

Du Bois on the Centralized Organization of Science

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1. Introduction.

There's so much we should like to know, and so little time to come to know. How can we, and how should we, use our finite time and resources so as best to carry out inquiry? This is, of course, a matter of great practical and economic importance -- we spend a lot of money on science, and scientific inquiry is using up the labour hours of many of our most educated citizens. We should aim to spend these precious resources well. But it is also clearly a matter of epistemic importance. What we inquire into now affects what (if anything) we shall be in a position to know in future, and in general what information shall be available to future seekers.

In this chapter I am going to argue that we have much to learn about these important questions for carrying out studying the practical and theoretical work of W.E.B. Du Bois. In addition to the literary and activist work for which he is famous, there was a period in which Du Bois ran the Atlanta Sociological Laboratory. This is plausibly the first dedicated sociological laboratory in the USA, but Du Bois' work there has been until recently highly neglected by scholars and historians of science (Morris 2017).¹ This means there are potentially insights to be found by examining this work that could be brought to light and applied to our situation today. For, Du Bois used the Atlanta Sociological Lab to carry out an ambitious project of long term social research. This meant he faced the practical and epistemic questions in the opening paragraph. I shall argue here that his manner of answering these questions reveals an interesting and under-explored strategy for deciding how to allocate one's epistemic resources which could bear fruit for contemporary science, or social epistemology more generally.

In order to appreciate Du Bois' work, it will be useful to contrast it with the status quo system in the United States and UK. Presently we mainly distribute grants and scientific awards, and thus decide what sort of work shall be done, through peer-review panels. Peer-review panels have people submit proposals which are then (in theory) evaluated by relevant experts in a meritocratic way. They are meant to help us work out what research to pursue, given the resources available to us and the information we desire. To do this they need to tell who will gather pertinent information via reliable means, while ensuring regular replication and cross-validation, insuring ourselves against error, and retaining legitimacy within the scientific community. However, it is fair to say that there is, at the least, room for improvement in how well we are achieving these goals. First,

¹ It may be interesting to compare and contrast Morris' story of US sociology's disciplinary formation with the account of disciplinary formation given in Kuchler's contribution to this volume.

there is widespread doubt about our ability to evaluate research in a meritocratic manner. For one thing, there is at least some evidence of anti-meritocratic demographic biases in the way individual grant proposals are evaluated (e.g. Tamblyn et al 2018). For another, the manner in which reviewer scores are aggregated can potentially introduce biases even when individual reviewers are successful in unbiasedly evaluating candidate research proposals on their merits (Lee 2015). What is more, there may be types of high-value research or unusually innovative researchers whose work is likely to be missed by purportedly meritocratic assessment methods (Avin 2019). This can form self-perpetuating cycles of under-investment in otherwise talented and interesting researchers (Heesen & Romeijn 2019). And, finally, the fact that we fail to properly incentivize replication work is a notorious feature of present science currently getting much attention (Open Science Collaboration 2015). Given such issues, it is not surprising that there is presently policy work going on to explore non-meritocratic alternatives to distributing research funds — and, in particular, explorations of the possibility of distributing funds by lottery (Avin 2015, 2018).

Du Bois took quite the opposite route from trying to introduce lotteries, with their embrace of chance randomization. In fact, to a very considerable degree he centrally planned the sort of research his group would carry out so as to form an interlinking whole. Where the status quo system allows for competition between scientists to give funding out piecemeal to whoever seems best at a given moment, Du Bois' work embodies the attitude that as far as possible our research activities should be coordinated, and not aimed at rewarding individual greatness but rather producing the best overall project. While ideas along these lines have not been totally without support in the history of philosophy of science (see e.g. Neurath 1946, Bernal 1949, Kummerfeld & Zollman 2015), it is safe to say the epistemic merits of this are relatively under-explored. Our brief examination of Du Bois' plan will thus hopefully form a spur to generate more consideration of this sort of holistic line of action.

2. The Atlanta Sociology Laboratory

To understand Du Bois' approach it will be necessary to understand both how he organized things in the lab and the general philosophical ideals that guided his scientific work. I begin by sketching the manner in which he organized his labs' research schedules, drawing from his own descriptions (Du Bois [1944] 1990, pg.38-42) and also the recent scholarship of Wright II (2017).

