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METHODS OF SOCIAL STUDY

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Methods of Social Study

by Sidney and Beatrice Webb

with an introduction by
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London School of Economics and Political Science
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INTRODUCTION TO THIS EDITION

T. H. MARSHALL

THE FIRST reference to *Methods of Social Study* occurs in Beatrice Webb's Diary on 18 March 1917. She was then recovering from a breakdown and wishing that she could retire to a quiet life in the country. 'Sidney meanwhile might complete those endless volumes of historical material which are almost finished, and the two of us together might write the two books we want to bring out before we die – *What is Socialism*? and *Methods of Investigation*.'[1] The 'endless volumes' were eventually completed, but not by Sidney alone – for Beatrice resumed her place in the partnership – and not until the end of 1928; it had been delayed because they had decided to bring the history of the Poor Law right up-to-date, and this had involved, not only much writing, but some new field-work. When, on New Year's Eve, they wrote the final words of the Epilogue of that great work, Beatrice declared that 'these two volumes will be the last big work of research'.[2] And so they were, apart from the unforeseen volume on Soviet Communism.

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Nevertheless Beatrice found time, in 1921, to turn her mind to the projected book on methods and to prepare some draft chapters for it. These (or at least some of them) have been preserved among the Passfield Papers at the London School of Economics.[3] They were written in a personal style, using the first person singular instead of the familiar Webbian 'we', but they were based in large part on the summary statement about their methods of investigation which they had included in the Preface to their joint work, *Industrial Democracy*, published in 1897. Then Beatrice changed her mind. She was feeling moved to express in writing what she termed her philosophy of life by formulating 'on the one hand my faith in the scientific method as applied to social institutions, and on the other my realisation that, without the religious impulse directing the purpose of life, science is bankrupt'.[4] There is some evidence to suggest that she had tried to include the first part of this philosophy in her drafts for the book on methods. What we have of hers in the Passfield Papers starts with Chapter II, the opening sentence of which is: 'In the foregoing pages I have tried to explain the reason of my faith in a science of society.' But this sentence has been crossed out and the 'foregoing pages' are missing. In their place is a brief note in Sidney's hand-writing, beginning 'What we study is social structure', which lists topics for investigation and offers

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some precepts to investigators. Whatever the explanation of this, the fact remains that Beatrice decided, as she later expressed it, to 'cast on one side' the draft chapters she had written 'in order to put my experience of investigation into *My Apprenticeship* in an autobiographical form'.[5] This was published in 1926, and she was still working on the second volume, which became *Our Partnership*, when she died.

So the book which 'the two of us together might write', was absorbed, for the time being, into 'this little book of my own – which is a big book in its high endeavour to explain my craft and my creed'.[6] But it reappeared in their immediate programme of work in June 1931, with its present title, *Methods of Social Study*. By that time, it seems, her interest in the project had somewhat diminished. Evidence drawn from her Diary has to be used with caution, as the entries sometimes express only a passing mood; but on this point they are frequent and consistent. In June 1931, she described the programme in which this book was an item as 'the plan for our dotage. I feel singularly lighthearted about it and regard it more as a way of keeping the old Webbs comfortably occupied than as an additional contribution to our output.'[7] When the time came to begin work, she gave Sidney the chapters she had written in the early twenties for him to use in 'preparing' the book.

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In January 1932, she noted that 'he has added to and amended these chapters and is hard at work on the statistical method applied to social facts. Now I have to review what he has written.'[8] But she found it a struggle: 'My aged brain is so tired and strained that I find it difficult to grasp the task.'[9]

