Table of Content:

Symposium "Personal Encounters with Serendipities"
Erkki Berndtson, Political Scientist as a Historian 1
Charles Crothers, Serendipitious Interpretation from a Fresh Empirical Occurrence 3
Till Düppe, Serendipity in Writing the Lives of Scientists 7
Raymond M. Lee, A Journey to the History of the Interview 11
Margaret Schabas, Serendipitous Moments in My Early Career 19

Articles
Jennifer Platt, Where is the Boundary Between Sociology and not-Sociology? 21

Forum
Peter Burke, Comparative History and Comparative Sociology 82
Andreas Hess, An Operation Called Comparison -- Some Critical Observations 89

Book Reviews
Mark Solovey and Hamilton Cravens (eds.), Cold War Social Science, reviewed by Matteo Bortolini 93
Dirk Kaesler, Max Weber and Jürgen Kaube, Max Weber, reviewed by Hans Henrik Bruun 96
John Holmwood and John Scott (eds.) The Palgrave Handbook of Sociology in Britain, Kirsten Haley and Gary Wickham, and Bryan Fanning and Andreas Hess, Sociology in Ireland, reviewed by Charles Crothers 101
Fernanda Beigel (ed.) The Politics of Academic Autonomy in Latin America, reviewed by Sari Hanafi 110

Material
Handbook of Indicators of Institutionalization of Academic Disciplines in SSH
Editors
Peter Baehr (Lingnan University, Hong Kong),
Fernanda Beigel (Universidad Nacional de Cuyo, Mendoza, Argentina),
Christian Fleck (University of Graz, Austria),
Andreas Hess (University College Dublin, Ireland),
Laurent Jeanpierre (Université Paris 8, Vincennes-Saint-Denis, France)
Olessia Kirtchik (National Research University, Higher School of Economics, Moscow, Russia)
George Steinmetz (University of Michigan, USA)

Associate editors
Matteo Bortolini (University of Padua, Italy)
Marcia Consolim (Universidade Federal de São Paulo, Brazil)
Christian Dayé (Alpen-Adria University Klagenfurt, Austria)
Verena Halsmayer (University of Lucerne, Switzerland)
Daniel Huebner (University of North Carolina at Greensboro, USA)
Albert Tzeng (Institute of Southeast Asian Studies, Singapore)

Book review editor:
Kristoffer Kropp (Roskilde University, Denmark)
Political Scientist as a Historian

Erkki Berndtson
erkki.berndtson@helsinki.fi

Social science theories are few and those we have are based on assumptions that are difficult to verify; Kuhnian revolutions are hard to find in the social sciences. We tend to develop new perspectives for research instead. These perspectives, however, are not always due to critical thinking, as we are often directed towards new ideas by accident rather than by consciously chosen theoretical work.

This has certainly been the case in my own research. I started my academic career as a methodologist in the 1970s. Soon after, however, I became interested in the history of political science, especially the history of American political science, which was still a hegemonic discourse in political science at the time. I read theoretical and empirical literature of the leading, and not-so leading, political scientists. I wrote about theoreticians, from John W. Burgess through Charles E. Merriam to David Easton, and I read empirical studies from The American Voter to Who Governs?

In 1983, I received a Fulbright grant to study the history of American political science at the University of Chicago. One of the reasons for applying to Chicago was my interest in the old Chicago School of Political Science, but perhaps more important was my wish to discuss political science with David Easton. However, when I came to Chicago, Easton had just left for Irvine and I was assigned as a Visiting Scholar to the History Department instead. As one of its senior members, Barry D. Karl had written a book about Charles E. Merriam. I was so happy to receive the Fulbright grant that I did not want to protest about my placement among historians!

When I met Karl, he took me into the University Library’s Department of Special Collections and suggested that I should take a look at Charles E. Merriam’s personal papers. When I started to read them I became fascinated with the picture that emerged. They contained Merriam’s correspondence with other scholars, with the people of power and money and letters from citizens while Merriam served as an alderman in the Chicago City Council as well as manuscripts, memos and organizational records. The only problem was that there were over 300 boxes of papers, which meant that during my time in Chicago I did not read much else. I had become a true historian doing archival research. I even went through the papers of Quincy Wright and Leonard D. White as well as those of scholars in the neighboring disciplines, such as Albion Small, Robert Park, T.V. Smith, Louis Wirth, George Herbert Mead, William Ogburn and Ernest Burgess.

I believe that my placement in the History Department changed my research perspective and eventually enable me to do deeper work than if I had continued my original focus solely on theories and approaches in the study of politics. In addition to the effect on my work, my archival research helped me to become a member of a small group of scholars, who, on the initiative of David Easton,
formed the *International Committee for the Study of the Development of Political Science* in 1985. So I met David after all!

The second transformative period for my studies of the history of political science began in 1999. At this time I was asked to become a member of the Steering Committee of the *Thematic Network in Political Science*, which later developed into *European Political Science Network (epsNet)*. One of the main objectives of *epsNet* was to focus on teaching in political science. I served the two networks from 2000 until 2005. As a member of *epsNet’s* Executive Council and its Co-ordinating Committee I came across problems arising from the Bologna Process about the agenda of European Higher Education. Through this work, I became more aware of how scientific disciplines have been defined by the structures of Higher Education institutions. Chicago had made me a historian, but *epsNet* made me a historical institutionalist. I also began to understand the immense variety of European political studies; how different cultural conditions still frame political science as a discipline.

My personal intellectual development has been heavily influenced by Chicago and *epsNet*. However, although I wanted to go to Chicago, my stay there turned into some other than what I had planned. *EpsNet*, on the other hand, was a totally unplanned project for me. I believe that social scientists are like driftwood. Nevertheless, as Machiavelli wrote in *The Prince*, “it may be true that fortune is the ruler of half of our actions, but that she allows the other half or thereabouts to be governed by us”. He compared fortune with a river, which can be furious and destructive, but when it is quiet, people can prepare themselves to defend against its future turbulences. It is the same with social sciences. One should consciously plan her/his research during times of quiet reading and thinking. However, one should not remain a prisoner of her/his theoretical models and approaches when new opportunities arise; one can find new only through the unexpected. For this, it is good to let the river take you to unknown territories.

Erkki Berndtson is an independent research in political science and lives in Helsinki, Finland.
Robert K Merton (RKM henceforth) defined serendipity as the event of observing an unanticipated, anomalous and strategic datum which becomes the occasion for developing a new theory or extending an existing theory. As with much of his sociology of science, a slew of examples from the natural sciences are outlined. However, there is a paucity of social science examples. It might well be expected that the social sciences (and even more the humanities) are infrequent amongst such examples since it is the disciplined reaction of the discoverer which is crucial in their creation and it is quite possible that the social sciences lack sufficiently precise frameworks for interesting recognition to occur. Merton points this out in his ‘Note on Serendipity in the Humanities’ (Merton, 2004: 223-229). My short contribution to Serendipities provides a case study of a serendipitous occasion, in a social science context, to complement RKM’s concentration on natural sciences. It also discusses technologically-enhanced serendipity-seeking.

It is important to pin down the ways in which the social sciences and humanities differ from the natural sciences in terms of the potential operation of serendipity. Merton says (2004: 223) that

Though collectors [of items of literary and historical interest – e.g. as in archive] and literary scholars …have to be prepared to make accidental discoveries – they must know in a general way where to look and what to look for – their stock of knowledge does not have the systematic quality of science, and the fact of their preparedness may, consequently, be less visible. Also, the nature of the happy accidents that befall them consist, frequently, of unexpectedly locating a desired item or of the unhoped for anticipation of others in the recognition of a valuable item; the human drama of such events may serve to conceal … the knowledge and effort necessary for making the discovery.

So, the key point is finding further material with which to stretch and fine-tune a theory or its statement of the conditions under which it operates, rather than to generate a ‘freshly-minted’ full-blown theory; moreover, the creative moment (cf. Koestler, 1964) is more likely to be muted.

I’ve been puzzled for some time (probably a few years) about the double-word term ‘the science’ as used in phrases such as:

- What is the science on dolphin reproduction?
- Do we have enough science to develop a plan for fish production?
Then I happened to hear someone at a political science conference that I was attending say ‘the literature on this says XXX’ and the story of these linguistic usages snapped into place. I realised that this was a synonymous-meaning phrase that I was more familiar with. I am not sure why my mind made the link at that time, since it was not at all obvious, but perhaps it arises from a cultivated mental habit of trying to place matters alongside each other and to spin out the consequences of co-locating them in my mind. I am much more familiar with the latter usage, and feeling ‘at home’ with it, I was then able to reach out and comprehend the other term. What follows is an analysis in a more developed form.

Both usages are more jargonistic, narrower versions of the common-language inquiry ‘what do we know about X’?. This usage is appropriate across a wide range of settings.

By asking what is ‘the science’? An applied setting is more likely to arise, especially where one can draw policy implications of scientific knowledge. The term implies a knowable body of knowledge about some portion of nature. Behind it lurks a hard-edged meaning; the science is seen as bounded, finite and existent. It suggests the possibility that the quantum of knowledge might be measured. However, the efficacy of the science can be qualified against what needs to be known - so gaps still requiring to be filled might be identified. The amount of knowledge to which this points is not disciplinary or field-specific, but rather an amalgam of whatever parts of science are relevant. It does, however, often exclude the social sciences, unless specifically mentioned and might, in some cases, include social knowledge. The science is animate, it has a voice and is almost ready to come to life. However, the location(s) in which this science resides are not specified, but left hanging. Presumably, appropriate scientists can be consulted who will [can?] provide this knowledge. It exists in the minds of the network of scientists with expertise in a particular [given] area, although on its own the phrase is unclear as to where the body of knowledge actually lies beyond this broad image. In contrast, the literature is more inert, emphasising where the science is located; in a library (or possibly in an electronic storage system). While it may be possible to get this literature up and running, clearly it will take more effort to do so. There does seem to be a tendency to site ‘ready for action’ in the minds of the networks with access to this knowledge. Within this, there lurks the idea that such science might reside in an underlying scientific literature, presumably sitting within a web of journal articles. However, it may also be possible to convey what ‘the science’ is through summary treatments, especially diagrams or other visual forms. ‘The Science’ is seen as possibly arguable, but generally sanctified; it is stamped with authority that flows from its carriers.

‘The science’ (with a small ‘s’) contrasts with the more general use of ‘Science’ (with a capital ‘S’), which is a far more general reference to an institutionalised activity. The image of science is – to some extent – populated with people; white-coated lab assistants in lab settings and bearded, thoughtful, pondering professors. It is a distanced image.

In the social sciences (and perhaps the humanities) it is more common to refer to ‘the literature’. Phrases might include asking a postgraduate thesis student about to embark on research what is the literature in their chosen area. This knowledge is much more ‘physically’ located in a set of journal articles and books, whose configuration can be described. The implication is that the specific knowledge remains rather more ‘distributed’ across this array of more general knowledge and that summative versions cannot be readily wrought, this being the task of a scholar in developing their specific literature review of a given area. The body of knowledge is more inert, not so likely to provide a springboard for policy consideration. In contrast to what I have said earlier about ‘the science’, ‘the literature’ implies a more scholarly mien; inhabiting libraries, combing
through stacks of journals and building up a somewhat personalised account of what is important with an emphasis on diversity within the field of knowledge and within particular summaries deriving from this.

These differences in usage seem appropriate in terms of what we know about these different bodies of knowledge. In ordinary life, knowledge can be acquired from a variety of sources. ‘The science’ refers to a more active and sanctified portion of applicable knowledge, whereas ‘the literature’ implies a more passive compilation. ‘Science’ (or its more extended form ‘Knowledge’) provides the more general setting.

My second point concerns the environments, (a crucial point in Merton’s self-exemplifying exegesis in his afterword – 2004), in which serendipity might occur and in particular the effects of technological change within them. Google Scholar has as its permanent ‘header’ the injunction ‘Stand on the Shoulders of Giants’, the provenance of which Merton spent so much time eliciting. But google (and other search engines) are serendipity generators of a high degree, to a point where the unintended material adduced is both overwhelming and annoying. There is even an ‘I’m feeling lucky’ option, which is purely random.

It was interesting to find this conception of a serendipity-machine spoken of by someone participating in a focus-group study on the internet – the respondent was asked to depict their use strategy. The response was:

I think I’m a functional explorer – because on my use of the internet there’s really practical purposes just to make my life easier, paying bills and banking and all the kind of stuff, but I actually use it a lot for research and exploring possibilities, learning and like sometimes like the serendipity of start with a link and click in another link and click in another link and see where it takes you, and that way you end up on YouTube, or on somebody’s blog or you end up in multiple places – so I really like that kind of exploring kind of thing, for fun and learning and then for practical purposes, like if you’re planning a holiday and then you can figure out where you’re going to go and book the best hotel.

This is a more technologically sophisticated version of Glaser & Strauss’s (1967) methodological admonition to see libraries as a similar serendipity-machine:

..the library researcher cannot help but stumble upon useful comparative data. He (Sic) is checking through the Readers Guide on one topic, when happily his eye lights on another relevant topic about when he never thought – or he wonders about an article with an intriguing title, and in checking it finds marvellously rich data. He ransacks books strung along several shelves, and not only finds books – perhaps even more useful – either as he walks toward those shelves or allows himself to browse through books on neighbouring shelves. Or after reading a magazine article which he has tracked down, he allows himself sufficient time to riffle through the remainder of the magazine (ibid: 174).

Technology also provides us with entries in a range of blogs and other information sources which may yield quickly-harvested and interesting information. Wikipedia informs us that [serendipity] “... was voted one of the ten English words hardest to translate in June 2004 by a British translation company”, although it does not go on to inquire why this is so, and the reference seems to have vanished into the nether realms of ancient websites.
Despite this difficulty, the Wikipedia entry goes on to suggest that “...due to its sociological use, the word has been exported into many other languages” and then in a footnote lists: “For example: Portuguese serendipicidade or serendipidade; French sérendipicité or sérendipité but also heureux hasard, "fortunate chance"; Italian serendipità (Italian Dictionary Hoepli, cfr.); Dutch serendipiteit; German Serendipität; Japanese serendipiti (セレンディピティ); Swedish, Danish and Norwegian serendipitet; Romanian serendipitate; Spanish serendipia, Polish: Serendypność; Finnish serendipiteetti”. Again, the mechanism of the “sociology effect” is not spelled out – although perhaps it is an indication that sociologists in these countries (or more technically writing in these languages) have translated and then used the term.

Finally, we are informed that since “Serendipity is a key concept in competitive intelligence because it is one of the tools for avoiding blind spots” chance has been turned into a tool in a social technology.

Returning to Glaser and Strauss, (1967) they raise the methodological and theoretical implications of the concept, which Merton did not revisit after having posited the methodological role of the serendipity pattern in the mid-1940s. Glaser and Strauss (ibid: 2 ftnt 1) tartly distance themselves from Merton: “Merton never reached the notion of the discovery of grounded theory in discussing ‘the theoretic functions of research’. The closest he came was with ‘serendipity’....that concept does not catch the idea of purposefully discovering theory through social research. It puts the discovery of a single hypothesis on a surprise basis. Merton was preoccupied with how verifications through research feed back into and modify theory. Thus he was concerned with grounded modifying of theory, not grounded generating of theory”. And they have a point. Although their widely popular methodological practise was itself generated in part from Merton’s methodological concerns, they did add a further more active element.

Finally, the theoretical circle needs to be closed. Throughout his career, Merton was fascinated with social structure. He complemented this with an interest in anti-structure or chance; the ‘flip-side’ of his structural interest. Structure at a broad abstract level concerns the reduction of chance and resilience (or lack of resilience) in taking advantage or coping with opportunistic events. So, one can see that this whole area of discussion relates back to the heart of Merton’s sociological concerns.

REFERENCES:


Charles Crothers is Professor of Sociology in the Department of Social Sciences at Auckland University of Technology, New Zealand.
Serendipity in Writing the Lives of Scientists

Till Düppe 
duppe.till@uqam.ca

When writing the life of Gérard Debreu, a celebrated and multi-layered mathematical economist, I encountered two cases of Serendipity. First, when Debreu spoke about his discoveries - a perspective that I could hardly neglect - he repeatedly spoke of a series of chance events, as though he never actually wanted to discover anything. It would not have been possible to say much more about what really happened to him without the chance events that his personal archives were recently opened at the Bancroft Library in Berkeley, and that I got in touch with a cooperative daughter, who had more to say about her father than he had said himself.

These two cases imply two questions: When writing the lives of scientists, how much should we allow our narratives to be infused by luck and chance? And what would be the consequences for the method of life writing?

***

The first question might seem trivial. It is palpable that a convincing account of a scientific life must avoid the cheap determinism of the ‘inborn genius’ which slowly but surely becomes manifest through great discoveries. Biographical determinism is at odds with the idea of the creative encounter with the novel that describes scientific discoveries. The ability to make ‘lucky discoveries’ while looking for something else, as Merton viewed Serendipities, has nothing mystical to it; it requires no more than a generalized alertness to the unexpected. The genius with the privileged gift of receiving truth is a character that better sits in writings about saints with mystical abilities than in science biographies. The genius does not help us understand science; on the contrary, it removes it even farther from ordinary life. Perhaps it is for this reason that life writings are often seen as an uncritical genre in the philosophy of science.

While avoiding overly predictive narratives, some chance events must be employed if only to give some value to context. The contingency of finding oneself ‘in the right place at the right time’ is at the heart of narratives that reveal the historical specificity of discoveries. Serendipity is thus an essential element of any historical narrative insofar as it gives value to the historical situation in which prima vista unrelated social domains are brought together in surprising ways. In science – precisely because it is supposed to be independent from other departments of cultural life – there are often surprising links to social, political, or moral orders that give occasion to scientific adventures. Moreover, if one wishes to avoid anonymous social structures, it is through the lives of the scientists that these surprising links can be described in experiential terms.

While it is well known that the historical contingency of such narratives contrasts with the idea of the scientific method, another aspect of Serendipity is essential to the modern image of science. Considering the places from which knowledge can be seen to arise, serendipity appears at the very root of its fruit: the spontaneity of truth. Truth must entail a spontaneous force in order to defy that which is the mere will of the scientist. Truth is that which is not merely made, but which shows itself, which surprises us, and which resists beliefs and hypotheses. Insofar as the spontaneity of truth-bearing beings is the defining element of realist sentiments in science, one could even go so far as to say that only that which one ‘discovers by chance while looking for something else’ can be true. It is the stumbling scientist without mission, a daydreamer searching for nothing in particular, who would be the most worthy of trust. The notion of serendipity would, in fact, be elementary for the modern production of knowledge; it is a symbol of the a-subjectivity of knowledge.

Serendipity as a symbol of modern knowledge takes different meanings in different disciplines. Consider mathematics and economics, the two disciplines Gérard Debreu combined. While the reasoning of mathematical proofs is fully controlled by logic, the Eureka moment of conceiving them remains a mystery. Since there are no other senses than our contemplation that create a context through which mathematical ideas can wander, the gap between the control of reasoning and the lack of control of discoveries is greater in mathematics than in other disciplines. Serendipity is thus a notion that comes naturally to a mathematician. The same applies to economics, but for a different reason; economic science always travels under a cloud of suspicion, the suspicion of ideology (Düppe 2011). Reference to serendipity in the context of economics might thus be viewed as a response to those naggers who see only intentions, strategies, and personal interests. Therefore, it comes as no surprise that economists were keen to turn chance into a method; stochastic models “tame” chance, to recall Hacking’s (1990) ground-breaking study on Serendipity. Economic regularities can only be serendipitous discoveries, as long as one prefers to avoid the specter of ideology.

These serendipitous constituents of truth cannot be ignored when writing about the lives of scientists because they are omnipresent in the self-descriptions of scientists. Modern truth urges an image of the scientific self as being somewhat a-intentional, if not absent. When reading the self-accounts of the lives of scientists, it is striking how often the decisive moments of discovery are presented as chance events. Robert Merton’s manuscript bears witness to how widespread this self-perception of scientists can be. In chapter eight of his manuscript, Merton considers the moral persona that corresponds with this self-image (2004: 149 ff.). The scientist tends to view his life as a random walk, which was indeed the title of Gérard Debreu’s autobiographical essay:

As a particle performs a random walk in a high-dimensional space, an observer may discover a subspace in which the projection of its path approximates a straight line. The observer may then be tempted to anthropomorphize the particle, and to believe that it has a ‘system which a person forms for the conduct of life’ [Debreu quotes the “philosophy” entrance of a 1909 dictionary]. In an inversion of roles, a scientist or humanist who is asked to expound his life philosophy must feel inclined to identify with that particle if he is aware of the many chance events that shaped his career, and of the inchoate system that he formed for its conduct as it began (1991b: 3).

Note how the conception of scientific truth feeds back onto the perception of the self. Scientists tend to view their lives, like their work, as a play between control and lack of control; a play for which serendipity might be the most striking symbol (cf. Düppe 2012).
When writing about the lives of scientists, we face an old philosophical problem: in order to understand how science springs from life we need to ignore the self-accounts of the scientist, or better, view them as symptoms of their acquired image of science. A material account of the knowing self — which is life-writing — must deal with the fact that the knowing subject does not know of those material conditions of knowledge. Is this a case of false consciousness?

Not entirely. The scientist does in fact know of his or her own involvement in science: he or she enjoys, and also suffers from it! When speaking of Serendipity, we should not only speak of the role of chance in the making of discoveries, but also of the role of affect: it is the happy, and fortunate event of discovery without searching for it. The absence of the scientist’s intentions is the liberation of a joyful play of truth that is otherwise buried by the protocols of the scientific method. The value of the notion of Serendipity for the writing of the lives of scientists is that it makes us aware of the affective level of the joyful play of truth.

***

What are the implications of the above for how we reconstruct the life of scientists; for the method of life writing?

If it is true that the rhetoric of chance is part of the self-image of scientists, they leave fewer traces of their lives than other professionals. Scientific writings are opposed to personal writings. Gérard Debreu has always avoided speaking about himself, even to his wife. Trained to think of the world rather than themselves, few scientists write intimate diaries. Historians must therefore rely on some luck when searching for relevant sources that reveal their motivations. In the case of Debreu, I was fortunate enough to establish contact with his daughter, Chantal Debreu, who wished to counterbalance her father’s secrecy about his own history. She spoke more openly about her father than he ever could have.

There is another problem when it comes to the witness of friends, colleagues and family. In contrast to the person who is the focus of study, others tend to be protective of their friend, father, mother, husband, wife, etc. and wish to see a narrative written that might resemble that of the genius mentioned earlier. The genre of life-writing is an integral part of the system of credit and reward in science (Hankins 2007). Chantal Debreu did in fact attempt to demystify the image of the genius and thus was crucial in providing evidence about the role of his work in his efforts to live a good life.

These and other obstacles make it difficult to anticipate the process of discovery or apply a rigid method of life-writing. The luck of being the first who sees an archive, of finding a telling letter exchange or a key contact that knows about the persona of the scientist makes life-writing an adventure of serendipity. But what is the difference between this sort of serendipity of the historian and that of the scientist? As much as one might experience luck, note that a science biography can never result in an image of a-subjective truth. Yes, biographies such as Skidelsky’s Keynes or Monk’s Wittgenstein give an air of definitiveness, but there will always be things to add or reconsider. A life cannot be told in terms of spontaneous truth that reveals itself in its entirety to the historian; writing the life of the scientist remains an encounter of two people.

Moreover, this encounter between the historian and the scientist is an equal encounter to the extent that neither the historian nor the scientist have the last word over what is to be said about the meaning of science in his or her life. By understanding the many modes of a knowing life, we might
also better understand the possibilities that we ourselves might seize upon. It is for this reason that the biographer Thomas Söderqvist has called science biographies an ‘edifying’ genre (Söderqvist, 1997).

Many historians consider that historical sources, once exploited by one historian, are no longer interesting. This is false. The serendipity of priority does not exist in history! It grants too much authority to those who are fortunate enough to win the race to newly opened archives, or are first to hunt down those who keep the sources. Historical research is personal, and open to many narratives.

REFERENCES

Till Düppe is Assistant Professor at the Department of Economics, Université du Québec à Montréal, Canada.
When I was 9 years old, my parents gave me a book about ancient Egypt as a birthday present. I read it avidly, although now the only thing I can remember about it is that it had a brown cover embossed with red and black Egyptian hieroglyphics. I was soon telling anyone who would listen that when I grew up I wanted to be an historian. It didn’t quite work out that way; I became a sociologist instead. Entering a shrinking academic labour market after graduate study I became a ‘research bum’ to use Howie Becker’s phrase (Molotch 2012), working over a number of years, mostly as a survey researcher, on a succession of short term research contracts. The focus here was relentlessly short-term and instrumental. Later, teaching research methods to undergraduate and postgraduate students I remained focused on the current and the technical. A turn towards studying the history of the interview would probably have struck me at this time as redundant and improbable. Yet, it was the topic that became a central focus for me as I moved towards the end of my career. 

Now ar mhuin na muice (literally ‘on [the] back of the pig’), the Irish phrase that denotes that one is retired or living at one’s ease, I can tell my 9 year old self that I fulfilled at least part of his ambition.

Having decided to pursue a PhD in sociology my first thought, for various reasons, was do a thesis on Church-State relations in Poland¹. In the end, however, that topic posed too many logistical and political difficulties to be feasible. I had to look for something else. The ‘Troubles’ in Northern Ireland had recently erupted and, since I was born there, I began to think that I should turn my attention to a topic related to the conflict. Unfortunately, I couldn’t think of anything suitable. What caused the eventual topic to emerge was a serendipitous encounter. The morning after some riots in Belfast I encountered a small group of women on the edge of a Catholic area in the city who were surveying the remains of houses burnt down the previous evening. One of the women commented that the couple who had lived in one of the houses were in a ‘mixed’ (i.e. interreligious) marriage. According to the women, the house had been singled by a Protestant mob intent, so the women thought, on punishing the husband for taking the Catholic side in deference to his wife. To them, this revealed the iniquity of the mob since they reasoned that the husband couldn’t possibly have sided with the Catholics since, after all, he was a Protestant. I began to think it might be interesting and enlightening to see how intermarried couples dealt with the tensions and conflicts inherent in a divided society.

¹ My interest in Poland arose as a by-product of well-developed links I had to the jazz scene in Warsaw. These links were facilitated largely through the good offices of the now deceased music journalist Stefan Zondek.
I did my PhD on interreligious courtship and marriage in Northern Ireland at the University of Edinburgh. I won’t dwell on the specific field experience to any great extent. (For more detail on the fieldwork and how I dealt with the immediate challenges involved, see Lee 1992.) What I will focus on instead are some of the lessons I drew, not always very consciously, from the research. I learnt many things in the course of producing the thesis. Most of them have probably been forgotten or faded in significance with the passage of time. A small number of issues have resurfaced, however, from time to time since, often in unexpected or serendipitous ways. One such issue relates to the analysis of data from unstructured interviews. I had interviewed in depth a sample of intermarried couples and transcribed the recordings of the interviews. Now I had to deal with a large volume of unstructured data. Over and over again I felt I was drowning in words, drowning in paper. I tried different ways of dealing with the problem, but nothing seemed to work. It was only, idly doodling one day, that I realised I could graphically represent the experiences the couples I studied had undergone as a set of linear or recursive trajectories. From this I was able finally to gain an analytic handle on the interview material, and was able to go back to the interview transcripts with something like a sense of direction. As have many researchers before and since, I came to think there “had to be a better way” to analyse qualitative data (Fielding and Lee 1998).

Probably not unrelated to this I began to question the centrality of interviewing as a research method within the social sciences. One reason for this, I suspect, is that I wasn’t a very good interviewer. Often I would look at a transcript and think “Why didn’t I ask the obvious follow-up question at that point” or “Why did I let [the interviewee] ramble on about that topic when it was unlikely to lead anywhere.” I also suspected in some instances that I was being presented with a facade of harmony that hid a degree of conflict and difficulty within a couple’s relationship that I could not dismantle. The upshot of this is that I developed what was to become something of a long-standing interest in the interactional dynamics of the interview but one embedded within a rather ambivalent view of the method. The interview in a way became for me something of a ‘false friend’; useful and helpful on the surface, but potentially treacherous underneath. A further factor in my view of the interview related to its social acceptability in certain situations. In Northern Ireland at the time I was working there it could be hazardous to ask questions or to appear too inquisitive (see Lee 1995). In this situation it became necessary to find other routes to achieve one’s research aims. Not surprisingly, then, I became very taken by some of the arguments in Webb et al’s classic (1966) book Unobtrusive Measures about the deficiencies of self-report, the use of multiple methods, and the need to find creative solutions to research problems.

I developed two particular interests in relation to unobtrusive methods. One had to do with the use of ‘institutional discovery’ methods (Marx 1984), that is research based on documents that become available through investigative, judicial, or legislative procedures such as investigative commissions and tribunals, court cases, and material obtained under freedom of information legislation (see, e.g., Lee 2001; 2005). My second interest in this field related to what can be called the ‘generative problem’ in studies using unobtrusive methods (Lee 2000). Beyond the injunction to be ‘creative’, Webb et al gave little guidance on how sources of unobtrusive measures could be identified for particular research problems. I began to wonder if there might not be a range of heuristic strategies that could be used in such circumstances. What I had in mind has some affinities with aspects of Becker’s (1998) work on ‘tricks of the trade’ and probably has some relevance to the creation of fruitful environments for serendipitous discovery (Merton and Barber 2004, 200-2), but for one reason or another the idea is one I have never been in a position to develop further.
While doing my thesis I obviously read the existing literature on interreligious marriage, although I found it not to be overly useful. This was because most of the then existing literature tended to take intermarriage as an indicator of the assimilation of immigrant groups. However, assimilation was hardly an issue in Northern Ireland. Since intermarriage was relatively uncommon and generally frowned upon, I turned instead to the literature on deviance. One of the books I read was David Matza’s *Becoming Deviant* (1969). The book is written in two parts and, although it is one of my favourite books, I must confess I’ve only ever skimmed through the second part. What I was struck by was how in Part I Matza picks apart the assumptions behind a number of different sociological conceptions of deviance. In particular, I was taken by his comments on the romanticising impulse in the Chicago tradition. Later I read Alvin Gouldner’s much neglected article on romanticism and classicalism in sociology (1973), and later still Colin Campbell’s *The Romantic Ethic and the Spirit of Modern Consumerism* (1987). In a strange way, reading this material helped me make sense of something that I found slightly puzzling. On visits back from the field I became aware that people often thought that doing research in Northern Ireland was a brave thing to do. But I never felt brave, quite the opposite. I often felt scared or apprehensive, as did most other people in the same situation. Talking to field researchers then and since and reading a number of first-person accounts of the fieldwork process, I seemed to detect in the way some researchers described themselves an implicit image of the fieldworker as an existential hero going out to do battle with the world. Far from ubiquitous but frequent enough to be noticed and often a matter of tone as much as anything else, this made me wonder about how fieldworkers’ constructed their own self-images, and in particular how far such self-images might be suffused by romanticism and heroicism. While, again, this is not something I have pursued in any detail since, at the time it did encourage me towards an interest in the sociology of sociology.

In the 1980s, a number of scholars in the United Kingdom embarked on work that re-evaluated the Chicago tradition in sociology. Martin Bulmer (1984) marshalled an impressive range of material to produce an institutionally-oriented account that emphasised both the breadth and depth of Chicago sociology. More polemically, perhaps, Harvey (1987) set out to undermine what he saw as the myths that had grown up around the Chicago School. In a number of articles Jennifer Platt (1983; 1985; 1994) challenged the assumption that participant observation as a method understood in the modern sense was one associated with the urban studies conducted at Chicago by Robert Park and his colleagues, and questioned the extent to which Max Weber had been an important influence on Chicago sociologists. Bulmer’s work, in particular, broadened and deepened my knowledge of Chicago but it was Platt’s writing that I experienced as serendipitous².

Platt suggests that anachronistic readings of the history of Chicago sociology provide contemporary researchers with an origin myth linking their methodological preferences to those of a revered, but mythical, ancestor. Some years earlier I had chanced upon a used copy of the *Introduction to the Science of Sociology*, the ‘Green Bible’ authored by Park and Burgess (1921), and had been struck when reading it by what seemed like a lack of congruence between it and contemporary methodological preoccupations conventionally presented as a legacy of Chicago. While thinking this was odd, I had never followed up the idea and tended to assume that if I couldn’t see a connection between past and present it was because I was too stupid to do so. Platt’s work was gratifying in the

---
² According to Merton and Barber (2004), the term ‘serendipity’ was coined by Horace Walpole in 1754. In a nice coincidence, I read Platt’s article on participant observation within sight of Strawberry Hill House, the house Walpole built that is now regarded as one of the finest examples of Gothic revival architecture in Britain. Recently restored, it is now open to the public. [http://www.strawberryhillhouse.org.uk/about.php](http://www.strawberryhillhouse.org.uk/about.php)
sense of rescuing me from this assumption, but it did more than this. It had never really struck me before that research methods were not timeless givens but practices embedded in specific historical contexts. Nor had it occurred to me that disciplinary histories could be seen as being socially constructed. The sense I had taken away from my thesis that methodological self-images were important could begin to be seen in a more interesting light as something that fed off and into the kind of origin myth Platt had identified. This was the point at which I began to take seriously the history of social research as an area of study.

That was a road I didn’t take, however. I did produce a number of books inspired by my thesis experience, *Doing Research on Sensitive Topics* (1993) and *Dangerous Fieldwork* (1995), but I also became involved in work on the use of computers in qualitative research. My original interest in this had been sparked by another serendipitous encounter. At a meeting of the American Sociological Association I encountered Renate Tesch. Originally from Germany but settled in California, Renate (who also called herself Renata) had committed herself to popularising the use of computer software packages as tools for analysing qualitative data. Given the problems I had analysing my thesis data I was rather receptive to some of her ideas and, although initially I didn’t take matters much further, this was soon to become highly consequential. Nigel Fielding, a well-known qualitative researcher at the University of Surrey had been asked to organise one of an annual series of methodology conferences at Surrey. When in the course of a conversation with him I mentioned Tesch’s work, the idea of a conference on qualitative computing soon emerged. Our fears that the conference would be of marginal interest were unfounded by the huge amount of interest it generated. Out of this was born the CAQDAS Networking Project. Funded over the years by a variety of grants from the UK’s Economic and Social Research Council, the project provides courses and workshops on computers in qualitative research as well as maintaining a highly popular online forum, the qual-software list. (CAQDAS stands for Computer-Assisted Qualitative Data Analysis. The pun on ‘cactus’ is deliberate. In the beginning, for many qualitative researchers computer use was a ‘thorny’ issue.)

The CAQDAS Networking Project was in a sense a very forward-looking enterprise, which was open to new ways of doing things especially as the opportunities and challenges opened by the Internet became increasingly apparent. However, a basic tenet of the project was that researchers should not simply embrace or reject the use of new technologies in research but explicitly and critically assess their methodological and epistemological implications. As a result I became quite attuned to the sometimes subtle ways in which technological affordances affected research practice. In this context, it was probably not surprising that my eye was drawn to an observation made in another paper by Jennifer Platt. In an article on the history of the interview Jennifer observed that “research on the consequences for practice of changing techniques and technologies for the recording of free answers is strikingly absent” (2001: 41). When a conference at the University of Sussex on ‘The History and Practice of Sociology and Social Research’ was announced to mark Jennifer’s retirement, I decided that it would be appropriate and a fitting personal tribute to Jennifer’s contribution to the field to try to fill in the gap she had identified.

There is a fascinating interplay between interview practice in the social sciences and methods for sound capture that extend from the use of wax cylinders through to solid-state digital recorders, not to mention in earlier times the use of (usually concealed) stenographers (see Lee 2004). If I thought of it at all, I probably assumed that the paper I wrote on recording technologies and the interview was a one-off exercise. However, as I delved into the topic, all sorts of wider issues and questions began to emerge. Why did the interview seem to have low status as a method in Chicago.
sociology? How was it that social workers rather than sociologists were regarded as innovators in interview practice in the 1920s? How had nondirective interviewing moved from being a clinical practice to one widely used by social scientists? Why did theorists like David Riesman and Robert Merton take an interest in interview methodology? There was nothing for it but to try to find some answers (Lee 2004; 2008a; 2008b; 2010; 2011). There is no overarching plan to this work. The topic simply took on a life of its own. Nor is the work intended to be explicitly revisionist in tone. It is, however, broadly in line with the view that the history of qualitative research in sociology tends to over-emphasise historical continuities with what is in effect a mythologised past. This in its turn reflects my conviction that the commitment to reflexivity generally found in contemporary work inspired by Chicago sociology should imply a willingness to examine how that tradition has been socially constructed. That it often does not is implicitly connected I suspect to the kinds of romanticised and heroicised self-images I mentioned earlier.

Until the advent of audio and video recording it was extremely difficult to study the interview in actuality and in real time. Information about how interviews were conducted in the past has had to be recovered for the most part from prescriptive or programmatic sources, such as textbooks and training manuals, likely to present a partial or idealised view of actual practice (Platt 2001). The development of the interview has also involved a great deal of borrowing from one discipline to another, much of it unacknowledged or unattributed. Bearing these caveats in mind, patterns of methodological innovation and diffusion relating to the interview can be tracked to a degree through the journal literature. As a result, I have made much use of resources like the Social Science Citation Index, JStor and, for older material, sites such as the Internet Archive (https://archive.org/details/texts) and the HathiTrust Digital Library (http://www.hathitrust.org/). There are, of course, clear limits of this approach, something that became readily apparent when I began to look at the history of the focus group.