The basic idea of the Atlanta program was to carry out a cyclical series of studies on an interconnected network of topics for (at least) a century. In the early stages, the idea was that each year would have a particular focus -- say, the health and physique of the black population in Atlanta -- and as much pertinent data on this topic would be gathered as capacity allowed, and this drawn from as many different modes or methods of research as possible. At the end of the year a conference would be held in which the results of that year's study would be presented to an array of international experts on the social sciences. This would allow for further refined analysis in light of their feedback, and the resulting work as well as insights gathered would be published in a book

edited by Du Bois. The next year a new focus would be chosen, and broadly the same plan carried out.

Du Bois indicated that at one point he envisioned the cycle as covering these topics in this order:

1. Population: Distribution and Growth
2. Biology: Health and Physique
3. Socialization: Family, Group and Class
4. Cultural Patterns: Morals and Manners
5. Education
6. Religion and the Church
7. Crime
8. Law and Government
9. Literature and Art

(Du Bois [1944] 1990, pg.40-41)

With the tenth year spent by Du Bois and his team on collating, organizing, and synthesizing, an overall picture in light of the previous decade's research was intended to emerge. This cycle would repeat for a century. Sheer limitation of resources meant that in the early days of the project, each year would really have to largely focus on just one of the topics, with only relatively little attention paid to the other themes. However, it was hoped that ideally the laboratory would grow in size and funding support, such that for each year active research would have been done on all facets of this cycle, with only the emphasis of the end-of-year conference changing from year to year.

It was thus a highly ambitious, and highly organized, project whose topics were chosen in advance for a century by Du Bois. He edited the end-of-year document collating and summarizing the research. This allowed him to highlight the scope and ambition of the project he was engaged in, and hopefully to draw in recruits. For instance, in the preface to *The Health and Physique of the Negro American*, he says:

A study of human life today involves a consideration of human physique and the conditions of physical life, a study of various social organizations, beginning with the home, and investigations into occupations, education, religion and morality, crime and political activity. The Atlanta Cycle of studies into the Negro problem aims at exhaustive and periodic studies of all these subjects as far as they relate to the Negro American. Thus far we have finished the first decade... (Du Bois 1906)

Du Bois thus intended the scientific community to know that there was a grand project of sociological inquiry going on and hoped to attract support for it as continuing venture that would out-

live him. In the meanwhile, the people who staffed the labs were his students, and he thus had final say as to the particular projects they were to carry out or the data they were to gather. He ensured that each year's findings were reexamined in light of the following year's results, and a variety of methods and sources of expertise were drawn from in gathering and interpreting these results.

To understand why Du Bois arranged things in this manner, we must review his philosophy of science. Central to his thoughts on scientific method is a distinction between the "mediate" and "immediate" aims of science (for a detailed discussion see Bright 2018 §3). The mediate aim is the ultimate purpose of science, the reason for which we ought to invest in it as an institution and incorporate it into our social concerns. Du Bois took the mediate aim of scientific inquiry to be guiding social reform: Science provides information that is useful in shaping policies, technologies, and attitudes, that tend to the common good. But the immediate aim of scientific inquiry was simply the discovery of truths (see Bright forthcoming for discussion of a related ideal). In sociology, especially, we seek truths about general patterns of behavior and discerning the extent to which people predictably respond to situations (Du Bois [1905] 2000).

Du Bois was hence insistent that the Atlanta sociological laboratory did in fact produce research that proved useful for social reform (and would draw upon this body of research in issuing policy advice (Du Bois 1906a)), but also that they were themselves aiming only to discern scientifically interesting truths about the lives of black Americans (Du Bois [1944] 1990, pg.39). Indeed, this was highlighted in all their publications, as the preface to their annual conference proceedings included a note from Du Bois saying "we wish not only to make the Truth clear but to present it in such shape as will encourage and help social reform" (Du Bois, [1940] 2007, 63-64). How, then, can one fulfill the immediate aim of science in such a way that it can likely aid the mediate aim of science? I have argued before that for his work to fulfill the mediate aim of science he needed to produce research that is both trusted and trustworthy (Bright 2018). That is to say, it must actually describe the conditions of life faced by people and how they tend to react to various stimuli, and do so in a way that is seen by people as a viable source of policy relevant information. Du Bois attempted to achieve this by identifying social problems faced by the groups he studies. He defined social problems as "the failure of an organized social group to realize its group ideals, through the inability to adapt a certain line of action to given conditions of life" (Du Bois 1898). For through the identification of their social problems one will be helping the group fulfill aims that it itself takes to be good -- and thus, if the work is honestly carried out, and presented in an attractive manner (Battle-Baptiste & Rusert 2018), earn one's status as trusted. And to do this accurately, and thus be merited as trustworthy, one would have to carry out precise research triangulated across multiple research methods.² What this means is that the work should be designed to en-

