing social scientists in both camps to adhere to this division of labor.

In Giddings’ text *The Principles of Sociology* (1896) he includes a section on the methods of the young field, observing that “Empirical generalizations in sociology may be made by two methods, namely the comparative and the historical.”<sup>77</sup> Although Giddings also advocates for statistical operationalization of these observations, he believed that its use was in confirming and elaborating pre-existing theories. If the comparative method is used prior to statistical analysis, then it is unlikely that Giddings would have been able to find anything that challenged prior beliefs regarding cultural hierarchy. Even from the progenitor of statistical modeling in American sociology, traveller’s tales and legends were countenanced as evidence of societal differences.

Perhaps the Spencerians’ ambivalence toward empirical detail is why Jessica Platt’s oft-cited *A History of Sociological Research Methods in America, 1920-1960* (1999) began 16 years after the Congress took place. As Platt writes: “The book takes as its remit the period in American sociology from around 1920, when university sociologists *started* to carry out empirical research and to write about research methods”.<sup>78</sup> Indeed, by starting with Robert Park’s

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<sup>76</sup> Lester F. Ward, “Relation of Sociology to Anthropology,” *American Anthropologist* A8, no. 3 (July 1895): 241–56, <https://doi.org/10.1525/aa.1895.8.3.02a00020>, 241.

<sup>77</sup> Giddings, *The Principles of Sociology*, 64.

<sup>78</sup> Jennifer Platt, *A History of Sociological Research Methods in America 1920-1960* (Cambridge: Cambridge University Press, 1996), 2 (Emphasis my own)

leadership of the Chicago School of Sociology, Platt takes the commonly-held view that empirical sociology in America only became widely practiced in the 20s.

Platt's decision to begin in 1920 is supported by Daniels and Wright's article comparing *American Journal of Sociology* articles to the publications of Du Bois' Atlanta Sociological Laboratory.<sup>79</sup> In their comparison, Wright and Daniels found that while only 22 percent of *AJS* articles in this time period specific a method of data collection, every single Atlanta School publication did so. This indicates both a lack of clarity on how data was collected and the possibility for popular assumptions to slip their way into sociological analysis.

In contrast, much like *The Philadelphia Negro* (1899), Atlanta School publications gathered empirical data through surveys, archives, and observation. Additionally, Wright and Daniels' article makes the point that Atlanta University research topics were decided upon based on input from the Black community throughout the South. As a result, methods were carefully selected in order to answer specific problems faced by the Black community, for example in their report on *The Negro Artisan* (1902), DuBois and his students relied on questionnaires sent out to Black people in the professional artisan class. Only a few decades after the passage of the Thirteenth Amendment, Atlanta University researchers wanted to better understand how millions of Black Americans were transitioning from slavery to freedom and from rural to urban life.

Despite spotty funding, Du Bois' leadership of The Atlanta School resulted in sixteen monographs on Black life in America between 1897 and 1910.<sup>80</sup> They tackled the structure of the

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<sup>79</sup> Kalasia Shqueen Daniels and Earl Wright, "An Earnest Desire for the Truth Despite Its Possible Unpleasantness": A Comparative Analysis of the Atlanta University Publications and American Journal of Sociology, 1895 to 1917," *Sociology of Race and Ethnicity* 4, no. 1 (April 29, 2017): 35–48, <https://doi.org/10.1177/2332649217706519>.

<sup>80</sup> Alford A. Young and Donald R. Deskins, "Early Traditions of African-American Sociological Thought," *Annual Review of Sociology* 27, no. 1 (August 2001), <https://doi.org/10.1146/annurev.soc.27.1.445>, 449-453.

family, education, economic and business development, and the church in the Black community in the South. Most Atlanta School studies featured a designated section on methods, scope, or something similar. W.I. Thomas was the first author of an *AJS* article to include such a section in 1912, but the practice did not become common until 1917.<sup>81</sup>

In this section, I have remained focused on one practice of knowledge production that differentiated Du Bois from the mainstream crowd of the ASS: empirical data collection. Du Bois was conscious of this difference and used it to author some of his critiques. In large part, this might be understood as a critique of Spencerian theory. After all, to make a claim about the superorganic nature of societal evolution, one almost *had* to resort to sweeping comparisons of different cultures. Such a grand theory can only be confirmed from 1000 feet above ground. However, this critique of methods was accompanied by a critique of his peers orientation toward science, which was broadly based on Comtean positivism. While Spencer had provided the theory of the day, it was Comte who had outlined the ideal relationship between the researcher and the social world. Du Bois' critique of the Comte was rooted in his own alternative theory of how one conducts scientific research. Particularly, it seems that Du Bois' frustration with a model of sociology that echoed the natural sciences.

It is often acknowledged that early sociologists were members of the Progressive Movement and interested in the power of science to clarify and remedy social ills.<sup>82</sup> This claim cannot be disputed. Sociologists like Albion Small were acutely aware of pressing social issues of the day, including the monopolization of industry, labor unrest, and more. They believed that

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<sup>81</sup> Daniels and Wright, "An Earnest Desire for the Truth despite Its Possible Unpleasantness" A Comparative Analysis of the Atlanta University Publications and *American Journal of Sociology*, 1895 to 1917", 43-44.