In a paper published in 1987 Robert Merton mused on the relationship between his wartime work on focused interviewing (see, e.g., Merton and Kendall 1946) and the growth of focus groups in consumer research, but noted that the topic had not been studied in any detail. I decided to try to trace the connections. This proved difficult because both the journal literature itself and norms relating to referencing and citation developed relatively late in the field of consumer research. Merton’s work, it seemed, had left no trail. I was frustrated by this until, idly searching the Web one day, I happened across a blog post that claimed the Edsel, a notorious and disastrous flop introduced by the Ford Motor Company in the 1950s, had been the first car to have been designed by focus group. In fact, this appears to be a myth (which might say more about public perceptions of focus groups than their historical development). It is the case that the Bureau of Applied Social Research conducted studies for Ford during the Edsel’s design phase (Brooks 2014), but these involved conventional survey and depth interviews with individual consumers rather than group interviews. What had seemed like an interesting and promising lead now looked like a dead-end until it occurred to me that it could be useful to look for other companies that might have commissioned work involving focus groups. This led me to a classic example of materials produced by institutional discovery, the Tobacco Industry Documents. Now totalling over 5 million documents, most of which are available online, the Tobacco Industry Documents came into the public domain as the result of litigation against tobacco companies once the link between smoking and cancer became

well-established. These documents have been widely used by public health researchers (Bero 2003), and although for various reasons difficult to use, proved to have much material relevant to the marketing of cigarettes in the 1950s and 60s when companies tried to shift away from their traditional market, working-class males, and towards women, young people, and ethnic minorities. The Tobacco Industry Documents included material documenting the introduction and growing use of focus groups, bridging the gap left by the absence of citation data.

The use of online resources raises quite broad issues for research on the history of the social sciences. Will we see, for example, a shift towards use of large-scale digitised archives at the expense of small-scale local archives? Or to put this more dramatically, towards the broad sweep at the expense of the detailed investigation of particular cases? Might the availability of citation indices and the ability easily to search long runs of journals encourage the production of more bibliometric studies? There are implications here for resource distribution, training, infrastructure, and what counts as expertise. (For an examination of how the growing use of textual databanks changed the nature of expertise in classical studies, see Ruhleder 1995.) More specifically, it is interesting to note that researchers in the digital humanities have for some time been worried about the extent to which existing tools and methods for searching online resources inhibit serendipity (see, e.g., Foster and Ford 2003). That concern in turn has encouraged information scientists towards the empirical study of serendipity and investigation of ways in which it might be encouraged, rather than inhibited, through the use of online tools (Martin & Quan-Haase 2014). What solutions will emerge and how useful they will be remains for the moment an open question.

REFERENCES


The major online repository for the Tobacco Industry Documents, is based at the University of California, San Francisco: http://legacy.library.ucsf.edu/


Raymond M. Lee is Emeritus Professor Criminology and Sociology at the Royal Holloway University of London, UK
When I became Head of my department in 2004, I was soon apprised of the fact that to hire well required the conjunction of opportunity and serendipity. Hiring does not just happen in a mechanical or pedestrian manner and serendipity without opportunity, or opportunity without serendipity, meant that hiring was far less likely to succeed. Opportunity is about doors being opened. The Dean has to agree to fund a position and the department has to agree on a field, a rank, and a procedure. Serendipity is about good turns of events falling from the sky without any effort on your part. Someone suitable has to apply and be ready to move, willing to accept the terms of the post, be acceptable to the department, the Dean, etc., etc..

I was lucky that my early career had a couple of serendipitous moments. In 1980, while in my second year as a doctoral student in the History and Philosophy of Science at the University of Toronto, I decided to write my dissertation on the Victorian polymath William Stanley Jevons (1835-82). I had already made the unorthodox decision to specialize in the history and philosophy of the science of economics and Jevons seemed like the perfect choice because in addition to his pioneering efforts to mathematize economics, he also wrote at length on the philosophy of science. I soon learned that he was a polymath, contributing to just about every field of science and even writing a treatise on music theory. My choice was bolstered by the fact that no one had yet written a book on Jevons (there are now at least four) and that Professor R.D. Collison Black of Queen’s University (Belfast) had spent twenty-odd years tracking down his correspondence and papers. And just as I decided to write the thesis, Black completed the last couple of volumes of his seven-volume set (1972-1981). I was able to purchase the books and make much use of the volumes on which the ink was still drying, so to speak.

Such was my brush with opportunity. The serendipity came in a different form. In the summer of 1982, I spent a few days in Manchester at the John Rylands Library, perusing the archival collection on Jevons (he had been a professor in Manchester in the 1860s). I made careful notes and returned home to Toronto. In one of my regular meetings with my supervisor, Samuel Hollander, I happened to mention the reaction of John Stuart Mill to Jevons’s efforts to popularize George Boole’s logic. He asked how I knew this and I mentioned a letter I had read in the Jevons archives. He jumped up and said that everyone had been looking for that letter, since the gist was known but the letter had disappeared.

In this way, my first publication fell into my lap. If it had not been for that exchange with Hollander, I might not have known the value of the letter. I was indeed writing about Jevons’s logic but I
might have missed the significance. I quickly wrote up a few pages of analysis of the exchange, gained permission to publish the letter, and hence I had my first article in The Mill News Letter.

But luck smiled on me again. The year after I finished my thesis a good friend, Judith Margles, called me from New York where she was pursuing a Master’s degree in Museum Studies. “Guess what,” she said, “a friend of mine, Stephen Novak, a student in history at NYU, has a part-time job in the archives at Seton Hall University. He mentioned that they had just spent $10,000 purchasing some letters by someone named Jevons. I remembered that you wrote your thesis on him. This may be of interest!”

I wrote to Black immediately and he then broadcast the find in the newsletter for the John Rylands Library. He was delighted to learn that the letters had been found, but also somewhat dismayed at the outright deception that had blocked them from being part of the published multi-volume set. He knew that the letters existed because some of them had been published in a collection edited by Jevons’s widow, Harriet in 18xx. Jevons’s younger brother Tom had moved to New York City and married into the prominent Seton family. That was the main reason the papers were purchased, although the archivist clearly knew that many of them (92 in total) were by a famous economist. Black had made a trip to New York to find the letters, and met with Jevons’s great nephew (Ferdinand Jevons), only to be told that they no longer existed. It turns out they were in Ferdinand’s house but he clearly had no intention of releasing them. He had no children and it is unclear what his motives were, but because there had been a hefty amount of mental illness in the Jevons family, Ferdinand may have wanted to protect the family name. In any case, I was lucky to make use of the new set of letters and all the more grateful that I have friends with good memories!

Margaret Schabas is Professor at the Department of Philosophy, at the University of British Columbia, Vancouver, Canada.
Where is the boundary between sociology and not-sociology?

Jennifer Platt
j.platt@sussex.ac.uk

Abstract

In work on the history of sociology, how may the boundaries of study be defined, and what requires explanation as part of sociology? Becker’s concept of the „art world” suggests thinking in terms of the „sociology world” which is needed to produce the sociological object. Three very different examples - Young and Willmott’s Family and Kinship in East London, a cross-disciplinary quantitative sociology study group, and Hodson’s use of amateur descriptions of workplaces as data - are discussed, and it is concluded that the practical methodological answer depends on the particular research topic and the resources available.

Keywords
Boundaries between disciplines; sociology, definition of;

Where is the boundary between sociology and not-sociology? This paper addresses the question put in relation to sociology, but it is equally relevant in principle to work on the history of other disciplines. The question is not raised to initiate a discussion of the correct definition of the „discipline”. Indeed, a serious effort is made to avoid that concept and the normative weight that it carries; boundary work is not being conducted here. The question addressed is a purely practical empirical one: for the historian of sociology, where does sociology begin and end, and what should be treated as the implications? It is noticeable that different writers make different implicit assumptions on this, most easily seen in whether or not authors from periods sometimes well before the existence of an institutionalised university sociology named as such are described as [really] sociologists avant la lettre rather than as predecessors – who may happen to be open to choice of them as ancestors for some parts of modern „sociology”, even if there is no traceable causal chain linking them.

A number of writers have done work which, explicitly or implicitly, raises questions about where to draw the boundaries of „sociology” when writing on its history. Two authors who have raised the issue explicitly are Howard S. Becker and Jean-Michel Chapoulie. Becker’s concept of the „art world” has had considerable currency, and has an obvious potential applicability to other „worlds“. What he does is to include in the account of artistic production all the cooperative activities
necessary to the appearance of the final art work which are carried out by others than the „artist“: „Works of art, from this point of view, are ... joint products of all the people who cooperate via an art world’s characteristic conventions to bring works like that into existence“ (Becker 2008: 35). He goes on to examine what the roles and conventions are for „art“, and clearly we can usefully emulate this for „sociology“. More recently, Chapoulie (2009) has advocated a much wider approach to the definition of the discipline in work on its history, one less confined to what is formally labelled as „sociology“, or the abandonment of „the discipline“ as the unit of study. He sees implicit hypotheses of „conventional history“, in which excessive personal familiarity with the field sets the trap of presentist approaches and disregard of causal factors not internal to the discipline as now defined, and sees the solution in a focus on the activities of the going concern(s) which produce sociological work rather than just on their products.¹

In this paper we look at some examples of sociological work which raise further questions about what may be involved in attempting to define the boundaries of sociology as an object of historical study. These examples have no claim to be representative; all that is claimed is that they show some ways in which the boundaries drawn around „sociology“ may seem to become questionable in historical work. The particular cases used were selected – or selected me - because they were ones which drew to my attention the issue of boundaries when I was working on them for other purposes. Additional examples would no doubt draw attention to further aspects of the issues, while in other cases those might be much less salient. One could envisage useful development of a typology of empirical situations, and research styles, where some work was and other work was not satisfactorily contained within disciplinary boundaries.

Considerable documentation is available on the book we look at first, so that we can learn a lot about its „sociology world“ – by no means all of which was „sociological“. This demonstrates some of the possibilities following from the location of research in its wider setting.

**Michael Young and Peter Willmott, Family and Kinship in East London (FKEL), 1957**

This book’s enormous success means that sales must have been made to many members of the general British public; at its publication academic sociology in Britain had not yet started its great expansion. Although the British Sociological Association had been founded in 1951, at its early conferences many invited speakers were still not sociologists but practitioners in areas of social policy. FKEL was concerned, in ways which fitted well into that context, with policies on social planning.

Stuart Laing (1986: 31) provides a useful summary of the cultural situation in the late 1950s, going somewhat beyond the research community:

(T)here was ... a body of work which in defining itself as sociological analysis of working-class life did present its material, in part, in a descriptive, evocative and experiential form – as, in effect, a kind of realist writing. At one end of the spectrum this shaded off into more “rigorous” quantitative and tabular modes of research and presentation, while at the other it gradually merged into documentary, personal reportage and semi-fictional narrative.

¹ For further discussion of his arguments, see the comments in the same issue, and his reply.
Young can in part be placed in that framework, though it does not take account of his very active policy concerns. He stood at the point of intersection of that genre’s world and the policy world of bourgeois and intellectual philanthropy.  

We start with Michael Young’s own life, and work outwards from that. Young was the lead author; comparable detail for his co-author Willmott is not attempted. Young was a remarkable social entrepreneur, and his academic life, on which we focus, was only part of his range of activities.

Born in 1915, he spent his childhood in a very disrupted family life until, in 1929, he was sent to the pioneering progressive Dartington Hall School, set up by Leonard and Dorothy Elmhirst, a wealthy and philanthropic couple, who almost adopted him (Briggs 2001: 17). They founded the Elmgrant Trust, which made a significant contribution to the funding of FKEL. Their milieu was a privileged and cosmopolitan one; Young was, for instance, at the age of 15 taken by them to stay at President Roosevelt’s White House (Briggs 2001: 11). On leaving school he joined a London firm of solicitors, and lived for a time in 1933-5 at Toynbee Hall settlement. He started going to the London School of Economics [LSE] as an occasional student, and then took a BSc (Econ) there - without any classes in sociology or anthropology - graduating with a 2.1 in 1938; he remained in touch with LSE until the outbreak of war. He was called to the bar as a lawyer in 1939. He then consulted Max Nicholson of Political and Economic Planning (PEP) about what to do next, was given the opportunity to write a piece for them, and as a result was given a job there (Briggs 2001: 49). PEP was a social-scientific think tank involving civil servants, businessmen and politicians as well as academics; it produced influential reports applying social science to current policy issues (which included evidence to the hugely important Beveridge Report, basis of the post-war welfare state), and was an important networking site (where the formation of the British Sociological Association appears to have been initiated). In 1945 he moved to the Research Department of the Labour Party, making a major contribution to its manifesto for the 1945 election, which it won; he remained there until 1951. Before 1950 he had started a doctoral thesis at LSE, under Harold Laski (a long-term activist in the Labour Party, and its chairman in 1945-6), on how parties operated at the local level. In 1951 he switched to Richard Titmuss, who had just become Professor of Social Administration, as supervisor, and to the family as topic area (Briggs 2001: 83, 107); the thesis was submitted in

---

2 Topalov (2003), analysing the background of comparable studies done in Britain (FKEL), France, and the USA, argues that their authors were marginal to academic sociology, which gave little attention to the issues they raised, and argued with the policy world of planning. This helped to account for the novelty of their contributions, redefining “slums” as “traditional [‘traditional’ because they were disappearing] working-class neighbourhoods.”

3 Peter Willmott came from a humbler background, starting out as an engineering apprentice and finding his way to Ruskin College (an adult education college, based in Oxford, for people who had not had the earlier opportunity to gain formal qualifications). Young had later recruited him to the Labour Party Research Department after he wrote in response to a pamphlet by Young.

4 For other aspects, see Briggs (2001)

5 In the same list of graduates is the name of Maurice Ash, who had become a good friend of Young’s and had another remarkable career entangled with his, using his substantial inherited money to support favoured causes. He became chair of the Town and Country Planning Association, and at his stately home ran a farm on Steiner principles and housed a small Buddhist-oriented community. Through Young he met the Elmhirsts’ daughter Ruth, and married her; he eventually became a Dartington trustee alongside Young.

The name of Raymond Goodman also appears on the same list (with a 2.2). He was Director of PEP for 1946-53, and for 1951-3 acted as the first Secretary of the British Sociological Association (BSA). Perhaps there would be some mileage in studying a cohort from this period.

6 Who was also, among other things, founder of the World Wildlife Fund and of the Nature Conservancy (of which he was Director from 1952).
1954. In 1953 he had set up the Institute of Community Studies (ICS) a research unit whose first publication was FKEL (with a foreword by Titmuss). The title of the doctoral thesis is „A study of the extended family in East London“ and, though not cited in the book, it clearly was, or became, part of it.7

The ICS was based in Bethnal Green, the area of its first research. (Initially it was in Oxford House settlement premises there, until it got its own base, though that was next door to University House settlement.) It was seen as created as a „sister organisation“ to the Tavistock Clinic8, where Young had spent a year (Briggs 2001: 82). Its aim was declared to be „to study the relationship between the social services and working-class family life“ (Young and Willmott 1961: 203), and it became prolific in the production of studies, some of them local, though many also going beyond the topic area originally defined. There was a clear policy focus:

We were young and naive enough to believe that if we could report, in a convincing way, on the needs and hopes of Labour supporters, even if only in one working-class district, it would help to bridge the gap ... with the leadership. (Young 2000)

THE BOOK AND ITS TEAM

FKEL, published in Britain in 1957, went through many editions and became enormously influential and well-known. To sketch an outline of its „sociology world“ it is useful to start from its acknowledgments. It is clear that there are some conventions about what may be noticed in acknowledgments, so in that sense some methodological caution in how much one infers from them is appropriate. However, what appears there may both reflect social patterns in the research work done, and in its less obvious names do something to define a personal milieu. Listed are people who had made at least five different kinds of contribution: funding sources, academics who gave support or methodological advice, academics who had shared research experience of the same area or topic, university support staff, research unit employees. Looking at these, we can see something of the construction of the „world“ which had characterised and generated this book. But there are also some people mentioned for undefined reasons; the implicit rules of acknowledgment politeness mean that their contributions may have been anywhere from negligible to significant.

People mentioned in FKEL’s acknowledgments, in order of appearance

<table>
<thead>
<tr>
<th>Thanked for</th>
</tr>
</thead>
<tbody>
<tr>
<td>Edward Shils, Richard Titmuss</td>
</tr>
<tr>
<td>Philip Barbour</td>
</tr>
<tr>
<td>Peggie Shipway</td>
</tr>
</tbody>
</table>

7 Various sources mention the relation between them in ways which do not clarify this. Many years later, Young himself said of FKEL that “The first draft of it was my own PhD thesis with Professor Titmuss, and the second draft was the one I did with Peter Willmott, and the third draft was the one I did with Peter [Townsend]... the quantitative stuff ... wasn’t very well brought in in my original PhD thesis” (Young and Thompson 2004).

8 The Tavistock had started as a psychiatric clinic, but by the end of World War II it was more socially oriented, and conducted work on ‘human relations’ which made important empirical contributions to industrial sociology.
Peter Townsend, Peter Marris  | Discussion
Daphne Chandler  | Typing
Margot Jefferys, Ann Cartwright  | Advice on design of main survey
John Mandeville  | Provision of Hollerith machines
Alan Stuart  | Advice on tests of significance
Ruth Glass, James Robb  | Access to their data on Bethnal Green
Raymond Firth  | Information on his study of kinship
Bott, Bowlby, Brome, Cooper-Willis, Donnison, Elmhirst, Madge, Moser, Peterson, Sheldon, Sparrow, Spencer, Stirling, Wilkins, [Phyllis] Willmott  | Unspecified advice.

Some notes on who these people were, with special attention to features relevant to the later discussion:

Those thanked for specific contributions

*Edward Shils* – prominent US sociologist, who spent much of 1940-45 in London on war work, and for 1946-50 held a readership at LSE in parallel with his Chicago professorship.

*Richard Titmuss* – first professor of Social Administration at LSE, very influential on policy in the Labour government’s creation of the post-war welfare state; he held no formal HE qualifications, but was charismatic and well respected for his important publications.

*Philip Barbour*: a conscientious objector and proponent of world government, who joined ICS as its statistician and treasurer after an LSE sociology degree (Barbour 2010).

*Peggie Shipway* [no information found]

*Peter Townsend* – founding colleague at ICS. He had Cambridge BAs in philosophy and psychology and in anthropology, then spent a postgraduate year in Berlin studying sociology in 1951-2. He appears in the programme of the 1953 first conference of the BSA as Secretary of the Social Policy Group of PEP, where he worked for 1952-4.

*Peter Marris* – early ICS colleague; BA in philosophy and psychology; after National Service in Japan, he spent two years in Kenya in the Colonial Service.

*Daphne Chandler* [no information found – secretarial background assumed]

*Margot Jefferys*: like Young, she had a 1938 BSc Econ. (described on early BSA membership lists as in Economic History). By 1960 her post was „lecturer in social science“ at the London School of Hygiene and Tropical Medicine, with „sociology of medicine“ listed as her field; she is regarded as a pioneering leader in medical sociology.
Ann Cartwright: had a PhD in Statistics, and before joining ICS was a lecturer in Social Medicine at Edinburgh University.

John Mandeville: started a career in the Navy, somewhere along the line gained an A.M.I.Mech.E, moved into work in the early diffusion of punched-card machines and so became an authority on the machine analysis of market research, worked in various ministries during World War II, and then set up as an independent consultant on punched-card systems (Mandeville 1946: 121).

Alan Stuart: spent his whole career as an LSE statistician, with survey sample theory as one of his main areas.

Ruth Glass: held a post at University College London; Glass (1939) was a well-regarded study of a housing estate, and she continued to work on urban planning and housing-related topics. When Young and Willmott started work she had already collected data on Bethnal Green (Glass and Frenkel 1946), not all published. In 1947-8 she had been a research officer at PEP. She was the wife of David V. Glass, who became professor of sociology at LSE; nepotism regulations prevented her to having a job there.

James Robb: New Zealander working in London on a LSE PhD in social psychology, collecting data in Bethnal Green; at the same time he worked on a Tavistock project on marital casework. His background then was in psychology and social work more than sociology, though he became a founding figure in NZ sociology on his return, and carried out a number of community studies there.

Raymond Firth: of New Zealand origins, from 1933 from lecturer to professor of anthropology at LSE, where he had obtained his doctorate; had already carried out a pioneering study of kinship in London (1956). His archived papers at LSE contain work in progress from Bott, and from Young with Shipway reporting on pilot interviews.

Those thanked for unspecified contributions:

Elizabeth Bott: Canadian social anthropologist working in London, who subsequently became a psychoanalyst; her project for the Tavistock Institute of Human Relations on families and their networks had several publications before her famous 1957 book on it. She worked on the same project as Robb.

John Bowlby: a psychologist/psychiatrist/psychoanalyst with a special interest in child development, and head of the children’s department at the Tavistock Clinic.

Vincent Brome: a writer; had also worked at the Labour Party Research Department (LPRD).

Euan Cooper-Willis: economist/banker; his wife was a ceramic designer who had studied at Dartington College of Arts, and they ran the Portmeirion pottery company. It is evident from Young’s papers, archived at Churchill College Cambridge, that Cooper-Willis had by the late 1960s for some time had a job in banking, and held responsibility for looking after the ICS’ investments, buying and selling shares for them. It sounds as though he may also have been a personal friend.

9 YUNG 5/14
**David Donnison:** from 1956 a professor of Social Administration at LSE; a strong Labour and welfare state supporter who worked on housing issues.

**Leonard Elmhirst:** from a landed gentry family, and by Cornell training an agronomist; he had a long concern with India and its rural problems. He and his wife Dorothy, who held a large family fortune, founded the Dartington estate to attempt to put their ideals into practice.

**Charles Madge:** Professor of sociology at Birmingham, though without any degree in the field; a founder of Mass Observation, and had worked with Young at PEP in 1943.

**Claus Moser:** a leading member of the LSE Statistics department, whose survey methods textbook (1958) was for many years widely used by sociologists.

**John Peterson** [no firm information found— but probably he is the Petersen mentioned in Robb’s preface as on the staff of the settlement University House in Bethnal Green].

**J. H. Sheldon:** physician with a special interest in geriatric medicine, and author of a well-received 1948 book based on a survey on the social medicine of old age, including the relevance of housing and support from neighbours and family.

**John Sparrow:** [Presumed not to be the one of the same name who became Warden of All Souls College at Oxford.] BSc Econ. specialising in accountancy and finance, LSE, 1954, then employed in a chartered accountancy firm. His later career was of great distinction, in accountancy and merchant banking, with several public functions, a knighthood in 1984, and vice-chairmanship of the Governors of LSE.

**John Spencer:** studied „social sciences“ [at that period this was in effect social policy/social work] at LSE before the war; on return from war service he became a lecturer in „social science“ there, and gained a PhD with a thesis on the effect of military service on crime. In 1953 he left LSE for a project in Bristol which involved community organisation and group work on an urban housing estate (Sinclair 1979).

**Paul Stirling:** anthropologist, who as an LSE research student started fieldwork in a Turkish village from 1949, including the collection of formal household data in 1950.

**Leslie Wilkins:** on leaving school he started a social-work course at the University of Southampton to train as a probation officer, but could only afford evening classes, while working as a clerk in the Ministry of Labour. He worked his way up, via wartime operational research, to become a Fellow of the Royal Statistical Society and work for the government Social Survey, becoming widely recognised as an expert on aspects of survey method and their application to policy issues.\(^\text{10}\)

**Phyllis Willmott:** Peter Willmott’s wife. She had qualified as a social worker, but came to play an active but informal role in ICS research through living with their children over the Bethnal Green offices. Young (2000) says that she „became the ethnographer in chief, while Peter and I interviewed random samples."

\(^{10}\) For much more on his fascinating career, see Wilkins (1999).
We note the conventional division of labour between such roles as secretaries who type, interviewers who interview, statisticians who do tests\textsuperscript{11}, and the more diffuse or informal academic ones included. The former roles are important ones, often consequential for such matters as the distance between the authors of the publications and their data, but may not be mentioned at all in the published presentation of the research.

It is striking how few of those listed were in any formal sense „sociologists“, even if it might seem that the character of their work gave them a plausible claim to that honorific title. Of those listed for reasons other than typing, interviewing or provision of machines, a maximum of six (24\%) have good claims to be regarded as „sociologists“ by discipline, if sometimes ones of recent vintage. Some of this can be imputed to the stage of development of British academic sociology, where very few formal posts yet existed. Books by some authors from psychology, anthropology and geography were generally used as sociology.\textsuperscript{12} Many people then whose careers started in other fields later held posts in sociology, a few changing affiliation as a result of work for ICS – but that does not seem to account for many of these, a majority of whom are known to have continued in other fields, even if those are ones like social administration, social psychology or anthropology which can be seen as very close. There is an emphasis on help which is methodological, or based on sharing data; those with related data could be unusual in their disciplinary community in having interest in relevant fields, and it is clear that there was widespread interest in policy-related research themes where practical problems could override disciplinary conventions.\textsuperscript{13}

We can see how many other people played roles, if not all essential ones, in the production of the book we have focused on – and that is without starting to look at anything specific to the process of publication and diffusion. FKEL, like many other social science monographs of the time, was published by Routledge and Kegan Paul in the „Library of Sociology and Social Reconstruction“ series initiated under Karl Mannheim\textsuperscript{14}, although ICS was identified there as a somewhat separate subgroup. It is interesting to see, from the Routledge archives at the University of Reading, that the publisher did not anticipate good sales!\textsuperscript{15} The deliberate effort made to write in a way accessible to the intelligent general reader must have helped. The reception of the book is not covered here, but the picture could usefully be extended in that direction.

\textsuperscript{11} It is not known precisely what Barbour's role was in relation to the book, but he was employed as ICS in-house statistician; given that a research statistician is also mentioned it seems possible that his role was to apply the tests recommended by Stuart.

\textsuperscript{12} This also went in the other direction. Monchaux and Keir’s (1961: 158-9) review of British psychology from 1945 to 1957 includes FKEL and Townsend (1957) as contributors to the field, and goes on to say how hard it is to draw a dividing line between sociological and social-psychological studies.

\textsuperscript{13} It is interesting to contrast these acknowledgments with those in books from the University of Liverpool’s Department of Social Science which were published at much the same time. This department in the 1950s had a distinctive pattern of organisation. On the one hand, it had permanent academic posts committed to empirical research, and on the other hand it had a commitment to work on local social problems, with the knowledge and consent of those researched. Collective expertise and local knowledge were developed with experience, so that external advice was not required as much as in other situations. Staff often worked cooperatively, so that formal authorship could be relatively arbitrary; some of the books have no individual author on the cover, but a list of those members who participated in various ways is given inside. The acknowledgments made are commonly to representatives of their subjects, both management and trade unions where applicable, while the penumbra of miscellaneous contacts and sources of advice is missing.

\textsuperscript{14} On which see Platt 2014.

\textsuperscript{15} The understanding that the authors would order ‘a very large number of copies’ themselves (which seems to have been a fairly common practice at the time) may have done something to tip the decision (RKP A131).
THE SETTING

Some of the social connections involved in Young’s trajectory can now be sketched in. The Elmhirst connection was of overwhelming importance, in places not all of which are the obvious ones: the solicitors Young worked for were used by Dartington; Leonard Elmhirst was a founding member of PEP, and its chair 1939-53; Max Nicholson had strong Dartington connections; a grant from Elmgrant, a foundation set up by the Elmhirsts, made it possible to set up the Institute for Community Studies and produce FKEL.16

A second funding source was Edward Shils, who taught at LSE from 1946-1950. He held a Ford Foundation grant, which he could use in any way he wanted, and some of this was used to support Young’s project. In effect he became a protégé of Shils. (Later, Young held his own Ford grant; this introduction may have helped in that.)17 As a doctoral candidate he attended some of Shils’ seminars in 1948-9, when he offered courses on „Primary Groups in the Social Structure“, „Social Structure“, and „Sociological Research“. Shils also gave informal „tutorials“ at ICS in its first year (Willmott 1985: 146-7). Briggs (2001: 84) says that they kept in touch when Shils returned to Chicago in 1950.18

The LSE context more generally was obviously also important. LSE had a group of statisticians who focused on social science concerns, especially aspects of survey method. They were certainly not sociologists, but some of them, especially Moser (professor of Social Statistics), produced work which was effectively incorporated into sociology. There was also a Research Techniques Unit to advise on methods, making another point where it might be worth tracking back in the sequence of events. (FKEL only provides simple tables in the main text, with significance tests in an appendix; Young’s thesis, based on fewer than a hundred cases, had nothing more sophisticated than percentages.)

Overlapping with LSE as a relevant node in the network was the Labour Party. LSE was very prominent in the formulation of the agenda of the 1945 Labour government, which was creating the welfare state.19 William Beveridge, director of LSE from 1919-37, was the author of the 1942 Beveridge Report which set out the proposals on which it was based, though he was not a Labour Party member. However LSE staff member Harold Laski chaired the National Executive

---

16 The research which became Benney et al. (1956) was also funded by a 1948 grant from Elmgrant, supervised by a committee which included Young; Benney (1966) said it was inspired by Shils. Other empirical work on rural areas (e.g. Saville 1957, Williams 1963) - much closer to the issues local to the Dartington estate - was commissioned and funded by its Trustees, and appeared in Dartington Hall Studies in Rural Sociology, also published by Routledge Kegan Paul.
17 Briggs (2001: 131) reports that projects listed in the original proposal for ICS included one with financial support from Shils, who wanted to do it himself, on Oxbridge and other universities.
18 A well-known joint article (Shils and Young) was published in 1952. For this Young ‘conducted some interviews with residents of the East End. With these data, information from newspapers, and my reading on monarchies, coronations and similar ceremonies we wrote an essay…’ (Shils 2006: 87) This article became notorious in England when later seen as a right-wing justification of the monarchy.
19 Its prime minister, Clement Attlee, taught ‘social administration’ at LSE until he was elected to Parliament in 1922, and before that was associated with settlements, including Toynbee Hall.
Committee of the Labour Party for 1945-6, and Richard Titmuss and colleagues were deeply involved in the agenda of the welfare state.\textsuperscript{20}

Labour ideology centred on the working class\textsuperscript{21}, and that focus was very much in the Anglo-American tradition of policy-related social research. ICS's location in Bethnal Green was closely consistent with the settlement-house pattern which in the US (e.g. Hull-House) and Britain had been associated with that concern.\textsuperscript{22} There was considerable policy-related research on connected topics going on among the working class in London at the time, often with a psychological rather than a sociological problematic, though Rorschach tests and concern with personality were sometimes supplemented with participant observation of a kind now more often associated with sociology.\textsuperscript{23} Some of the main books based on it are these:

- Robb (1954): in the Preface Robb, a New Zealander, declares his greatest debt to be to Shils' help and encouragement while he was carrying out the fieldwork; acknowledgments are also made to Bott and John Spencer among others. The book was based on his doctoral thesis.
- Spinley (1953): this was a doctoral thesis in Psychology, and she too was from New Zealand. The slum area she studied was in London, but outside the East End. (Her slum group was compared with a public-school one.) The preface names Robb as a friend who read her first draft.
- Bott (1957): Bott belonged to a research team which also included Robb (who returned home to New Zealand in 1954, so his contribution is less salient in the book than it might otherwise have been); the acknowledgments include Firth, and Young and his ICS colleagues, as well as many others, especially anthropologists. In 1949-51 she held an assistant lectureship in anthropology at LSE.
- Jephcott, Seear and Smith (1962 - publication was delayed by problems): Jephcott, born in 1900, had a Wales BA in History, and in 1946-8 was a PEP research officer. In 1954 she was appointed as a research assistant in Titmuss' Social Administration department, after considerable earlier work organising and researching working-class girls' clubs, and worked with John Smith, an LSE graduate and later a professor of sociology.

It is clear from the repeated recurrence of some of the same names that this was quite a small-world research community, with mutual support and exchange of ideas across disciplinary lines.\textsuperscript{24} (Smith told me that there was considerable interaction among the researchers working in East London at the time.) The same names of institutions - LSE, PEP, LPRD, Dartington, Tavistock Clinic - and of individuals (Shils, Titmuss, Glass, Robb, Bott) appear, as has been shown, in several contexts, so that their work needs to be treated explicitly as a product of specific social relationships, not all of them within the gates of „sociology“.

\textsuperscript{20} By 1957 Labour had lost power, and government policies differed. It has been argued elsewhere (Platt 1971: 139-140) that the Institute’s somewhat romantic picture of Bethnal Green can be seen as a counter-Utopia, presenting an alternative to the new policies’ model.

\textsuperscript{21} David Glass, in his foreword to the famous LSE collective work \textit{Social Mobility in Britain} (1954: 3), declared that its focus was on middle-class groups because so much had traditionally concentrated on the working class.

\textsuperscript{22} It could be of some interest to consider how far ICS might be regarded as a modern settlement.

\textsuperscript{23} Another book which in many ways seems to belong in the same set is Kerr (1958), a study of personality development in a slum, but this slum was in Liverpool and the author acknowledges Liverpool assistance.

\textsuperscript{24} The list above is sufficient to draw attention to the legacy of empire, on which Steinmetz (2014) has written very usefully, and the social categories of exile/refugee and Jew, not developed here, were also a salient part of the social setting.
For someone for many years commonly regarded as a sociologist, surprisingly little of Young’s activity was formally sociological; his BSc was not in Sociology, his doctorate was in „Social Administration“, and he drew on anthropology rather than sociology for ways of approaching the family. He taught sociology named as such for only three years, as a lecturer at Cambridge in 1961-3, and this did not seem to suit him well. His interest was always in policy applications, and his writing was addressed to the general reader, or such groups as teachers, social workers and town planners, not just academics. Much of his institution-building, such as the founding of the Consumers’ Association, was outside academia. Was Young „really“ a sociologist? Perhaps we might apply to him a comment made by Marshall in an obituary appreciation of Titmuss: „we may begin by asking whether Titmuss was a sociologist. The question is permissible provided one does not demand a ‘yes’ or ‘no’ answer.“ (Marshall 1973: 137). Whatever his personal identity, Young’s published works have certainly been used as sociology.

A brief look at the career path of another prominent British „sociologist“ – chosen for contrast, admittedly – whose prominence has overlapped in time with Young’s, is enough, even with much less data, to show how different a prominent career trajectory can be even under many of the same environmental conditions. John H. Goldthorpe’s UCL BA was in history, but he rapidly moved into sociology. After some years at Leicester and Cambridge universities he has for many years been at Nuffield College Oxford; he works in a highly quantitative style on empirical, theoretical and methodological aspects of social stratification. Normative judgments and policy relevance have not been absent, but it is clear that his central involvement is with the academic and disciplinary communities. There is a network of connections among individuals with whom he has done cooperative work (e.g. Breen, Erikson, Whelan), who are connected with a journal of which he was one of the founders (the European Sociological Review25), whose departments belong to an association of others with similar styles and resources (the European Consortium for Sociological Research26), who have been active in an important international group working on issues of social stratification (Research Committee 28 of the International Sociological Association), who cite and are cited by his publications, and who have been involved in the research groups associated with an EU „Network of Excellence“ („Equalsoc“) with a research group on educational inequality. Numbers of the people involved with these groups have also been student, staff, or visitor at Nuffield College or other parts of Oxford University, and probably also at appropriate institutions in northern Europe. There can be no doubt that Goldthorpe is now a sociologist, though he dissociates himself from much of contemporary British sociology. (The sophisticated level of quantification in his publications cuts many British sociologists off from the possibility of serious engagement with them, and his writing is not aimed at the general reader.) The intellectual „world“ he has chosen, indeed to some extent created, coexists with sometimes overlapping and sometimes distinct other worlds for sociologists. It is obvious that the boundaries of his and Young’s worlds have not been in the same places.

The areas sketched above for Young are ones which could apply to any empirical work if the data were available. They exemplify some directions in which study can be taken in order to explore the

---

25 Its first issue states its aim to provide a forum for an informal network of European sociologists ‘who have in common a commitment to empirical research and an interest in comparative studies, and who have often followed styles of sociological analysis that are distinct both from those characteristic of their own national traditions and from the reigning North American paradigms’ (Mayer, Goldthorpe and Ringen 1985: 1).

26 This admits as members units which ‘have a demonstrated capacity to achieve and maintain high standards in all relevant aspects of the research process.’
relevance of non-disciplinary factors in the creation of „sociology“. To learn about both the wider social context and the relationships involved in the production of FKEL sketched above is surely to understand more of the meaningful history of Young’s work. Two other examples below show, in much less detail, less ordinary ways in which not strictly sociological elements may appear in association with sociological work.

The BSA’s Quantitative Sociology Group [QSG]

The QSG started in 1967, and aimed „to synthesise statistical, mathematical, computing and substantive aspects of quantitative sociological research“ (Network 6: 9, 1976). This was, of course, the period of the emergence of the computer (but not yet the word processor) into practical academic life, and many of its members were active in that.27 There was still excitement about survey method, and within survey work convention made this as much the sphere of social statisticians as of sociologists, though social statisticians were by no means available wherever there was university sociology. The arrival of SPSS, much easier to use for social data than previous computer programs, figured prominently in the lives of some. Among sociologist members survey work bulked large, but network analysis and conversation analysis were also present. In practice, the QSG was strongly interdisciplinary, and cut across other conventional boundaries too by including governmental research workers, survey researchers from market research and other commercial research outfits, and members of university computer centres whose roles were formally in service, rather than research or teaching. The January 1978 issue of the BSA newsletter Network said that at the recent annual colloquium of the QSG „At many sessions there were no sociologists present, and where they did attend they were easily outnumbered by physicists and mathematicians."