² For a statement of his methodological pluralism see the preface to Du Bois 1899 -- see Heesen *et al* 2019 for a detailed discussion and defense of Du Bois' methodological pluralism.

sure multiple lines of inquiry by different methods are brought to bear on the same question, and one should take answers that one can consistently reach by this means.

The Atlanta Sociological Lab can be seen as having carried out a research project along these just lines. Multiple of its areas of focus were explicitly dedicated to discovering and spelling out the group ideals of the black Americans he was studying. For instance, Du Bois thought that such things could be discerned from the artistic products of a people (Du Bois [1903] 1994), and this was one of the topics of the ten-year cycle. Questions were addressed from multiple angles and results were scrutinized at a conference attended by experts from various different fields, ensuring methodological pluralism and thus facilitating the triangulation he thought essential. And the exacting statistical details the Atlanta lab produced and worked through were always checked and rechecked due to the cyclical nature of the project. Finally, this cyclical design meant that long term trends in black Georgians group life could be identified: facilitating the identification of patterns underlying human social behavior was what Du Bois thought to be the central goal of sociological study (Du Bois [1905] 2000). Given that it so well approximated his own philosophical ideals, it is no wonder that even years later he thought that if the project had been carried through it would have produced “a foundation of carefully ascertained fact [that] would build a basis of knowledge, broad and sound enough to be called scientific in the best sense of that term” (Du Bois [1944] 1990 pg.40). Alas. however, as Du Bois became an increasingly controversial political figure, maintaining the funding for any lab associated with his name became possible. And so, for sheer lack of money, the Atlanta Sociological Laboratory was shut down before it had even completed its second decade of work.

3. Advantages and Disadvantages of Du Bois' System

Granted that the design of the Atlanta Sociological Laboratory series of studies seems well suited to Du Bois' goals, we may still ask how well this project fares when measures against the concerns of contemporary social epistemologists. And, to give the game away, I think it can fairly be said to have a number of advantages that would be attractive to many contemporary social epistemologists.

First and most obviously, there is a concern for replication and multi-method research as a central feature of the whole program. The emphasis on approaching the same question from multiple angles, and subjecting proffered answers to scrutiny from expert representatives of multiple fields, ensures that (at the least) conceptual replication of any given claim is secured. A conceptual replication is one which which “attempts to establish the same theoretical conclusion as an original experiment with different experimental manipulations or measures” (Machery 2020). Taking “experiment” in a broad sense to include observational sociological studies, insisting upon methodological triangulation ensures conceptual replication of their results was guaranteed. What is more, methodological triangulation of the kind proposed and carried out by Du Bois has itself

been proposed as a means of directly addressing many of the problems which replications were meant to achieve (Munafò & Smith 2018).

Additionally, the somewhat dictatorial nature of Du Bois' position as the lead investigator, means that one can simply mandate that direct replications take place. Direct replications are those which "aim to be identical to an original experiment save for its sample of participants" (Machery 2020). These are viewed to be low status, and so are notoriously undervalued and under-rewarded in contemporary science (Heesen 2018). If one's CV consists mainly of replications of other people's work, which will have been difficult to get published in the first place, then this sparse and unimpressive track record will make it hard to get a permanent job and hard to secure funding in meritocratic grant competitions. Whereas something organizationally similar to the Atlanta lab would have tasks allocated by a PI like Du Bois. This person can simply mandate that some lab workers allocate at least some time to direct replication. In this way the Atlanta lab structure managed to approximate methods of labour allocation that have recently been suggested as attractive for ensuring replication in contemporary science (Romero 2018).