<sup>82</sup> See, for example, Ross, *Origins of American Social Science* and Calhoun, "Sociology in America: An Introduction".

sociological study could clarify the causes of social issues, but that it was the job of a disinterested and objective researcher to do so. In order to produce sociology, they must adopt the Cartesian view from nowhere. In *An Introduction to the Study of Society* (1894), Small and his co-author George Edgar Vincent trace the lineage of their discipline to August Comte.<sup>83</sup> Drawing on Comte's positivist methodology, they make the argument that sociologists must remain disinterested toward their objects of study. They write: "Sociology is not, therefore, a resort for social visionaries, so eager to reform social evils that they cannot stop to take advantage of available knowledge of social condition."<sup>84</sup> The sociologist may apply what they learn to the world, but they may not do so until after they have finished in their scientific duty.

In its most basic form, Du Bois' approach to science eschews the natural scientific model of observation. In this view, the scientist takes an 'objective' stance towards their object of inquiry. This does not change between the physicist, viewing an object in motion, or the social theorist, viewing a society in the process evolution. For a social scientist to identify with their topic of research would raise the possibility of bias or 'me-search'.<sup>85</sup> The term 'me-search' is new, but as Morris observed, Du Bois was also critiqued by his white peers on the same grounds. The most famous instance of this was the Carnegie Foundation's passing over of Du Bois for Gunnar Myrdal to write *An American Dilemma: The Negro Problem and Modern Democracy* (1944). According to Morris, the selection of Myrdal was because a Black scholar with stakes in

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<sup>83</sup> Albion W. Small and George E. Vincent, *An Introduction to the Study of Society* (Dubuque: Brown Reprints, 1971), 25.

<sup>84</sup> Small and Vincent, *An Introduction to the Study of Society*, 32.

<sup>85</sup> For discussion of 'me-search' see 1. Julian Go, "Race, Empire, and Epistemic Exclusion: Or the Structures of Sociological Thought," *Sociological Theory* 38, no. 2 (June 2020): 79–100, <https://doi.org/10.1177/0735275120926213>, 89 and 1. Victor Ray, "Me Studies Are Not Just Conducted by People of Color," Inside Higher Ed | Higher Education News, Events and Jobs, accessed May 21, 2024, <https://www.insidehighered.com/advice/2016/10/21/me-studies-are-not-just-conducted-people-color-essay>.

the ‘Negro Problem’ could not approach it objectively. After Du Bois’ long career with the NAACP where he managed *Crisis* magazine, it was clear that Du Bois was not a disinterested academic.

Du Bois’ orientation toward science can also be seen in his early work in Atlanta. As Daniels and Wright observed, the topics of Atlanta University research reports were not decided upon randomly. They were in fact solicited from the Black community in the South: “Atlanta University administrators and faculty members were flooded with communications from alumni and community leaders from around the South informing them of the need for scientific inquiry into the conditions affecting Blacks in America.”<sup>86</sup> Thus, when Du Bois and his team of volunteers distributed their surveys and wrote their reports, they did so as scientists with a particular point of view—the point of view of the Black community.

The distinction between Du Bois’ and Small’s program is quite clear. In the essay “My Evolving Program for Negro Freedom”, Du Bois recalls that “In my quest for basic knowledge with which to help guide the American Negro, I came to the study of sociology”.<sup>87</sup> Regarding Small and the ASS, the inverse may be true. They approached their Progressive political analysis as social scientists, whereas Du Bois engaged in social science for the purpose of clarifying politics. Science was instrumental in determining a way forward for the Black community. As Jose Itzigsohn and Karida Brown argue, it was this orientation to sociological knowledge that

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<sup>86</sup> Daniels and Wright, “An Earnest Desire for the Truth despite Its Possible Unpleasantness” A Comparative Analysis of the Atlanta University Publications and *American Journal of Sociology*, 1895 to 1917”, 43.

<sup>87</sup> Du Bois, “My Evolving Program for Negro Freedom”, 35.

made Du Bois “critical”. It forced him to turn to empirical reality first and then to social theory after in an attempt to disprove widely held beliefs.<sup>88</sup>

### **Conclusion:**

Over the past three sections, I have endeavored to engage in two broad comparisons. While my case-specific empirical conclusions stand on their own, both of these comparisons are also relevant for the larger argument of this thesis regarding heterodox idea formation. The first comparison was between Boas and Du Bois on the one hand, and their intra-disciplinary interlocutors on the other. Both Du Bois and Boas generated heterodox paradigms for researching race and culture, and they did so by both collecting their own empirical data and then developing new ways of interpreting.