This was, thus, despite its formal status, by no means a group confined to „sociologists“. Where had its members come from, and what had become of them when the group folded in the early 1980s? Some suggestive light can be thrown on this by data on its post-holders.28 Only 40% of the jobs they held while in QSG were in Sociology departments; the others were in computing services, social statistics, survey units... (As those became more institutionalised as distinct fields of work, some of the people concerned no longer defined their interests as within sociology.) Few of their first degrees were in sociology – only three of the 19 whose degree was unequivocally identified, one of those in sociology joint with maths. They often came from fields with much stronger quantitative traditions, whether in natural science, mathematics, or partly social-scientific fields such as psychology or geography, so these people did not need to rely on what they were taught about quantitative methods within sociology. Their shift into sociological work was part of the movement of excitement about sociology which led to the great expansion of the late 1960s and early 1970s.29 (Others moved in from fields such as philosophy, and were much more often represented in the Theory study group.) The divisions within „sociology“, documented for the US

27 A division of labour within a team in which one researcher had all the responsibility for computing - at that period, before the development and diffusion of programs such as SPSS, necessarily learned from scratch for the particular project - seemed to encourage obsessive perfectionism, and the production of data in forms of such complexity that they were in effect unusable, as well as incomprehensible for the colleagues who were meant to analyse and write up the material (Platt 1976: 91-92).

28 No complete membership list has been identified.

29 Two members embarked (independently) upon doctorates aiming to mathematize Parsons’ theory; perhaps it is not surprising that neither completed a thesis.
by Ennis (1992), set limits to social integration, but in this case the integration cut across several boundaries. During the transitional period of this group, it is far from clear how to describe its members’ disciplinary status.

**Hodson’s set of workplace ethnographies**

Sociologist Randy Hodson has collected every case he could find of book-length workplace ethnographies in English with sufficient information on groups of workers for the themes he was interested in to be coded.\(^{30}\) He has assumed that the information provided is adequate data, and used the set of coded material to provide both quantitative and qualitative data for a number of papers in sociological journals. But what is the set of cases that he has chosen? It is a set of books providing information on workplaces, not a set of work by „sociologists“; for a few writers, the data were not at all addressed to an academic constituency. A noticeable proportion of the studies were done by anthropologists, and there is a scattering from other corners of academia; but there is also an interesting subgroup, on which we focus here, by people who were academically entirely amateurs, sometimes writing autobiographically. Does their work that Hodson has used *ipso facto* become „sociology“? or should it perhaps be regarded as having been sociology even before Hodson had noticed it?

If one were, unlike Hodson, interested primarily in the history of academic sociology, arguably the amateurs would have to be omitted – except to the extent, at least, that their work has been retrospectively absorbed into the social science literature. His publications on this set of cases have in effect absorbed them, but is that just one idiosyncratic use? An impression of the extent to which the „amateur“ works have been absorbed into social science was gathered by looking at the first five pages found on some of them in Google Scholar. This is quite suggestive about the range of ways in which such materials may be incorporated, and the factors which can influence the level of attention received. One book received many references, but the largest numbers came from labour history, and from legal scholars, sometimes in association with issues of drinking in the workplace (mentioned in the text, though not its main topic). The book had also been used in teaching of rhetoric/ creative writing/literacy, and as offering data on gender identities. Another book received few social-science references, but appeared used as educational descriptive material for undergraduates rather than as research findings or theorisation to build on. Social-science references to another, written by an active trade unionist, were mostly from sources on class and trade unionism, while others were cited by hobbyists in the substantive fields of the books’ topics (railroads, local history) as well as by academics. Thus such studies have been used in academic social science for a variety of purposes – and were probably used quite as much as many single empirical studies by professors. But their potential status as social science is achieved by cooption, rather than volunteering. Are they (perhaps especially if they offer some general explanations for what they report) „really“ sociology, or social science more broadly, rather than merely potential data for it? To find oneself asking such a question is to recognise that it cannot sensibly be answered, since „really“ is a social construction independent of the historian rather than an intellectual matter – though the socially made distinctions are in themselves of historical importance.

\(^{30}\) This is discussed in more detail by Platt, Crothers and Horgan (2013).
Concluding discussion

Two main kinds of not-sociology have been discussed: intellectual material identified by boundary work as belonging to other disciplines or none, and material on factors - a necessary or occasional part of the practical production of any intellectual work - ranging from the availability of data-collection resources to the system of distribution of the results. Various issues emerge from this discussion of examples. How problematic it is to deal with the first kind depends on the extent to which disciplinary boundaries are emphasized; if the question is not contextually raised, one can perhaps ignore it. But in teaching it is hard to avoid the question; then, a tidy artificial presentist boundary can seem to meet the need better than one with trampled barbed wire to trip over, and several metaphorical goats invading from the next field. Serious historical work, however, needs to look at the messy bits – and treat them as messy. There were in fact, for instance, numbers of other studies of new housing estates and their effects, but most of them have been forgotten by sociologists – though some remain noted as part of the literature of town planning, while others which started as town planning were absorbed as „community studies“. The pattern of retrospective shifts in disciplinary identity has certainly occurred elsewhere. For instance, the volume Politics, Social Networks and the History of Mass Communications Research (Simonson 2006) gives a surprise to anyone who thought that Personal Influence (Katz and Lazarsfeld 1955) was a classic of empirical sociology; it has become „one key text in the history of mass communication theory“ (Simonson 2006: 9). Has it to be deemed to have left sociology, then? No reason why it should, but the dual claims made on it undermine consideration of what it „really“ is; in this context, at least, it really is whatever it is used as.

There are many ways in which academic work will almost inevitably be inextricably entangled with practical contributions from non-academics. Yes, we type our own papers now (but we don’t run the internet), some universities have devolved internal budgetary systems (but their financial resources depend on wider social processes), and a few journals have been run by founding groups whose members voluntarily do the production work themselves without bringing in commercial publishers; to the extent that such conditions hold, some disciplinary boundaries may be maintained. When it comes to such matters as the availability of funding, or the nature of the publication system, there may be no problems of definition; the question is merely whether data on such points should be drawn on in historical description and explanation. The answer there is surely that it depends on the extent to which what happened in the particular cases studied was simply the normal pattern of their time and place, though attention may need to be drawn to that for readers not familiar with the general literature on it. Where what happened is more idiosyncratic, it merits more attention. We have shown a number of points at which Young’s personal contacts were significant; other researchers have had very different connections, and have made different choices, so the „worlds“ of their books look rather different. The world of „sociology“ can in that sense be composed of numbers of overlapping worlds occupied by different „sociologists“, which make it hard to reach convincing empirical generalisations about the work of a period or a geographical unit – and if their absence is what the data show, so be it. However, there may be varying levels of homogeneity between schools of thought, generations, or sub-fields, and where the sets in the Venn diagrams intersect is of considerable interest in itself.

31 We may also note that several authors of such studies changed disciplinary identification to become ‘sociologists’ as the discipline became established in universities and rapidly expanded.

32 The prior question of whether the relevant research has already been done, however, arises – and quite often it has not. For some discussion of the gaps, see Platt 2013.
It is not always easy to gather data on factors such as those listed for Young; even such a well-documented case has gaps in its account, so that it may not be possible to meet in practice the historical standards to which one adheres in principle. Regrettably as this may be, one can deal with it by choosing where to set the boundaries of one’s problems - or circumstances may choose them for us. The salient methodological task may then shift to how to make the best use, without over-interpretation, of the resources that are available. Where the boundaries of social science lie must, thus, in the end depend on the research topic addressed, rather than being an answerable general empirical question for the historian. Thus it becomes an ad hoc methodological question, rather than a substantive one, which is perhaps as it should be.

**BIBLIOGRAPHY**


Platt, Jennifer (2013) What Have We Done, and What Remains to Be Done, In the History of Sociology?, *Sartoniana* 26: 115-140.


Jennifer Platt is emeritus professor of sociology, University of Sussex. She researches the history of sociology and its methods; major works include *A History of Sociological Research Methods in America, 1920-1960*; *The British Sociological Association: a Sociological History*; „Women’s and men’s careers in British sociology“ (British Journal of Sociology).
A “Not Particularly Felicitous” Phrase: A History of the “Behavioral Sciences” Label

Jefferson D. Pooley
pooley@muhlenberg.edu

Abstract

The article reconstructs the history of the "behavioral sciences" label, from scattered interwar use through to the decisive embrace of the newly prominent Ford Foundation in the early Cold War. The rapid uptake of the label, the article concludes, was the result of the Ford Foundation’s 1951 decision to name its social science unit the “Behavioral Sciences Program” (BSP). With Ford’s encouragement, the term was widely adopted by quantitative social scientists eager to tap the foundation’s social science funds. The label’s newness and its link to the gigantic foundation’s initiative generated much suspicion and resistance as well.

Keywords

Behavioral sciences, Ford Foundation, Cold War

There are few behavioral scientists today. But as recently as the 1950s and 1960s, self-identified “behavioral scientists” occupied the elite ranks of American social science. The rapid uptake of the label was the result of the Ford Foundation’s 1951 decision to name its social science unit the “Behavioral Sciences Program” (BSP). With Ford’s encouragement, the term was widely adopted by quantitative social scientists eager to tap the foundation’s social science funds. The label’s newness and its link to the gigantic foundation’s initiative generated much suspicion and resistance as well.

This paper reconstructs the label’s career from scattered interwar use through to Ford’s embrace. Existing histories trace the term back to psychologist James Grier Miller’s Committee on the Behavioral Sciences at the University of Chicago. The term, however, was already in limited circulation by the mid-1930s, deployed in distinct but overlapping ways by political scientist Arthur Bentley and psychologist Clark Hull.

Drawing on Ford Foundation archives, the paper draws connections between Hull, Miller, and Hull student Donald Marquis, who played a pivotal role as the key social science planner at Ford. For Marquis, the label was a layabout alternative, an encumbrance-free near-neologism that could, on the one hand, avoid the recurrent conflation of “social science” with “socialism” by anti-New Deal-
ers in Congress, but also signal a linguistic break with the speculative, unscientific legacy that allegedly remained a drag on social scientific progress. The term quickly became a flash-point around which clashing visions of postwar social science were organized.

The “behavioral sciences” label has largely escaped historical scrutiny, especially relative to other postwar formations like “cybernetics” and “systems science” with which the term was complexly entangled. One reason for the neglect is that the “behavioral sciences” term was never coherently defined, in part due to internal Ford politics. From the beginning the term had no stable referent, and was often used generically as a substitute for the more common “social sciences” designation. Throughout the postwar era, moreover, Miller clung to an idiosyncratic definition centered on his “living systems theory.”

Even so, the “behavioral sciences” did refer to a more-or-less distinct intellectual agenda, centered on enthusiasm for cross-disciplinary, team-based research employing quantitative methods. The Ford Foundation’s 1950 “behavioral sciences” christening, moreover, put a name to a movement that was already underway, with roots in World War II. Many of the social scientists who had mobilized for war service had returned to their campuses with the good-faith belief that the owl of Minerva was set to take flight. Though federal funding fell off initially—the social sciences struggled in vain to win a prominent place in the planned National Science Foundation—the heated-up Cold War of the late 1940s brought substantial military and State Department spending. The massive Ford investment began in this period too, backed by some of the same Cold War exigencies.

The social scientists on the receiving end of government and foundation funding constituted a new elite that would, in the early 1950s, start calling themselves “behavioral scientists.” Based on their wartime service, these scholars were far more sanguine about the potential scientific yield from problem-based team research than, say, quantitative enthusiasts from the interwar years. They were also more likely to embrace general theory, mathematics, and modeling than their interwar counterparts. Bound by interwoven funding streams, wartime service, and excitement about the near-term potential to uncover general laws, the social scientific elite of the early postwar years was already in gestation when Ford proposed its “behavioral sciences” label.

If the intellectual coordinates were in place first, why bother with a name that was tacked on later? The paper argues that the history of Cold War social science—a good deal of it, at least—is suspended in language. Terms like the “behavioral sciences,” in short, do more than designate. They are the raw material that scholars use to fashion their intellectual self-concepts.¹ As tokens of allegiance, labels help to organize academic space into distinguishable (and simplified) groupings. Descriptors like “behavioral sciences” provide, to those who don the labels, orientation and membership—and for dissenters something similar, an identity-affirming contrast. Some terms, “behavioral sciences” included, have messy backstories and connotative associations that linger to significant effect.

The “behavioral sciences” label is an especially rich case, given its supernova-like arc: sudden prominence followed by slow decline. The term’s fortunes, moreover, were yoked to the Ford Foundation’s BSP, which skittish trustees shuttered in 1957. With Ford sponsorship effectively withdrawn, the term’s strategic value to fund-seeking scholars waned even as the label remained prominent throughout the 1960s. In other words, the history of the “behavioral sciences” term implicates a mix of overlapping factors: funding and the Cold War, certainly, but also intellectual

¹ On the importance of intellectual self-concepts for academic identity, see Gross (2008), ch. 1.
commitment. In that sense the now-orphaned term reflected—and also reinforced—the curious blend of opportunism, genuine excitement and geopolitical resolve that characterized American social science in the early Cold War.

The sudden and widespread adoption of the term is testimony to the enormous influence of patronage, at least in this instance and within the relatively narrow context of nomenclature. Other scholars had promoted the “behavioral science” moniker before Ford, but the term’s remarkable 1950s purchase was the direct result of the foundation’s surprisingly insouciant language choice. So successful was Ford’s lexical alternative that scholars unconnected to the foundation were already employing the phrase without comment—as authorless doxa—by the early 1950s. For two decades the label served as a viable rival to the established “social science” terminology. By the time Ford withdrew its funds in 1957, moreover, the term was already sufficiently lodged to thrive without the foundation’s sponsorship. It was only with the waning of what Hunter Heyck has recently called the era of “high modern social science” in the 1970s that the label’s hold began to weaken.2

The article proceeds in four parts. First, I trace the term’s early history to political scientist Arthur Bentley and psychologist Clark Hull. Next, I detail the debate that led to the adoption of the term at the Ford Foundation in the late 1940s and early 1950s. The article then turns to frequency-of-use data from Google Books Ngram Viewer and JSTOR, to help establish the foundation’s major role in propagating the term. In the paper’s last section, I track the term’s medium-term durability in the face of often virulent criticism, even after the Ford crutches had been kicked away in 1957.

I. ‘The So-Called Social Sciences’: Arthur Bentley and Clark Hull

When the Ford Foundation launched the “behavioral sciences” terminology into wide circulation in the early 1950s, the phrase was received as a heavy-handed neologism. Ford did not, however, coin the term. Political scientist Arthur Bentley (1870–1957) had already peppered his writings with the “behavioral science” label more than 15 years before Ford’s adoption. With no apparent link to Bentley, Yale psychologist Clark Hull (1884–1952) was also using the label as early as 1940, a full ten years before Ford.

Neither Bentley nor Hull is credited with inventing the term. Instead, existing histories mistakenly trace the label’s origins to James Grier Miller’s late 1940s plans for the Committee on the Behavioral Sciences at the University of Chicago.3 One reason is that Miller, on behalf of the Chicago Committee, claimed credit for the “behavioral sciences” terminology. “To refer to the biological and social fields involved,” he wrote in 1955, “we coined the term ‘behavioral sciences’” (Miller 1955: 513). A number of tributes and obituaries repeated the erroneous assertion after Miller’s 2002 death (Pickren 2003: 760; Harris 2003: 227; Swanson 2007).

But Bentley and Hull were already using the term in the late interwar years. Both scholars turned to “behavioral science(s)” because they found the prevailing “social sciences” catch-all to

---

2 Heyck argues that post-war social science—with its embrace of mathematics, modeling, general theory, and systems conceptions—constituted a “high modern” era (roughly 1955–1975), itself rooted in a broader and older set of social and intellectual changes that Heyck labels the “organizational revolution.” See Heyck (2014) and Heyck (2015).

3 Berelson (1968), 43; Crowther-Heyck (2005), 154; Hammond and Wilby (2006), 431; Somit and Tanenhaus (1982), 183. A partial exception is Senn (1966), 110, 113, which mentions Hull’s 1943 use.
have problematic connotations. Both Bentley and Hull, moreover, sought to signal the distinctiveness of their respective intellectual projects.

Neither scholars’ deployment of the term caught on at the time. Instead, these early uses constituted a kind of linguistic time-capsule. Post-war scholars, averse to “social science” for their own intellectual and strategic reasons, would go on to pluck the pre-existing but dormant “behavioral science” label among alternative candidates also already in limited circulation—including “human relations,” “social relations,” and “human resources.”

**ARTHUR BENTLEY COINS “BEHAVIORAL SCIENCE”**

Though Hull’s use of the “behavioral sciences” label was probably the direct antecedent to post-war adoptions, Bentley introduced the term first. In his 1935 book *Behavior, Knowledge, Fact*, Bentley repeatedly referred to “behavioral science” to designate his idiosyncratic vision for the study of man.4

Bentley was a committed neologizer. A curious figure in the history of American social science, he is normally remembered as a political scientist despite his repudiation of the discipline (Kress 1970). Bentley earned his doctorate from Johns Hopkins in 1895 after studying with Georg Simmel and Wilhelm Dilthey in Germany (Menand 2002: 379–380). Like many other social scientists trained in the late 19th century, he initially identified as an economist (Ward 1981: 222). His 1908 book *The Process of Government* was neglected by the then-emerging discipline of political science, but later helped seed interwar interest in groups and pluralism (Hale 1993: 2). In the 1950s, *The Process of Government* was embraced by David Truman and other quantitative political scientists as a key proto-behavioralist tract.5 (I address the complex overlap between “behavioralism” in political science and the broader “behavioralism sciences” below.)

In his own lifetime Bentley was estranged from organized academic life. He fell into depression after publishing *The Process of Government*, and soon retired to an Indiana fruit farm where he spent the rest of his life (save a brief stint at Columbia in the 1940s) writing with promiscuous range on philosophical and social scientific problems (Kress 1970). His main project, arguably, was developing an original philosophy of social science, the context that gave rise to the “behavioral science” terminology.

Bentley, like Harvard philosopher Alfred North Whitehead, regarded the relativity revolution in physics as a crucial watershed for academic inquiry in general. He rejected the imitative scientism of many interwar social scientists who sought to mimic the natural sciences with verifiable, quantitative methods. Instead, in *Behavior, Knowledge, Fact* and follow-up work, he asserted that the validity of any given science rested on the internal consistency of its own categorical schema. Mathematics was an exemplary model of formal consistency, but only a model: each science required its own categorical system.

---

4 Bentley (1935). Of course, there may be uses of the term that predate Bentley. My comprehensive, full-text search included Google Books, JSTOR, and PsycARTICLES.

“Behavioral science” was Bentley’s self-conscious neologism for his recast science of man, designed to distance his approach from the more common “social” and “psychological” labels. The book is filled with an absurd-seeming parade of new terms chosen, he explained, to free his scheme from the lexical baggage of prevailing academic language. Bentley rejected the idea of static facts and social entities, and insisted instead on a processual—his word was “transactional”—ontology. The task of “behavioral science” was to systematize its own categories into an internally coherent system—the only knowable truth about human life in a relativistic world.

Though Behavior, Knowledge, Fact was well-received by philosophers of science, Bentley’s 1935 book was ignored by contemporary social scientists. It probably did not help that the book’s key section was presented in the form of a dialogue. He was, moreover, bucking the pronounced empiricist orientation of interwar social science. If anything, his philosophy of social science was ahead of its time, anticipating the full-fledged analytic realism of Talcott Parsons after the war.

Indeed, even as Parsons was working with Edward Shils and others on the late-1940s Carnegie-funded work summarized in Toward a General Theory of Action, Bentley published a high-profile book with John Dewey, Knowing and the Known. Dewey’s concept of “trans-action” (elaborated in Experience and Nature) had been a major influence on Bentley, and Dewey claimed that his 1938 Logic was influenced by Bentley’s Behavior, Knowledge, Fact (Ward 1981: 224). In their 1949 collaborative book, Dewey and Bentley argued for a post-Newtonian “transactional” epistemology largely consistent with Bentley’s earlier work (though shorn of analytical realism). Notable is the authors’ insistence on new, unencumbered terminology, prominently including “behavioral science.”

The collaboration with the famed philosopher was a career-capping vindication for Bentley, and soon enough his 1908 book would get rediscovered by Truman and other behavioralists. Even so, the Dewey-Bentley book was not a major factor in the postwar vogue for the “behavioral sciences” label. By 1949 the post-war adoption of the label was already in motion, and none of the relevant figures cited Knowing and the Known as inspiration. The more direct link to Miller, Marquis, and the Ford Foundation was probably Clark Hull’s use of the term at Yale in the early 1940s.

**Clark Hull and the Yale Institute of Human Relations**

Clark Hull, the neo-behaviorist psychologist, arrived at Yale in 1929, the same year that the university opened its ambitious, lavishly-funded Institute of Human Relations. Hull was the central intellectual figure in the Institute’s mid-1930s crisis-driven overhaul after an amorphous and ineffectual first five years of operation. Under Hull’s de facto leadership, the Institute embarked on a remarkable 15-year effort to generate a unified theory of social life. Though dominated by experimental psychologists like Hull, the initiative was characterized by an organized division of theoreti-

---

6 Part III of Behavior, Knowledge and Fact elaborates his categorical schema. See the excellent summary in Ward (1981).
7 See Ward (1981), 224. George Lundberg, the sociologist and quantitative evangelist, did review the book enthusiastically: Lundberg (1936).
8 Parsons and Shils (1951); Dewey (1925); Dewey (1938); Dewey and Bentley (1949). See also Dewey and Bentley (1964).
9 The term, and the general insistence on new terminology, is also prominent in Dewey and Bentley (1947).
10 Bernard Berelson did later refer to the Dewey-Bentley book to establish the term’s legitimacy at the time of Ford’s adoption, but—in the absence of other evidence—the claim comes off as an ex-post facto justification, Berelson (1968), 41.
cal labor that mixed Hull’s learning theory with psychoanalysis and—later and less resolutely—social and anthropological theory.\textsuperscript{11}

As early as 1940, passing references to the “behavioral sciences” began to appear in the Institute’s published work. Institute scholars later considered labeling their unified theoretical approach “behavioral science,” but could not agree. Instead the Institute’s published summaries employed unwieldy terms like “the unified science of behavior and social relations” and even—half in jest—“lesocupethy” (from LEarning, SOciety, CUlture, and PErsonality THeorY).\textsuperscript{12}

Still, the “behavioral sciences” language was in relatively wide circulation at Yale. Donald Marquis, the architect of the term’s embrace at the Ford Foundation, was a member of Hull’s circle in these years. It is likely, though far from certain, that Marquis inherited the term from Hull and the Institute. Casting about in the late 1940s for an alternative to “social science,” Marquis—on this theory—seized on a lexical remnant from his Yale years.

The Yale Institute, founded in 1929, was successor to a near-decade’s worth of initiatives and programs at the university, most funded by Rockefeller philanthropies.\textsuperscript{13} When James Rowland Angell, the functional psychologist and past president of the Carnegie Corporation, was named the university’s president in 1921, he set out to recast Yale as a research university in the mold of Chicago or Johns Hopkins. His plan involved expanding Yale’s professional schools and integrating them with university’s then-languishing Graduate School (Geiger 1986: 203–206). In the early 1920s, Angell helped secure grants from Rockefeller philanthropies for an Institute of Psychology (founded in 1924) and a new Department of Psychiatry and Mental Hygiene with an unusual social science mandate.\textsuperscript{14} In 1926, Angell began talks with Rockefeller officials to expand the Institute of Psychology to encompass the “fundamental problems of behavior” (Biehn 2008: 30). Soon two professional school deans, Robert Hutchins in Law and Milton Winternitz in Medicine, took an avid interest in the idea and spearheaded an application for an “Institute of Human Behavior” to serve as the research hub of a sprawling Human Welfare Group to include most of the university’s professional schools, social science departments, and biology programs. In 1929 the newly merged and reorganized Rockefeller Foundation awarded Yale an enormous 10-year, $4.5 million grant to much fanfare and press attention.\textsuperscript{15}

The prominent involvement of the Law School’s Hutchins, who left in 1929 to become president of the University of Chicago, is curious. Hutchins’ advocacy for the planned Institute was coupled with published calls for a reimagined legal training that stressed the importance of “scientific data” and the study of “individual behavior and social behavior in all their aspects” (see Morawski 1986: 228 and May, 1950: 46–47). At Chicago in the 1930s, however, Hutchins would go on to aggres-

\textsuperscript{11} On the Institute’s history, see the superb treatment by Morawski (1986). Mark May, the Institute’s director from 1935 to 1960, provides a detailed narrative in the appendices of May (1950), 35–70. A revised version appeared as May (2012).

\textsuperscript{12} Anthropologist George Peter Murdock (1949: 377) proposed “lesocupethy” for the Institute’s “emerging unified science,” adding, “Perhaps it will irritate some reader into proposing a more satisfactory name.”

\textsuperscript{13} On the Institute’s 1920s labyrinthine history of forerunners, and its early years, see Morawski (1986), 225–232; May (1950), 35–61; Biehn (2008), 22–33; and Viseltear (1984).

\textsuperscript{14} Angell was well-connected in the foundation world. He was a trustee of Rockefeller’s General Education Board, which seeded the new Psychiatry Department (Biehn 2008: 31). The Institute of Psychology was funded by the Laura Spelman Rockefeller Memorial’s Beardsley Ruml, who had been Angell’s assistant at Carnegie (Geiger 1986: 149).

\textsuperscript{15} On the application process and negotiations with Rockefeller, see especially Morawski (1986), 227–230; Biehn (2008), 30–33; and May (1950), 39–40.
sively challenge the university’s quantitative social scientists, leading to several high-profile departures (Dzuback 1991: 172–174). In the early 1950s, Hutchins—by then a Ford Foundation’s associate director—was the chief internal critic of the foundation’s planned BSP.

In its first five years, Yale’s new Institute of Human Relations—“Behavior” was dropped from the title on the objection of an unnamed dean—was a failure on its own terms. Plans for interdisciplinary research were thwarted by the Institute’s funding structure (direct disbursements to individual departments and programs) and by senior scholars’ apparent indifference (May 1950: 54–61). In 1935 the Institute was overhauled, with support from Rockefeller officials. Administration and funding were centralized under a director, psychologist Mark May, while the Institute’s ties to other, chiefly biological units were largely severed. Clark Hull, with May’s support, soon emerged as the central figure in an Institute now dominated by experimental psychologists.

Under Hull’s leadership, an aggressive and coordinated theory-building initiative began, centered on weekly seminars and multiple-author research projects. From the beginning the group’s goal was a unified science of human behavior on the model of the physical sciences. Hull furnished both the theoretical framework and philosophical underpinnings. He had elaborated his neo-behaviorist learning theory in a series of papers from the early 1930s, on the conviction that the theory could be expanded to cover human behavior in general. He also established at the center of the Institute’s approach his nomothetic and deductivist philosophy of social science—resembling, but developed independently of, European logical empiricism.

The Institute’s mid- to late 1930s theory-building included a highly organized effort to absorb and operationalize psychoanalytic theory into Hull’s schema. Later, the group incorporated social and anthropological theory, yielding a “unified” four-theory synthesis. But the core of the Institute’s theoretical project was always Hull’s learning theory. In the late 1930s, Hull began to lay out his fully elaborated theory of behavior, first in the co-authored *Mathematico-Deductive Theory of Rote Learning* (Hull et al. 1940) and then in his magnum opus, *The Principles of Behavior* (Hull 1943). Both books include prominent references to the “behavioral sciences.”

Hull’s core claim was that a mechanistic account of conditioned habits and adaptations could account for purposive, creative behavior, without recourse to “the old idealistic philosophy and its various modern attenuations.” His long-held view was that a set of logical postulates, tested by experiments, could describe overarching laws of behavior. In *Mathematico-Deductive Theory*, he moved to express those laws in terms of symbolic logic. The book’s elaborate equations, in fact, were explicitly modeled on Alfred North Whitehead and Bertrand Russell’s *Principia Mathematica*.

16 Morawski (1986), 229. The dean complained that “behavior” was too Freudian.
17 May, accounts suggest, was Friedrich Pollock to Clark Hull’s Max Horkheimer. On the dominance of psychology—the only social science department physically housed in the Institute building—see Morawski (1986), 220; and May (1950), 54.
18 The best treatment of Hull’s theory of behavior, as it developed in the 1930s and 1940s, is Mills (2000), 83–122.
19 On Hull’s philosophy of social science, and his intellectual history more broadly, see Smith’s excellent (1988), 147–256.
20 The most prominent published result was Dollard (1939).
21 The four-theory synthesis was championed by anthropologist George Peter Murdock, and featured in the Institute’s summary publications. Murdock (1949); May (1950), 4–27.
22 Hull, quoted in Smith (1988), 156.
ca, though Hull required the help of a Yale logician to make sense of Whitehead and Russell’s system.

Hull’s turn to symbolic logic and mathematical expression was intended, at least in part, to produce an aura of scientific authority. As Laurence D. Smith discovered, Hull had once privately admitted that scholars are “impressed by the mere external appearance of rigor” in his equations: “This is a factor of considerable importance in the matter of propaganda. I shall certainly heed the evident moral by emphasizing this aspect when I write up the system as a whole.” In that sense Hull’s mathematical expressions prefigured the manner in which the “behavioral science” phrase would be worn, in the postwar years, as a breastplate of scientific rigor.

Mathematico-Deductive Theory (1940) employs the “behavioral sciences” phrase just twice. In both cases, Hull is making a qualified prediction that his deductive approach could be successfully applied throughout the social sciences. In place of “social sciences” language, he substitutes “behavioral sciences,” though in the second instance with a mid-phrase parenthetical, “behavioral (social) sciences”—apparently to signal his synonymic intent (Hull et al. 1940: 12, 305).

In a conference paper delivered the same year, Mark A. May (the Institute’s director) repeatedly invoked the “behavioral sciences” phrase. Appealing to Hull’s formal logic approach, May argued that the “common problem” of the behavioral sciences is to “understand, control, and predict human behavior at all levels and in all complexities.” May predicted that a “general theory of behavior,” once found, will “serve to unify the behavioral sciences as the biological theories have unified the structure of the medical sciences and as the theories of physics and chemistry have tied together the structure of the engineering sciences.”

In Principles of Behavior (Hull 1943), Hull issued an even more forceful call for the ascendent “behavioral sciences” to surpass and supplant the traditional “social sciences.” Hull first invokes the term to assert the unity of science: the difference between the physical and behavioral sciences, he writes, is “one not of kind but of degree—of the relative amount of the figure still embedded in the unhewn rock.” As long as behavioral scientists maintain a “consistent and rigorous objectivism,” they can aspire to match the progress of physics (Hull 1943: 28).

The “behavioral sciences” label—along with “behavior sciences”—appears occasionally throughout the book, but moves to center stage in the book’s rousing conclusion. Hull asserts that the “systematization of the behavior sciences” requires fellow scholars to embrace the “incomparable technique of symbolic logic” and “precise mathematical statements.” He applauds the “increasing tendency, at least among Americans, to regard the ‘social’ or behavioral sciences as genuine natural sciences rather than as Geisteswissenschaft”—evoking the late 19th-century German Methodenstreit. Hull praises the “growing practice of excluding theological, folk, and anthropomorphic considerations,” in favor of “explicit and exact systematic formulation, with empirical verification at every possible point.” There is “good reason to hope,” he adds, that “the behavioral sciences will presently display

25 Mark A. May, “Coordination of the Sciences of Behavior,” paper presented at the annual meeting of the American Sociological Association, Chicago, December 1940, box 11, folder 11, Mark A. May Papers, Ms 1447, Manuscripts and Archives, Yale University Library, New Haven, CT: 11. Thanks to Dennis Bryson for sharing this manuscript.
26 Ibid., 12. For an excellent account of May’s IHR directorship, see Bryson (2015).
a development comparable to that manifested by the physical sciences in the age of Copernicus, Kepler, Galileo, and Newton” (Hull 1943: 399–400).

With increasingly martial rhetoric, Hull reminds his readers that the task will be “arduous and exacting.” Behavioral scientists must “not only learn to read mathematics understandingly—they must learn to think in terms of equations and the higher mathematics.” Expect fierce resistance, he warns, from traditional scholars:

The so-called social sciences will no longer be a division of belles lettres; anthropomorphic intuition and a brilliant style, desirable as they are, will no longer suffice as in the days of Williams James and James [sic] Horton Cooley... There will be encountered vituperative opposition from those who cannot or will not think in terms of mathematics from those who prefer to have their scientific pictures artistically out of focus, from those who are apprehensive of the ultimate exposure of certain personally cherished superstitions and magical practices, and from those who are associated with institutions whose vested interests may be fancied as endangered.

“Hope lies,” he concludes, “as always in the oncoming youth” (Hull 1943: 400-401).

Hull's history-on-the-cusp narrative anticipates postwar rhetoric, which similarly consigned “speculative” social science to a discredited past. Indeed, the Ford Foundation’s adoption of the “behavioral sciences” label was intended to signal the same kind of break with a pre-scientific legacy. For Hull, as for Ford, a new science called for a new name.

Hull continued to use the term (along with “behavior sciences”) until his 1952 death. Nevertheless, the label did not catch on, even within the Institute. In the late 1940s Institute scholars did consider “behavioral science” as an overarching label for their four-theory synthesis, but ultimately rejected the term. “Behavioral science” was judged to be too psychological, with “too strong a connotation of behaviorism.” (The fear was prescient: The mistaken conflation of “behavioral sciences” with “behaviorism” would go on to plague the label in the 1950s and 1960s.) Institute members dismissed other candidates—“human relations,” “social relations” and even “social science”—for the opposite reason: slighting psychology. Hence the half-serious proposal for “lesocupethy” (Murdock 1949: 377).

By this time, the Institute was already in decline. Key figures had left campus for war service, and many—including Marquis—took up posts at other universities after the war. Rockefeller funds dried up in 1949, followed a few years later by Hull’s death. Most of Hull’s “laws of behavior” were undercut by subsequent empirical work even as his brand of neo-behaviorism fell out of favor (Baars 1986: 60-61).

To a remarkable extent, the Yale Institute prefigured the values and practices of early Cold War social science: claims for the unity of science, interdisciplinary team research, aspirations to use mathematics and build general theory—even the physics envy. Though its intellectual influence was arguably weak, the Institute did serve as an organizational model for cross-disciplinary projects in the postwar years. Talcott Parsons, James Grier Miller, and the Ford Foundation planning team all...
studied the Institute precedent while preparing their own high-profile initiatives at Harvard, Chicago, and the foundation.\textsuperscript{29} The Institute’s most important legacy, arguably, was its diaspora of distinguished psychologists, including Ernest Hilgard, Robert Sears, O. Hobart Mowrer, Neal Miller, and Marquis himself—all of whom would serve as APA president.\textsuperscript{30}

Marquis, who earned his PhD at Yale in 1933, taught in (and, after 1941, chaired) the Psychology Department before leaving for Washington war service. Since the Institute’s reorganization in 1935, he had been an active, though lesser, member until his departure from Yale.

As I have already suggested, Marquis may have carried the orphaned phrase with him to the Ford Foundation. It seems reasonable to conclude that he was at least exposed—in published work and in discussion—to the “behavioral sciences” label while at Yale. The fact that the term failed to gain purchase at the Institute may, ironically, have enhanced its appeal to Marquis and the other Ford planners. Here was a term mostly unencumbered with the freight of past associations, save a welcome residue of scientism. A phrase too closely identified with the Hullian project, after all, would have been disqualified by its particularity.\textsuperscript{31} Instead Marquis and his colleagues at Ford had at their disposal a layabout term, abandoned and forgotten, to put to their own uses.

Still, it is impossible to know for certain how Marquis came upon “behavioral sciences.” He may have independently invented the term, or else borrowed it from his friend James Grier Miller’s Chicago initiative. He could have picked it up from Bentley’s published work, or even from a few other scattered uses—unconnected to Bentley or Hull—that were beginning to appear in 1946 and 1947 in the published literature.\textsuperscript{32}

Marquis later claimed that he, or perhaps Miller, had coined the term. In a 1972 Ford Foundation oral history interview, Marquis was asked about the label’s origins. Ford’s use, he admitted, “was almost simultaneous with James Miller at Michigan [sic] who was also looking for this same class of areas, and he will probably say that he thought of it.”

“I’ll probably say I thought of it,” Marquis continued. “He and I are very close friends and were interacting much at that time, so I don’t know which is accurate.”\textsuperscript{33}

We know that neither man originated the term, but its path to Ford in the late 1940s remains a partial mystery. Far easier to establish is the Foundation’s outsized role in propagating the label.

\textsuperscript{29} On Parsons, see Lagemann (1992), 168; on Miller, see Fontaine (forthcoming).

\textsuperscript{30} Of the Institute’s younger generation of celebrated psychologists, only Carl Hovland and Leonard Doob finished their careers at Yale.

\textsuperscript{31} Indeed, the Yale-era book that Marquis co-authored with Ernest Hilgard cited Hull frequently but did not endorse Hull’s full-fledged theory of behavior: Hilgard and Marquis (1940).

\textsuperscript{32} The philosopher of science Charles Morris (1946) used the singular “behavioral science” label in his synthesis of pragmatism and logical empiricism. Arthur Bentley was among Morris’s chief critics. See Reisch (2005). In 1947, the philosopher Wilfrid Sellars also used “behavioral science” in apparent connection with, but without direct citation of, Morris (Sellars 1947: 197, 202). On Sellars’s engagement with Morris’s book in these years, see Olen (2012), 146–156. Generic uses of the “behavioral sciences,” with no cited link to Bentley, Hull, or even Morris, also first appear in 1947. See, for example, Harper (1947a), 297; Harper (1947b), 82; Curtiss (1947), 315, 317; Burns (1947), 156.

II. ‘Becoming More Scientific all the Time’: The Ford Foundation and the ‘Behavioral Sciences’

During the war and into the early postwar years, the behavioral sciences remained a stowaway term. The intellectual movement that “behavioral sciences” sought to name, after all, predated the Ford Foundation. Its roots were in shared World War II service, which furnished for many campus-bound scholars a sense of methodological and intellectual excitement.