She was then 74 and advancing years were probably one of the causes of her very limited participation in the work, although it is true that she had made similar complaints more than once in the past, beginning as far back as 1917, and she proved able to cope with the visit to Russia and the two volumes devoted to it which still lay ahead. She also felt, no doubt, that she had already done her share of the work by writing the draft chapters, two of which (the crucial ones on note-taking and interviewing) she had published as Appendices to *My Apprenticeship*. In addition she was absorbed by a new interest. Russian Communism, which in 1926 the Webbs had regarded as being of the same species as Italian Fascism, had, by 1930, begun to look as if, in spite of its unacceptable features, it might contain the essential elements of the new social order for which they were looking – and she was reading everything she could lay her hands on about it. The entry in her Diary for 1 March 1932, sums up the story: 'S.W. with a little help from me, has finished the book on *Methods of Social Study*, whilst I have been reading

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Lenin's works and various odds and ends on Russia.'[10]

It is important to assess Beatrice's share in the composition of this book because social investigation was very much her subject or, as she termed it, her 'craft' which, combined with her 'creed', had provided the inspiration for her professional, as distinct from her political, life. She had chosen the career of social investigator in her middle twenties, and had studied and practised the craft both on her own and also in close collaboration with Charles Booth. She had discussed with him, not only the methods of enquiry, in the sense of the techniques for assembling and handling data, but also its methodology, including the interplay of induction and deduction in the reasoning process. But though she regarded Booth as the boldest pioneer in the methodology of the social sciences, she did not find in him a warm response to her eagerness to press forward all the time with the creation of a science of society as the ever-present objective. Booth's idea was to build a statistical framework, not a theoretical structure, into which to fit the facts he discovered.[11] By the time Beatrice met Sidney she was the expert in social investigation, and he the novice. In 1891 she almost apologised for accepting his help in the study of trade unionism; the experience would be of value to him in his projected political career.[12] In 1919 *she* was helping *him* with the final chapters of the

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new edition of the *History of Trade Unionism*.[13] By that time Sidney had added his remarkable gifts to Beatrice's intelligence and acquired skills to produce the complete and balanced partnership whose methods are described in this book. Outstanding among his abilities were his astounding reading speed, the rapidity and fluency of his drafting, his photographic memory and his powerful, and above all orderly, mind. Mary Agnes Hamilton said of him: 'One glance at a printed page stamps its contents on the tables of his mind, and no accumulation produces either congestion or disorder there.'[14]

In the case of this book, however, the famous partnership did not function quite as it had done in its prime. Tawney once described it as 'one complex personality communing with itself', so that ideas 'were struck out in a continuous duologue in which each was flint and steel in turn'.[15] But this time there was little or no *continuous* duologue while the book was actually being written. The bulk of the material on methods was drawn from Beatrice's earlier writings, principally the draft chapters now in the Passfield Papers, *My Apprenticeship*, the Diaries (nearly the whole of Chapter VIII), and her article in the *Nineteenth Century* about the Royal Commission on Labour.[16] Sidney incorporated all this into the book with remarkable fidelity, expanding it considerably in places, but rarely altering the text – except that he replaced

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the autobiographical 'I' by the Webbian 'We' everywhere apart from the direct quotations from the Diaries. Consequently the general tone is more personal than in the other works produced in partnership, and there are places where Beatrice's more passionate temperament has not been toned down as much as (she admits) it generally was by Sidney's more down-to-earth approach. Chapter VII, for example, reproduces, not only the arguments, but also in places the tone, of her savage, and rather reckless, attack on the Labour Commission, which provoked the Chairman, Sir Geoffrey Drage, to reply with an article in which he said that 'anyone who has been present, as I have, through the whole of the inquiry conducted by the Labour Commissioners, and who knows the facts, can hardly find the patience to read the endless string of inaccuracies' in her account of the proceedings.[17] There certainly were inaccuracies in it, but her case was fundamentally sound and of vital importance. Royal Commissions did rely far too much on oral evidence, and too little on verified facts.