Finally, as is obvious, the Atlanta model allows for very ambitious projects! This is because, if it were to work out, a research group would attain support for an extremely long-term project, with a sort of perspective-taking exercise every decade to keep things on track when one takes the more synoptic view. As mentioned, peer review panels are often wary of overly ambitious projects, and this goes especially when there is an interdisciplinary component (Lamont 2009). By contrast, something similar to the Atlanta cycles would give an ambitious PI wide remit to design a project as they see fit and allocate tasks and funding within that broad project so as to ensure it is able to paint a comprehensive picture of whatever is being studied.

The structure of this project thus allows it to overcome worries about lack of multi-method approaches, under-funding of ambitious projects, and under-incentivization of replication studies. Further, while the actual Atlanta cycle project was shut down early for lack of funding, scholars who have studied it have persuasively argued that its scientific achievements were very impressive and have a reasonable claim to being foundational for American sociology (Morris 2017). The downside, however, is obvious. To put the point mildly, this sort of design is heavily dependent on the character, charisma, organizational talent, and insight of the leader for its legitimacy and success. Du Bois was an unusually skillful sociologist, with an explicitly articulated guiding philosophy to structure his research plans, and charismatic author who could garner support for the project. However, the whole project was essentially destroyed by a combination of general hostility to honest study of race relations in America and Du Bois' conflict with Booker T Washington, making it hard to attract funding for anything associated with him. The fate of a 100-year research project was thus sensitive to the popularity of just one person. This is because the Atlanta Sociological Laboratory was simply not designed to be robust to the changing fortunes of the PI; if the latter unexpectedly dies, becomes otherwise unable to work, or falls out of favor, much can be lost.

4. Concluding Discussion.

The Atlanta Sociological Laboratory was a pioneering and ambitious project of social science in America. Its highly centralized mode of organization under Du Bois allowed for a unified and cyclical pattern of research to be projected and partially carried out over a century. Features of its design ensured that methodological triangulation could be carried out, and replications regularly carried out. Its scale and longevity meant it was capable of producing research that would both identify long term patterns of human behavior, and also the social problems faced by those under study, which may then in turn represent useful information for that very same group. Its impressive achievements are testament to these desirable social epistemic properties, which are themselves at the forefront of contemporary discussion of problems in science.

The idea that diversity-of-ideas trumps competence-of-ideas in epistemic life has perhaps been overplayed in recent philosophical and scientific discussions (Grim *et al* 2019). However, one may reasonably feel uncomfortable at the kind of homogenization of perspectives organizing a research project in the same manner as Du Bois' Atlanta Sociological Laboratory would entail (Dang 2019). There are good reasons to want to avoid our epistemic fate being so tied to the competence of a single scientist (Heesen 2017).

The question remains whether there is some way of gaining the advantages of the Atlanta model while avoiding its disadvantages. Perhaps it could be fruitfully combined with some of the proposals for democratizing decisions about how research is directed that are existent in the literature (e.g. Brown 2009, Kitcher 2011, Douglas 2017). Or perhaps the study of the social dynamics of large research teams (e.g. Knorr-Cetina 2009, Winsberg *et al* 2014) will yield insight as to how tasks and responsibilities may be allocated in such a way as to reap the benefits while minimizing the homogenization apparently inherent to his model. Finally, I have not said much about how the funding is secured for such a large project beyond noting that in the case of the historical Atlanta school it was tied to Du Bois' reputation. As social epistemologists appreciate, the funding structure of a science is not at all irrelevant to its trustworthiness or the degree to which it will be trusted (e.g. Holman & Elliot 2018, Weatheral *et al* 2018). As such, given that any such project would require a large investment, this would have to be addressed.

Difficulties and questions thus remain for anyone who hopes to take up, renew, and apply the Atlanta Sociological Laboratory's cyclical model of allocating research tasks. I hope to have shown, however, that it has desirable features, and despite being quite opposite in approach from some presently popular responses to the failures of the present system, it might yet go some way to helping us yield the reliable body of scientific knowledge that Du Bois so earnestly sought — and which we still seek today.

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