Even before Pearl Harbor, social scientists were flocking to Washington to service a fast-expanding morale and propaganda bureaucracy. The networks formed through overlapping collaboration at over two dozen government and military agencies—cross-pollinated by frequent re-assignments and the spread of new methods—in essence brought the post-war behavioral sciences into existence. Virtually every important figure in the post-war social sciences—certainly among quantitatively oriented psychologists, political scientists, and sociologists—served at some point in the federal government’s wartime propaganda effort.

The quantitative social scientists of the early postwar years were optimistic, but wary about prospects for continued funding. Genuine excitement about wartime methodological innovations—best exemplified by Edward Shils’ heady survey of the postwar landscape (Shils 1948)—was tempered by persistent doubts internal to the disciplines and among key foundation figures. Social scientists’ confidence was, if anything, anticipatory: excitement about incipient technical breakthroughs and emerging generalizations. They harbored no illusions about public or Congressional esteem. After halting attempts to tout wartime achievements on the model of Vannevar Bush’s 1945 *Science: The Endless Frontier*, they emerged from the debates surrounding a proposed National Science Foundation chastened by dismissals not just from conservative Congressional quarters but from key figures in the natural science establishment (Klausner 1986). In the first few postwar years, federal funding had slipped back to stingy pre-war levels, and grant-making by Carnegie and Rockefeller—although significant—could not make up the difference (see Solovey 2012 and Solovey 2013).

Though not yet fully expressed, a set of intellectual “family resemblances” linked early postwar social scientists to one another, with roots in the shared war service. These included (1) an embrace of new and established quantitative methods; (2) a preference for abstract, often formal, general theory; (3) faith in mathematics as a key social science tool; (4) enthusiasm for cross-disciplinary team research, (5) often organized around applied problems which, however, were deemed conducive to theoretical and substantive progress.

When heightened Cold War tensions in 1947 and 1948 convinced military and government officials to, in effect, re-mobilize the wartime morale and propaganda networks, social scientists who shared these convictions found themselves in a stronger patronage position. The Ford Foundation would soon provide more funds, along with the “behavioral sciences” label itself.

34 On the Rockefeller Foundation’s investments from 1939 through 1941 in propaganda and morale activities that were, at the time, politically unacceptable for the Roosevelt administration, see Gary (1996).
35 The best summary is Crowther-Heyck (2005), ch. 5.
ROWAN GAITHER’S STUDY COMMITTEE AND THE MODERN FORD FOUNDATION

The Ford Foundation’s decision to establish a “Behavioral Sciences Program” originated in Ford’s late 1940s transformation from a minor regional philanthropy into the world’s largest foundation. To guide the transition, Ford commissioned a study team led by H. Rowan Gaither to plot a vision appropriate to the foundation’s new wealth and national stature. Gaither’s committee of academics quickly concluded that the social sciences should be Ford’s main focus. From the beginning, however, committee members expressed discomfort with the prevailing “social science” terminology. Their meetings in 1948 and 1949 coincided with dramatic and fast-developing Cold War escalations. The geopolitical backdrop was a decisive influence on their overall plans, but also affected word choice. Conservatives in Congress, and even some natural scientists, had repeatedly conflated “social science” with “social reform” and “socialism.” Committee members, already eager to promote the quantitative and “scientific” end of the social science spectrum, cast about for alternative language. After considering a number of candidates, the foundation ultimately settled on “behavioral sciences.”

The “behavioral sciences” choice solved two overlapping problems: (1) the term could not be confused for “socialism,” and (2) signaled an intellectual break with a “speculative,” pre-scientific social science past. There were other advantages: (3) the label was judged more palatable to potential participation from biologists and other non-social scientists, and (4) inclusive of those psychologists who remained resistant to the “social science” moniker. It was, however, the first two benefits—cover from the “socialism” charge and the linguistic mark of intellectual leave-taking—that proved decisive for Ford’s embrace.

The Ford Foundation, created in 1936, was for its first decade a relatively small, Detroit-based regional philanthropy. This all changed in 1947, when Henry Ford’s death left the foundation with 90 percent of Ford Motor Company’s stock. With the dramatic recovery of Ford Motor’s fortunes in the immediate postwar years, the foundation instantly became the world’s wealthiest philanthropy by far, with an estimated $417 million in assets by 1951 (compared to the Rockefeller Foundation’s $122 million and Carnegie Corporation’s $170 million) (Sutton 1987: 52).

In the fall of 1948 Karl Compton, Ford trustee and president of MIT, recommended H. Rowan Gaither to preside over a Study Committee charged with generating a plan to recast the suddenly gigantic foundation. Gaither, an attorney who had served as Compton’s assistant at MIT’s wartime Rad Lab, had just led the process to recharter what had been Project RAND into the nonprofit RAND Corporation. He remained chair of the Air Force-funded think tank throughout the Study Committee period (Kaplan 1991: 60–62; Smith 1966: 56–60).

Gaither soon recruited the Committee’s six members, all academics and each charged with representing a topical “division.” For our purposes the key selection was Donald Marquis, who—along with Gaither and his staff assistant William McPeak—were the pivotal figures in the foundation’s social science thinking and “behavioral sciences” word choice.36

36 In addition to Marquis (social science), the other five committee members were Thomas Carroll (business), Peter Odegard (political science), Charles Lauritsen (natural science), Francis Spaulding (education), and T. Duckett Jones (health). A seventh member, Yale’s William DeVane, was later named to represent the humanities, in response to public complaints about the Committee’s neglect of the humanities. Gaither, “Activity Report for the Period Ending January 31, 1949,” 31 January 1949, folder 19, box 2, series I, 20003, FFA: 12.
DONALD MARQUIS AND EARLY COLD WAR SOCIAL SCIENCE

As in the case of Gaither, it was Compton who recommended Marquis. A year earlier Compton, also chair of the military’s Research and Development Board (RDB), had recruited Marquis to chair the RDB’s new social science section, the Human Resources Committee.

In these years Marquis’s career was in the ascendance, owing in part to his multiple connections to the military’s emerging constellation of Cold War social science projects. During the war Marquis had led the Office of Psychological Personnel before accepting the chair of Michigan’s struggling psychology department. He successfully revived the department, helping to bring Kurt Lewin’s group dynamics team to Michigan in 1948 and later helping James Grier Miller to reconstitute his stalled behavioral sciences project as Michigan’s Mental Health Research Institute (Capshew 1999: 195–198).

1947 was an important year for Marquis. He was appointed chair of the Human Resources Committee, elected president of the American Psychological Association (APA), and awarded a $10,000 Carnegie grant to produce a “fresh appraisal of the place and functions of the social sciences” (Carnegie Corporation 1947: 32). He was one of just two psychologists—the other was Hull—to attend the Cold War–drenched Project RAND “Conference of Social Scientists” in 1947 that led to the establishment of RAND’s Social Science Division, directed by Hans Speier—who would soon join Marquis in 1951 as a consultant-planner for the BSP. (Bernard Berelson, the future BSP head, was also in attendance.)

Throughout his Ford Foundation service Marquis was an active participant in Cold War psychological warfare research, at the RDB, as a member of Project Troy, and as a consultant to the Psychological Strategy Board (Needell 1993: 401–408; Lucas 1996).

Marquis delivered his 1948 APA presidential address, “Research Planning at the Frontiers of Science,” just three months before joining the Ford Study Committee. The address, a heady manifesto for postwar quantitative social science, served as a blueprint for his Study Committee work. He laid out a six-stage process—“scientific method in its full and complete form”—for coordinated, cross-disciplinary team research, citing Hull’s work as an example. Noting that the military branches are “now the biggest customers for research of all kinds,” Marquis counseled psychologists to seize the opportunity with an “increased number of large and well planned research programs” to contribute

39 Marquis (1944) reported on an Office of Psychological Personnel survey that documented the massive mobilization of psychologists for war service.
40 Conference of Social Scientists [R-106] (1948), 20. The conference’s verbatim transcript shows that Marquis was an especially active participant. He called for a public opinion study of Americans’ goals and values: “[S]uch a study seems to be absolutely basic for any planning of, not mere propaganda, but what can one get away with in national policy, and what are the best approaches” (118). Later he proposed an opinion study to identify “the extreme pro-Russian group” in the U.S. population (123), and endorsed a proposal by Harold Lasswell for a compendium of anti-Marxist arguments: “People have recognized the need for it ever since Marx threatened our stable institutions” (161). For an excellent history of the conference and RAND’s Social Science Division, see Bessner (2015). Speier’s Washington-based Social Science Division, Bessner shows, dissented from the quantified systematization prevailing at RAND’s Santa Monica headquarters—notably including the institute’s Economics Division, based in California. Speier and his Washington colleagues were far more open to historical analysis and close textual reading. The relative ecumenism of RAND’s Social Science Division, established in 1948, may help explain why the unit never adopted the “behavioral sciences” language.
to the "growing integrated body of scientific knowledge." In Ford memos and statements Marquis inserted passages from this address word for word, and the Study Committee's final report would bear its verbal stamp.\footnote{Marquis (1948a), 430, 433–435, 438. A companion paper published the same year stressed similar themes, including the six-stage research process (Marquis 1948b: 412–413).}

**Social Science by Another Name**

Beginning in late 1948 and into spring 1949, the Committee convened for four three-day meetings.\footnote{See "Staff-Committee Memorandum No. 4," 27 December 1948, folder 1, box 1, series I, 20003, FFA: 3; Marquis, "Report of the Social Science Division," January 1950, FFA: 10–15; and Gaither et al. (1950), 95–96.} Though Gaither and his small staff were frustrated by Committee members’ posturing and lack of focus, the meetings did produce an early and surprising consensus that the Ford Foundation’s mission should center on the social sciences.\footnote{For a more detailed account of the deliberations, see Pooley and Solovey (2010), 202–215.}

The decision was apparently reached at the Committee's second meeting in January 1949. Notes from the meeting report a "strong and virtually universal feeling" that "the place to work is in the social sciences."\footnote{On Gaither’s frustration, see Gaither, memo to McPeak and Dyke Brown, 28 April 1949, folder 25, box 3, series I, 20003, FFA; and Gaither to Thomas Carroll, 20 April 1949, folder 23, box 2, series I, 20003, FFA.}

Throughout the spring, the social science focus was justified, obliquely but unmistakably, by the Cold War context; the Berlin Blockade ended just days before the Committee’s final meeting in May.\footnote{"Notes for Discussions with Trustees," 14 January 1949, folder 19, box 2, series I, 20003, FFA: 2. The social science focus remained constant throughout the four meetings. In the fourth and last, the minutes conclude that the final report will "indicate the importance of operation in the general area of the social sciences." “Staff-Committee Memorandum #14,” 12 May 1949, folder 1, box 1, series I, 20002, FFA: 6.}

In justifying the social science recommendation, committee members also repeatedly cited the likely exclusion of the social sciences from the planned National Science Foundation. At Marquis’s suggestion, the Committee commissioned a funding report, completed by sociologist John Riley, that concluded as expected that the social sciences were grossly underfunded.\footnote{"Notes for Discussions with Trustees," 14 January 1949, folder 19, box 2, series I, 20003, FFA: 2.}

In his official “Business Division” report, Committee member Thomas Carroll...
observed that “prevailing attitudes and some misunderstandings of ‘social research’ on the part of business men create special problems of terminology and sensitivity.”

A key fear was plainly the recurrent conflation of “social science” with “socialism,” especially by anti-New Dealers in Congress (Solovey 2012: 410; Solovey 2013: 44, 53; Miller 1955: 513). The Committee’s allergic response to “social science” was closely tied to the socialism slander. As Marquis observed in his “Social Science Division” writeup, there is a “fairly common confusion of social science with ‘social reform’ or even ‘socialism.’” In the report, he aggressively refutes the association: the “spirit” of a “total system” like Marxism is “foreign to that of the social scientist,” who is “more akin to the physician … sober, pedestrian, undramatic.” In a 1972 oral history interview Marquis confirmed that the Committee had worried that the “word social would be confused with socialism and so for [sic] and tried to come up with something else.” Other accounts confirm the central role that the socialism conflation played in the foundation’s search for alternative language.

The other major motivation for a new label—clearly related to the first—was the Committee’s desire to signal a clear intellectual break with the body of social science they deemed speculative and historical. In an early 1948 talk Marquis had referred to the “traditional social sciences” as a “mixture of common sense, speculative philosophy, historical scholarship, religion, wise advice, and some science.” The identical sentence appeared in Marquis’s “Social Science Division” report. Talking points prepared for a presentation to Ford trustees refer to the “many shortcomings” of the existing social sciences. “In many ways they are not scientific enough... consist[ing] of ordinary common sense or personal views rather than verified knowledge,” the document reads. “Too frequently” social scientists have proposed “some sweeping world reform which they thought good.”

Throughout the spring meetings the Committee deployed terminological stand-ins—though not as yet “behavioral sciences.” Instead, the most frequently proposed candidates were “human relations” and “social relations.” Both terms were already in wide circulation by the late 1940s, in part due to the Yale Institute of Human Relations and Harvard’s post-war Department of Social Relations. Marquis had repeatedly favored the “human relations” term in his 1948 publications, and

50 Marquis, “Report of the Social Science Division,” January 1950, FFA: 20–23. The metaphor of the physician—and the broader claim that social scientists can provide technical advice, but not guidance on ultimate values—has roots in the “liberal managerialism” of interwar social science. See Crowther-Heyck (2005), 43–47.
52 Berelson et al., “Proposed Plan for the Development of the BSP,” December 1941, Report No. 002072, FFA: 14. In a 1964 talk Ralph W. Tyler, director of the Center for the Advanced Study of the Behavioral Sciences, admitted, “Another reason for seeking a substitute for the older terminology is the identification on the part of some laymen of the social sciences with social work and with socialism. In several situations, this confusion has had irritating consequences. One way of avoiding this misunderstanding is to rename this group of academic disciplines” (Tyler 1964: 28). James Grier Miller also pointed to the problematic association with socialism as a key factor in his Chicago initiative’s choice of “behavioral sciences”: “we foresaw a possibility of someday seeking to obtain financial support from persons who might confound social science with socialism” (Miller 1955: 513).
53 The February 1948 talk was published as Marquis (1948b).
54 Marquis, “Report of the Social Science Division,” January 1950, FFA, 10. The only change was that scholarship was, in this version, misspelled as “scholorship.”
56 Thomas Carroll was already using the “human relations” terminology in the Committee’s initial December meeting. “Staff-Committee Memorandum #4,” 27 December 1948, folder 1, box 1, series I, 20002, FFA: 4.
Committee member Thomas Carroll had been affiliated with Harvard Business School's Committee on Human Relations.57

Carroll—Gaither's cousin and an especially active participant in the Committee's deliberations58—soon proposed the awkward “social (human) relations” phrase.59 A lengthy memo jointly authored by Carroll and Marquis in advance of the Committee's final meeting in May continued to employ the “social (human) relations” language.60 That term's obvious inadequacy prompted the Committee and its staff to generate a parade of increasingly cumbersome prospective labels. In May, as Gaither prepared to brief trustees on the Committee's social science vision, his staff floated “social relations and human behavior.”61 Gaither's late May memo to trustees referred to “human relations and social organization.”62 As if to exhaust every possible permutation, a June staff memo made use of “human relations, social organization and human behavior.”63

THE GAITHER REPORT AND THE TURN TO “BEHAVIORAL SCIENCES”

William McPeak and a hired staff writer labored on a draft of the Committee's final report over the summer. In addition to the social science focus recommended by the Committee, the draft report called for an expanded program to include economic, political, educational, and international issues. In the new plan proposed by the draft report, support for social science shared billing with four other named programs: Area One (“The Establishment of Peace”), Area Two (“The Strengthening of Democracy”), Area Three (“The Strengthening of the Economy”), and Area Four (“Education in a Democratic Society”). To designate the social science–oriented Area Five, Gaither and his staff selected yet another compound phrase: “Individual Behavior and Human Relations.”64 Area Five was positioned as a basic scholarly unit intended to service the other four, more substantive areas.


60 Carroll and Marquis, “Suggested Program Area- Social (Human) Relations,” May 1949, folder 26, box 3, series I, 2004, FFA. Aside from a brief introduction written by Carroll, the document's sections were drafted separately: a lengthy write-up from Carroll and three short memos by Marquis. Marquis's memos are suffused with a familiar mix of unqualified scientism and applied Cold War urgency. He called for a “systematic attempt to formulate” principles “in rigorous fashion,” while also nodding to heightened geopolitical tensions: “Such slowness in the acquisition of new information in this area and slowness in application of what is known is tolerable in stable times. It could be disastrous in the present unstable ones. The deliberate modification of some aspects of the behavior of large segments of the population of the world may be the best answer to some of the threatening aspects of the world situation.” Ford support is vital, Marquis wrote, because of the military's fixation on short-term results, but also because “government agencies are peculiarly vulnerable to charges of promoting propaganda.” Marquis, “Modification of Behavior through Education and Training,” 26 April 1949, folder 35, box 3, series I, 20004, FFA: 1.


62 Gaither, memo to trustees, 23 May 1949, folder 20, box 2, series I, 20003, FFA.

63 Dyke Brown, memo to Gaither, 10 June 1949, folder 20, box 2, series I, 20003, FFA.

64 An undated “Table to Rank Program Areas,” circulated by Gaither at some point in the summer, refers to “Individual Behavioral and Human Relations.” Table to rank program areas, n.d., 1949, folder 25, box 3, series I, 20004, FFA.
Archival records do not detail the reasoning that led Gaither and his small staff to expand the plan’s scope beyond the social science focus recommended by the Study Committee. It is likely that the change was prompted by the revelation that the foundation would possess far greater resources than Gaither or the Committee originally assumed (cf. Sutton 1987: 47, 52–53). It is also possible that trustees had insisted on a broader, more substantive mandate, though memos exchanged between Gaither and his staff in early summer still indicate an exclusive focus on social science. Perhaps the task of translating the Committee’s disjointed recommendations into a coherent narrative persuaded Gaither and the staff to rethink the structure of the report.

Regardless, by the fall of 1949 a draft of the Committee’s final report was completed. It was William McPeak who wrote the social-science oriented Area Five narrative, and who emerged as its most effective champion. A former journalist, McPeak had served as field staff director in Samuel Stouffer’s wartime Army Research Branch. The Research Branch’s four-volume American Soldier study, published after the war with Carnegie Corporation support, served as a celebrated model for large-scale cross-disciplinary research projects.

McPeak had written a spirited defense of the Study Committee’s social science vision in preparation for Gaither’s May appearance before the trustees. McPeak later wrote and delivered the key pitch on behalf of Area Five to trustees in February 1950. On his return to the foundation in 1953, he served as the BSP’s supervising officer until its 1957 closure, winning plaudits for his effective oversight.

Though the fall 1949 draft of the final report does not survive, it is unlikely that the document made explicit reference to the “behavioral sciences.” The term appears nowhere in the Committee deliberations nor in the staff correspondence exchanged during the report’s drafting. The published version of October 1950 does mention “behavioral sciences,” though otherwise hews to the “indi-
individual behavior and human relations” language (Gaither et al. 1950: 94). The “behavioral sciences” references were probably the result of a later edit, since the term only appears in the archival record in early 1950.

It was Marquis who seems to have introduced the “behavioral sciences” label. In preparation for his “Social Science Division” report, he organized a small conference of social scientists in August at the offices of the SSRC in New York and interviewed a series of leading scholars. Marquis’s division report, dated January 1950, contains the first recorded reference to the term. In the report’s conclusion, Marquis writes, “If a program of research in the behavioral sciences of any considerable magnitude is contemplated, it is necessary to consider the resources available” (Gaither et al. 1950: 34). It is unlikely that McPeak or Gaither suggested the label, since Marquis later confirmed that he had introduced the term, and archival evidence supports his claim.

In his 1972 oral history interview, a somewhat regretful Marquis recalled that he selected “behavioral sciences” to avoid the socialism conflation, and also to mark off Ford’s science-oriented approach. “I was eager,” he said, 

to make a distinction between soft science and hard science. That happens to be my bias to approach psychological and social problems with the standard methods of science and a different label enabled us to define an area rather than to accept largely defined areas. So, behavioral science, was in our thinking, in fact, interpreted as a scientific approach to social problems and originally much too narrowly. This view was shared by [Bernard] Berelson who was appointed to head it [in 1951]. I wouldn’t try to do that now, I would put in things like policy and history and so forth. But I was young, eager, overconfident, and not nearly as disillusioned as I am now that the scientific approach to social problems would be the best way to attack them. I wouldn’t defend that now.

As already noted, Marquis was equivocal about the term’s origins, conceding that his friend James Grier Miller might have coined the label. In an oral history interview, he observed that Ford’s use was “almost simultaneous” with Miller’s Chicago initiative, and that the two were “interacting much at that time.”

Miller was among the social scientists that Marquis had consulted for his division report. As Philippe Fontaine has detailed, Miller was the central figure (along with neurophysiologist Ralph Gerard) in a cross-disciplinary University of Chicago “Committee on Neural-Mental Problems” convened the same fall to foster conversation among the “mental sciences.” By November 1949, the nascent Chicago group had changed its name to the “University Committee on Mental and Behav-

73 On the conference, see Odegard, “Report of the Political Science Division,” November 1949, FFA: 11–12. On Marquis’s interviews, see Marquis, “Report of the Social Science Division,” January 1950, FFA: 1–4. Among the many scholars that Marquis consulted were Dorwin Cartwright, James Grier Miller, Talcott Parsons, Kenneth Boulding, J.M. Clark, Philip Hauser, and former Yale colleagues George Peter Murdock and Robert Sears. Marquis also appointed an “advisory panel” consisting of sociologist Robert Merton, economist Theodore Schultz, and political scientist Pendleton Herring, the SSRC president.

74 A 1951 BSP planning memo implied that Marquis had introduced the term. Included among a list of remaining tasks for the summer of 1951—as the BSP plan was being developed—is: “Try to secure memo from Marquis defining behavioral science.” Hans Speier, Marquis, Dyke Brown, memo to Gaither, 12 June 1951, folder 75, box 7, series V, 20046. There is no evidence that Marquis ever furnished a definition.


ioral Sciences,” and by mid-December Miller had drafted a proposal for an “Institute of the Behavioral Sciences” (Fontaine forthcoming). Marquis’s division report, with the first recorded Ford mention of “behavioral sciences,” appeared the next month.

Through a number of permutations Miller’s much smaller initiative—the planning group for these was often called the “Committee on the Behavioral Sciences” after 1952—was eventually reconstituted in 1955 as the University of Michigan’s Mental Health Research Institute.77 Fittingly, Marquis played a key role in facilitating the move and securing funds for the Michigan Institute (Capshew 1999: 197–198). Miller’s idiosyncratic definition of “behavioral science”—with its biological, “living systems” inflection—would help sow confusion over the already-nebulous term, especially after Miller co-founded the systems science-focused journal *Behavioral Science* in 1956.78

By February 1950, Gaither, McPeak and Marquis had settled on the “behavioral sciences” label to describe Area Five. Judging by the 18-page script he prepared, McPeak issued a hard-sell brief for the social science initiative.79 He pointed to the social sciences’ likely exclusion from the planned NSF, and claimed that Rockefeller might reduce its support by half. Citing the Committee’s social science funding report, he asserted that Carnegie would only fund applied, and not basic, research—a dubious assertion given Carnegie’s ongoing funding for both the *American Soldier* series and Talcott Parsons’s major theory-building initiative (Lagemann 1992: 168–172). With the government and the other big foundations bowing out, “[t]here is no one else to look out for” the social sciences. McPeak pointedly quoted James Conant—chemist, Harvard president and major figure in the natural science establishment—endorsing more investment in social science research.80

McPeak also stressed the Cold War urgency of “scientific and systematic” knowledge. Because of the “extreme differences between our beliefs and those of others,” we will, he said, “have to get to the bottom of such differences by understanding.” Armed with that knowledge, he continued, “we will be better equipped to deal with either peace or war.”81

At this point in the script, McPeak offered a somewhat tortured explanation for the Committee’s aversion to the “social science” label. “Just as those who do research to get knowledge of nature are called natural scientists, those who try systematically to learn about man and his behavior are frequently called social scientists,” he admitted. But “here the parallel breaks down,” because the “social sciences” term, “as we would like to use it, appears to be loosely constructed.” He noted that in addition to the five major academic social science disciplines, the term often refers to a broad range of professionals, including lawyers, accountants, public relations counselors, “or many other things.” But since Area Five’s emphasis is on knowledge of “human behavior,” the Study Committee is “not concerned with all of these groups to the same extent.” Rather, the focus is on those social scientists who are “becoming more scientific all the time,” who have “borrowed many techniques from the natural sciences, and ... devising more of their own.” Rather than refer to “social scientists,” McPeak explained, “we would like to use the term behavioral scientists.” In a nod to

77 Ibid.
78 “Editorial,” Behavioral Science 1, no. 1 (1956): 1–5; and Hammond (2003), 143–196; Hammond and Wilby (2006). Marquis was among the 10 scholars who signed the initial editorial in Behavioral Science, though does not seem to have been otherwise involved.
79 William McPeak, “Presentation of Program Five,” 13 February 1950, folder 74, box 7, series V, 20046, FFA.
80 The quote is from Conant (1948), 80.
81 McPeak, “Presentation of Program Five,” 5. From there also the following quotations.
trustees’ likely disinterest in such terminological nuances, McPeak half-apologized: “If you will let us use this term for the rest of the discussion, we won’t use more technical terms.”

McPeak, however, offered two additional justifications. He noted, first, that the foundation hoped to also support “certain medical men, and psychiatrists, geneticists and other natural scientists, and social workers”—all of whom research behavior, but do not identify with the “social science” label.

He also justified the new term by restricting its application to just three of the social science disciplines, to the exclusion of economics and political science. “We are,” he said, “very much interested in the sociologists, the anthropologists and the psychologists, particularly the more scientific ones.” Tellingly, economics and political science had appeared on the longer list of “various interpretations” of “social science” that McPeak had already cited as evidence of the term’s overly broad connotations. The Area Five write-up in the published Study Committee report also explicitly lists sociology, psychology, and anthropology, with no mention of the other two disciplines (Gaither et al. 1950: 92).

There was precedent for the three-field restriction: the Yale Institute (which, however, included few sociologists) and Harvard’s Social Relations department. But for reasons that Mark Solovey and I have detailed elsewhere, the exclusion of economics and political science was almost certainly the result of internal Ford Foundation politics. Political science and economics had, in effect, already been “claimed” by Area Two (“The Strengthening of Democracy”), and Area Three (“The Strengthening of the Economy”). We concluded that the anthropology-sociology-psychology formulation was likely a post-hoc rationalization.82

The three-field formula introduced a definitional ambiguity that would go on to plague the BSP throughout its existence.83 At many other points, after all, the “behavioral sciences” term was plainly defined to include all the social sciences, or at least their “scientific” wings. The exclusion of economics proved less problematic, if only because economists secured their own substantial funding stream for basic research through Area Three, after resisting overtures from the BSP. The Area

---

82 Pooley and Solovey (2010), especially 213–217. Bernard Berelson, in his 1957 postmortem on the BSP, also suggested that turf disputes were responsible for the narrow, three-field definition. He observed that the “behavioral sciences” term was chosen over “social science” because other foundation program areas had jurisdiction over key social science fields: “The familiar term ‘social sciences’ includes at least three major disciplines—economics, political science, and history—that were not typically included in the ‘behavioral sciences,’ if for no other reason simply because they were dealt with elsewhere in the Foundation.” Berelson, “The Ford Foundation BSP Final Report, 1951–1957.” September 1957, Report No. 010548, FAA: 3.

83 One example of the staying power of the three-field definition—with influence, perhaps, from Miller’s definition—from 1964: “In some circles, membership in the behavioral science club is limited to psychology, sociology and anthropology. In other circles, membership is extended or perhaps limited to mathematics, psychiatry, and neuron-physiology. In many circles, however, membership is denied to such fields as history, economics and political science” (Kramer 1964: 192). In its 1965 official report to a Congressional science committee, the National Academy of Sciences adopted the three-field definition, complete with awkward qualification: “The term behavioral sciences is of relatively recent origin and emphasizes those parts of social science that attempt to solve their problems by empirical and scientific methods. It includes most of contemporary psychology, sociology, anthropology, and certain aspects of political science and economics” (National Academy of Sciences Committee on Science and Public Policy 1965: 203). Even the History of the Behavioral Sciences Newsletter—predecessor to the Journal of the History of the Behavioral Sciences—used a modified form of the three-field formulation: “It deals only with historical aspects of the behavioral sciences and is directed toward all those working in this area—primarily anthropologists, psychiatrists, psychologists, and sociologists, but also biologists, neurologists, historians and any other interested individuals.” See Senn (1966), 107–108.
Three opportunity, along with pre-existing professional and intellectual cleavages centered on the rationality assumption, convinced many economists to reject the “behavioral sciences” label.84

For political scientists, however, the quasi-exclusion from Area Five proved more troubling. Political scientists, after all, were fully integrated into the World War II network of propaganda and morale service that formed the core of elite postwar social science.85 A younger generation of empirically inclined political scientists were caught up in the same intellectual currents captivating other social scientists. Soon after the appearance of Ford’s published report in 1950, complaints about the discipline’s apparent exclusion began to surface. Confusingly, however, political scientists were relatively well-funded by the BSP. Indeed, they received a far larger proportion of grants than anthropologists.86

Later documents associated with the BSP try to resolve the discrepancy—between the definition referring to science-oriented social science writ large and the other definition referring to just three disciplines—by designating anthropology, sociology, and psychology as the “core” behavioral sciences.87 Still, Ford’s definitional gymnastics may have contributed to political scientists’ terminological exceptionalism—their preference for the “behavioralism” label that in most respects designated an intellectual worldview common to self-described “behavioral scientists.” As Emily Hauptmann has recently concluded, “The story of the rise of behavioralism in political science ... is best told as a subplot in the bigger story of the rise of the behavioral sciences.”88

At their February 1950 meeting, Ford trustees formally approved Area Five. The Study Committee’s final report—The Report of the Study for the Ford Foundation on Policy and Program—was finally published in the fall of 1950.89 The Gaither Report, as it came to be known, struck a delicate balance between stated commitments to peace and universal welfare, on the one hand, and the escalating Cold War conflict, on the other. The “individual behavior and human relations” language remained dominant, but “behavioral sciences” appeared four times—all on a single page (Gaither et al. 1950: 94).

**THE BEHAVIORAL SCIENCES PROGRAM**

By the time that planning for Area Five picked up in the spring of 1951, the “behavioral sciences” label was already established as Ford’s term of choice. Throughout 1951 Gaither shepherded the program’s planning process, retaining Marquis and Hans Speier—the head of RAND’s Social Science division—as consultants. In late summer, Gaither, Marquis and Speier were joined by Bernard Berelson, the library scientist hired to lead the program. The four men collaborated on a detailed planning document, whose extensive revisions were informed by a frenetic series of cross-country

---

84 Pooley and Solovey (2010), 219–230. Cherrier (2009), however, argues that we over-emphasize the unity of economists in this period, in her own analysis of episodes in Ford’s early history.

85 Farr et al. (2006), 580–583. The political scientists who worked with Harold Lasswell, principally at his Library of Congress content analysis operation, constituted a significant share of the early behavioralists in political science, including Heinz Eulau, David Truman, Ithiel de Sola Pool, and Sebastian de Grazia.


interviews and conferences in the fall of 1951. Ford trustees formally approved the plan in early 1952, and the new Behavioral Sciences Program (BSP) was announced to the world.

With his Study Committee duties complete, Gaither agreed to lead the planning effort for Area Five. Throughout the planning process Gaither was an aggressive advocate for the program, in the face of indifference and outright hostility from the foundation’s newly appointed leadership. Gaither had no formal social science training, but for reasons that remain unclear he repeatedly protected the program, first in the planning process and the BSP’s early years, and later as the foundation’s president from 1953 to 1956 (Sutton 1987: 84; Solovey 2013: 112–127). Berelson, in a 1972 oral history, speculated that Gaither’s exposure to social scientists at RAND—notably Speier—and his Study Committee experience had cultivated his “deep faith” in the BSP. “If it hadn’t been for Rowan [Gaither] at that time, I think it’s very likely that the behavioral sciences part of the Foundation program wouldn’t have got activated at all.” Throughout his tenure at Ford, Berelson recalled, Gaither “protected,” “nurtured,” and “ran interference for” the BSP in response to internal and external critics alike. One reason that trustees succeeded in closing the BSP in 1957 was that Gaither had by then stepped down.

Gaither’s early protection was necessary because the foundation had appointed, in late 1950, Paul Hoffman as its first president. Hoffman, a former businessman and administrator of the Marshall Plan, was by all accounts indifferent to, and perplexed by, the developing behavioral sciences program. He even referred to the behavioral sciences as a “good field” to “waste millions and get nothing” (Raucher 1985: 85, cited in Solovey 2013: 116). Though he grudgingly supported the BSP establishment, a memo from the period notes that “in his questions and comments Mr. Hoffman indicated that he was not clear in his own mind about the limitations to be placed upon the subject matter covered by” the BSP.

Robert Maynard Hutchins, meanwhile, was in open revolt. Hoffman had appointed Hutchins, the former University of Chicago president, as one of his associate directors. Hutchins was withering in his criticism of plans for Area Five, calling the behavioral sciences “utter nonsense” (Sutton 1987: 72). He opposed Berelson’s hiring, even insisting that the foundation document his objection. By the early 1950s Hutchins had long since abandoned the quantitative evangelism that characterized his Yale Institute proposals from the late 1920s.

90 An outline of pending grants from the spring of 1950 records that trustees, when they approved Area Five in February 1950, had appointed Gaither to head the project. “Grants Applications Pending,” April 1950, folder 56, box 4, series I, 20007, FFA: 18. Largely at Compton’s urging, Gaither was formally appointed as a (part-time) Associate Director in 1951, with oversight of the BSP. See Sutton (1987), 62, 71–72.

91 Gaither was the “one who protected [the BSP] always,” Berelson added. Oral history interview of Bernard Berelson, 7 July 1972, Ford Foundation Oral History Project, FFA: 3–6. Solovey (2013: 125–127) confirms Gaither’s advocacy but questions how effective his protection was as McCarthyite pressure on the BSP mounted in the mid-1950s. Soon after leaving Ford, Gaither chaired the Security Resources Panel, the Eisenhower administration task force that produced the secretive Deterrence & Survival in the Nuclear Age (Washington, DC: Science Advisory Council, 1957)—the other “Gaither Report.”

92 Francis Sutton, based on Ford documents, his own experience, and a number of interviews, recalled that Hoffman had “little understanding or sympathy” for Area Five (Sutton 1987: 62, 71–72). See also Berelson, oral history, 5; and Solovey (2013), 119.

93 Speier, Marquis, and Berelson, memo to Gaither, 20 December 1951, folder 75, box 7, series V, 20046, FFA.

94 In his oral history interview, Berelson read from a May 1951 staff memo: “Mr. Hutchins requested that the record note that he had nothing to do with this appointment.” Berelson, oral history, 1–3.
By spring 1951 Marquis and Speier had begun meeting to discuss the plans for Area Five.95 Gaither had come to respect Speier—the Karl Mannheim student turned avid Cold Warrior—through RAND. Gaither had even consulted Speier, a political sociologist, on the plans for the Study Committee before its members were appointed in late 1948.96

The Speier-Marquis conversations were marked by undisguised Cold War concerns.97 The Korean War was in full swing, and both men had been working on the State Department’s Project Troy (Needell 1993: 401). In early exchanges the two scholars discussed how the behavioral sciences program could collaborate with Troy and the government’s Psychological Strategy Board.98 Much of Speier’s attention was trained on developing a proposal for an “Institute of International Communication” focused on psychological warfare research.99 The next year Ford approved $875,000 for what became the Center for International Communication, led by political scientist Ithiel de Sola Pool as a division of MIT’s Center for International Studies (CENIS).100

By September, Speier, Marquis, Gaither and Berelson—who had been hired in August—completed a draft plan for a “Program in Behavioral Science Research.”101 The four men, and especially Gaither and Berelson, spent the fall soliciting feedback on the draft and making revisions. After five full revisions and a pair of late November conferences in New York and Chicago, Gaither finally submitted the “Proposed Plan for the Development of the Behavioral Sciences Program” to officers.102 Early the next year trustees gave their formal approval to the Behavioral Sciences Program.

Later Berelson would refer to “behavioral sciences” as a “not particularly felicitous” phrase, but as it turned out the label proved far more durable than the short-lived BSP.103

III. ‘A Genuine Need for a Collective Term’: Tracking the Term’s Rise

The advent in recent years of large-scale, full-text databases has drastically simplified the process of tracing the careers of specific terms. An often-overlooked application is the ability to trace the aggregate spread of phrases over time—until recently, an impractical task. By tallying the use of

95 Marquis, memo to Gaither and Speier, 24 April 1951, folder 74, box 7, series V, 20046, FFA.
96 “Memo: Conference—November 18,” 19 November 1948, folder 19, box 2, series I, 20003, FFA. As Berelson later observed, “At RAND they developed a Social Science Division, [Gaither] became relatively close, I think, to Hans Speier, came to have a very great respect for such people—and followed very closely what went on at RAND along this line as a kind of counter to the hardware aspect of RAND.” Oral history interview, 3–4.
97 They were well primed by Gaither. In an early 1951 memo to Hoffman, Gaither wrote, “War is not necessarily inevitable or immediately imminent; however, the threat of total war is so great that the United States and the free peoples of the world must mobilize their economic and human resources to deter aggression, to achieve peace, and to assure victory should war come.” Gaither, memo to Hoffman, 2 January 1951, folder 57, box 4, series I, 20007, FFA.
100 See Anonymous (1954), 358; Schwoch (2009), 62–65. On the larger CENIS context, see Blackmer (2012).
102 See Gaither’s exhaustive account. Ibid.
“behavioral sciences” in databases like JSTOR and Google’s massive book corpus, it is possible to chart the label’s rapid uptake in the early 1950s. The resulting data strongly support the claim that the “behavioral sciences” term was launched into wide academic circulation by Ford’s BSP.