In the chapters on methods, though there may be some differences of style or tone, there is complete unanimity of ideas and principles; in the case of the first and last chapters of the book, this is not equally true. At the beginning of the Preface they say that the book as a whole is about 'the methods of investigation' (its original,

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1917, title) used by the Webbs themselves in their studies, but add that 'in our first chapter and in the last, we have attempted to define the scope and necessary limitations of the science of society' (p. xliii). The first sentence refers clearly to what Beatrice called her 'craft', and accurately describes the core of the book. The second refers, somewhat evasively, to the two halves of her 'creed' – faith in social science and belief in the importance to man and society of other values lying beyond the range of science. The final chapter does, in fact, expound the second half of her 'creed', using some of her own phrases in doing so. But only the last four pages are devoted to the subject. This section begins with a quotation from the Introduction to *My Apprenticeship*: 'Is man's capacity for scientific discovery the only faculty required for the reorganisation of society according to an ideal? Or do we need religion as well as science, emotional faith as well as intellectual curiosity?' (p. 255). The sentence that follows is an obvious echo of Beatrice's criticism of Herbert Spencer for trying 'to apply the scientific method to the *Purpose* as distinguished from the *Processes* of existence'.[18] But the treatment of this great issue is tame, and even at times apologetic; it reads very much more like Sidney (out of his element), than like Beatrice. The rest of the chapter offers some not very impressive examples of the beneficial effects of sociological research on

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social development, and describes some of the obstacles to planned social change and to accurate sociological prediction.

The first chapter is more substantial, but it steers clear of involvement in any theoretical discussion of the possibility of applying exact scientific methods to social data. Its centre-piece is an acceptable, but elementary, definition of sociology as the study of relations between people, their associations, and the structure and function of social institutions. This was, indeed, the basis of the subject as taught by Hobhouse and Ginsberg at the London School of Economics, but one would not gather from it that students there were also engaged in studying and discussing the theoretical issues raised in the works of Durkheim, Pareto, Max Weber and Marx. The chapter ends with a rather curious classification of institutions according to their origin in animal instinct, religion, humanism or deliberate planning, a classification which must surely have been invented by Sidney. It would seem, then (though we can only guess) that Chapter I as we have it is Sidney's substitute for the missing draft chapter, referred to in the cancelled sentence in the Passfield Papers, which Beatrice wrote to explain the reasons for her faith in a science of society. It must not be inferred that Sidney did not hope that such a science would be established. But, like Charles Booth, he thought it was as yet very much in its

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infancy, and he fought shy of philosophical argument about its nature. Beatrice tells us that he found much of her autobiography far too subjective, and 'all that part which deals with 'my creed' as distinguished from 'my craft' seems to him the sentimental scribblings of a woman, only interesting just because they are feminine'.[19] We might call this a fanciful description of a genuine difference between two intellectual temperaments, of which Sidney's was the stronger influence in their partnership. When, for instance, Beatrice stated emphatically in 1900 that 'our effort is now directed to one end – to establish on a firm basis a science of society', she was referring in the first instance to the foundation of the London School of Economics, and only in second place, and with many doubts and reservations, to her 'creed' and the possibility of fulfilling it through their own research work.[20] This might contribute some useful material, but the task itself must be entrusted to others, and to an institution which would outlast its individual members. Here she was undoubtedly voicing Sidney's opinion both about the role of their partnership and about the importance of the London School of Economics.

The basic principles of the Webb method of social investigation are: First, every statement made, whether by way of description, explanation, prediction or precept, must be based on facts. Second, the investigator must assemble

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and review *all* the facts in each case he studies. (This does not preclude sampling, where the cases in a particular class are very numerous. They used it themselves for parishes, and thought Charles Booth should have used it in his London survey.) Third, all facts and statements must be verified. Fourth, the investigator must be totally objective. He cannot (and should not try to) rid himself of bias, but he can, and he must, when at work 'put his bias out of gear'. These requirements are normal for any piece of empirical research, or would be so, if one were to insert the word 'relevant' before 'facts'. But this the Webbs were reluctant to do, and their discussion of the point is interesting and significant. It turns on the problem of how to start an investigation.