The second comparison is between anthropological and sociological communities more broadly. In particular, I have contrasted their institutional spaces and their practices of knowledge production. In the case of anthropology, the institutional space changed dramatically in the years surrounding 1902, moving from the Bureau of American Ethnology to various university departments, and in particular Boas’ Columbia. Their practices of knowledge production were altered as well, but in a very particular manner. The use of ethnography as a means of collecting data was constant. In this way, Boas and Powell were doing the same thing—venturing forth into foreign cultures, collecting artifacts and observations, and then synthesizing their findings into

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<sup>88</sup> Itzigsohn and Brown, *The Sociology of W.E.B Du Bois: Racialized Modernity and the Global Color Line*, Introduction.

theories. The primary distinction was only at the level analysis. Instead of a comparative method of analyzing observations, Boas was engaged in a historical one.

One way to explain the transition between the stage-ist theory of culture grades and Boasian sociocultural anthropology would be as a Kuhnian paradigm shift.<sup>89</sup> Over time, anthropologists simply noticed anomalies that could not be explained by the stage-ist theory, eventually preferring Boas' for that reason. This explanation incorporates the social context of knowledge claims to a certain extent, but still relies on one theory being "better" at explaining the empirical world. It seems difficult to make this claim for one reason in particular: Powell's view of culture was quite popular up until his death, and then in a very short period between 1902 and 1910, Boas shifted the conversation dramatically. The fact that this shift becomes most dramatic after the combination of Powell's death, the decline of the BAE, the rise of Columbia, and Boas' ascendancy in the AAA implies that it was more than the ability to "explain anomalies" that contributed to Boas' success. It was also the opening up of a space within anthropology that was not beholden to Congressional funding to research the causes of American civilizational superiority in relation to indigenous tribes.

But what about in sociology? There was no such control imposed by the state over the American Spencerians. In the case of anthropology, the institutional format of the BAE proved to constrain the impact of Boas' methodological and theoretical efforts. What stopped sociologists, who were not state employees and were in fact rather disparately organized until the founding of the ASS in 1905, from abandoning the evolutionary model of culture and race? This becomes

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<sup>89</sup> Thomas Kuhn, "Chapter VI: Anomaly and the Emergence of Scientific Discoveries," in *The Structure of Scientific Revolutions* (Chicago, Illinois: University of Chicago Press, 2012).



especially puzzling, when one considers that these American Spencerians weren't oblivious to the arguments being made by Boas. As James McKee noted, both Lester Ward and W.I. Thomas were in tune with the anthropological literature on race, and in particular with the work of Boas himself.<sup>90</sup> In a world where cranium size was still a widely accepted metric of determining intelligence, Thomas and Ward agreed with Boas that races could not be ordered in terms of their innate mental capacities. And yet, as was demonstrated in section two, Thomas and Ward held fast to notions of cultural hierarchy long after anthropologists had begun to disregard that notion.

If sociologists were not constrained by their relationship to the American settler policies, and they were willing to accept some of Boas' premises, the only remaining source of difference between their theories and those of the anthropologists is in their practices of knowledge production. This was the purpose of comparing sociology and anthropology. The primary difference was summed up by Ward himself, in the aforementioned article on the "Relation of Sociology to Anthropology".<sup>91</sup> Sociologists were not in the business of collecting data on other societies, merely interpreting it. Not only did they rely on the comparative method to argue over Spencer's grand theory of social evolution, but they did so almost exclusively from their offices. There were no moments in the field to challenge commonly held notions of Anglo-American superiority nor for their subjects to speak for themselves.

With these comparisons in mind, allow us to return to the more general question posted at the beginning of the paper: how is it that heterodox ideas form within scientific fields? Before continuing, I would note once more that the theoretical conclusions of this paper are preliminary.

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<sup>90</sup> McKee, *Sociology and the Race Problem*, pg. 28-32

<sup>91</sup> Ward, "Relation of Sociology to Anthropology", pg. 241

The examples of how Boas and Du Bois generated their theories are only individual examples, and thus will not hold in all scenarios. Nevertheless, by way of the broader comparison made between anthropological and sociological fields, I hope to draw a few important conclusions.

Chief among these is the importance of institutional space in popularizing new concepts. The best piece of evidence for the importance of this causal mechanism is the timing of Boas' rise to prominence. Quickly after Powell's death in 1902, Boas' version of sociocultural anthropology hits the anthropological mainstream. While Boas had been making similar critiques prior to 1902, the timing suggests that a combination of a decline in the BAE's importance and the rising importance of Boas' department at Columbia were necessary for his ideas to become popularized within American anthropology.

A corollary to the importance of institutional space is that neither the discovery of new data nor the adoption of new methods is sufficient to generate new theories on their own. Boas and Du Bois both generated their critiques of evolutionary thinking by rethinking the relationship between empirical reality and theoretical conclusions. In doing so, they were able to rethink fundamental premises held on to by their peers. Thus, it was only through a conjunction of all three factors: (1) new data, (2) new methods of interpretation, and (3) an institutional space amenable to new ideas that Boasian sociocultural anthropology and Du Boisian sociology were able to flourish. The primary difference, of course, being that Du Bois' program flourished primarily at Atlanta University, while Boas' was not segregated from the rest of anthropology and became widespread through American academia. In any event, this model requires more testing; it well explains the cases of heterodox idea formation for Boas and Du Bois, but the extent to which it applies to other cases will require further research.

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