**GOOGLE BOOKS NGRAM VIEWER**

One useful measure is supplied by Google Books Ngram Viewer tool, which plots terms’ frequency of use over time within the more than 20 million English-language scanned books in the Google Books corpus. The results as plotted are normalized by the number of books published in each year.

Fig. 1 shows the relative frequency of “social sciences,” “human sciences,” and “behavioral sciences” from 1890 to 2000, with the y-axis displaying the percentage of matches among the entire corpus. The consistent dominance of “social sciences” is notable, as is the term’s steady increase in the early 20th century. In the late 19th and early 20th centuries, the “social sciences” label was itself a contested and unstable term, even as the individual disciplines were establishing themselves from diverse origins (see Manicas 1990: 45–48).

The “human sciences” term is used relatively infrequently, though it has registered significant gains in recent decades—a rise that partly reflects, perhaps, the term’s softer, more humanistic connotations as an alternative to the more scientistic associations of “social sciences” and especially “behavioral sciences.”

The “behavioral sciences” label, as already discussed, did not surface until 1935, with a trickle of appearances over the next 15 years. The Ngram Viewer records near-zero use up until the 1950s.

Fig. 2 displays the frequency with which either “behavioral sciences” or “behavioral science” appeared in Google’s corpus, from 1940 to 2000.

---

The term first registers in 1951, and appears with increasing frequency over the next two decades. The label’s frequency of use—normalized for the total number of books published—plateaus in the 1970s, followed by a definite decline over the 1980s and 1990s. The sharp increase in the term’s early 1950s use closely tracks the BSP’s 1951 establishment and its active advocacy for the term.

Fig. 3 tells a similar story, with results from 1945 to 1960. The term’s use suddenly accelerates in concert with the Ford program.

**JSTOR**

Another useful database is JSTOR, which provides a digital archive of over 1,600 academic journals. One benefit of JSTOR is that its results are confined to scholarly research. JSTOR’s full-text

---

105 “JSTOR,” Ithaka, accessed October 2, 2012, http://www.jstor.org. Psychology journals are not well-represented in JSTOR, but the overall pattern would not likely change significantly were more psychology journals included.
results, moreover, include links to each item’s matched pages whose contents can then be subject to traditional close readings with links to each item’s relevant pages.

Fig. 4 plots the sheer frequency of articles or reviews in the full JSTOR database that include “behavioral sciences” or “behavioral science” in their contents. (Note that the results are absolute, and have not been normalized by the total number of database items each year.) As expected, there are very few articles or reviews that include the phrase up through 1945.

The chart provides striking evidence for a Ford-linked upsurge in the term’s use: In the five post-war years, there were under 15 mentions of the term. The next five years, up through 1954, registered almost 125 uses. That number jumped to 1130 over the second half of the decade, and then on to over 2000 appearances from 1960 to 1964. The “behavioral sciences” matches continued to rise steeply from 1965 to 1969, with well over 4500 mentions in the period. The term’s use plateaued in the 1970s with slight declines through 1994, even as the total number of annual items in the JSTOR database grew steadily.⁶⁶

Fig. 4: Number of articles including “behavioral sciences” OR “behavioral science” from 1900 to 1994. Source: “JSTOR,” Ithaka, accessed February 29, 2016.

⁶⁶ Results after 1994 are not included, since many publishers opt out of JSTOR to restrict access to recent journal issues.
Fig. 5, a year-by-year picture of the term’s uptake from 1945 through 1960, provides a more granular view. Published mentions of “behavioral sciences” jump significantly in 1953 and continue to grow sharply for the remainder of the decade. The 1953 uptick is consistent with a significant Ford role, taking into account the time lag from submission to publication typical in academic publishing.

The dramatic upswing in the “behavioral sciences” language could reflect uses linked to Bentley, Hull, philosopher Charles Morris (whose 1946 book featured the term prominently; see note 32), or even James Grier Miller’s behavioral sciences initiative at Chicago and Michigan. That is, the surge of mentions may have little to do with Ford’s early 1950s embrace of the term. To test that possibility, I tagged each matched article or review up through 1955, to record those items related to Bentley, Hull, Morris, and/or Miller. An item was judged “related” if the “behavioral sciences” use was authored by, or explicitly cited to, or plainly implied, one of the four scholars or their projects.

Fig. 6 displays the results of the tagging, plotted year by year. Because there were so few items “related” to any of the four scholars, the Bentley, Hull, Morris, and Miller totals were combined, and plotted against the untagged items.
In no single year did the total number of items linked to any of the four authors ever exceed three. The first two items, in 1937 and 1939, are a review of Bentley’s *Behavior, Knowledge, Fact* and an article authored by Bentley (Eldridge 1937: 440, 442; Bentley 1939: 412). Only seven of the additional 17 articles or reviews appearing in the 1940s are linked to Bentley, Hull, Miller or Morris. Beginning in 1951 and sharply accelerating in 1953 are reviews and items that make no explicit or implied reference to the four scholars. Indeed, by the mid-1950s the vast majority of matched JSTOR items are generic and uncited. It was Ford that introduced and popularized the relatively inclusive definition of the “behavioral sciences.” Of Bentley, Hull, Miller, and Morris, only Hull’s sometime use of the term is comparatively catholic;[^107] Bentley, Miller and Morris each advanced much more particularized meanings.

The “behavioral sciences” label, in short, was in limited circulation many years before the Ford embrace. Indeed, I have argued that one of those early uses—Hull’s—was the likely source for Marquis and the other Ford planners. Even so, the evidence suggests that “behavioral sciences” only gained significant traction after—and arguably as a result of—Ford’s decision to champion (and underwrite) the term.

[^107]: Of course both Hull and Ford still used the term to exclude social science work judged to be “unscientific.”
IV. ‘The So-Called Behavioral Sciences’: The Term Spreads Despite Objections

Over the course of its brief lifespan, the Behavioral Sciences Program managed to disburse almost $42 million.\(^{108}\) Still, the program largely failed to fulfill its initial aims. Ironically, it was the Cold War—or at least its McCarthyite by-product—that sealed the BSP’s fate. The “behavioral sciences” language—chosen to avoid the “socialism” conflation and to signal scientific rigor—failed to shield the program. The socialism accusation was made anyway, and the avowed objectivity only attracted charges of amoral “scientism” from right-wing critics. In the mid-1950s, aggressive scrutiny from conservative lawmakers in Congress and controversy over a handful of BSP grants and proposals convinced skittish trustees—with no Gaither to convince them otherwise—to shutter the program in 1957.\(^{109}\)

In a bitter postmortem, Berelson laid some of the blame on the “behavioral sciences” term itself. Since it was “not only unfamiliar but associated with a center of power, it disturbed a number of people and undoubtedly made a certain amount of trouble for the Program both in and out of the Foundation.”\(^{110}\) Ford’s sponsorship of the term—so plainly the source of its widespread adoption—also contributed to the demise of the program it named.

Indeed, the term generated suspicion from the beginning, even among social scientists otherwise sympathetic to the foundation’s vision. For scholars already hostile to what they viewed as a spreading scientism, Ford’s “behavioral science” neologism was an especially loathsome metonym.

And yet the term gained purchase anyway. Withering denunciation from opponents—and a lukewarm, half-sincere embrace from its putative supporters—were not enough to stall its momentum. More surprising still, the term survived the BSP closure. By 1957, and despite the criticism, the label had already settled into the linguistic sediment.

Resistance from Within

The term’s early uptake undoubtedly benefitted from the strategic calculation of would-be grantees, some of whom concealed their otherwise tepid reaction to the new label. The muted resistance was already evident in the program’s very first initiative, issued before the BSP was formally established. In the summer of 1950 the foundation had awarded unsolicited grants of $100 to $300 thousand to 13 universities to conduct “self-studies” of existing and planned initiatives related to the “behavioral sciences.”\(^{111}\) Each grant recipient was asked to convene a committee responsible for a report which, in turn, would get reviewed by a visiting panel of outside scholars. Outside this


\(^{111}\) The $3 million grant, known as the “5.1 Program,” was administered by the SSRC. “Grants Applications Pending,” April 1950, folder 56, box 4, series I, 20007, FFA. Pendleton Herring, memo to Gaither, 25 September 1951, Report No. 010834, FFA.
framework the foundation did not provide detailed guidance, not even a definition of “behavioral sciences”—perhaps because Gaither and the others had not yet settled on one.112

Neither Ford nor the universities were satisfied by the results. Though all 13 accepted the grant, a few of the schools evidently bristled at Ford’s unsolicited approach and vague instructions. Many, and perhaps most, likewise failed to meet Ford’s high expectations for the self studies.

In an early evaluation, Ford claimed that the program “stimulated a great deal of faculty thinking about the development of the behavioral sciences,” but that in “some cases … it seems that the spirit of the grant, if not the letter, was not observed.” Some of the funds granted to Yale and Chicago were applied to “routine expenses,” Harvard used some of its monies for “overhead,” and a number of other universities spent funds outside the scope of the grant. As a group, moreover, the schools employed wildly different criteria for classifying faculty as “behavioral scientists.” Of the $3 million grant, the evaluation concluded, “at least one-fourth (guess) is not in accord with intent.”113 One recipient, UC Berkeley, even sat on its grant for eight years, only organizing a report after a reminder from an irritated Berelson (Hauptmann 2012: 166).

Ford followed up in 1953 with $50,000 grants to five of the original 13 universities—Chicago, Harvard, Michigan, UNC, and Stanford—to underwrite more comprehensive reports. Even in 1953 Ford’s “behavioral sciences” definition remained vague—“all those intellectual activities which contribute to the scientific study of human behavior”—with a sweeping call for recipients to catalog their “total resources for the scientific study of man’s behavior.” The Michigan report noted, pointedly, that the “behavioral sciences” term is “of recent coinage, and its meaning is neither precise nor stable.” The label was “still in process of creation and definition,” Chicago’s committee reported, with a trace of pique.114

Berelson judged the university self-studies a “failure.” In his “Five-Year Report,” he listed the self-studies and various other efforts to stimulate interdisciplinary research as “particularly disappointing.”115 The initiative was based, he later concluded, on an inflated sense of Ford’s sway that probably backfired. He cited his own amateurishness in not knowing that you can’t do that in universities—you can’t reform universities in that way…. the University Survey was a disappointing and, in a way, traumatic episode. It wasn’t a good idea. It got a lot of people sore. The Foundation was trying to tell the universities how to reform themselves. It was bringing outsiders in to look over their shoulder and push them around. The outsiders weren’t going to do it; they were their colleagues, they weren’t mine…. They thought they had to do this in order to get our money.

112 Ibid.
113 “Report on ‘A Program in Behavioral Science Research,’” September 1951, Report No. 010818, FFA: 1–3. The Ford comments are a sober rejoinder to a much more upbeat report from the SSRC, which had received $300,000 to administer the grant. Pendleton Herring, memo to Gaither, 25 September 1951, Report No. 010834, FFA.
114 On the second round of grants, see Macmahon (1955), 857–863. In his thorough review, Macmahon noted the “vogue of the term ‘behavioral sciences,’” adding that Ford “notably helped to give it currency by applying the name Behavioral Sciences Division [sic].” 859. Harvard University published its report as The Behavioral Sciences at Harvard (1954).
115 Geiger (1993), 102–103. As Geiger concludes, the five-year report’s stated disappointments “seem to be associated with the original, rather ambitious Foundation aims for reforming the behavioral sciences; the successes largely buttressed existing practices in the disciplines.”
Despite the push-back, and Berelson’s regrets concerning Ford’s heavy hand, the self-studies may have succeeded on a more a basic level, at least in linguistic terms: Elite social scientists around the country were talking about the “behavioral sciences,” however grudgingly. What started as grant-seeking opportunism could become, with enough repetition, a worn piece of the lexical furniture.

The BSP’s most high-profile project was the Center for the Advanced Study for the Behavioral Sciences (CASBS), which opened near Stanford in 1954 under director Ralph Tyler. Despite the $10 million that the BSP spent to establish and endow the Center—easily its biggest outlay—Berelson considered it an unhappy legacy. "I meant [CASBS] to be a seminal spearhead of new developments in the behavioral sciences,” he stated later. Instead, the Center “became a kind of retreat. And Ralph [Tyler] made it into that.”

Here again, though Berelson’s ambitions were unrealized, the Center’s high-profile existence in itself probably helped spread the “behavioral sciences” phrase. Even today the Center—which indeed functioned more like a scholars’ colony—is perhaps the most prominent linguistic remnant of the “behavioral sciences.”

By 1953, with Hoffman and Hutchins gone, Gaither newly installed in the presidency, and much of the original Study Committee staff—including William McPeak—in officer slots, the BSP had every reason to thrive. Social scientists otherwise sympathetic to the foundation’s initiatives, however, continued to express misgivings about the “behavioral sciences” label—even as the term spread.

The same year, social scientists affiliated with Michigan’s Institute for Social Research published a methodology collection, Research Methods in the Behavioral Sciences (Festinger and Katz 1953). The book’s title signaled acceptance for the new term beyond immediate Ford circles, though Marquis was linked to the project.

Prominent Harvard psychologist Gordon Allport—another presumed ally—instead danced around the label. In a 1955 speech, Allport said, “Personally, I am not entirely happy with [the term] since the science we seek is a science of feeling, of thought, of dreams and of silence, quite as much as of behavior. But philanthropic foundations seem to like the name behavioral science, and we shall raise no objection to it lest Cinderella miss her chance to ride in a golden coach provided by the Foundation” (quoted in Herman 1996: 133).


118 "We are indebted,” the editors wrote, “to Donald Marquis, who participated in the planning of the project and who bears much of the responsibility for the circumstances which made the book possible” (Festinger and Katz 1953: viii).

119 Price (1954), 919. Another reviewer made a very similar observation: Chapanis (1955), 199.
The term, in short, continued to circulate despite an ambivalent reaction from its intended audience. That ambivalence was on awkward display in *The State of the Social Sciences*, a 1956 collection based on a symposium at the University of Chicago (White 1956). The book included a version of Miller’s 1955 “Toward a General Theory of the Behavioral Sciences” essay, along with a chapter from Berelson. Referring to public opinion research, Berelson contrasted “today’s specialist” with bygone scholars who “learnedly” studied in “broad, historical, theoretical and philosophical terms and wrote treatises.” The field has become—in Berelson’s enthused serial—“technical and quantitative, atheoretical, segmentalized and particularized, specialized and institutionalized, ‘modernized’ and ‘groupized’—in short, as a characteristic behavioral science, Americanized” (Berelson 1956: 300, 304).

The book’s editor, Chicago political scientist Leonard D. White, struck a different note. In his introduction, White observed that the “contemporary scene” emphasizes “the scientific rather than the humanistic or the prudential aspects of the subject matter. The center of the stage is now held by the so-called behavioral sciences.” White, whose historical method had fallen into disfavor, registered the slightest unease with the “so-called” behavioral sciences. “Older forms of inquiry,” he wrote, “will nevertheless persist, for wisdom and understanding come from other sources as well as mathematical analysis” (White 1956: xi).

Another prominent Chicago figure, anthropologist Robert Redfield, tied the new phrase to the “millennialistic myth” that the social sciences will one day mirror the natural sciences. “In a time when men become like machines and machines are made like men,” he wrote, “behavioral scientists find it easy to think of the two as much the same” (Redfield 1954: 37, 38).

RESISTANCE FROM WITHOUT

Despite misgivings from within the social science community, the term continued to diffuse rapidly—appearing, for example, in the titles of a number of late 1950s books. In 1962, the “Behavioral Sciences Subpanel” of the President’s Science Advisory Committee published a statement on “Strengthening the Behavioral Sciences” in *Science* (Behavioral Sciences Subpanel 1962). In the statement, the subpanel of social-science luminaries called for a boost in research funding and policy consultation, with “behavioral sciences” defined in vague but resolutely scientific terms. The next year, the National Academy of Sciences renamed its Division of Anthropology and Psychology to the Division of Behavioral Sciences (Gordon and Negri 1966: 48).

The term survived often savage criticism from non-social scientists, too, none of whom were restrained by grant-seeking opportunism. The critic Dwight Macdonald, in his popular book on the Ford Foundation, dismissed the program and label as modish and incoherent (Macdonald 1956: 7, 80–85). Hannah Arendt, the emigre philosopher, attacked the “all-comprehensive pretension of the social sciences which, as ‘behavioral sciences’ aim to reduce man as a whole, in all his activities, to the level of a conditioned and behaving animal.” The rise of “the ‘behavioral sciences,’” she continued, “indicates clearly the final stage of this development, when mass society has devoured all strata of the nation and ‘social behavioral’ has become the standard for all regions of life” (Arendt 1958: 41–42).

120 The statement concedes the “enormous scope and variety of its problems and its methods,” but insists that the “general aims and criteria of evidence of the behavioral sciences are the same as they are in other sciences.” Subpanel members included Neal Miller, James S. Coleman, Leon Festinger, George A. Miller, and Herbert Simon (Behavioral Sciences Subpanel 1962: 233).
The bilious ridicule that often trailed the term is captured by this 1965 parody:

B—Be sure to give esoteric names to pedestrian notions—
E—Even if you must adulterate the notion in the process.
H—Have as little empirical correspondence as possible;
A—Avoid pragmatism as it may be useful.
V—Violate all rules of logic—unless they will prove your point.
I—Invent your own logic—you too can be a scholar!
O—Objectivity is to be ignored. It may prove you wrong.
R—Rationalization is a good word; it describes other people quite well.
A—Always dwell on exceptions. Normality is so boring.
L—Leave the study of business to the trade schools; we would rather be educated.
S—Scientific means hard to understand.
C—Clinical means rats and monkeys—never people.
I—Isn’t this stuff interesting?
E—Even though it’s useless.
N—Neuroses is what we all have. Except me.
C—Culture is different in Scarsdale and Samoa.
E—Emotions are important—especially on quizzes.
S—Stratification, as in social. Looking up, it’s caste. From above, it’s class.


Pitirim Sorokin, the Russian-born Harvard sociologist, attacked the term as an example of “obtuse jargon and sham-scientific slang” (Sorokin 1956: 12, 28). A scathing review of Berelson’s *Human Behavior: An Inventory of Scientific Findings* (“what the behavioral sciences now know”) quipped that the book should be called “The Nature of Intellectual Failure in the Behavioral Sciences.”

In 1963—six years after the BSP closure—a clearly defensive Berelson was still trying to justify the term’s ongoing relevance. Conceding that Ford launched the label—“it was then that some people began to wonder whether they too were not behavioral scientists after all!”—Berelson complained that it has often produced “unduly hostile counteractions.” It is true that the “edges” of the concept are “fuzzy,” but no more so than other terms including “social science” itself. “Accordingly,” he

121 Berelson and Steiner (1964); Henry (1964), 129. Berelson’s book grew out of his failed “inventories” BSP project, which commissioned a series of “systematic inventories” of established postulates from high-profile social scientists including Robert Merton, Paul Lazarsfeld, and Herbert Simon. “None of them came off, none of them,” Berelson recalled, “and in the end I did it for the whole damn field.” Oral history interview of Bernard Berelson, 7 July 1972, Ford Foundation Oral History Project, FFA: 46–47.
pleaded, “I ask for my colleagues here the same detachment and sympathy that are given to students of other subjects that do not carry the emotional load of this effort ...” (Berelson 1963: 3–4).

Despite the criticisms from within and without, the term remained in wide and growing use throughout the 1960s. Berelson himself remarked on the term’s durability. The label, he wrote in 1968, “survived the termination” of the BSP, probably because of a “genuine need for a collective term in addition to the traditional ‘social sciences’” (Berelson 1968: 43).

Conclusion

As Kurt Danziger has observed, the word “behavior” has long been favored for its scientific sheen. By the second decade of the twentieth century, psychologists were already splicing the word into book titles. Far from being a “neutral category,” Danziger wrote, “behavior” had become the “flag of a movement... a quasi-political token.” (Danziger 1997: 94–96).

For Marquis and the Ford Foundation, the “behavioral sciences” near-neologism accomplished something similar. A gathering movement of self-proclaimed scientific rigor secured itself a name and an identity. For opponents of postwar scientism, the same baptismal act helped draw the lines of battle.

Berelson once wrote that the “behavioral sciences” are “here to stay” (Berelson 1963, 11). He was at least partly right: the phrase, though in slow decline by the 1970s, remains in wide use. Nevertheless, the intellectual movement that “behavioral sciences” once named is mostly dissipated. Enthusiasm for quantitative methods, nomothetic theory, modeling or any of the other intellectual features of “high modern” social science remains high in many quarters. But the cross-disciplinary networks of war-trained behavioral scientists embraced them all, with an anticipatory confidence and faith in the reconcilability of science and service that their successors could never maintain. As Hunter Heyck has argued, the decline of the “behavioral sciences” formation began in the early 1960s as a new, more discipline-focused and basic-research-oriented patronage system started to displace the foundation- and military-funded, “broker”-driven system that characterized the early Cold War years (Crowther-Heyck 2006: 420–446).

Revelations about covert military and CIA entanglements in social science research in the mid-1960s, epitomized by the Project Camelot controversy, also contributed to the weakening of the “behavioral sciences” movement (Solovey 2001). By the early 1970s, New Left veterans had joined the faculty ranks, especially in sociology and anthropology. To the insurgents, behavioral scientists’ Cold War service plainly contradicted their putative commitment to value freedom. At the same time a diverse set of challenges to postwar unity-of-science aspirations came to prominence, helping to establish “behavioral science” (and the catch-all “positivism”) as pejorative refrains.

Beginning in the late 1970s, the label took on a ghost-like afterlife in federal funding nomenclature, as a pejorative, and in the vestigial names of journals and scholarly associations. The term remains in circulation, but mainly as an unreflective synonym for psychology (often with a biological inflection) or in conjunction with the wildly successful new field of behavioral economics. In

122 E.g., The Journal of the History of the Behavioral Sciences, the American Behavioral Scientist, Cheiron: The International Society for the History of Behavioral & Social Sciences, and CASBS itself.

123 An interesting example is the name that a group of social scientists aiding the 2012 Obama campaign playfully gave themselves: the “consortium of behavioral scientists” or COBS (Carey 2012: D1).
2015, President Barack Obama issued an executive order formally establishing a “Social and Behavioral Sciences Team” charged with improving government through behavioral science “insights”—defined as “research findings from behavioral economics and psychology about how people make decisions and act on them.”

In its period of ascendance, however, the “behavioral sciences” language was a viable alternative to “social science.” The fact that the term’s spread was almost certainly the direct result of Ford Foundation strategy makes its career an especially interesting case study in what Robert Merton called “sociological semantics.” Unlike, say, the lapidary fortunes of “serendipity,” as traced by Merton and Elinor Barber, “behavioral sciences” arrived in a Ford-sponsored big bang. The term continued to thrive, moreover, in the face of criticism and long after the removal of the Ford training wheels.

In part owing to its sudden emergence and checkered afterlife, the label has been neglected by historians. This paper has sought to restore the term to its important place in the history of postwar American social science.

124 Obama (2015). The same month, the Social and Behavioral Sciences Team issued its first annual report, which uses the same definition (“research findings from behavioral economics and psychology about how people make decisions and act on them”) and whose first two citations are to Kahneman (2003) and to Thayler and Sunstein (2008) (Subcommittee on the Social and Behavioral Sciences Team 2015: xi). Sunstein, the University of Chicago law professor, famously served as head of the Obama administration’s Office of Information and Regulatory Affairs from 2009 to 2012.

Bibliography


Conference of Social Scientists [R-106] (1948) Santa Monica, CA: Project RAND.


Ford Foundation Archives (FFA), Rockefeller Archive Center, Sleepy Hollow, NY.


Jefferson Pooley is associate professor of Media and Communication at Muhlenberg College, USA. He is coeditor of *The History of Media and Communication Research* (2009) and *Media & Social Justice* (2011). His research interests include the history of media research, the history of social science, scholarly communications, and consumer culture and social media.
FORUM

Comparative History and comparative Sociology

Peter Burke
upb1000@cam.ac.uk

I’d like to begin with a paradox. There is a substantial corpus of work that makes use of the comparative method and might be equally well described as social history or historical sociology. However, the view of comparison itself that is held by historians and sociologists, at least the ones of my generation or earlier, is rather different, not to say diametrically opposed; sociologists take the value of comparison for granted, while many historians remain suspicious of it. When they do practice comparison, sociologists are bolder, happily moving across great distances in space and time, like Michael Mann, for example. Historians are much more cautious hence this paper, without my intending it, has turned into a comparative study itself.

Comparative history, in the sense of parallel histories, goes back to the ancient world, to Plutarch and his parallel lives of Greeks and Romans. The approach was revived in the early modern period, especially in Italy, and used to study the virtually simultaneous revolts of the 1640s, extending from Catalonia and Portugal, via Naples and Sicily, to the Ukraine and Istanbul. For example, in 1652 count Maiolino Bisaccioni published an account of these revolts and an attempt to explain them in a book entitled *Guerre civili de gli ultimi tempi*.

In the eighteenth century, when the idea of system was becoming important, Montesquieu, whether or not we follow Raymond Aron in calling him a sociologist, compared and contrasted monarchies and republics as political and social systems, while Adam Smith contrasted what he called the ‘mercantile system’ with the system of free trade.

In the early 19th century, some historians produced parallel histories of states. Ranke wrote on the Ottoman and Spanish Empires (1827), for instance, and Joachim Lelewel, the Polish historian in exile in France, on Spain and Poland in his *Historyczna paralela* (1831).

If one take the comparative method in a more precise sense, however, it was a new discovery beginning in the natural sciences (comparative anatomy, for instance) and spreading to linguistics, sociology and literature as well as to history. In Britain, John Stuart Mill produced a classic discussion of the comparative method. In France, it was advocated by Durkheim, and in Germany by Weber, who considered himself a historian but has been described by posterity as a sociologist (favourably by sociologists but pejoratively by some historians).

In the early twentieth century a comparative approach was advocated by historians of the calibre of the Belgian Henri Pirenne and Marc Bloch. In the USA, Crane Brinton and Roger Merriman pub-
lished comparative studies of revolution, following in the footsteps of Bisaccioni (whether they were aware of this or not).

The comparative approach may be said to have been institutionalized when a journal devoted to the subject was founded over half a century ago, in 1958: *Comparative Studies in Society and History*. It would be interesting to carry out an analysis of the articles, to discover how many are written by sociologists and how many by historians, and also how many are genuinely comparative rather than mini-monographs that the editor juxtaposes to others on similar themes.

Since that time, the comparative method has been used regularly by historical sociologists, notably by three North Americans, Barrington Moore (1966), Theda Skocpol (1979) and Jack Goldstone (1991). Intriguingly, and in a repetition of the first use I cite here, all of them are concerned with revolution. Robert Bellah also uses this method, continuing Weber's work with a study of Buddhism and capitalism in Japan, while other scholars have made comparative studies of bureaucracy and the process of industrialization.

On the other hand, [pure?] historians have been relatively slow to follow the examples of Bloch and Pirenne. In the 1970s, when I edited a series of studies in comparative history, it came to an end rather quickly after only four titles had been published, because I could not persuade more historians to collaborate. Many historians imagined comparison as a simple and doomed search for similarities and dismissed comparison, as some still do, with the phrase 'you can't compare apples and oranges', an idea that irritated the Belgian classicist Marcel Détienne into writing his brilliant essay, *Comparer l'incomparable* (2000).

However, the tide may be turning. Recent British examples include three distinguished contributions. Another classicist, Geoffrey Lloyd, has written about the study of the natural world in ancient Greece and ancient China. The global historian Felipe Fernández-Armesto both compares and contrasts the histories of North and South America. Sir John Elliott, a scholar with a high reputation among both conservative and innovative historians, has long defended comparison and recently published a book about the British and Spanish Empires.

II

In a sense all historical writing is comparative, at least implicitly. Historians like to say that they are concerned with the particular, leaving generalization to social scientists, but as Max Weber once remarked, it is only possible to establish 'what is specific, say, to the medieval city ... if we first find what is missing in other cities (ancient, Chinese, Islamic)'.

Explicitly comparative history comes in a number of varieties. Comparisons are usually made between two or more places, regions or social groups during the same period, leaving comparisons between different centuries to historians of literature and art, scholars who are less tied to a single period than usual for general historians.

However, to focus on the same period for both halves of a comparison may not always be the best strategy. I had to face this problem in my essay on the patricians of Venice and Amsterdam in early modern times, since the apogee of the two groups occurred at different moments, that of the Venetians in the 15th and 16th centuries and that of the Amsterdammers in the 17th century. I decided to study them over the same period, the 17th century, in order to see how the two groups, who were both involved in international trade, responded to the changing economic situation.
Today, one grand comparison between different cultures in different periods awaits a historian ambitious enough to undertake it. I am thinking about comparisons and contrasts between the Christian Reformation of the 16th century and the Muslim Reformation of our own time. Where the first Reformation was associated with the printing press, the second Reformation depends on newer technologies such as the video cassette, allowing the laity to listen to and to discuss sermons in their own homes. I hasten to add that this is not my idea: Ernest Gellner used to speak about Muslim Puritanism, while Eickelman has considered the place of new media in new forms of Islam, but the theme is surely still in need of development.

Again, there are different forms or levels of comparison. Between whole cultures or aspects of them (such as birth-rates, literacy, etc), or between events such as revolutions (including scientific revolutions), and structures, such as feudalism. Or between what Bloch called ‘neighbourly comparisons’, such as kingship in England and France, and distant comparisons, such as feudalism in France and Japan.

Some historians prefer precise, systematic, quantitative comparisons (the size of cities, for instance, or the number of calories consumed or the number of hours worked in different places, periods or social groups). Other scholars practice more intuitive, qualitative comparisons – between ideas, for instance, between systems of education or even between individuals (such as the 17th-century statesmen, Richelieu and Olivares).

Some scholars look mainly for similarities, others for differences. Paradoxically, though, a focus on apparent similarities sometimes generates awareness of underlying differences, while an emphasis on difference may lead to the discovery of similarities, especially functional equivalents, a concept that has, I think, outlived the demise of functionalism.

Comparison makes absences more visible and a few comparative studies have been explicitly concerned with such absences. Werner Sombart, officially an economist but active in sociology and history as well, published a famous article entitled: Why is there no socialism in the United States? To the example of Sombart we might add those of Durkheim’s follower Marcel Granet on the absence of notions of sin and law in China; Joseph Needham on the absence of a Scientific Revolution, again in China, leading to a great debate on the ‘Needham question’; or Ross McKibbin’s essay, inspired by Sombart, ‘Why was there no Marxism in Great Britain?’

III

On this interdisciplinary occasion it may be useful to return to first principles and to ask: Why compare at all? What are the uses or advantages of comparison? I can think of two main answers to this question.

In the first place, we need to make comparisons in order to avoid campanilismo at its different levels (city, nation, or the whole of the west). Détienne makes this point eloquently: “Il y a une valeur éthique de l’activité comparative ... C’est qu’elle invite à mettre en perspective les valeurs et les choix de la société à laquelle on appartient ...on apprend ... à porter un regard critique sur son propre tradition” (Détienne 2000, 59).

It was for this reason that Arnaldo Momigliano praised Toynbee, despite the many weaknesses of A Study of History, as a contribution to deparochialization. As Elliott recently re-iterated: ‘Even imperfect comparisons can help to shake historians out of their provincialisms’.
It is easy when beginning research, all too easy, to think of one’s chosen topic as special, singular or even unique, especially if the topic comes from one’s own culture.

In Britain, for instance, historians of the early modern period have taken considerable interest in the Grand Tour to Italy and elsewhere undertaken by British aristocrats, often without realizing that nobles from other countries, especially in northern Europe from the Netherlands to Poland, undertook similar tours for similar reasons. Whether there were national or regional differences in routes taken or places seen remains to be discovered – a comparative study is yet to be undertaken.

The question of singularity is – in yet another paradox – a general one. A comparative historian will [might?] note that exceptionality has been asserted by a number of different nations such as the British, French, Spanish, Germans (with their famous Sonderweg), Russians (with their Russian soul), Americans, Japanese (with the nihonjinron tradition) and so on.

Island-nations such as Britain and Japan have been guilty of insularity. In the Spanish case, we might speak of peninsularity, but the cases of France, Russia, Germany and the USA remind us that the problem is a more general one – cultural nationalism. The Belgian historian Pirenne, after the First World War, advocated the comparative method precisely as an antidote to nationalism. As a recent study by the French historian Anne-Marie Thiesse points out, the process of the creation of national identities, with their stress on the unique qualities of each nation, is a process with many common features.

On a smaller scale than the nation, note the idea of the singularity of Venice, which goes back at least as far as Francesco Sansovino’s Venetia città nobilissima e singolare (1581). Rather than asking whether or not early modern Venice was singular, it would be more useful to ask: In what ways, and to what extent, was it singular? Venice differed from Florence in the same period in some ways, from Genoa in other ways, from Amsterdam in others, from ancient Rome in others, from Bangkok or the Japanese port of Sakai in still more, but yet it still had something in common with each of them. Viewed within comparative perspective, Venice appears to be a unique combination of elements, most of which have parallels elsewhere.

On a larger scale, think of the idea of the singularity of the West. Today, one of the leading crusaders against such Eurocentrism is Jack Goody, the British social anthropologist who successfully reinvented himself as a historical sociologist or cultural historian. Goody has criticized both the sociologist Norbert Elias and the historian Keith Thomas for overemphasizing the uniqueness of the West.

More generally, he has denounced what he calls the ‘theft of history’, that is the description by western historians of humanism, individualism, capitalism, modernity and so on as if they were completely western discoveries or inventions, ignoring parallels in China, the Islamic world and elsewhere.

So the first argument in favour of comparison is a rather general one, that it discourages collective narcissism.

Secondly, and more precisely, it has often been pointed out that a comparative approach, including [that encompasses?] contrasts, allows us to test explanations. Whenever we offer historical explanations of anything, we depend on implicit comparison. The question: ‘why did the French Revolution happen?’ implies, why not in England? Why not 50 years earlier or later? And so on. Making the comparisons explicit makes explanations easier to test.
Again, the recent interest in what is known as ‘counter-factual’ history – if the Spanish Armada had landed in England, for example – depends upon comparison, between what we believe to be the consequences of something that happened and the possible consequences of something that didn’t happen.

As Goody puts it: “Comparison is one of the few things we can do in the historical and social sciences to parallel the kind of experiments the scientists do”. (cf. S. F. Nadel (1951) Foundations of Social Anthropology and Piasare, L’etnografo imperfetto).

An obvious topic for collective research of this kind is emigration and immigration. One might study the history of emigration from different places (such as Italy, the Ottoman Empire and Japan in the later 19th century) to different places (USA, Australia, Argentina, Brazil, etc.), to see how far the reception of the emigrants varied with the culture of the hosts or the guests.

We also need comparison, as I suggested earlier, to draw our attention to significant absences. In Brazil, the absence of the press and of universities in colonial times leaps to the eye as soon as we look at Brazil’s neighbours, the viceroyalties of Mexico and Peru. The Spanish Empire decentralized, while the Portuguese colonial strategy was to centralize both printing and higher education in the metropolis.

IV

It is time to turn to the negative side to problems with, and critiques of, comparison.

A recent critique comes from the supporters of connected history or histoire croisée especially, but not exclusively, in France. Their sharpest criticisms concern the creation of artificial entities such as Protestantism and capitalism, homogenizing what is, in fact, varied. This is a danger for all historians, comparative or not, since we find it difficult to do without concepts such as ‘Britain’ or the ‘Renaissance’, but it is especially acute where comparisons are involved.

The supporters of histoire croisée offer the study of connections, especially intercontinental connections, as a substitute for comparison (‘beyond comparison’, to quote the title of one of the manifestos). In my view, connected history is to be welcomed, but not as a replacement for comparative history. The two approaches are complementary and we need them both.

One obvious danger of comparison is that of ignoring the cultural context of the practices or institutions that one is studying. In the early days of the discipline of anthropology, James Frazer exemplified an ambitious comparative approach in his huge book The Golden Bough, ranging from ancient Greece to modern Africa in search of similar practices. In what has become a classic critique, Bronislaw Malinowski pointed out that Frazer ignored cultural contexts and so misunderstood both the function of the institutions and the meaning of the practices. As a critique of Frazer, he was surely successful, a comparative approach, however, is not tied, or does not have to be tied, to a lack of interest in context.

A second problem or danger is the problem of treating as static groups practices or situations that are in reality always changing. This kind of freezing is not inevitable, however, it is perfectly possible for a comparative historian to focus on process. In the case of my book on the patricians of Venice and Amsterdam, for instance, I focussed on a process that has been described as ‘aristocratisation’. In a culture in which the social group with highest status was that of noble landowners, it was
tempting for successful merchants to leave trade and buy land, perhaps even a title, which would certainly raise the status of their children, if not their own.

Another serious danger is linked to one of the greatest successes of the comparative approach, the discovery of significant absences. The danger is that of viewing a given culture only [purely?] in terms of absences. In a famous study that is now over half a century old, the French historian Philippe Ariès noted what he called the absence of a sense of childhood in the Middle Ages. More precisely, what he observed or thought he observed was the absence of the idea that children behave differently from adults; that in a sense they belong to another culture. Specialists in the Middle Ages, provoked by Ariès, have produced a more nuanced account of medieval attitudes to children, distinguishing those of males and females, for example.

Distant comparisons in particular raise this problem, as the case of Max Weber illustrates. In his day, Weber seemed to escape Eurocentrism by placing his investigation of the rise of capitalism in an Asian context. Today, by contrast, he is criticized for Eurocentrism because his comparative study assessed other cultures essentially in terms of their lack of what the West possessed (capitalism, individualism and even rationality - as he defined it).

Max Weber was struck by the fact that in Germany in his time, around the year 1900, the North was both Protestant and capitalist while the South was neither. He went on to argue that Protestantism encouraged a ‘worldly asceticism’ that allowed merchants to build up their capital in a way that Catholicism, Islam or Buddhism did not.