'The false start which pseudo-investigation usually takes', they write, 'is the plausible one of asking a question' (p. 34). Hypothesis on the other hand, is indispensible at all stages, 'and not least at the very beginning of the enquiry' (p. 60). Why so sharp a distinction between two things so closely related? For a hypothesis implies an antecedent question to which it offers a provisional and, if it is to serve its purpose, a testable answer. The explanation is that the kind of question the Webbs had in mind was one loaded with assumptions drawn from the investigator's personal prejudices or political ideals. They make this quite clear when they say that

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the subject of study must not be 'a social problem to which a solution has to be found, or a question to which the investigator desires an answer' (p. 56). For in such a situation the investigator will unconsciously narrow the area within which he looks for the answer. Hypotheses, on the contrary, enlarge the area of search, provided you take as many as possible into account, including, as they did, 'crazy ones, plausible theories and fantastic ones, the dicta of learned philosophers and those of "cranks" and monomaniacs' (pp. 61–2). A question, in the Webb's sense, imprisons an enquiry, whereas hypotheses (in the plural) liberate it, by 'delivering the investigator from the strangle-hold of the old categories of thought', as Beatrice expressed it in her original draft chapter.[21] But hypotheses, they realised, do more than this. They serve to guide the investigator in his collection of facts, and are 'one of the various handles by which the students can, so to speak, take hold of the part of the external world that is to be investigated' (p. 61). They are invaluable, provided one is always ready to change or to revise them. The only 'right way' to enter on research in sociology is to choose a particular social institution and collect all possible information about it, 'with the sole purpose of discovering every fact concerning its constitution and its activities, together with every ascertainable action and reaction between it and its environment'. Out of this investigation

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of the facts may emerge – in the end – 'the final hypothesis to which you apply your last test of verification' (pp. 41–3).

This problem of the relation between facts, questions and hypotheses provides a key to the understanding of the work of the Webbs, and in particular of this book about the methods they employed in it. They were operating in two fields which had to be kept apart (research and politics), and in two disciplines which had to be brought together (history and sociology). Tension was a possibility in both cases. On the first, Dame Margaret Cole has explained that they were engaged in 'labour research', which meant two things, both to them and to their colleagues. It meant 'research into subjects of importance to the Labour movement' and also 'research designed to establish conclusions in accordance with the general tenets of the Labour movement'.[22] Both used facts, the former for scientific ends, the latter (often on the same subject) for political purposes. Their determination to keep these two activities separate was perfectly genuine, and one of the main objects of this book was to show how this could be done and how, in fact, they managed to do it.

'Most orthodox historians', wrote Professor Postan in 1968, 'cling to the belief that their real business is to study facts.'[23] Sociologists, though they may also study facts, are particularly addicted to hypotheses. By these criteria

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the Webbs fall into both categories. They described what they called the 'Webb speciality' as 'a study, at once historical and analytic, of the life-history of particular social institutions during the last three or four centuries' (p. 89). The words 'historical and analytic' cover the two disciplines, but what follows shows a bias towards history. And it was indeed through history that they approached the study both of trade unionism and of local government, because they believed that, without a detailed knowledge of its history, one could not hope to understand the structure and function of a social institution. Furthermore, their method, based on a vast accumulation of facts, lent itself to this approach, and also, as their work demonstrated, to that kind of study of the present which one may class as contemporary history. It was, as they found, extremely difficult (or even virtually impossible) to manipulate this mass of material (as a sociologist would today, with the help of a computer) so as to extract from it explanatory and theoretical, as distinct from classificatory and descriptive, results. What they in fact produced, in the main, lay somewhere between straightforward historical narrative and full sociological analysis – of the kind Beatrice, no doubt, had in mind when, in 1886, she was 'puzzling over the methodology of social science', and making up her mind 'to try an article on social diagnosis'.[24]

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On both points (the separation of science from politics and the passage from history to sociology) the story of their first two major works, *The History of Trade Unionism* and *Industrial Democracy*, is particularly illuminating. The former, generally acclaimed as an historical masterpiece, original and complete, seemed to them, according to Beatrice, 'little more than an historical introduction to the task we had set before us: the scientific analysis of the structure and function of British Trade Unions'.[25] So they started on the second book, in search of a thesis. But, 'when we come to the thesis we find the facts, tho' they can be used as illustrations, are not much good as the basis of our structure – they are only the ornament', and the argument turns out to be deductive.[26] In the same entry in her Diary she noted the tendency of the sociological element to develop political affiliations.

> Our work suffers from being an almost unconscious attempt to unite three things: (1) a descriptive analysis of modern trade unions. . . ; (2) a criticism of trade unions (for the good of the unionists!); (3) an apology for, or defence of trade unions (for the enlightment of the middle-class and economists). These three objects do not amalgamate well.

These are, of course, only Beatrice's impressions, jotted down at the most trying moment in the passage from the first book to the second. But they identify correctly the issues they had to face, and the obstacles they had to overcome, or to circumvent. In *Industrial Democracy* they

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attempted to use the methods they had perfected in writing the history to produce an analysis of the contemporary scene which was sociological in the sense of being, not only descriptive, but also explanatory and, to a certain extent, theoretical. They never did this again. In the Preface to *The Consumers' Co-operative Movement* (1921) they say that it 'may be regarded as analogous to our *Industrial Democracy*'. This is true in so far as it is a contemporary, and not an historical, study. But there the analogy ends, for it is, as they rightly say, only a 'descriptive analysis', whereas the earlier work set out to be more than that.[27]

The book *Methods of Social Study* is presented to us in the Preface as offering a definition of the particular department of *sociology* studied by the Webbs and a description of the methods of investigation they employed in studying it. But all the chapters on method (Chapters II to X), except for Chapters VI and VIII (on *The Spoken Word* and *Watching the Institution at Work*) could be used, and were used by them, also in *historical* research. The main interest of the book to us today lies in the light it throws on how the Webbs worked and what their aims were; it is not a text-book for modern students, at least not in a technical sense; and (according to the Preface) it was not intended as such. But it clearly did have a didactic, and especially a cautionary, purpose. It is rich in common-sense

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and frank in its account of the pitfalls which that common-sense taught them to avoid and out of which, when they had fallen in, it helped them to climb. Their open-mindedness (that is to say their receptiveness to the lessons of experience) is remarkable, and their purpose is to pass on the fruits of this experience to the younger generation. The cautionary note is strong in Chapter II where they list the qualities needed for the daily tasks of social investigation, which are often tedious and boring, and always call for great patience and persistence. These qualities, they add, are, in the England of today, more often found in people with only a second-class education (pp. 50–1). Did Beatrice remember that, shortly before she married Sidney, she had written: 'we are both second-rate minds, but we are curiously combined. I am the investigator and he the executant?'[28] A nice example of the quick response to experience is the advice to the interviewer to remember that 'the mind of the subordinate in any organisation will yield richer veins of fact than the mind of the principal', not only because he is less on his guard, but because he knows what is actually happening (p. 137). This reflects what G. D. H. Cole called her 'determination to study directly the practical *behaviour* of persons and institutions'.[29]

The pivotal point on which their method revolved was their system for the recording and handling of the factual data on which all

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their work was based. And central to this system were the famous quarto sheets of paper, with only one fact entered on each, docketed with source and date and classified under a subject heading. The solemn tone in which they wrote (and spoke) about these pieces of paper has given rise to some jokes at their expense, and this has diverted attention from more significant points about their method. There is nothing odd about what we may call the 'unit-fact' method of note-taking; it has been, and still is being, used by many investigators, especially historians. In both the earlier versions of the chapter on note-taking (the draft in the Passfield Papers and the Appendix to *My Apprenticeship*) Beatrice quoted a passage from the *Introduction to the Study of History* (1898) in which Charles Langlois and Charles Seignobos state that 'everyone admits nowadays that it is advisable to collect materials on separate cards or slips of paper . . . furnished with the precicest possible indication of origin'. The detachability of these notes, they continue, 'enables us to group them at will in a host of different combinations; if necessary to change their places'.[30] This is exactly how the Webbs describe and explain their own method, including the vital process of constantly 'shuffling' the quarto sheets. So it is curious that the relevant footnote in *Methods of Social Study* (p. 84) should state that 'we have not found any book informing the social investigator how to

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handle his notes'. It is true that the French authors, and also the German author cited by them in the same footnote, were writing about historical method; but the Webbs used their similar method in both historical research and social investigation.