However, as the American sociologist Robert Bellah has argued, a similar ethos may develop in different religions. Writing during the rise of Japanese capitalism in the 1950s, which Weber did not live to see, Bellah claims that there was a Japanese Buddhist equivalent to the Protestant ethos.

The question of absences is one aspect of a larger problem; the problem of ethnocentrism. Michel de Certeau’s famous question, ‘Where are you speaking from?’ becomes especially acute; do you come from one of the cultures compared, or neither?

A notorious example of ethnocentric comparison comes from the history of feudalism. Western scholars discovered analogies to the feudal society of the West in Japan and India, either exaggerating the similarities or treating the differences as deviations from the norm. Within Europe, many scholars treated French feudalism as the model into which to fit other forms of medieval society, from Scandinavia to Italy.

The example of feudalism illustrates the way in which western ethnocentrism is encouraged by the western origin of the conceptual apparatus with which historians work. Even apparently unspecific terms such as ‘university’, ‘portrait’, or ‘grammar’ were originally coined with the European experience in mind, with the consequent danger of forcing Islamic institutions, Indian artefacts or Chinese texts to fit a western model.

There seems no third way between using this western apparatus of comparison and refusing to compare at all. To undertake comparison while remaining aware of the danger of Eurocentrism appears to be the lesser evil.
One precaution that we can take, though, is to follow what might be called the principle of rotation. That is, we can take different regions as the norm in their turn. Weber, Bloch and other famous comparatists began with the West. It is equally legitimate to invert the procedure. For example, one well-known concept in economic history that has moved from East to West is that of ‘industrious revolution’, which Jan de Vries borrowed from Akira Hayami.

Again, we might discuss whether or not seventeenth-century Spain was a ‘closed’ or ‘secluded’ country on the model of Japan in the age of sakoku. To a lesser degree, given [their?] expansion in the Americas. Or perhaps look at the pleasure quarters of early modern Venice or Rome, Paris or London as western examples of the ‘floating world’ (ukiyo) to be found in Japanese cities such as Edō, Kyōtō or Ōsaka. Similar catalogues of talents, addresses and prices (Venice, Amsterdam, the Covent Garden ladies). Using concepts that originated outside Europe encourages a certain distancing from our own culture.

To summarise, my basic argument is that comparison is risky, but that lack of comparison is even more dangerous. It is dangerous because it encourages us to take for granted ideas that need to be tested.

Take the case of Norbert Elias and his famous study of what he called the ‘Civilizing Process’, more precisely perhaps the rise in early modern Europe of social pressures towards increasing self-control (which he believed to be linked to the centralization of government). Elias virtually ignored the rest of the world in his study. This was a pity, since the history of some other regions supports his theory.

However, similar pressures and similar results can be found in in Japan (in the age of the taming of the samurai in the Tokugawa period) and perhaps China, when it was unified under the Han dynasty.

The traditional grand narrative of western civilization was an account of uniquely western events: renaissance, reformation, scientific revolution, enlightenment and so on. In short, modernity was made in Europe and exported elsewhere. It is more realistic, as well as more humble, to think of these major events in the plural: renaissances, reformations, enlightenments and modernities. This is not to say that all reformations are the same, but that they reveal what Wittgenstein called ‘family resemblances’.

In short, a comparative approach helps historians to test their explanations and also to liberate themselves from current assumptions in their own culture, thus taking a few steps towards the polyphonic history that is needed in our increasingly multicultural age.

Peter Burke, Professor emeritus of Cultural History and Fellow of Emmanuel College, Cambridge, UK
An Operation called comparison - Some critical observations

Andreas Hess
a.hess@ucd.ie

It might perhaps be going too far to suggest that it is part of the human condition, but in our daily actions and observations we cannot help but compare and contrast. Walter Benjamin’s funny observation is to the point: in winter we usually notice people who are thin; in contrast, in the summer it’s fat individuals who attract our attention. If this is true, then that we can’t help comparing and contrasting. The next questions for those concerned with intellectual and disciplinary matters is, of course: How qualified are our observations and conceptualizations? What exactly should be compared and contrasted? How explicit do comparisons have to be?

Comparisons go back a long time; Plato and his distinctions of different forms of rule and governments, for example, and Herodotus and Thucydides from classic history writing immediately come to mind, although there are many others. At the beginning of modern times, Montesquieu and Jonathan Swift made deeper inroads into the multiple uses and meanings of comparisons, to great effect. In his *Persian Letters* (1721), Montesquieu famously invented a *dramatis persona*, Usbek, who held up a mirror to what was supposed to be the more advanced society (France). Swift, in his distinctively satirist style, famously used Gulliver’s travels (1726/1735) to imaginary places to show differences and similarities between peoples, tribes and nations. These early studies show that one could travel either horizontally in terms of space to identify differences or commonalities – as was the case of Montesquieu in his *Persian Letters* or the *Spirit of the Laws* (1748) and Swift’s dystopian novel – or vertically in time, as Rousseau did after Montesquieu when he drew on the distinction between natural and civilized men in his *Discourse on the Origins and Basis of Inequality among Men* (1755).

Alexis de Tocqueville and Gustave de Beaumont wrote the first modern works of systematic comparison to which the attribute ‘sociological’ can be applied. Actually they almost qualify as a mini department of comparative sociology. Their comparisons move constantly between France, the US, the UK (in which they include Ireland), Switzerland and Germany. For better or worse, this included also the comparative analysis of colonial entities and relations such as India (in the case of the UK) and Algeria (in the case of France). In their analyses, Tocqueville and Beaumont tried to address the whole package: culture, religion, politics and social conditions. It is perhaps worth pointing out how Tocqueville saw his approach. He regarded *Democracy in America* (1835/1840) as part of a comparative study in the development of modern democracy rather than a study of America *per se*. The United States, having been the first modern democracy, simply provided the concrete background for such a study. In this context, it is also revealing that Tocqueville wrote the
book with a principally French and European audience in mind. (That he would later become one of the honorary intellectual American Founding Fathers is indeed an historic irony that in itself is worth a comparative study.)

As any reader of *Democracy* will note, the comparisons in it are often, though not exclusively, implicit. For example, when Tocqueville talks about the Sovereignty of the People, he has another contrasting model in mind; the Westminster model of the Sovereignty of the Parliament. On other occasions, his comparisons are explicit.

Tocqueville’s companion Gustave de Beaumont, who wrote a less well-known book about Ireland, also used explicit and implicit comparisons contrasting English conditions with those in Ireland. Ireland, in turn, provides a further contrast with conditions in America. Ireland in the nineteenth century gave little reason for hope, in stark contrast to the U.S. In a way, *L’Irlande* (1839) can be read as the description of a darker side, omitted, oppressed or neglected in both *Democracy in America* and democracy in general. It is a model for comparing like with unlike, as opposed to like with like. Having said that, most of what was left out during the birth pangs of modern democracy was only hinted at in Beaumont’s work; the differences have to be teased out by the attentive reader. For example, even if one only takes a brief look at the table of contents of *L’Irlande*, one finds that the Irish study is apparently structured after Tocqueville’s *Democracy*. This was, of course, not accidental.

As many commentators have pointed out, Tocqueville and Beaumont’s studies stood in the tradition of nineteenth century political economy. It is sometimes forgotten that Mill and Nassau Sr. were friends of Tocqueville and Beaumont. However, while Tocqueville’s and Beaumont’s approach also includes political economy, it is not limited to it. Their studies have become modern classics because their work also contained discussions and topics that clearly transcended political economy. Religion, long-term ‘habits of the heart’, civilization, language and other important cultural distinctions enriched their analyses. Some of that openness was later narrowed-down as political economy, becoming more ‘scientific’, utilitarian and rigid in its approach. In this sense, even Marx’s critique of political economy is bifurcated in its analysis. Indeed, ‘critique of political economy’ does have a double meaning. It can mean, either, that one is critical, or a critic, of political economy and wants to criticize and perhaps replace it with a better approach, or that one stands firmly in that tradition of using political economy as a theoretical and conceptual tool, albeit perhaps not wholly uncritically.

Famously, the subtitle of *Das Kapital* (1867) was ‘a critique of political economy’. Consequently one finds Marx constantly moving between the two viewpoints mentioned. Marx talks of ‘capital’, ‘labour’, ‘labour power’, ‘surplus’, ‘production’ etc. At the same time, he attempts to look at what lurks behind those allegedly economic terms. By doing this, Marx moves with great ease around the globe, making comparisons and highlighting distinctions. Yet, in the end he becomes trapped in his own attempt to outwit political economy and trying to enlighten us about the hidden semantics of certain ‘economic’ terms. Logically, then, his comparison soon begins to serve only one cause — to define how modern capitalism has emerged and has conquered the world with everything else becoming subservient to that notion. The end product is a strange ‘nostrification’ process whereby the leading and most developed capitalist countries — in Marx’s case first England later the U.S. — show their less developed and poorer relations their future. Countries and regions, even entire continents, are now inhabited by people without history whose only task is to function as predecessors to the capitalist regime. (Such systems thinking would later re-emerge in Wallerstein’s world system theory, to no great intellectual benefit and distinction it must be said).
Comparison served an altogether different purpose for Weber and Durkheim. Max Weber was perhaps the first to point out that sociological concepts were almost designed to contain comparative and general elements. It was not by chance that, epistemologically speaking, Weber emerged out of the neo-Kantian tradition; there is no perceivable world and no meaning without concepts. Terms such as ‘class’, ‘lifestyle’, ‘bureaucracy’ or ‘charisma’ are abstractions (ideal types in Weber’s language) and, precisely because of that very quality, help distinguish certain common characteristics and properties of observable phenomena from any other (Economy and Society, 1922). This attempt to make sense puts nomothetic sociology in opposition to other academic undertakings, particularly history which, at least in its origins, was more idiographic and attempted to explain singularities and single events (hence Ranke and hence historicism and the critique directed against them). In the course of the development of both disciplines, such extreme juxtaposition was later relativised, in some cases to great effect. Today, social and cultural history would be poorer without Weber’s coinages and sophisticated conceptual reflections.

In the case of Durkheim and the Durkheimians (and most of social anthropology in its wake), we probably encounter the most productive use of the modern comparative method. For Durkheim, comparison helped identify ‘social facts’. Over time his own use of the comparative method became more differentiated. One can clearly distinguish between Durkheim’s early, and rather simplistic, use in The Division of Labour in Society (1893) and a later, much more sophisticated approach in the Elementary Forms of Religious Life (1912). In the early work, he contrasts the simple division of labour in primitive societies (and, as a consequence, a mechanic form of solidarity) with a modern society, which was based on a complex division of labour (and what Durkheim termed organic solidarity). In the later, he tried to master (mainly with the help of Marcel Mauss) an array of phenomena, resulting in a number of shorter ‘thick descriptions’ of cultures, tribes, groups and societies and their social practices.

It is interesting that Durkheim’s comparisons are located on both horizontal and vertical axes, and that they are both applied to space and time. If one had to choose only one outstanding example of how successful Durkheim’s comparative method worked, and how rich and insightful his conceptual coinage turned out to be, it would have to be the crucial distinction between ‘sacred’ and ‘profane’. Durkheim moves back and forth in time and space to reveal deeper, multiple meanings of the sacred/profane distinction. Perhaps the most notable feature of Durkheim’s method of comparison is that we come to know the deeper meaning of the word and its social and cultural notions and functions at the end of the study; we do not know it at the beginning. In this he differs considerably from Max Weber, who has been accused of a degree of conceptual imperialism; i.e. using concepts that are beyond space and time and so ‘travel light’ and can be applied to all times and circumstances but yet miss some crucial features because of their abstractness.

Not even the shortest meditation about what comparison in sociology or cultural history entails can be complete without mentioning a modern classic that, like no other study, has the advantage of permitting a look into the engine room of the social scientist who relies on comparisons: Levi-Strauss’ Tristes Tropiques (1955). As is well known, Levi-Strauss’s work would be unthinkable without Durkheim and the Durkheimians (even though Rousseau was his real hero). For our context, it is not important whether we regard Levi-Strauss’ own structural anthropology as having been successful or having stood the test of time. What is important is that in TT Levi-Strauss raises some of the most pertinent questions about what it means when we study another culture or country. He points out the mediating and Jeremiad-type role that the social scientist, and in particular the social anthropologist, plays. Neither at home in their home country, which s/he often despises,
nor fully integrated in the culture they are studying, the social anthropologist is left in a precarious position which s/he constantly has to try to balance. Yet, as CLS stresses, it is exactly this tightrope walk, this constant oscillation between an insider and an outsider position, which will make other cultures and countries more intelligible. Levi-Strauss’s critical insight into the discovery process is driven by genuine curiosity and the will and wish to know more about the other culture; the one the social anthropologist is not part of. In the end, Levi-Strauss succeeds because he makes an important contribution of how to avoid ‘nostrification’; i.e. that those studied are treated and studied as if they were on the way to becoming more like ‘us’. In the same vein, Clifford Geertz has stepped into Levi-Strauss’ footsteps by asking whether our concepts can still remain unchanged despite a world that has radically changed. In the light of the changes we have [our generation has?] seen, from the fall of the Berlin Wall to the post-September 11 wars, can we still maintain that country and culture are the same and that culture and consensus are identical (Available Light, 2001)? This surely must have consequences for any future attempt at comparative analysis.

This short discussion throws up the ultimate question of whether comparison can be taught; can it be acquired in the classroom or in the seminar? Like any good analysis, comparative study worthy of the name will have to address what other sociological and historical studies do. In other words, it will have to address the famous What, How and Why questions: What exactly is happening? How is it happening? And why is it happening? But apart from that, comparative study must also show sensibilities towards differences in language, culture and custom. Famously, Marcel Mauss, one of the founders of the comparative approach, never travelled and has been denounced as an ‘armchair social anthropologist’. But yet it is not necessary to have lived and conducted fieldwork with every tribe and nation to achieve intellectual distinction. The answer as to whether the comparative approach can be taught (and whether there even exists a method that can be called comparative) probably lies somewhere in between these two extremes. Let’s start with the obvious. The operation called comparison would have a better name if some of its advocates and practitioners would speak at least a second or third language, perhaps with a passive knowledge of a few more. The same applies to academic disciplines; those who only know one probably don’t even know that one very well. After all, as the short Benjamin remark that I mentioned in the beginning shows, comparison is the foundation of intelligence.

Andreas Hess, Senior Lecturer at the School of Sociology, University College Dublin, Ireland.
BOOK REVIEW

Solovey and Cravens, Cold War Social Science

Matteo Bortolini
matteo.bortolini@unipd.it

Mark Solovey and Hamilton Cravens (eds.), Cold War Social Science: Knowledge Production, Liberal Democracy, and Human Nature
New York: Palgrave Macmillan 2014
Price: €31

This paperback edition of Cold War Social Science, originally published in 2012, includes 13 chapters divided into three sections: knowledge production, liberal democracy, and human nature. The collection surveys the state and the development of the American social sciences—mainly sociology, anthropology, psychology, linguistics, future studies—during the Postwar period and the Cold War years. In a dense introductory essay, however, editor Mark Solovey warns scholars against a too-facile use of the “Cold War social science” label, which should be understood more as an hypothesis than as a conclusion. In fact, in both the introduction and Theodore M. Porter’s foreword, the complex web of government-driven research, purely scientific and theoretical interests, forms of funding, technological gizmos, mass media, and changing understanding of the public role of the social sciences is sketched as a paradoxical, often contradictory figuration of structures and processes which defy a single simplistic definition—let alone any one-way kind of causal explanation, as the very expression “Cold War social science” might suggest.

These warnings are justified by a multiplicity of facts which continuously pop up from individual chapters. First, the Cold War did not reshape the social sciences in an immediately evident and coherent way, for different and often diverging streams of research, analytical models, and empirical conclusions were elaborated from intellectuals working within different scholarly traditions—many of which were rooted in Prewar and Interwar developments (see, for example, the development of creativity studies recounted by Michael Bycroft in chapter 11). Second, many scholars did not participate in the military-academic-industrial complex, but rather took a critical stance towards it—among them neo-evolutionist anthropologists (chapter 9) and leftist critics (chapter chapter 8). Third, local and international academic, political, and interactional factors all mediated the impact of “the Cold War” on social scientific practices and ideas, so that no generalization “in the last instance” can be advanced. At the same time, Solovey underlines that the Cold War climate exerted a clearly detectable influence on the social sciences by creating new objects of study and “entire fields of inquiry,” unseen institutional designs, and innovative forms of extra-academic patronage.
Individual chapters oscillate between a skeptical illustration of the relationships between the Cold War and the developments of social scientific disciplines and a general sense that, in Noam Chomsky’s dictum, “everything was connected.” I would like to call attention on Jane Martin-Nielsen’s essay on the use of computer in linguistics (chapter 4) and Edward Jones-Imhotep’s piece on the training of maintenance technicians (chapter 10). In spite of their modest length, both essays treat the connections between technical devices, theoretical schemes, and empirical results in a way reminiscent of some STS studies of the interplay between technology, scientific thinking, and human practices. Here machines are seen as both means of structuring theoretical hypotheses and human behavior, of educating, in a way, the habits of, respectively, practicising scientist and maintenance technicians in order to gain scientific and/or technical goals in the face of uncertainty, limited rationality, and the possibility of human error. On its part, Joel Isaac’s study on epistemic design (chapter 5) focuses on the understanding of the relationship between analytical schematas and the collection and interpretation of empirical data at the Department of Social Relations at Harvard. In this view, social scientists should develop models and schemes in order to treat and combine raw data with the goal of gaining “higher” (or “deeper”) theoretical knowledge of social phenomena. Isaac uses some examples drawn from the so-called “Ramah project,” designed and headed by Clyde Kluckhohn as a comparative study of values across five cultures, to show the generalizing tendencies of Harvard-trained anthropologists and sociologists, and to underline typical Social Relations obliteration of history in favour of reified, simplified theoretical constructs. The same tendency can be seen in Martin-Nielsen’s section on Chomskian linguistics and in Hunter Heyck’s essay on the creation of institutional methods for assuring the rationality of choices in the face of uncertain environments and failing human decision makers (chapter 6). At the end of the day, my general impression is that social scientists were, on the whole, driven more by scientific concerns than by political or social ones, even if Marga Vicedo’s interesting essay on mother/child relations and Hamilton Cravens’ essay on the creation of institutional methods for assuring the rationality of choices in the face of uncertain environments and failing human decision makers (chapter 6). At the end of the day, my general impression is that social scientists were, on the whole, driven more by scientific concerns than by political or social ones, even if Marga Vicedo’s interesting essay on mother/child relations and Hamilton Cravens’ essay on the creation of institutional methods for assuring the rationality of choices in the face of uncertain environments and failing human decision makers (chapter 6). At the end of the day, my general impression is that social scientists were, on the whole, driven more by scientific concerns than by political or social ones, even if Marga Vicedo’s interesting essay on mother/child relations and Hamilton Cravens’ essay on the creation of institutional methods for assuring the rationality of choices in the face of uncertain environments and failing human decision makers (chapter 6). At the end of the day, my general impression is that social scientists were, on the whole, driven more by scientific concerns than by political or social ones, even if Marga Vicedo’s interesting essay on mother/child relations and Hamilton Cravens’ essay on the creation of institutional methods for assuring the rationality of choices in the face of uncertain environments and failing human decision makers (chapter 6). At the end of the day, my general impression is that social scientists were, on the whole, driven more by scientific concerns than by political or social ones, even if Marga Vicedo’s interesting essay on mother/child relations and Hamilton Cravens’ essay on the creation of institutional methods for assuring the rationality of choices in the face of uncertain environments and failing human decision makers (chapter 6). At the end of the day, my general impression is that social scientists were, on the whole, driven more by scientific concerns than by political or social ones, even if Marga Vicedo’s interesting essay on mother/child relations and Hamilton Cravens’ essay on the creation of institutional methods for assuring the rationality of choices in the face of uncertain environments and failing human decision makers (chapter 6). At the end of the day, my general impression is that social scientists were, on the whole, driven more by scientific concerns than by political or social ones, even if Marga Vicedo’s interesting essay on mother/child relations and Hamilton Cravens’ essay on the creation of institutional methods for assuring the rationality of choices in the face of uncertain environments and failing human decision makers (chapter 6). At the end of the day, my general impression is that social scientists were, on the whole, driven more by scientific concerns than by political or social ones, even if Marga Vicedo’s interesting essay on mother/child relations and Hamilton Cravens’ essay on the creation of institutional methods for assuring the rationality of choices in the face of uncertain environments and failing human decision makers (chapter 6). At the end of the day, my general impression is that social scientists were, on the whole, driven more by scientific concerns than by political or social ones, even if Marga Vicedo’s interesting essay on mother/child relations and Hamilton Cravens’ essay on the creation of institutional methods for assuring the rationality of choices in the face of uncertain environments and failing human decision makers (chapter 6). At the end of the day, my general impression is that social scientists were, on the whole, driven more by scientific concerns than by political or social ones, even if Marga Vicedo’s interesting essay on mother/child relations and Hamilton Cravens’ essay on the creation of institutional methods for assuring the rationality of choices in the face of uncertain environments and failing human decision makers (chapter 6). At the end of the day, my general impression is that social scientists were, on the whole, driven more by scientific concerns than by political or social ones, even if Marga Vicedo’s interesting essay on mother/child relations and Hamilton Cravens’ essay on the creation of institutional methods for assuring the rationality of choices in the face of uncertain environments and failing human decision makers (chapter 6). At the end of the day, my general impression is that social scientists were, on the whole, driven more by scientific concerns than by political or social ones, even if Marga Vicedo’s interesting essay on mother/child relations and Hamilton Cravens’ essay on the creation of institutional methods for assuring the rationality of choices in the face of uncertain environments and failing human decision makers (chapter 6).
ideas and the theoretical tools used by other social scientists as they try to improve the strictly scientific, as opposed to the political or the social, condition of their disciplines.
BOOK REVIEW

Kaesler, Weber -- Kaube, Weber

Hans Henrik Bruun

hh.bruun@tdcspace.dk

Dirk Kaesler, *Max Weber. Preusse, Denker, Muttersohn*
Hardcover, 1007 pp.
ISBN 978-3-406-66075-7
Price: € 38

Berlin: Rowohlt 2014
Hardcover, 496 pp.
ISBN: 978-3-87134-575-3
Price: € 26,95


The occasion, therefore, is obvious. But why write the biography of a scholar at all? Do not his or her works tell us all that we really need (or indeed want) to know? Honorably, the authors of both of the biographies reviewed here try to furnish us with an answer to this question, albeit along different lines. Dirk Kaesler, emeritus professor of sociology and a Weber specialist of long standing, argues that we cannot separate Weber’s life from his works, and that both should be seen in their historical context. Kaesler maintains that if we are at all interested in Weber as a scholar, we should also consider his life and times - a unitary approach which is also reflected in Kaesler’s somewhat clumsy subtitle *Preusse, Denker, Muttersohn* (”Prussian, Thinker, Mother’s son”). Jürgen Kaube - for many years professor of sociology, subsequently cultural editor of the Frankfurter Allgemeine Zeitung, but new to Weber studies - argues the other way round: Max Weber (1864-1920) is placed between two historical epochs. For a proper understanding of these epochs, Kaube says, both Weber’s works and his life are valuable sources of information, because he was intensively preoccu-
Kaesler has literally for decades steeped himself in information concerning all aspects of Max Weber's life, and this is clearly reflected in his book. Its length - more than 900 pages of fairly small print - is in itself significant. Over and above this, there is a bibliography of almost 60 pages and an index of names, but - and this must be considered a serious shortcoming - no source notes at all. Kaesler himself thanks his publishers for having "slimmed down" the book considerably, and the source notes may, regrettably, have fallen victim to this slimming process. However, it would have been preferable if the text itself had been subjected to more extensive pruning. This is particularly true of the first part, where far too much space is allotted to, for instance, descriptions of remote members of the Weber family, of political conditions in Germany before Max Weber's birth, and of the details of student life in the 1880s. It is only around page 300 that we get as far as Weber's first proper academic work (his dissertation on the commercial partnerships in the Middle Ages).

Another circumstance which contributes to the book's length, but not to the same extent to its value, is Kaesler's constant habit of quoting long excerpts from Marianne Weber's classical biography. This is occasionally done with the aim of showing us where Marianne was prejudiced in her judgement (for instance, of Max Weber's father); but mostly, the quotations simply serve as lengthy complements to - or even substitutes for - Kaesler's own text. The result is often over-long - quite apart from the fact that Marianne's literary style sometimes seems rather more convincing than Professor Kaesler's.

Kaufe's book is only half the size of Kaesler's, but it is certainly not a lightweight piece of work. There are fewer details, but Kaufe covers the ground well, and often adopts useful and interesting points of view. Moreover, he manages to supply the reader with brief, but sufficient source notes.

Major parts of the story of Max Weber's life are essentially of a private nature. This is true of his relations with his parents and with his wife, Marianne; his deep depression in the years 1897-1902; his relationship with the pianist Mina Tobler; and his passionate affair with Else Jaffé-Richthofen in the last years before his early death. But their private nature certainly does not imply that they are devoid of interest in themselves. In some cases, they have also left important traces in Weber's works; and they are often - as Kaufe, in particular, is at pains to demonstrate - symptomatic of their epoch, and thus interesting from a historical perspective.

As far as Weber's relations with his parents are concerned, he increasingly, in his twenties, orients himself towards his unusually pious and self-abnegating mother, Helene, and distances himself correspondingly from his father's easygoing, traditional and patriarchalist lifestyle. The tension between these two attitudes comes to a head in 1897, where Max has a violent quarrel with his father about the right of the mother to visit Max and Marianne without being accompanied by the father. The father dies suddenly shortly afterwards, without having seen Max again. It is understandable that some authors (particularly Martin Green, The von Richthofen Sisters (1974)) have succumbed to Freudian temptation, interpreting these dramatic events as a classical Oedipal situation. Kaufe refers to these Freudian interpretations, but considerably downplays the relevance of this episode for our general understanding of Max Weber. Kaesler does not discuss the issue of relevance, but hedges his bets with four pages of verbatim quotation from Marianne, who was an eyewitness to the scene.
Max and Marianne's engagement only becomes a reality after a series of dramatic confrontations between Marianne and Max's mother, where Marianne finally prevails over a complex barrier of moral and conventional objections. But the most remarkable feature of this whole process is that it takes places over the head and behind the back of Max Weber himself. As Kaube nicely puts it: "Max Weber is given in marriage". The marriage between Max and Marianne is marked by deep mutual affection, and by unquestioning admiration and considerateness on her part; but viewed from the outside, the dominant feeling between them seems to be comradeship rather than love. There was no offspring, and whether the marriage was in fact ever physically consummated is a question which has titillated the imagination of certain commentators, but which the grand old man of Weber scholarship, Johannes Winckelmann, contemptuously dismissed as "aunts' gossip".

In the years leading up to 1897, Max Weber assumed an almost impossibly heavy burden of work. But a few months after his father's death, he falls into a deep depression. For years, he can do little else than sitting idle in a chair. No reading. No discussions. He resigns from his professorship, and in fact holds no remunerated academic position until shortly before his death. It is only in 1902 that he feels able to resume work. Many reasons have been advanced for this nervous breakdown: the clash with his father; the immense workload; sexual problems; feelings of guilt. Both Kaube and Kaesler wisely refrain from looking for firm answers in this respect. However, this long crisis is particularly interesting for three reasons: First, we possess a steady stream of letters between Marianne and Helene, in which the state of health of the husband/son, including his sexual problems, is discussed in surprising detail (a circumstance which Kaube, in particular, finds somewhat shocking). In this phase of his life, Weber is clearly a weak and passive object in the hands of two strong women. Secondly, Kaube sees the signs of a significant development in Weber's work when he starts writing again after his long depression. This view, which is not uncommon among Weber scholars, is not shared by Kaesler. What is certain is that, when Weber does resume work, the rate of production is in itself stupendous: from 1903 to 1905, he writes a number of long and difficult methodological studies, including the important article on "Objectivity", and at the same time, he produces his best-known work: the long article on "The Protestant Ethic and the Spirit of Capitalism". If one compares these works with Weber's production before his breakdown, in particular his strongly nationalistic and harshly polemical Inaugural Address from 1894, it is difficult not to agree with Kaube that the personal crisis has matured Weber's views to a significant degree.

And thirdly, it is fascinating to learn, from both biographies, about the perplexity and apparent helplessness of the medical specialists consulted about Weber's condition. Every remedy is tried: cold baths, hot baths, aeroatherapeutics, hypnosis, electrotherapy (though not, happily, applied to the brain), encouragement to conjugal sexual relations, anti-erection medication, ban on alcohol, gymnastics, massage, plasticine modelling - coupled with prescriptions of narcotics and stimulants: bromine, trional, veronal, arsenic and heroin. We know from Weber's letters that he remained dependent on some of this medication for many years; and against this background, his colossal scholarly achievements seem all the more impressive.

In 1909, Max Weber meets the Swiss pianist Mina Tobler and is captivated by her youth (she is 16 years younger than he is) and her musical sensitivity. We do not know for sure, nor is it of any great consequence, whether the relationship remains platonic. What is certain, though, is that it is accepted by Marianne, who for a long time, and with apparent equanimity, sees her Max leave every Saturday to visit Mina. Marianne believes, no doubt correctly, that Mina can unlock new and gentler aspects of Max Weber's character. At the same time, this new relationship is academically fruit-
ful, resulting in a long study by Weber of the sociology of music (which is, however, only published after his death).

While doubts may remain as to the degree of intimacy of Weber's relationship with Mina Tobler, no such doubts can be entertained when it comes to his liaison with Else Jaffé-Richthofen from 1917 until his death three years later. She was an unusually gifted woman who wrote her dissertation under Weber's supervision. She was unhappily married and carried on a number of affairs, including a protracted one with Weber's younger brother Alfred. For his part, Max had almost broken with Else in 1909. But when they meet again in 1917, he is overwhelmed by a stormy passion for her. Henceforth, Else is the absolute centre of Weber's emotional universe; but this does not endanger his marriage to the heroically tolerant Marianne; and it is only at the very end that he makes it clear to Mina that his relations with her must take a more secondary place. Kaesler is right in saying that Weber's letters to these three women in the years 1917-20 are not attractive, being full of "purely tactical comforting words, duplicitous half-truths, [and] flowery recollections of past events".

Apart from the private passion, strong echoes of Weber's relationship with Else can also be found in his academic production. In the "Intermediate Remarks" in his sociology of religion, there is a major section on erotic love as a separate value sphere - a thought which has its origins in remarks by Else. And tellingly, he later writes to her that this section in its revised form should really carry the following footnote: "Improved on the basis of a more intensive study of the subject-matter".

The two biographies of course also contain accounts and discussions of Weber's academic work. In this respect, Kaesler's book is considerably more detailed than Kaube's (for instance, Kaesler discusses in much greater detail the importance of Weber's two doctoral theses for his later work). Occasionally, though, Kaesler's account becomes so "pedagogical" that it almost interrupts the general narrative flow. His discussion of Weber's basic sociological concepts, for example, could almost have been lifted from a (very good) academic textbook.

Naturally, both authors focus strongly on "The Protestant Ethic and the Spirit of Capitalism" - which Kaesler aptly describes as "one of the grand narratives of the 20th century". The two authors are also at one in pointing out that Weber's long journey to the United States in the autumn of 1904 did not, contrary to what many commentators have supposed, leave strong traces in this article. In fact, Kaesler and Kaube agree that Weber was more interested in the European secularizing influence on the United States than in the possible American (Puritan) influence on Europe. But the trip to the USA did furnish Weber with central elements of his later article on the Protestant sects (a work that should always be read alongside the "Protestant Ethic").

Marianne Weber says that politics was Max Weber's "secret love". He did write a number of long and well-argued political articles and, during the Great War, voiced surprisingly scathing criticism of German policies. After the defeat of Germany in 1918, he was a member of a consultative committee for the preparation of the new German Constitution, and here argued strongly, and successfully, in favour of the election of the Reich President by popular vote - a provision which was to have disastrous consequences fifteen years later. Otherwise, he was less successful in the political field. His participation in the German delegation to the Versailles was useless, and his attempt to win a place on the ticket of the German Democratic Party for the elections to the Reichstag quickly bogged down in a quagmire of party-political intrigue. Actually, as noted by both Kaesler and Kaube, Weber's attitude towards politics was out of step with reality: as he himself admitted in sev-
eral letters written in April 1920, he was an academic, not a politician. Truth was more important to him than political compromise.

In conclusion, I feel that Kaesler's biography will no doubt, because of the impressive amount of material that it presents and the breadth of its scope, gain a lasting place as a standard work of biographical reference in the literature on Max Weber; but its lack of a persuasive unifying perspective, in conjunction with Kaesler's wish to share every bit of available information with the reader, have made it longer and more detailed than necessary. Kaube's book is shorter, but thorough, well-proportioned and crisply written, and may perhaps offer a better choice for the ordinary reader with no special previous knowledge of the subject.

So far, both books exist only in the original German. Until further notice, therefore, those who look for detailed, up-to-date information in English on Weber's life must go to the English translation of Radkau's biography (which is, however, less than persuasive in drawing parallels between aspects of Weber's life and his academic work). But for anyone with a sufficient knowledge of German, the Max Weber gap on the biography shelf has now been more than adequately filled.
BOOK REVIEW

The Palgrave Handbook of Sociology in Britain
Australian Sociology
Sociology in Ireland

Charles Crothers
charles.crothers@aut.ac.nz

John Holmwood and John Scott (eds.) The Palgrave Handbook of Sociology in Britain
Basingstoke: Palgrave Macmillan 2014
Hardcover, 631 pp.
Price: € 234.33

Kirsten Harley and Gary Wickham, Australian Sociology: Fragility, Survival, Rivalry
Basingstoke: Palgrave Macmillan 2014 (Sociology Transformed)
Hardcover, 123 pp.
ISBN 978-1-137-37974-0
Price: € 69.54

Bryan Fanning and Andreas Hess, Sociology in Ireland: A Short History
Basingstoke: Palgrave Macmillan 2015 (Sociology Transformed)
Hardcover, 89 pp.
Price: € 69.54

1 Introduction

Placing together reviews of these three is useful since they are linked: whereas the UK volume copiously describes the sociology of the ‘mother country’, the Irish and Australian volumes report on the sociologies of two ex-colonial offshoots, and as a result instructive similarities and differences in their sociologies are highlighted. This then facilitates work on historical/comparative sociology of sociologies which is sorely lacking in this field of specialisation. The three are also linked in that the first is a ‘mother volume’ from which the series encompassing the other two has spun off. The Palgrave series notes that “the field of sociology has changed rapidly over the last few
decades ... and the series ... seeks to map these changes on a country-by-country basis and to contribute to the discussion of the future of the subject”. The series is concerned “with [sociology’s] many variant forms across the globe”. A big difference is that the series is apparently aimed only at post-1945 putative national sociologies. The trio also sit under the shadow of the earlier US volume on Sociology, and Turner’s polemic which is the first volume in the Palgrave series (see also the symposium discussing this in the pages of The American Sociologist.)

The sociology of sociology faces some particular difficulties. Just as sociology produces social knowledge usually together with some hope of supporting social betterment, the sociology of sociology produces knowledge about sociology as a knowledge production system usually with some hope of supporting future improvement. However, the mix of description and prescription can be dangerous or at least difficult. The former driver can lead to such plunging into the depths of historical detail with little or no relevance to the present that it may become Antiquarianism which can undoubtedly be the antithesis of presentism or even futurism which is the end-point of the second thrust. The format of the series encourages polemic, which is useful since the UK volume flounders under a considerable weight of historical material. Another crucial test of the quality of works in the history of sociology is Robert Merton’s concern about amateur history being conducted by sociologists: do these volumes pass this test or any other that is self-imposed?

A synoptic rubric of the questions posed in sociology of sociology might be posed as “Who (from what social backgrounds) with what resources and organised in which institutional and organisational frameworks produces what outputs (teaching, research, scholarship, services) to what audiences with what intended/actual effects and with which allies/enemies?” This framework is broadly used to guide the following discussion.

Each volume is also focused on a putative ‘national sociology’ (rather than fields of specialisation) if only given that this an assumed field behind each volume (see Fanning and Hess pp. 4-5 for a brief discussion of these points). Issues concerning boundary maintenance between sociology and other knowledge structures are also pertinent and will be featured when found.

2 UK

This massive 600 pp. collection of 26 chapters is modelled on Calhoun’s US collection, with one of editors’ aims being an “emphasis on multiple histories and discontinuities”. Alternative theories of disciplinary development are mentioned - that sociology emerged from social work/social policy or that sociology is an extension of ‘political arithmetic’ compared to being able to provide more deep-seated analyses – but these are not replaced by newer analytical frameworks for understanding disciplinary trajectories. This volume should reveal the scholarly effect of being able (to some extent) to draw on a dedicated cadre of ‘professional’ historians of sociology – a luxury only afforded by few national sociologies. (Oddly enough, UK historians of sociology have paid much attention to American sociology, although this attention is not represented in the Calhoun volume.)

There are some minor irritations. The absurdly over-reaching title of ‘Handbook of UK Sociology’ is clearly misnamed and ‘historical development’ or words to that effect were required in the title to avoid misrepresentation. Savage’s chapter is incredibly important and yet through some editorial oversight is rather intensely written (mainly being concerned with refuting criticisms of his book in this area) and addressed to some unnamed workshop.
The editors begin (p. 1) with noting the fascination of (UK?) sociologists with their history, although they suggest that there has been little consideration of British sociologists and their work which hardly seems correct and is possibly misleading as they do not attempt to engage systematically with this legacy (e.g. by providing a literature review of studies in this area).