Whatever others may have been doing at the same time, it is clear that the Webbs invented, or discovered, this method independently for themselves. They started collecting facts for their trade union history by means of a questionnaire, which they print in full in this book so that the reader can see for himself why it was a failure. It was, as they frankly admit, based on a quite inadequate knowledge of the field. It was too complicated, at many points inapplicable to the situation it was investigating, and it begged questions to which it was supposed to be seeking answers. Rather surprisingly this experience led them, not to try to improve their use of the questionnaire as a technique of investigation, but to abandon it altogether in their major research. It could provide, they thought, raw material for the statistician, but could not be used for qualitative analysis (p. 75). For a time they seem to have grouped their notes under the source from which they were drawn, until their assistant, F. W. Galton, switched to a system of classification by topic. They at once saw the benefit of this, but found that they could not take full advantage of it until the notes had been

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entered on separate sheets, so that those relevant to each topic could be brought together in such manner as the analysis of the material required; and so the Webb method was born (p. 162 note). Here, as elsewhere, they tackled their problems in a kind of self-sufficient isolation. In her early drafts for the book Beatrice wrote that she was often asked what was the use of this elaborate method of note-taking. 'Here I find myself driven into an uncomfortable corner', she continued. 'Short of having as intimate a connection with the work of other sociologists as I have had with the Webb investigations. . . I am compelled to be frequently taking these latter as proof of the validity of the methods of research which I am advocating.'[31] The same point is made on pp. 88–89 of this book, but in a much weakened form.

There is no doubt about the value of this method; the question is to decide in what circumstances and in what types of research it can be profitably, or even safely, employed. Nowhere as yet has the 'unit-fact' method of note-taking been used on a grander scale, or with more highly developed technical means of sorting or 'shuffling' the data, than in the *Human Relations Area Files* inaugurated by George P. Murdoch in the 1940s to receive cultural data about primitive societies. Yet even here some anthropologists hold that the use which can be made of factual information isolated in this way from its context is limited, and that there is a

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danger of it being misused through neglect of the qualitative differences between outwardly similar unit-facts. It is a question of the conceptual framework into which the facts can be fitted. As Clyde Kluckhohn put it, 'an altogether adequate organization of comparative data must await a better working-out of the theory of the universal categories of culture, both structural principles and content categories'. Present methods of organising and comparing data 'beg questions which are themselves at issue'.[32] The Webbs were, of course, working at a much lower level of generalisation and in a much more limited field, but they faced a similar problem, as the failure of their questionnaire showed. They hoped, by 'shuffling' and re-shuffling their data, eventually to discover the necessary principles and categories, even in the then uncharted seas of trade union history. Were their hopes justified?

When one browses among the boxes of original notes in the L.S.E. Library one finds it impossible to believe that these could really have been the source of their great works on trade unionism and local government. They are written by hand, in a variety of styles, and many are almost illegible. They range (especially in the early days) from brief items from a committee's minutes to literary extracts covering several quarto sheets. They are often classified under headings so broad that further sub-classification must surely have been necessary; but, as they