The arc of chapters is firmly anchored in a magnificently sociological study of the Scottish Enlightenment, and then gets somewhat bogged down with coverage of many aspects of UK Sociology in the late 19th and earlier parts of the 20th century, before slicing the breakfast sausage in a different angle by looking at a few key fields. Nine fields are covered: community, race, methods, religion, criminology, work, cultural studies, class, and body - before suddenly arriving in its concluding chapters at the present and near future. The otherwise all-UK writing crew is supplemented by 2 American and an Anglo-French writer.

Analytical frameworks are provided only by the first and last chapters. Brewer (p. 19) discusses the differences between spaces of writing and reading and supplies an alliterative framework of serendipity, space and social structure as drivers of disciplinary development. Holmwood’s concluding chapter plunges into several pertinent frameworks for discussing Sociology’s social characteristics compared to other disciplines and to drive ‘policy recommendations’. There is little development of systematic data apart from Platt’s chapter.

The organisational setting is nowhere drawn out (cf. Platt on a key publisher) yet UK sociology is nested in what seems to be a user-friendly set of institutions, ESRC funding seems considerable, and there are a host of supportive institutions such as academy of social sciences etc. Gaps include lack of discussion of organisational apparatus including the BSA and its sections, journals, research units, research funding, international benchmark reporting all of which might have provided a firmer systematic description.

The historical chapters have great stuff:

- In the US the connection between religion and sociology has long been recognised and here discussion of this connection is extended to the UK;
- Evidence is produced that Interwar sociology was stronger than usually realised;
- An interesting portrait is drawn of the small influx of Continental sociologists around WW2 (which argues that they had a more complex effect than some received accounts that they were conservative and more British than the British, and reinforced empiricist trends);
- The amazing book production of the ILSSR is documented;
- The development of an interwar textbook tradition is specified;
- The role of ‘colonial sociologists’ in the imperial rebound of immediate post WW2 years is examined;
- A useful discussion is presented of how sociology fits into the Snow/Leavis debate as part of a ‘third culture’ (although this chapter then diverts into far broader consideration of sociology’s humanistic side).

However, there seem to be gaps in what might otherwise be a definitive coverage. Spencer and Marx/Engels not adequately covered and nor is the recrudescence of the 1970s when British sociologies with Giddens in particular updated theory and the UK in general became an entrepôt between Continent and USA, supported by a strong publishing infrastructure. Historical sociology was a particularly important development that is here missing in action.
The volume exhibits renunciation of empiricist sociology and theory of the drivers of disciplinary developments (apart from the opening and closing chapters). There is an ‘historical fade’ which fails to engage with the most recent periods, perhaps because appropriate data is not developed.

Some passages in the volume rise to providing broad characterisations. Savage is most upbeat: suggesting that leading UK sociologists were mobilising around histories of discipline and that there was also interest in rethinking earlier sociologies – as in restudies. However he also relates that (p. 361):

In recent years, British sociology has been convulsed by a major identity crisis, driven by increasing anxiety about its academic standing and further prospects. ... even in the changed climate of the 1980s the discipline dealt apparently easily with the Thatcherite challenge through significant intellectual contributions to debates about class and state, gender, race and ethnicity, and cultural change associated with post-modernity. Into the early 2000s student demand was strong, leading sociologists such as Giddens and Bauman became prominent on the public stage and the remarkable rise of sociologists to Vice Chancellorships around the UK was a striking demonstration of their managerial skills. Sociological ideas were widely influential across the health sciences, development studies, education studies and business schools.

However, fewer enrolments, declining RAE scores and few RAE submissions together with high profile department closures (although more often of teaching departments) have tarnished this hopeful future although there has also been investment in sociology amongst higher status universities, and invigorations with cultural class analysis. Drop-outs of some significant subfields from the discipline include science studies and sociologies of health and organisations. Methodologically a fundamental issue has been posed if research tools remain adequate in emerging era of ‘big data’. This all cumulates in Holmwood’s mournful (but arguably realistic) assessment that: “I characterise it as a discipline that is potentially ‘fading’ from the scientific field in the UK, to be replaced by a variety of applied social studies” (p. 602).

3 Australian Sociology

Kirsten Harley (more recently cruelly afflicted with motor neuron disease) and Gary Wickham (2014) have produced a workpersonlike and meticulously organised text which summarises and extends (largely drawing on Kirsten’s own previous research) the considerable effort which went into recalling the history and current condition of Australian sociology in the late 2000s, particularly under the leadership of John Gemov. The book is laced together by attention to three themes that the authors suggest characterise Australian sociology: fragility, survival and rivalry (although the first two are opposites, so the themes in effect come down to 2). The substantial chapters cover the earlier period up to 1959, descriptions of teaching, research and important books published during the main period under review – most of the tabulated material relates to the present (or more precisely the recent past). Data illustrate staff and student numbers over time. That 29 introductory texts are noted suggests a major investment in this genre and hints at the size of the sociological teaching establishment over the period. To inject more in-depth insight, a resume is given of two debates that point to contention within the discipline: Bryson’s 1970s account of generational conflict with Australian sociology and Bryan Turner’s sermon of the late 1980s about the endless need of sociological theory to continuously innovate new waves of theory.

A further chapter investigates theory use in Australian sociology while the final substantive chapter provides a case study of the development of sociology at the University of Sydney. However, such
attention to theory really needs to be balanced by similar attention to empirical work and the links between the two.

Flowing from this analysis they argue that currently Australian sociology is in reasonably good shape (with some signs of cracks appearing) but its potential is held back by rivalries concerning content (seen in terms of fields) in various ways in the discipline as a whole and that it has spread its wings too widely: thus undermining disciplinary cohesiveness and thus impact. The authors argue that Australian sociology needs the reflexivity which would be injected by a higher historical consciousness.

Much is made of pre-1960 history of Australian sociology which is characterised as arrogant and ineffective, but above all as barely visible. A wonderful collection of early-period snobbish comments on sociology from other academics have been collected, although the authors suggest this was somewhat in retaliation for the arrogance of sociologists of the day. However, I’m not convinced that this early prehistory was anything other than a long-forgotten early skirmish which involved very little activity (a few courses, mainly taught under the auspices of the Workers Educational Association, WEA). So it really did not cast the pall over subsequent developments suggested. Moreover, there are interesting early episodes which could have been mentioned which would have spiced up the tale: e.g. the later-important UK economist Jevons who was in Sydney in the early 1850s and carried out a survey.

The treatment is resolutely internalist, but the volume makes a good attempt to cover the bases: staff and student numbers (seen as the main resource base) are tabulated and outputs classified by sociological field. An important, but quickly covered, feature is the spirited passage which enunciates Australian sociology's international stature, with 17 contributors being noted together with explicit mention of the international stature of Raewyn Connell, Peter Beilhartz, Bryan Turner and Robert von Kreiken (who spent some time as a professor in Ireland). There is also quick discussion of the extent to which an Australian national sociology has developed (with the collective rating of ‘Most Important Australian’ books being seen as a good indicator of some consensus around this). Skrbis and Germov’s claim is cited (p. 57) that this consensus constitutes a particular brand of critical sociology that inherently strives to relate social issues to power, public policy, and social reconstruction – although to be frank this seems a rather bland characterisation. There is also rather too much of an assumption of a collective solidarity and stronger external boundaries behind the discipline than seems warranted.

Some numbers are interesting: Sociology is taught in 35 of 37 universities – although not always directly as ‘sociology’; TASA has 620 online directory members; coverage of courses taught reveal that treatment of Australian society constitutes 12% compared to the 5% for methodology. For articles, quantitative methodology ran at 28% in the 2000s compared to 54% in the 1960s, while qualitative articles rated at 36% from a 1960s base of 0%. There is a recent decline in non-empirical articles now standing at c30% compared to 40% across whole period. Amongst fields to decline is the study of stratification.

A few other stories about Australian sociology of course might’ve been told that space limitations prohibited (although perhaps hints could have been dropped) – many of which parallel some of the UK developments: the Exodus overseas of scholars in the ’20s, the small post-war group of émigré Continental sociologists, secret police surveillance of some early research; the Sydney/Paris axis of translations with the advent of post-structuralism/post-modernism and indeed the breakaway of the pomos from the sociology department at UNSW (unity has since been restored), the shameful
I find some difficulty with the thematic interpretations. Australian sociology seems quite robust to me, and certainly TASA's own propaganda is that the discipline is ‘robust’. No particular evidence is offered that departments escalate their offerings for competitive reasons. Differentiation is more likely to have arisen through internal pushes by staff to accommodate diversity. Anyway, how might the diversity be cut back? Abandonment of core because of competitive pressures from other disciplines (e.g. criminology) is more likely an issue.

4 Irish Sociology

The study of Irish sociology is well structured and well delivered. It is organised in three tranches:

- The earlier development of sociological writing on Ireland over several centuries;
- A tour of key events, writers and units since sociology in Ireland ‘took off’ stretching into the beginning of the concluding chapter;
- A proposed agenda for the future of sociology.

Early statistical work set off a tradition beginning with William Petty's account and included many studies from the 1830s onwards, many sponsored by the Dublin Statistical Society and the Belfast Social Inquiry Society. Other writers on Ireland included Malthus, Martineau, Beaumont and others are covered in a particularly interesting discussion. This tradition tended to be English and framed by a ‘liberal’ political economic approach. There was also stream of academic visitors concerned particularly with preservation of the Gaelic language. It is claimed (p. 5) that earlier discursive sociology framed later debates, but it is not shown how.

From the turn of the 20th century a more indigenous but strongly delimited Catholic sociology subsumed the earlier writing as part of a wider Catholic development of social thinking that sought to address social concerns while carefully segregating itself from secular sociology and in opposition to Marxism in particular. This highly religiously-circumscribed tradition was able to meld with state-building ideology and eventually spawned some empirical work. It was supported by a society and journal. At the end of this era a more sophisticated but still religiously orientated journal was established but this was over-taken by the establishment of secular sociology from the 1970s.

There was a parallel research trajectory from the 1930s when the (US) Rockefeller Foundation funded American anthropologists who studied rural communities in Ireland - drawing international attention to their studies of Irish communities - and set in motion a continuing but small sequence of similar studies. The studies (carried out by ethnographers Kimball and Arensberg) were spin-offs from the Yankee city studies. (Interestingly, the Yankee studies had an Australian connection from ex-pat Elton Mayo and Lloyd Warner who had previously researched in Australia.)

Over the period between the late 1960s and 1970s a more academic sociology became established, becoming more open to world-wide sociological content and involving the importation of some international scholars. An association and journal were established and cumulative development
since has ensued, although with all the vagaries attendant on a small sale enterprise. The trajectory of growth to the current Ireland-wide deployment of sociology with perhaps 200 professional sociologists is a blurry story: the speed of growth is undocumented although some more contemporary numbers are provided (in a footnote). There was something of a quantitative/qualitative split between the ISER (run particularly by economists although becoming less important as it necessarily became market-orientated) and the more divergent university departments. This account of social research is not updated with attention to more recent developments such as the Irish Platform for Social Research and related infrastructure initiatives. The more significant outcomes of Irish sociology (in terms of wider recognition) are claimed as the Famines and their effects and the Northern Island ‘troubles’, although we are given scant detail of what these studies involve or the social circumstances of their production.

The book’s final agenda-setting thrust engages with international ideas concerning the possibilities for sociology. The authors want to see more consciousness amongst Irish sociologists and for them to frame their work as part of world sociology wherein they may have particular contributions to make. They wish to foster more conceptual and interpretative work (although no strong arguments advanced as to why this modality of sociology needs boosting; it doesn’t seem in any danger within the sociology so comfortably bedded-in within Irish academic sociology departments). I’m afraid that that their study is only partially self-exemplifying given the authors’ posited standards for good sociology: Irish sociology isn’t depicted in comparative perspective (beyond a rather distant comparison with Vienna’s rather more advantageous position in the Germanic sphere and the asymmetric domination of Dublin by London). The authors suggest both clinging to the deep problems of core sociology and reaching out to the various peripheral sociologies (e.g. sociology of education). And they also call for rapprochement with the powerful tradition of mainly literature-orientated ‘Irish Studies’ through more cultural sociology.

The two difficulties I found with the account are that the organisational underpinnings aren’t adequately handled despite invocation (although then largely untreated) of Baehr’s distinction between discursive and institutional leaders in sociology: for example consideration of the historical development of Irish universities is provided only in a brief footnote. And the Northern Ireland/South Ireland nexus is not deftly handled as it is covered at some points but leaves the reader often wondering what was happening north of the border. An island divided into two states (with the North problematically still connected with UK) offers some enticing ‘research design’ opportunities: have different institutional developments (including different research assessment arrangements) north and south of the border led to different effects?

The volume insinuates but does not explicitly confront a continuing domination by UK sociology. Its broader geo-intellectual setting is not further elucidated. However, it might be interesting in this respect for readers to contemplate the early-Medieval situation where Irish monasteries protected the heritage of Western civilisation during a widespread cultural desert not only keeping the flame of civilisation alive (Cahill, 1996), but then replanting it throughout Europe. Perhaps Irish sociology should consider a similar role!

5 Comparisons/Similarities
All three volumes share similarly guarded views of future: there is little in the way of brimming hope for future sociology. None examine (other than fleetingly) a possible national sociology or its interrelations with other national sociologies. The Australian case study does at least boast briefly
about international theoretical accomplishments, and the Irish volume does so even more briefly, but the UK one is strangely silent on this point despite recent official reviews being upbeat on accomplishments. Such official accounts are not referred to apart from slight attention in the closing UK chapter – odd given the considerable involvement of both editors of that volume in these exercises. This suggests a major boundary-fence lies between academic sociology and non-academic sociology.

Another question lurking behind the volumes is the extent to which sociology is a working class discipline. The UK volume (p. 591) fleeting but obliquely refers to this point in noting that “during its expansion phases sociology was attractive to students and faculty alike who were first in their families to attend university and were orientated towards the new opportunities afforded”.

The present stage of development of historical sociologies of national sociologies, then, is that it is still in data assemblage mode: finding out what happened. And the tools used are scholarly: archival and administrative residues are mined with possibly some drawing on (auto) biographies but without recourse to oral histories or systematic data collection (e.g. collective biography, or even surveys: apparently only Australia has carried out surveys of sociologists amongst this trio of cases and this was not referred to). Recourse to conceptual frameworks is also needed to develop further. And the comparative aspect needs to be systematically built up.

In terms of being a midwife to further development, sociology of sociology needs to attend more to disciplinary ideologies which involve the ‘higher goals’ of a sociological community: especially what they hope to achieve and how this might be accomplished. Then an analysis can be made of how these yearnings interrelate to environing social conditions it is interfacing with. This needs ongoing discussion and also raising of the capacity of that community to deliver.

Some lessons from the three volumes are possible, although it may be unfair to place burden on such short books.

- There seems to be a reverse scale effect: the smaller the country the more relatively intense the development of its sociology: at the social interaction (e.g. conference going) end if not in terms of the formally published research output. Similarly, in the historical trajectory of national sociology development there may be a critical mass threshold which in particular triggers an unleashing of publications which seemed to be reached quite early in Australian sociology.
- External influences on Sociology tend not to be adequately considered and boundaries other than those occurring in early history are unattended to.
- There are similar concerns across the three countries with the imposition of an ‘audit culture’ although the precise impacts of this seem nowhere spelled out.
- We (the historians of sociology) seem to excel at telling interesting stories concerning our past, although without drawing much in the way of lessons re the sociology of sociology or of its effects on later developments. But we falter when tasked with depicting current sociological scene and not utilising available information represent situation. The provision of advice in these volumes is limited by personal predilections, or certainly not explicitly argued.

Hopefully, further country case studies in this series will provide further grist to the mill but also generate further data and ideas.
6 References


BOOK REVIEW

Fernanda Beigel (ed.) The Politics of Academic Autonomy in Latin America

Sari Hanafi
sarihanafi@gmail.com

Fernanda Beigel (ed.) The Politics of Academic Autonomy in Latin America
Farnham: Ashgate 2013
Hardcover, 290 pp.
ISBN 9781409431862
Price: € 120

This edited volume deals with the autonomy of the social science academic field in Latin America in 60s and 70s: historical formation, structural factors boosting or creating hurdles for the consolidation of this field. Through more than 30 years, it explores a current phenomenon – not only because most of the interviewees are still alive, but also because those past structures continue to play an active role today. It is the outcome of a successful research project with the participation of 12 authors from Argentinian universities. This concentration of researchers led sometimes to a reflection which is Argentinian centered and did not take into account specificity of some Latin American countries such as Brazil. These four sessions and 13 chapters have subtly combining theoretical reflection and empirical studies deploying ethnographical field investigating life stories, prosopography and statistical analysis (using primary and secondary data).

The four sessions and 13 chapters question much dichotomist opposition: central vs periphery and “internal” dynamics vs “exogenous” forces that shape intersections of the academic field with other social spaces. In the introduction, Beigel distinguishes three empirical levels of the notion of “academic autonomy”.

The first level is the “institutionalization and the effective specialization found in the construction of “academics,” largely manifest in full-time teaching and/or research positions at universities. At the same time several cross-section phenomena occurred, such as massive university enrolment, “feminization” and “modernization” of universities. Regional academic centers, graduate schools and research institutes also date back to this period, largely supported by foreign aid”.

This level is thus associated with university autonomy-- a long-established tradition in Latin America, featured in many national constitutions, starting in 1918 with Argentina’s university reform movement.
The second level of academic autonomy refers to the existence of a set of beliefs and narratives that separate the academic world from other cultural realms. Professors in universities shared certain values such as altruism, loyalty, and “teaching freedom”, that Bourdieu (1999) labels as “illusio,” which he views as a specific aspiration shared by academics engaged in a field and in the search for peer recognition.

The third level of academic autonomy refers to the effects of “internationalization” and the fact that several forces participate in the professionalization process. Fernanda Beigel discusses the “internationalization” process in order to show a heated debate about the ability of peripheral intellectual communities to create innovative knowledge and to craft their own research agendas. International networking is seen as an asset by a part of the authors while others sustain it creates “intellectual dependency”, this time not vis-à-vis an external force such as the State but Western power.

Part of the internationalization is transfer of material and symbolic resources – financing selected research agendas or introducing theoretical and methodological models. Fernando Quesada in his chapter (8) challenges the simplistic view that considers these activities as “unilateral transfer” that undermined the recipients’ autonomy. For him, the Rockefeller Foundation funding the University of Chile research cannot be understood as an institution replicating North American political dominance over the periphery. This perspective denies self-determination of the beneficiary institutions and assumes that they enter into philanthropic relationships with “zero degree of historicity,” as if they emerged socially solely only upon receiving funds.

The term “academic dependency” refers thus to domination scenarios stemming from the Latin American national field’s positioning in the international academic system. While the book discusses this uneven structure of the system and how it affects knowledge production in the periphery, it rejects the temptation of central/periphery theory and its deterministic view which argues that ”structure determines practice which reproduces the same structure” and argues for the emergence of peripheral centers. For Fernanda Beigel and her colleagues, the position in the structure determines the social strategy, and the determinisms applying to a given position operate through the complex filter of dispositions acquired and articulated over the whole social and biographical trajectory of the academics in the social science, and of the history of their structural position in social space.

Since the second half of the 1970s, many studies formulated a theory on the international structure of uneven academic exchanges (Arvanitis and Gaillard 1992; Altbach 1977; Díaz et al. 1983; Gareau 1988). Nonetheless, according to Beigel, the fall of dependency analysis and the theme shifts of social sciences in the 1980s and 1990s from academic dependency analysis to a more complex scheme eventually casted this issue aside. Nowadays, talking about “cultural imperialism” is deemed old-fashioned, and not even Bourdieu could rally attention to it.

In the area of the history of social science, these analyses have reviewed the role played by technical assistance policies and aid programs instituted by international agencies and private foundations (for a review in Latin America see Feld et al. 2013). As a result, the notion of “internationalization,” which had been virtually left behind in globalization studies was revisited. There is no consensus in the available literature on possibilities and paths to overcome dependency, largely because there are scarce empirical studies on academic professionalization in the periphery. Keim (2011) believe social sciences in peripheral countries can yield anti-hegemonic traditions. Her study of South African sociology enables her to plot an autonomous academic development process that follows a
pattern: a) it starts with the emergence of a public sociology, as social scientists shift their attention from international concerns to locally relevant issues; b) it continues with the dawning of a critical sociology, followed by a professionalization process; and c) it finally leads to an integrated community that interacts with the international community on more egalitarian and even terms. This process resulted in “disconnection” with the North Atlantic dominance and enabled the emergence of an autonomous tradition that “completes local sociology” (2011:131). Keim views this path as more conducive to autonomy than the strategies depicted by Jacques Gaillard (1996) as “catching-up” strategies through internationalization strategies brought about by individual. Nonetheless, as a recent survey on international collaborations driven by Gaillard and Arvanitis (2013) shows, “the asymmetrical relations in the main sectors of international scientific collaboration, which was highlighted as a burning issue in the 1970s and 1980s, have turned into a more equal partnership between [Europe and Latin America].” This is empirically visible in the way scientific activities and interests in cooperation as well as advantages and disadvantages of such collaborations are perceived by scientists in the two regions.

Fernanda Beigel advocates for the study of “cultural transferences” which in her view proved to be fruitful, as they focus on mediation processes (and mediators) involved in the international circulation of ideas: publishing (and publishers), translating (and translators), libraries, intellectual networks, and scientific missions, among others. For the most part, the analysis of cultural transferences tries to overcome the limitations of traditional comparisons that identify national cultures as isolated entities, rather than exploring their interactions.

For her a typical transference is “scientific missions” of Europeans and North Americans that proved to be particularly interesting since it became a relevant phenomenon in peripheral centers from the second post-war period until the 1960s. Also the national UNESCO commissions and leadership disputes within this organization to show that these agencies were instrumental in the creation of an academic regional circuit bred in the 1950s (see Chapters 2 and 3).

The use of a center-peripheral approach in social studies of science may lead to the assumption that a dependent economy goes hand in hand with an equally subordinated knowledge production “state,” which, in turn, means that peripheral contributions to international scientific development are expected to be null (Kreimer and Zabala 2007). At the very end, these categorizations tend to have a counterproductive effect in the history of science, preserving images of a universal science supported by symbolic violence.

The treatment of international knowledge circulation through the notion of “import–export”, is a simplistic approach, ineffective in analyzing notional and intellectual exchanges from the perspective of the periphery. It mainly reinforces the very idea that there is a dominant science, grounded in European or American traditions, that wields “originality,” rendering the peripheries as passive scientific spaces necessarily “lacking originality”, and merely consuming imported knowledge. Many debates on “intellectual dependence” according to Beigel prove to be rather fruitless, as they rely on a “nativist” benchmark that assumes the existence of national knowledge based on a unique “indigenous” outlook. We can also add to her acute analysis that a very similar debate was triggered in Latin America and Worldwide on technology transfers, that was initially based on the theory of dependence and had arrived to a dead-end, that was overcome only by changing the perspectives by introducing the analysis of technological in-house developments of firms, and the consequent spreading of new sociological and economic ideas in the continent (De la Garza, 2010). As Arvanitis (2010) mentions, the dependence theory as well as its opposition (for
example with the notion of ‘technological learning’) both appeared in Latin American and were
genuine local theoretical constructions.

So we are now in front of a problematic issue which is more complex than the classical Foucaultian
problematic of power. Working the nexus between power and knowledge in the case of Brazil,
Cláudio Costa Pinheiro (2010) brought two compelling examples: first, the case of French
demographer Alfred Sauvy who introduced the idea of the “Third World”, developed in Brazil and
published first in 1951 in a prestigious Brazilian academic journal. Nonetheless, the notion became
a ‘universal’ concept only after it was published the following year in the French magazine
*Observateur*. “Third World” theory obliterated Brazil from the debate. For Costa Pinheiro, the
development of social theories reproduce the cleavage between the North (theory developer) and
the South (theory consumer) and the memories of this debate were erased from the wider narrative
of the concept – “Third World” – and its consequences. However, in contrast with the transitivity
of “Third World” theory, Pinheiro gave an example of “dependency theory” that was able to directly
travel from Brazil to the global south and north. So “dependency theory”, whether we agree or not
with some of its explanations, is an example of the originality of the South and the possibility of a
theory to move from there to become international. Yet, for the North, this theory was blamed as
being an “ideology” although it was a truly intellectual movement with a largely shared problematic
by most economists in the continent. Pinheiro is in line with our thesis defended here, as well as
world with strong hegemonies. In the international Sociological Association’s conference
«Sociology in an Unequal World» (2009), there was a sort of consensus on the existence of a type
of domination of Western sociologies over the national ones, although it did not amount to a
Hegemony, in the sense of Antonio Gramsci. The conference also painted a more complex picture
than the mere existence of a Centre and dependent peripheries, because it considered that there are
peripheries at the center, and centers in peripheries, or semi-peripheries (e.g. Australia). To
understand this multi-leveled World system (as was proposed quite early by a Chilean sociologist
in France Polanco 1989), one needs to render the “invisible” knowledge “visible” in each national
sociological traditions. This organic form of knowledge has the ability to unfold the colors of reality
and the historical development in each context, although it does not preclude cooperation between
the North and South nor does it impede the search for new social theoretical directions.

We predict (maybe imprudently) the decline of the waves of post-modernism and post-
structuralism that shattered most thinking without theory, and the return to a more central role for
social theory, yet a flexible theory allowing pluralism and able to include the empirical variety that
is feeding constantly our literature. Hence, Singaporean sociologist Sayed Farid Alatas calls for the
“Indigenization” of social theory. He invokes the example of how to take theoretical advantage of
Ibn Khaldun’s thoughts about the development and ‘asabiyya, and not just in cosmetic and
folkloric way (to show that one is proud of the Arab social thought tradition). Having pointed out
that this Indigenization is part of the process of universality, it is not in the name of specificity
(Alatas 2010). Alatas call is therefore very different from the calls for the Islamization or
Judaization of knowledge, advocated by some scientists in the Islamic world and in Israel. These
latter calls have not only led to an ideological eclectism in social research but to a sterile attempt of
reading, even the local reality.
Militant academicism

Beyond this dear idea of debunking the center/periphery theory, the book of Beigel delves into the empirical observations to gain a better understanding of the historical trends of academic autonomy in Latin America, namely the politicization and return of exilic social academics.

In Bourdieu’s *Homo Academicus* we saw the close relationship between science and politics at the university. Politicization served as a “compensating strategy,” which enabled an “escape from university or scientific market’s specific laws” (Bourdieu 1984: 34). This politicization especially emerged in times of crisis (such as May 1968), driven by conflicts of interest over positions held in the field, a time when the political division principle prevailed over other criteria formerly polarizing groups in university life (Bourdieu 1984: 244–5).

This politicization in LA does not concern only professors but also students. The militant dispositions and skills and student engagement were greatly influenced by primary Catholic socialization. A phenomenon that we find in the Arab world, but with the influence of Islamism and sometimes leftist movements.

For Beigel, it is in the 1960s, during second-generation reform movements that this reconversion fueled the expansion across university bodies with this new kind of symbolic capital that changed the “illusio,” career-building paths, and academic recognition sources. Thus, the student movement laid the foundation for a “militant academicism” that spread across universities in certain times. Thus, in the 1960s militant capital turned into academic capital and vice versa. This prevailing collectivism largely explains the assembly-like operations within many research institutes, the politicization of regional centers, and new forms of “militant academicism” sometimes disguised with anti-academia motions but rarely venturing outside universities.

Regionalization

What I found extremely interesting is how the exilic scholars become the force of LA scientific community integration. The Christian Democrat candidate won the 1964 presidential election and Chile underwent a process of deep social change, including land reform. Military coups in Brazil and Argentina consolidated Chile’s leadership within the regional academic circuit, because hundreds of exiled intellectuals escaped to Santiago. Most of them were young Brazilian social scientists that were also involved in student activism and had taken part in the student movement at the National University of Brasilia.

Many had participated in the resistance against the dictatorship, and some of them had been arrested. They became affiliated to CEPAL, FLACSO, the University of Chile, and the University of Concepción, making a major contribution to social sciences and to the “Latin-Americanization” of Brazil.

With Chile’s military coup in 1973, Latin America’s social science circuit hub moved to Mexico, along with a sizable share of the South American social scientists who had resided in Chile. This constituted the second big wave of regional academic exile. The Southern Cone’s military regimes brought about an increasing segmentation process at universities, with the creation of tiered, isolated consecration circuits. A deliberate social sciences “de-institutionalization” policy was pursued, with visible repercussions lasting until the present.
Part of the process of regionalization is the backing home of Chilean exiles with the help of The World University Service-United Kingdom (WUS-UK) Return Program. This was the object of the Chapter 11 of Paola Bayle.

WUS-UK become later on the Council for Assisting Refugee Academics (CARA) and played a major role for relocating the Chilean exile scholars and their backing home. Bayle’s study reveals that Chilean exiles’ return to their homeland’s labor market was driven by organizations and individuals willing to contribute to the struggle for democracy in Chile and, accordingly, the extent to which politics and academia have been historically linked in this particular phase of Chilean history.

WUS-Chile’s labor inclusion policy for returning Chilean exiles was meant to complete the rescue cycle for academic agents and institutions initiated in 1973 by AFC and WUS-UK in the UK. This program brought together scholars from several political affiliations which came together in a single, progressive movement that sought to help exiles to rejoin Chilean society. I hope Tunisian, Egyptian, Libyan scholars will learn from the Chilean experience in backing home.

Conclusion
The only criticism I would have is that Fernanda and the other contributors did not account for the importance of the constitution of research teams. One cannot discuss institutionalization, professionalization and internationalization without addressing the role of research team consolidation on that. This is very crucial for the academic autonomy and scientific community.

The book of Fernanda is more than an empirical investigation of Latino American academics in 60s and 70s. It is an attempt to provide an experimental demonstration for the necessity and potency of a genuinely reflexive sociology: Beigle’s and her colleagues’ aim is to show that sociologists can overcome the antinomy of objectivist explanation and subjectivist understanding and account for the very world within which they live. This process involves reflecting on their own practices and positions using scientific tools for objectivation that they routinely employ upon others so as to neutralize the biases inscribed both in the contemplative relation between the social observer and her object and in the fact of occupying a particular location in the universe under investigation.

Bibliography


BOOK REVIEW

Morris, The Scholar Denied: W.E.B. DuBois

Daniel R. Huebner
drhuebne@uncg.edu

Oakland, CA: University of California Press
Hardcover, xxviii+282 pp.
ISBN: 9780520276352
Price: US $ 29.95

W. E. B. Du Bois (1868-1963) is the subject of a burgeoning body of research and reappraisal in American sociology. In this new study, Aldon Morris advances several major new contributions in understanding Du Bois and evaluates his unique position in the history of the social sciences. In the introduction and first chapter, he provides a useful outline of much of this new research on Du Bois and on the importance of the concept of race to the development of American sociology. Through considerate and well-documented analyses over seven chapters, Morris argues that Du Bois developed what may be considered the first major empirical research project and founded the first identifiable scientific school of thought in American sociology, advanced a comprehensively sociological analysis of race, and influenced the thinking of Max Weber. These accomplishments would seem to place Du Bois at the center of the burgeoning field of sociology, but Morris traces how Du Bois was systematically excluded from institutional resources and rewards and how early American sociologists were complicit in this racism. The contradiction this seems to present – a scholar fundamentally excluded from the resources that produce high-level scholarship who nevertheless develops a major body of pioneering work and develops a school of thought – is utilized by Morris as an opportunity to contribute to the sociology of knowledge, above and beyond the important historical worth of the study. He develops the useful notion of “liberation capital” as a way of conceptualizing the unique intellectual possibilities and challenges of people in subjugated social positions.

In chapter two Morris presents a synthesis of Du Bois’s scholarship and gives details about his education and experiences that contribute substantially to understanding their context and importance. A particular highlight is Morris’s examination of the influence of the Gustav Schmoller’s branch of the German Historical School of Economics on Du Bois. As an exchange student at the University of Berlin, Du Bois attended Schmoller’s seminars, and drew from the Historical School’s “quintessentially sociological” emphasis, as Morris puts it, on empirical multi-method analysis of economic institutions over historical time rather than abstract concepts, deductive reasoning, and grand theories. This critical, empirical approach, which including training in statistical methods, continued to distinguish Du Bois’s work from other American
sociologists of the period, especially when examining race. In the secondary literature, there is considerable commentary regarding whether Du Bois fully developed an adequate, social constructivist approach to race. Morris deftly argues, against critics, that the larger body of Du Bois’s research refuted the notion of inherent inferiority or superiority of races and treated biology as having no essential role in the causal analysis of racial inequalities. Instead, he stressed economic exploitation and political oppression as the producers and sustainers of racial inequalities. Thus, racial inferiority is caused by historical social forces subject to empirical investigation and political agitation rather than biological racial differences. Morris argues that Du Bois is perhaps the key classic reference point (more thoroughgoing than Franz Boas or Max Weber) for contemporary social constructivist theories of race. In addition, Du Bois was a “preeminent public sociologist,” who sought to make sociology relevant to social change, and Morris points out how Du Bois’s editorship of *The Crisis*, the journal of the National Association for the Advancement of Colored People, for a quarter century provided a vehicle for the dissemination of a sociological perspective to millions of readers perhaps more effective than any that has since been seen. A major claim of the work is that Du Bois’s 1897 study *The Philadelphia Negro* is the first major empirical sociological research project conducted in the United States. For that study, Du Bois employed a massive, multi-method approach in order to examine African American institutions and cultural processes with a depth and precision that was unparalleled elsewhere in the social sciences of the time in the United States. In addition to Du Bois’s pioneering individual scholarship, Morris argues that at the resource-poor, historically-black Atlanta University, Du Bois pioneered the first “scientific school of American sociology” beginning in 1897. In Atlanta, he developed a sociology department and taught sociology to a generation of eager African American students. The text highlights the careers of this “hidden” or “erased generation” of African American sociologists (Monroe Work, Richard R. Wright Jr., George Edmund Haynes, and others), who embraced Du Bois’s empirical sociology as a weapon of liberation. Du Bois also developed a “research laboratory” in sociology at Atlanta University that produced the most thorough and reliable sociological analysis of race at the time. The laboratory held unique annual conferences, which led to a convergence of scholars and leaders (including Jane Addams and Franz Boas) every spring, during which they would debate the intellectual and political implications of research findings from the previous year’s work on some aspects of African American urban life. Morris argues that what he calls the “Du Bois–Atlanta School” can truly be called a unique and pioneering school of thought, because it presented a novel theoretical position that differed from existing paradigms, and because its novel methodological approach enabled it to generate empirical findings that challenged dominant paradigms.

Despite his impressive credentials and body of work, and despite his continued attempts to get cooperation and employment from elite, white universities, Du Bois faced systematic racism that prevented even the most accomplished African American scholars from equality of opportunities and resources. Morris traces numerous ways in which the scholarly community was complicit in this racism, and especially noteworthy is his detailed examination of the relationships of Du Bois, Booker T. Washington, and Robert E. Park in chapter four. Washington served as a “gatekeeper” for resources to projects concerning African American higher education in the late nineteenth and early twentieth centuries. He was head of the Tuskegee Institute, a historically-black institution that promoted practical industrial and agricultural education. The “Tuskegee Machine,” as Morris (following others) calls it, was bankrolled by many of America’s wealthiest philanthropists, and dominated the press about African American higher education. Washington could, through his
dominant position and influence, silence or co-opt rivals and virtually control the allocation of resources. This had a profound effect on Du Bois and his sociology program at Atlanta University, because he and Washington were engaged in an “epic ideological struggle,” as Morris terms it, in the first decade of the 1900s over the proper course of action to achieve racial equality. While Washington favored a compromise position that emphasized advancement of African Americans through vocational education and labor, Du Bois favored immediate, full political equality and critical, liberal arts education that sought to expose inequalities. Robert E. Park stepped directly into the middle of this rivalry when he was hired in 1905 as director of public relations for Tuskegee Institute and ghostwriter for Washington. It was Park’s “occupational duty,” Morris points out, to be a political ally of Washington by exploiting his newspaper contacts and influencing coverage of Tuskegee and other African American institutions. In this chapter, Morris carefully documents the available evidence and concludes that Park was directly involved in negatively affecting Du Bois’s image in the press.

Park was later hired by the sociology department at the University of Chicago and became the “king of race studies,” and Morris shows how Washington’s mentorship was crucial to the sociological understandings of race that Park developed and taught. Through a side-by-side comparison of Park’s and Du Bois’s scholarship, Morris shows how Park’s “Chicago School” of studies of race implicitly adopted elements of Social Darwinist thinking, most directly in Park’s continued emphasis on biologically-based racial temperaments, his attempts to discern natural laws in racial conflict, and his notion of civilizational progress that uncritically accepted the hierarchical evaluation of cultural traditions. Moreover, Morris points out that Park inherited important institutional resources at the University of Chicago (including networks of colleagues, graduate students, and research funds) unavailable to Du Bois at the resource-starved Atlanta University. Because Park and his students were not ignorant of Du Bois’s research, as the text shows through an examination of citations, Morris is led to the conclusion that the Chicago School marginalized Du Bois from mainstream American sociology by ignoring his scholarship and excluding him from scholarly networks. Morris documents other major examples of the marginalization faced by Du Bois in the final chapter, such as the Carnegie Foundation’s decision to fund Gunnar Myrdal’s 1944 study An American Dilemma: The Negro Problem and Modern Democracy as its major contribution to race relations instead of Du Bois’s proposed Encyclopedia of the Negro, at least in part because of advice from white American social scientists who were concerned with the radical advocacy and supposed biases of Du Bois. Again, Morris provides historical documentation and traces the alternative epistemological and political implications of Myrdal’s and Du Bois’s scholarship.