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gained in experience, the recording became more systematic. In one respect their method differed from that recommended by Langlois and Seignobos. Immediately after the passage quoted by Beatrice in her footnote they say that, if items are interesting from several points of view, 'it is sufficient to enter them several times over on different slips'. This obviously reduces the amount of 'shuffling' necessary, and they presumably thought this desirable, because, as they say, 'in virtue of their very detachability, the slips or loose leaves are liable to go astray'.[33] The Webbs do not appear to have duplicated their notes and, considering the vast scale of their operations, and the relatively primitive methods available, this would hardly have been possible. So they had to rely on 'shuffling' and accept the risk of vagrant slips. Nevertheless there can be little doubt about the greatness of the books produced by these technically rudimentary methods. Their major works, said Tawney, 'stand out, amid the trivialities of their day and ours, like Roman masonry in a London suburb'.[34] While Clapham, reviewing *Statutory Authorities for Special Purposes*, said that their account 'can neither be summarized, because of its infinite variety, nor criticized, because of its complete originality'.[35] This is high praise from the two leading British economic historians of the day. How did the Webbs manage it?

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One can imagine the method working satisfactorily where the subject-matter of the enquiry is clearly limited and the research is focused on some specific aspect of it. It could, as they themselves say, be used by ordinary intelligent and industrious people to produce monographs (see below, p. xxxi). What is so staggering about their own use of it is the scale of their operations and the immense volume of material which they were able to handle with what Tawney called 'new standards of comprehensiveness and precision'.[36] One must remember, however, that in their local government studies chronology, geography and the type of institution provided some easy bases for preliminary classification, so that they could, to some extent, deal with one thing at a time. In the co-operative movement they had a fairly homogenous subject, but not so in trade unionism which was marked by sharp differences of structure, function and policy. It was hard enough to describe and classify these differences, and harder still to try to explain them, as they did in *Industrial Democracy*.

They did not, of course, rely solely on the 'shuffling' of 'unit-fact' notes on quarto sheets of paper. When they had completed a research operation, like the study of a series of documents or the observation of an institution at work, they would often make an immediate summary report on it, and they found these reports very useful when they were writing the book. Then

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they had an index – a copious one, by all accounts, but not very efficiently compiled. One of the oddest features of this book is that it contains no reference at all to the role of an index in the handling of data, a striking omission. We must then add the personal qualities of the authors – Beatrice's dedication, energy and endurance (in her younger years) and Sidney's fabulous memory. Galton, their first research assistant, said of their system of note-taking, filing and retrieval that it 'worked quite satisfactorily on the whole, but could not have done so without Webb's remarkable memory'.[37] Nevertheless there can be little doubt that slips did sometimes go astray, or get overlooked, in the course of analysis by the 'shuffling' of pieces of paper. But, with such an immense volume of evidence to draw on, it is unlikely that defects of this kind would have any significant effect upon the conclusions. What is quite certain is that they took infinite pains to correct errors when they discovered them and to improve their analysis when they saw how this could be done. On one occasion they decided to re-sort all their data about the Parish, and Beatrice wrote: 'I shall index the whole of the material under the four new heads.'[38] which must have been a formidable task. On another occasion they made 'an uncomfortable discovery' – they had failed to take account of some important sources of information. 'It is not too

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late, though troublesome, to remedy it', wrote Beatrice. 'But I shudder at the thought of how bad our work was three years ago. We ought to have known better.'[39]

In the Preface to *Industrial Democracy*, after setting out the basic principles of their methods of investigation, the Webbs went on to speak of the uses to which they could be put.

> By the pursuit of these methods of observation and verification, any intelligent, hard-working and conscientious students, or groups of students, applying themselves to definitely limited pieces of social organisation, will certainly produce monographs of scientific value. Whether they will be able to extract from their facts a new generalisation applicable to other facts – whether, that is to say, they will discover any new scientific laws – will depend on the possession of a somewhat rare combination of insight and inventiveness, with the capacity for prolonged and intense reasoning.[40]

The point they are making here is that both these products of the scientific method are of value, and that both are, in their different ways, scientific. But they themselves do not fit neatly into either category. Their 'sociology' was not of the kind that is directed to the discovery of new generalisations 'applicable to other facts', nor could one properly describe their works as the monographs of conscientious students.

In *Methods of Social Study*, written thirty-five years later, there is a similar passage, but with a significant difference. It comes (p. 41) just after they had insisted that the investigator must not start with a question, but by choo sing his subject

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