Max Weber, in contrast to dominant American sociologists, took Du Bois very seriously. Lawrence Scaff’s 2011 Max Weber in America traced important aspects of the relationship between the two men, but Morris goes much further in his analysis in chapter six. Weber’s and Du Bois’s graduate schooling at the University of Berlin overlapped, but their real intellectual contact began in 1904 when Weber visited the United States in connection with the International Congress of Arts and Sciences and asked Du Bois to write an article for his Archiv für Sozialwissenschaft und Sozialpolitik addressing the relationship between race and caste. Weber subsequently made repeated requests for publications and syllabi from Du Bois, which he apparently eagerly read. Morris makes a convincing argument, on the basis of the available documentation, that Du Bois strongly influenced Weber’s thinking about race in the early twentieth century, and that Du Bois’s influence can be seen in Weber’s mature writings on the notion of social status, especially as it relates to ethnicity and caste.
Chapter seven of the text develops the implications of this case for the study of schools of thought and scholarly fields. The Du Bois–Atlanta School lacked the conditions that sociological studies have identified as necessary for scholarly productivity and influence, such as positive reward structures and access to scholarly networks, and yet it developed innovative scholarship for a decade and retained a certain influence despite suppression and mainstream invisibility. Building upon Gramsci’s and Bourdieu’s work, Morris argues that subaltern intellectuals, who have been systematically excluded from recognized intellectual discourse, can develop “counterhegemonic” or “insurgent” intellectual networks, by drawing on a counterhegemonic form of capital, which Morris deems “liberation capital.” This liberation capital consists of donated resources and volunteer labor by scholar-activists, including providing scholarly tools to previously untrained students and creating media to make scholarship visible. The promise of group liberation through their efforts to develop and validate counterhegemonic ideas serves as a compensation for unremunerated work, perhaps even when faced with professional sanctions. By mobilizing liberation capital scholars may hold off resource challenges long enough to allow new schools of thought to take root, but such efforts cannot fill the need for resources indefinitely, nor can they ensure that the best scholarship will always be produced. While liberation capital was the “basic form of currency” that made possible the Du Bois–Atlanta School of sociology, it also depended on charismatic leadership and a minimal institutional infrastructure. When those resources are lacking the school of thought will ultimately fail to develop. With this theory, Morris has made a unique contribution to the sociology of knowledge, offering a critical tool that scholars can use to conceptualize and reevaluate the contributions of marginalized intellectuals who have until now been erased from the history of the social sciences and perhaps other disciplines.
Handbook of Indicators of Institutionalization of Academic Disciplines in SSH

WP2: Patterns of institutionalization of SSH

Workpackage Leaders
University of Graz (Austria)
& John Wesley Theological College (Hungary)

Grant Agreement n°319974
# Table of Content

- **Introduction** ............................................................................................................................... 3
- **1 The formation of a discipline (qualitative report) (HIGH)**................................................................. 7
- **Organisation of Disciplines (Research + Teaching)**............................................................................ 8
- **2 Research......................................................................................................................................... 8**
  - 2.1 Academic/university entities (HIGH)......................................................................................... 8
  - 2.2 Professional associations / learned societies (MEDIUM)............................................................ 9
  - 2.3 Research institutions and arrangements (MEDIUM)................................................................. 10
- **3 Teaching (secondary & tertiary education).................................................................................... 11**
  - 3.1 Academic curricula/degrees (HIGH)......................................................................................... 11
  - 3.2 Conditions of access to the discipline (MEDIUM)................................................................. 12
  - 3.3 Secondary education (LOW).................................................................................................... 14
- **4 Output............................................................................................................................................... 15**
  - 4.1 Reputation of publication outlets (qual.) (MEDIUM)................................................................. 15
  - 4.2 Publishing landscape (timespan: today) (HIGH)........................................................................ 16
  - 4.3 Academic journals (timespan: 1945-2015) (HIGH).................................................................. 17
  - 4.4 Content of journals (LOW)........................................................................................................ 19
  - 4.5 Handbooks (MEDIUM)............................................................................................................ 19
  - 4.6 Non-academic output (science to public) (MEDIUM).............................................................. 21
- **5 People (tertiary education & research)............................................................................................ 22**
  - 5.1 Personnel (HIGH)..................................................................................................................... 22
  - 5.2 Social background of professors/research staff (MEDIUM)...................................................... 23
  - 5.3 Student numbers (in tertiary education) (HIGH)..................................................................... 24
  - 5.4 Job market (LOW)................................................................................................................... 25
- **6 Public and academic evaluation and distinction .............................................................................. 25**
  - 6.1 Prizes, recognition and excellence (MEDIUM)...................................................................... 25
- **7 Funding............................................................................................................................................ 27**
  - 7.1 Funding schemes for people and projects (timespan: today) (MEDIUM)......................... 27
  - 7.2 Remuneration (MEDIUM).......................................................................................................... 28
  - 7.3 National research and development/innovation indicators (LOW)...................................... 29
- **8 Misc (LOW)................................................................................................................................... 30**
Introduction

In work package 2 the aim is to develop a concise set of indicators for the comparative analysis of the development of the social sciences and the humanities (SSH) in Europe since 1945. Mapping the recent history of the SSH in various European countries with reference to their main intellectual partners outside the continent is indispensable to envisage its future prospects. The objectives are:

1) to identify **national patterns of institutionalization** which might explain the **relative isolation** of national traditions in the SSH but also the operating patterns of **crossed influences** and **international cooperation** (competition, national self-assertion, efforts to ‘catch-up’, etc.);

2) to assess the importance of the disciplinary **division of labour** within the SSH in order to reflect upon the historically changing **power relations** between branches of study, processes of professionalization of new disciplines, the reshaping of traditional forms of scholarship and the potentials of new mechanisms of intellectual and institutional **collaboration** and exchange with or without consequences in terms de-disciplinarisation of disciplines concerned;

3) to find out to which extent the **varying institutional (or academic) division of labour** within the SSH is an **obstacle to cooperation among actual research branches** and in which way its transformations can be a source of scientific innovation.

Tools and methods for a global sociology and history of the SSH have to be established, by identifying major social factors – including political ones – of their level of development as measured by **objective empirical indicators**. While a comparative approach is necessary in order to interpret the institutional and morphological aspects of diverging national traditions, a transnational approach combining entangled and connected history is required to account for the role of supportive agencies such as the UNESCO, public national foundations and private funds of sponsorship (such as philanthropic foundations) in processes of institutionalization and professionalization, as well as in the exchange and circulation of research achievements and scholars, including the forced or strategic (market oriented) migrations of the latter.

The aim in this part of the project is essentially to collect data and analyse this data to produce indicators for the comparative study of the development of SSH disciplines, including the social and intellectual characteristics of their research staffs, their dominant topics and study targets, the technicalities and methods applied as well as their preferential forms of both scholarly and popular communication in various periods. This is to bring about systematically designed and well-structured overviews per country. The sources to be exploited range from available monographic studies to university statistics, administrative archives and a variety of policy statements and reports issued by ministries, national academies, professional associations and international agencies (OECD, UNESCO, European Science Foundation, etc.), the combined evaluation of which has never received serious academic attention.
To reach this aim this handbook of indicators should lead every partner in their efforts to collect as much comparable data as possible for their country and all the disciplines selected for this project. It will not be possible to gather all the data for all disciplines in every country as the degree of differentiation and detail of data available on the national scale differs considerable. The structure of the handbook mirrors the data collection and organisation process. The handbook includes information on every set of data to be collected including a short description of the rationale of the indicator. Also, the kind of data to be gathered and the different descriptors/variables are defined. For qualitative indicators questions that should be answered in reports are detailed. Finally, the handbook provides information on templates that were or will be provided by the coordinators of the work package and a short notice on the repartition of work between the partners and the coordinators.

Generally speaking, the indicators are organized along five horizontal pillars and one cross-cutting dimension. The five pillars are:

1. **Research** focuses on research institutions, professional associations and other research institutions in the different disciplines and their historical development.
2. **Teaching** deals with academic curricula and degrees awarded in the different disciplines, the conditions of access to higher education training in these and the representation of different disciplines in secondary education.
3. **Output** concentrates on academic output, which is obviously closely linked to the research dimension, and non-academic/popular science output and the roles as public intellectuals etc. of representatives of the different disciplines. Common practices of publishing will be contrasted to quantitative measures of publishing in journals, books and other media.
4. **People** includes numbers on personnel and their social background, the development of student numbers in tertiary education and some basic information on the job market for academics in the fields under study.
5. **Recognition** deals with awards, rankings and other forms of public and academic evaluation.
The vertical dimension **funding** is dealing with national systems/procedures leading to funding for research, academic teaching and publishing as well as the development of salaries of academic staff on the one hand, and larger financial indicators for research and innovation on the other hand.

### The Formation of a Discipline

<table>
<thead>
<tr>
<th>Research</th>
<th>Teaching</th>
<th>Output</th>
<th>People</th>
<th>Recognition</th>
</tr>
</thead>
<tbody>
<tr>
<td>academic institutions</td>
<td>tertiary education:</td>
<td>publication practices</td>
<td>research/teaching</td>
<td>prizes and awards</td>
</tr>
<tr>
<td></td>
<td>curricula / Degrees</td>
<td></td>
<td>personnel</td>
<td></td>
</tr>
<tr>
<td>non-academic</td>
<td>accessibility of</td>
<td>academic publication</td>
<td>student numbers</td>
<td>evaluations</td>
</tr>
<tr>
<td>institutions</td>
<td>disciplines</td>
<td>(quant)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>professional</td>
<td>student numbers</td>
<td>non-academic publishing</td>
<td>social background</td>
<td></td>
</tr>
<tr>
<td>associations</td>
<td></td>
<td></td>
<td>of researchers</td>
<td></td>
</tr>
<tr>
<td></td>
<td>secondary education</td>
<td>media coverage</td>
<td>job market for graduates</td>
<td></td>
</tr>
</tbody>
</table>

**Funding** – funding schemes and financial indicators

### Practical Information

All partners should take into account the following information for data-gathering and organisation:

- This Handbook understands itself as a maximalist framework and we cannot expect to fill in all the demands for every national case, therefore the coordinators of WP2 are in close contact with the partners to adjust templates, to find alternative measures and to make sure that a great deal of the data is internationally comparable.

- The data will be gathered and organised separately for every discipline (economics, sociology/demography, political science, anthropology, philosophy, (national) literature and psychology/psychoanalysis) and every country, i.e. in separate Excel/Word files.

- The timespan covered by the indicators depends on the type of data gathered. In the case of numerical/time-series data the standard procedure is to capture annual data if available. Where this is not possible the sample taken should proceed at least in ten year steps starting in 1950. However, depending on the efforts necessary to gather the data, five year steps are preferred:
  - cover all “0” years, i.e. 1950…1960…n…2010 or, where possible in five year steps.
  - Other indicators either only deal with the current situation or offer qualitative reports of historical developments taking into account important milestones. What is “important” might differ from one country or discipline to the next and can not be pre-defined.
Three levels of priority for data gathering are defined (high, medium and low). Data with high priority should be gathered by September 2014, data with medium priority by December 2014 and data with low priority by the end of March 2015. [Deliverable II: Mid-term workshop in May 2015 with data-presentation]

To organize the data, templates (tables and structures or sample reports) will be provided by April 1st 2014 for the data of high priority and until May 1st 2014 for all the remaining indicators.

Next to the availability of time-series and specific datasets for every discipline the comparability and interpretability of the data is closely linked to the understandings of boundaries of disciplines implicitly or explicitly stated in the sources used by the project. As a consequence it is of utmost importance that in the report on “the formation of a discipline” an explanation is given describing the borders of disciplines in the different national contexts over time. On top of that the spread-sheets should contain memos describing issues of institutional boundaries, changing boundaries, etc. that are necessary to interpret the data.

### Repartition of Tasks

<table>
<thead>
<tr>
<th>Task 2: SSH in Great Britain (UCAM)</th>
<th>Task 6: SSH in Northern Europe: the Netherlands (EUR)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Task 3: SSH in France (CNRS)</td>
<td>Task 7: SSH in Eastern European Countries: Hungary (WES) [with references to Romania and Slovakia]</td>
</tr>
<tr>
<td>Task 4: SSH in Germany, including GDR (GRAZ)</td>
<td>Task 8: SSH in Latin America: Argentina (CONICET) [with references to Brazil]</td>
</tr>
<tr>
<td>Task 5: SSH in Southern Europe: Italy (UNIBO)</td>
<td>Task 9: SSH in the US in comparative perspective (CNRS)</td>
</tr>
</tbody>
</table>

### Disciplines to be covered

Anthropology, Economics, Literature, Philosophy, Political Science, Psychology/Psychoanalysis, Sociology/Demography.
1 The formation of a discipline (qualitative report) (HIGH)

Rationale and descriptor
The short reports on the formation of a discipline should serve two purposes: 1) it should be used as an internal report for the contextualisation of the data gathered for work package two; and 2) it should serve as a basis for an introductory chapter on every country and discipline for the final report and analysis of work package two.

The short report on the formation of every discipline should take into consideration the milestones in the institutionalization of a discipline and go back beyond 1945 and take into consideration initiatives not necessarily bearing the name of any of the disciplines under scrutiny, i.e.:

- First institutional affiliation of university departments;
- First professors;
- First journals;
- First professional associations;
- Names of important forefathers, and;
- Descriptions of phases of expansion of the discipline in terms of institutions, students and/or public recognition;
- Descriptions of the degree of centralization of decisions concerning the academic institutionalization with special attention to the influence of political players/institutions on disciplines (formal vs. actual influence taken by these institutions, i.e. did ministries interfere in academic matters when they were formally part of decision-processes?)

This report can be as long as necessary for internal use, i.e. include all kinds of particularities that might be important to know to interpret all the other data later on. For the final report of the project a short version will be used to introduce the data on indicators of every country and discipline, the remaining information will then be used to contextualize the data of the indicators.

Template
Report.
Organisation of Disciplines (Research + Teaching)

2 Research

The aim of this set of indicators is to show patterns of institutionalization of SSH research in different disciplines. It deals with the creation of institutions of research reaching from research departments, non-academic institutions to publication outlets and the organisation in professional associations.

2.1 Academic/university entities (HIGH)

Rationale
Ideally this part of the indicator should show whether and when disciplines became independent from other disciplines on the institutional level and under which larger institutional arrangement the SSH disciplines are categorized (e.g. faculty). The aim is to gather data on the institutional development of disciplines and to show when and how they became independent entities on the organisational level. The most interesting timeframes in this context are the first years of institutionalization of a discipline and the developments taking place in phases of massive educational expansion (from 1960s). Whereas the data for the first period should be comprehensive, the study of the second timeframe can concentrate on pointing out changes and/or important developments in the organisational arrangement. At a later stage or as a spin-off project, data could be gathered for newer disciplines or sub-disciplines like Gender Studies, Cultural Studies or European Studies.

This indicator captures the degree of independence of a discipline in universities and faculties on an institutional level.

Descriptor/Variables

1) List of first disciplinary institutions
   a. Institutional arrangement at the creation of SSH unit going as far back in the history of a discipline as necessary;
      i. type of unit: faculty, school, department, “Lehrstuhl”;
      ii. date of creation of the institutional unit.
   b. institutional arrangement in the timeframe of 1945-55 & during educational expansion:
      i. type of unit: faculty, school, department, “Lehrstuhl”;
      ii. date of creation of the institutional unit.

2) Report dealing more generally with the institutional arrangements of SSH disciplines and their categorization in universities (i.e. of what faculties are they part). Also report on major changes that occurred over time.
If the boundaries of disciplines are unclear the focus should be on the “core” of a discipline, i.e. only when a discipline can be identified easily through their nominal designation (for example in the World of Learning).

**Data-gathering method / Sources**

Literature, i.e. institutional histories of disciplines. “World of Learning” contains division of universities. For more recent periods it is more important to describe significant changes than to enumerate all university departments.

**Templates**

Timelines; i.e. Excel table + visualisation.

**Tags for literature**

“academic entity“

### 2.2 Professional associations / learned societies (MEDIUM)

**Rationale**

The goal of this indicator is to show the degree of professionalization of a discipline and its community. It captures the number of professional associations, their date of creation, their membership rules, the number of members and their activities. The different partners should focus on the national associations as information on international/European associations will be gathered centrally (especially by Thibaud Boncourt). The aim of the indicator is to show what kind of funding was available for the creation of a professional association and whether or not these associations contribute to an internationalization of a discipline.

**Variables/Descriptor**

1) Table of (national) professional associations (see World of Knowledge data)
   a. date of creation;
   b. place of creation (country/university);
   c. founder(s)/first president;
   d. funding for initial creation and currently (if available);
   e. internal organisation (i.e. sections like “sociology of culture, economic sociology”);
   f. disciplines covered.

2) Table of membership numbers (5 year steps from creation of association to present).
   a. total n;
   b. % women;
   c. % non-national members (countries where available).

3) Selectivity of association laid out in short report (2-3 sentence per association)
   a. membership rules;
   b. changes in membership rules.
4) Checklist and report on activities of professional associations [Comment: Checklist will be provided by coordinators]
   a. academic activities (summer schools/training courses, academic journals, national annual/bi-annual conferences, awards, funding of young research, own research, job market tools);
   b. political representation (is the organisation part of a political representation group (e.g. initiative for science in Europe), are members of the association representing the political interests of their members on the political level (“taking the role of trade unions”; licence to do a certain job; protection of profession);
   c. Science2Public (adult education, counselling/expertise, press conferences, presence in media as association, exhibitions);
   d. other non-academic activities.

Data-gathering method
Team Austria provides a list based on “Handbook of learned societies”/World of Learning Partners check (with ‘expert’) if the list is complete. Partners complete missing information (websites, experts, archives?).

Templates
Excel table.

Tags for literature/files in Zotero & online workspace
“professional associations“

2.3 Research institutions and arrangements (MEDIUM)

Rationale
The aim of this indicator is to show the arenas of social knowledge making of the different disciplines other than traditional universities. That can be networks of organisations like CNRS, Max Planck or Academies of Science, or single institutions like museums or think tanks. In the context of institutionalization processes it is necessary to know when these organisations were created, which disciplines are covered in these organisations and what role they play on the national (or also international level). In contrast to a mere list of organisations the chosen descriptors should provide information on the degree of interdisciplinary interweaving of these organisations. This indicator should also show when and where hybrid arenas of knowledge-making, positioned between the academic, public and economic/cultural/… sectors, were created in the social sciences and humanities.

Variables/Descriptor
Relevant institutions are defined as those that have some kind of research activity and produce a (perceivable) output of their research (which must not necessarily be disseminated
in academic journals etc.) i.e. the output can also be museum exhibits, economic forecasting reports etc.

1) List of current national non-university institutions that make SSH knowledge on the national level [Comment: also name important institutions that do not exist anymore. Concentrate on nationally visible entities; ignore local institutions]

2) Table with profile of national institutions
   a. date of creation;
   b. disciplines/research areas covered (one/pluri-disciplinary);
   c. type of institution: for example consultancies, corporations, think tanks, NGOs, etc.;
   d. weight in national context (large, medium, small);
   e. full-time equivalent of research and development personnel (where available);
   f. short description of activities (mission statement);
      i. research projects;
      ii. journals associated;
   g. key figures (if known);
   h. funding (public, private, foundation etc.).

3) List of non-national institutions, i.e. institutions founded (and funded) by international organisations, a number of different national organisations/institutions or national states. [Comment: examples would be the IIASA (International Institute for Applied Systems Analysis), UNESCO sites, the EUI (European University Institute) in Florence etc.]

Data-gathering method
Partners should make lists based on METRIS country reports and own research.

Templates
Table.

Tags for literature/files in Zotero & online workspace
„research institutions“

3 Teaching (secondary & tertiary education)

This set of indicators deals with the development of curricula, conditions of access to PhD research and the interweavement of disciplines in secondary education.

3.1 Academic curricula/degrees (HIGH)

Rationale
The aim of this indicator is to identify when and where academic curricula/degrees were first introduced and how the situation looks now. Next to showing the process of institutionalisation, also the beginning of a diversification of curricula (after Bologna) will be highlighted. However, the data gathered for this indicator captures only the main disciplines
covered by this project in detail. Disciplines and sub-disciplines connected to these will not be taken into account here, however, future studies could gather data for these more specialized fields using the indicators developed in the INTERCO-SSH project. This indicator shows how the institutionalisation process takes place on the level of teaching a discipline in higher education institutions.

Variables/Descriptor

1. Table of academic curricula/degrees per university
   a. date of creation of academic curricula and or degrees per university;
   b. undergraduate;
   c. master;
   d. graduate (specific graduate schools or doctoral programmes).

2. Number of universities where a discipline is taught
   a. number of teaching vs research departments;
   b. number of departments offering doctoral degrees.

3. Report on the development of disciplines, the situation at the start and when the degrees/courses started to diversify, i.e. to form sub-disciplines like for example “Peace Studies, Social Policy, Criminology, Gender Studies or Cultural Studies”

4. Report on the procedures leading to new courses/degrees to be created
   a. what is the procedure on the national level?
   b. who controls the accreditation of degrees?

Data-gathering method

Partners research the data.
Interviews with ministry officials.

Templates

Table.

Tags for literature/files in Zotero & online workspace

“curricula”

3.2 Conditions of access to the discipline (MEDIUM)

Rationale

This indicator captures selection procedures for PhD students that want to take courses of a specific discipline. Connected to that is the selection/formation of elites that are often selected through PhD affiliation. If the selection procedures for PhD courses and/or elite building of a discipline take place at an earlier stage these mechanisms of selection should also be described in the reports. The indicator aims at identifying the selectivity of a discipline.

The report should deal with different mechanisms regulating the access to PhD education in a certain discipline. The mechanisms leading to hierarchical structures inside a discipline and
also in-between disciplines are not necessarily imminent to PhD education, i.e. the choice of university at an earlier stage might be of larger importance to having access to elite PhD education than passing an entry-exam for example. Still, these processes should be taken into account.

**Variables/Descriptor**

1) Report describing access to disciplines with special focus on PhD level
   a) competition for access (i.e. numerus clausus; entry exams, limited number of spaces);
   b) tuition fees;
   c) elite schools vs. “ordinary” schools (incl. list);
   d) effects of selection procedures on hierarchical structures inside and in-between disciplines, i.e. are there specific career-paths that are dependent on early educational choices (e.g. elite universities/schools enhancing the chances to become professor in certain institutions or more generally the reproduction of elite structures in a discipline);
   e) other selection procedures.
   f) Selection for access to research / discipline…

2) Qualitative report on the value of a PhD in a discipline answering the following questions:
   i) What is the value of a PhD in a country; in a discipline etc.?
   ii) Who has the right to deliver a PhD (e.g. universities vs. Grandes Écoles)
   iii) Are there differentiations between doctorates (official or unofficial doctorates)?
   iv) Are there professional PhD programmes (like professional MA-programmes)
   v) When does an academic career start?

**Data-gathering method**

Interviews with experts (partners). Database by coordinators based on “The World of Learning”.

**Templates**

None.

**Tags for literature/files in Zotero & online workspace**

“distinction”
3.3 Secondary education (LOW)

Rationale
This indicator captures whether disciplines are present in upper secondary education (ISCED 3; approx. starting age 16) and to what degree they are implemented in secondary curricula. The aim is to evaluate the differing implementation of SSH disciplines in upper secondary education in Europe and beyond. On top of the apparent information this gives on the institutionalization of a discipline beyond academia, this data can also be used to contextualize student numbers, the development of university curricula etc. Also conclusions on the status and the ‘public understanding’ of a discipline might be drawn from this data.

Variables/Descriptor
1) Report on inclusion of disciplines in upper secondary education (ISCED 3)
   a. Yes/No;
   b. target age group or level of secondary education;
   c. status/profile;
      i. intensity of inclusion in curricula (the curricula taken into account should cover at least 50% of students leaving school with the entitlement to study at an institution for higher education/university);
         1. number of years a disciplines is taught;
         2. number of hours/week a discipline is taught;
            a. part of school leaving examination (entitling to tertiary/university education) & whether it is an optional/compulsory part of that exam;
      3. optional: scope/extent of inclusion in curricula;
         ii. arrangement: are disciplines taught in combination with others? (i.e. are there subjects at school that deal with several SSH?);
   d. level of professional qualification necessary for teaching on the upper secondary level; including a report on the organisation of teacher training;
      i. are teachers trained in the same courses as students studying these disciplines not leading to a career in upper secondary teaching?
      ii. do teachers in upper secondary education need training in the disciplines they teach or not?
      iii. other specificities of teacher training.

Data-gathering method
Partners gather data. Mainly for the present (only for the past if there were very significant changes).

Template
None.

Tags for literature
“secondary education”
4 Output

This set of indicators deals with different forms of research output. Next to aspects of institutionalization of a discipline the qualitative work on reputation of different forms of output per discipline and country will give insights into national developments of a discipline, borders of a discipline and rules of disciplinary cultures that are often ignored in rankings of universities and/or researchers. Finally, also communication with and for a larger public, the roles as public intellectuals for example, will be dealt with in the context of producing (research related) output.

4.1 Reputation of publication outlets (qual.) (MEDIUM)

Rationale

The aim of this indicator is to describe the publication practices of SSH researchers. The production of (textual) academic output remains the most important measure of reputation in the SSH. It is less obvious, however, what forms of output are preferred and most highly renowned in a discipline. The report can and should highlight differences inside a discipline, i.e. concentrating on epistemic cultures, adherents to certain theories, methods etc. Again, the aspect of national/international development/institutionalization of a discipline should be treated here by taking into account the degree of internationalisation of the media used by researchers (linked to later indicators) and the issue of translation. All this should be presented in form of a qualitative report.

Variables/Descriptor

Report answering the following questions:

1) What types (monographs, journal articles, co-authored texts etc.) of publications have the highest reputation in a discipline (timespan: today)?
   a) Is there a strong bias towards a certain type of publication or not? Are there dominant publishing practices?
   b) Are there important exceptions to these ‘rules’? (i.e. is there a psychologist that publishes books and is renowned?)
   c) What are the normative expectations to young researchers that aim at a research career?
   d) What are the most highly renowned national and international journals/publishers in a discipline?
      i) journals;
      ii) publishing houses.

2) Report on the major changes/events that occurred since 1945.
   a) What were the changes? (language, medium etc.)
   b) Specification of year/timespan of when changes occurred if possible…

Data-gathering method

Interviews. Reports and existing literature?
Data format
Qualitative reports taking into account national peculiarities and table with some basic information for comparative purposes.

Tags for literature
“publication practices”

4.2 Publishing landscape (timespan: today) (HIGH)

Rationale
The aim of this indicator is to provide for an overview of the key actors on the publication market of a country/discipline. The production of academic output is organized on the national and the international level by different kinds of publishers and the size, number and disciplinary affinity towards these publishers differs considerably from one country to the next. The procedures leading to an academic book publication and the general diversity/homogeneity of the publishing market can provide for valuable information concerning the organisation and reputation of different kinds of publishing in a country/discipline.

Variables/Descriptor
Report (+ table/list) answering the following questions:
1) How does the publishing landscape on the national level look like?
   a) Who are the main actors (publishing houses, journals, book series, editors…)
   b) Are there university presses? (what is their reputation)
   c) What are the differences between trade and academic publishing?
2) What is the procedure leading to an (academic) book publication?
   a) Are there review processes? How do these look like? Who is responsible for these?
   b) Who is paying for the publications? Are there subventions?
   c) Who is taking the (financial) risk? (publisher, editor, author…)

Data-gathering method
Interviews. Reports and existing literature?

Data format
Qualitative reports taking into account national peculiarities + table with some basic information for comparative purposes.

Tags for literature
“publication practices”
4.3 Academic journals (timespan: 1945-2015) (HIGH)

Rationale

This indicator aims at showing when journals were created, when (how?) they internationalized, when thematic journals were introduced in the different disciplines and how the prestige of journals changed over time. The indicator also deals with existing categorizations/rankings of journals in the different disciplines and countries. The data of this indicator will be combined with the data on organisational institutionalization and the creation of degrees/curricula to form a general indicator of academic institutionalization.

Also, this data will provide information on changes in the publication practices and the availability of knowledge through open-access, the concentration and specialization of disciplines through thematic journals and special issues and finally, the internationalization of disciplines.

Variables/Descriptor

1) List of nationally rooted journals + profile table
   a. Date of creation / End year;
   b. Profile of the journal:
      i. thematic/general;
      ii. national/International (link to WP3; international is defined as having editors from different countries);
      iii. review practices;
      iv. periodicity (monthly, bi-annual, annual…);
      v. circulation numbers;
      vi. ISSN;
      vii. open access (see for example http://www.wiso-net.de/download/SOLI.pdf).

2) Analysis of internationalization of national journals (based on previous work done by Thibaud Boncourt)
   a. Selection of journal samples based on list of nationally rooted journals
      i. affiliation of editors;
      ii. affiliation of authors;
   b. Analysis of a five-year period (x2)
      i. content analysis of articles (if possible);
      ii. background interviews with editors and past editors on the development of their journal (to be specified).

3) Report on rankings/ratings of journals in disciplines/countries:
   a. List of national/international systems of journal rankings/ratings;
   b. rules/methodology used for ranking/rating.

For rankings of business journals see: http://www.harzing.com/download/jql_subject.pdf → a A+ category of “world elite” journals seems to make sense next to normal high-quality journals); for a humanities ranking based on expert advice see http://www.esf.org/index.php?id=4813.
Data-gathering method
Team Austria provides a list of journals. Partners are responsible to cross-check whether these lists are correct (i.e. is the categorization of disciplines correct, is the date of creation correct, are there no important journals – especially historic perspective – missing?
For international journals: Basis will again be the Ulrichs database. Partners will receive list and check this list for accuracy.

Templates
None

Tags for literature in Zotero and files in workspace
“journals”
4.4 Content of journals (LOW)

**Rationale**
This indicator captures thematic trends in (major) academic journals. The goal of this indicator is to develop a methodology to capture thematic trends in the SSH and to contextualize historical trends with other developments. By using national journals of “general nature” (i.e. British Journal of Sociology) it will be possible to make statements about trendsetters, followers, interdisciplinary salient topics etc. Furthermore, a contextualisation with public debates in newspapers or thematic priority programmes like the EU framework programmes would be interesting.

**Variables/Descriptor**
1) List of titles of major (general) journals from 1945-2015
   a. list of abstracts of these journals (according to availability);
   b. articles (PDF) where available.
2) Journal profile from indicator 4.3.

**Data-gathering method**
The journals will be selected following the lists of journals provided by the partners/coordinators for indicator “4.3 Academic journals (timespan: 1945-2015)”. Following this informed selection the coordinators of the work package establish a database of abstracts and articles. Where access is restricted the help of the partners will be sought to gather this data. The analysis will be restricted to articles of scientific nature, i.e. essays, scientific reports and full academic articles. This would exclude any reviews or conference reports.

Data analysis: The data will be analysed quantitatively either using some form of automated quantitative text analysis (wordscores etc.) and/or thematic coding of topics.

Possible databases are IBSS, JSTOR, UNESCO, Sociological Abstracts (etc.).

**Templates**
None.

**Tags for literature in Zotero and files in workspace**
“journals”

4.5 Handbooks (MEDIUM)

**Rationale**
This indicator aims at showing when the first handbooks of a discipline were published, what these were and when there were phases of expansion. Handbooks are defined as publications aiming at providing an overview over a field of study. In contrast to textbooks,
The early handbooks of a discipline will provide information on the process of institutionalization of a discipline by showing the disciplinary background of the authors and the institutions they were adhering to. At later stages the process of internationalization of disciplines can be highlighted by considering the background of the authors on the one hand and whether handbooks are translated from other languages or not.

The last step of this analysis will be a report on the most widespread international handbooks used in a discipline in a country. This report will make use of union library catalogues to track these handbooks and deal with the degree of internationalisation of a discipline but also of local university courses etc.

**Variables/Descriptor**

1) List of general handbooks of a discipline  
   a. date of publication;  
   b. publisher;  
   c. authors/editors;  
   d. academic affiliation of authors;  
      i. country;  
      ii. university;  

Conclude from the information of point 1.d) whether a handbook is international or national.

2) Did, and if yes, at what point did the number of handbooks increase considerably?  
   a. Are there major increases in handbook publications during, after phases of educational expansion? (e.g. after the introduction of new curricula)

3) List of translated handbooks (if applicable)  
   a. list of translated handbooks (incl. information as above);  
   b. source language;  
   c. translator;  
   d. publisher;  
   e. date of publication.

4) Report on the most used international handbooks in a discipline in a country (LOW)  
   a. use union library catalogues (i.e. worldcat.org for individual countries);  
   b. number of (university) libraries holding the handbooks;  
   c. recording date into database/library catalogue (if available) [will be compared to the number of universities hosting a discipline].

**Data-gathering method**

In a first step the partners only provide for the information of points 1-3. Point 4 will only be relevant at a later stage (LOW). Include national and international handbooks in the list. If the number of handbooks exceed 10 per discipline please contact the coordinators.
Templates
A document with examples will be provided.

Tags for literature in Zotero and files in workspace
“publication practices”

4.6 Non-academic output (science to public) (MEDIUM)

Rationale
This indicator deals with the publication practices with regard to broader intellectual and cultural journals, newspapers and other media. It identifies (major) arenas of public communication and the most important figures of a discipline present in these arenas. The identification of arenas for this indicator will concentrate on television programmes on the one hand, and arenas for commentaries in major national newspapers on the other hand. The third measure that will take into account the disciplines as a whole are popular book publications of SSH researchers that are listed in bestseller lists. The second part of this indicator will concentrate on samples of 1-3 researchers out of the top 10% of researchers of a discipline and track the science to public communication chosen by these researchers. In combination this should provide for a rather comprehensive picture of the arenas of communication between SSH research and society.

Variables/Descriptor
1) Qualitative report on the arenas for commentaries on research/current issues in major national newspapers by SSH researchers active in the academic sphere (timeframe: now)
2) Content analysis of two/three daily newspapers preceding the European Parliamentary elections taking place in May 2014.
   a. selection of two/three daily quality newspapers (criteria to be defined);
   b. gather contributions made by SSH researchers;
      i. discipline;
      ii. institution;
      iii. gender;
      iv. topic;
      v. research/non-research relevancy of article.
   c. report on these including an explanation/assessment of the sample of newspapers made for the content-analysis.

Data-gathering method
Literature on the topic. Research by partners.

Template
Detailed instructions for the content analysis will be provided by the coordinators of WP2.

Tags for literature
“Science2Public”
5 People (tertiary education & research)

5.1 Personnel (HIGH)

Rationale

This indicator captures the number of professors/tenured research staff per discipline, the proportion of women and foreign professors/tenured research staff and numbers of other research and administrative staff per disciplines since 1945. It also includes typical career-paths and changes in these since 1945. The aim is to gather as much existing data as possible and, where necessary, highlight deficiencies in centralized datasets available for research and the evaluation of research.

These numbers will be compared to developments of student numbers/graduates/doctoral students etc.

Variables/Descriptor

Tables (HIGH)

1) Number of professors (head count) per disciplines since 1945
   a) % of women;
   b) % of foreigners;
   c) if available also add numbers of full-time equivalent professors.

2) Number/percentage of tenured faculty members per discipline
   a) % of women;
   b) % of foreigners.

3) Number of other personnel, administrative staff if easily available.

Report/flow-diagram (MEDIUM)

4) Typical career path(s) to reach tenure/full professorship:
   a) See LERU hyperlink for general university career paths-diagrams for: Belgium (Flanders), Finland, France, Germany, Italy, The Netherlands, Sweden, Switzerland, United Kingdom (England) http://www.leru.org/index.php/public/extra/careermapseurope/ [Comment: Argentina/Hungary are missing]
      i) make these kind of graphs on the level disciplines taking into account more peculiarities where necessary;
      ii) make these kind of graphs for 1950, 1970 and 2010 (now)
         (1) Highlight the most important changes in a comment;
         (2) If there are only very minor changes only add a comment to the initial graph.
   b) average age of staff when reaching tenure (5/10 year steps if available);
   c) average age of students when reaching PhD (5/10 year steps if available).
Data-gathering method
Research by partners.
Concerning the report/flow-diagrams closer cooperation with the coordinators of the work package is necessary.

Templates
Table. Flow diagram (low priority).

Tags for literature
“personnel”

5.2 Social background of professors/research staff (MEDIUM)

Rationale
This indicator captures literature and existing surveys on the social background of professors and other academic personnel in general and provides a table of data available in census statistics. Some of the data from these existing studies will be used for the contextualisation of the data on professors. The partners should try to find social surveys amongst professors/university staff/researchers save the datasets to the project intranet where possible and make a short report on the types of variables found in the datasets with special consideration of the variables listed below. Also, the lack of more comprehensive studies of this kind in Europe should be highlighted for different national contexts.

Variables/Descriptor
1) List of social surveys amongst professors/researchers at the national level.
   a) Examples for the USA would be:
      i) HERI Faculty Survey (UCLA, CIRP Freshman), 1989-2013, 3-year intervals (n 20000 – 25000); http://www.heri.ucla.edu [Comment: includes a lot of information on teaching practices etc. that would not be of interest here]
      ii) National Study of Postsecondary Faculty (NCES), 1984-2004 (n 20000 – 350000); https://nces.ed.gov/surveys/nsopf/
   b) Examples for Europe would be:
      i) EUROAC: http://euroac.ffri.hr/en/?page_id=17
      ii) Hochschullehrerstudie (German): http://www.uni-kassel.de/wz1/pdf/BMBF_Hochschullehrerstudie2011_Druck.pdf
   c) Interesting variables:
      i) occupation of parents;
      ii) education of parents;
      iii) migration of professors/parents;
      iv) religious affiliation;
      v) career path/mobility;
      vi) sources of income: primary/secondary employment.
2) list of literature using these surveys/analysing their data;
3) short report of the contents of these surveys.
The aim is to make a repository of these (national) reports and try to find relevant data that could be compared between disciplines and countries.

**Data-gathering method**
Research by partners.

**Templates**
None. A folder in the intranet will be provided to save reports, (available) datasets and literature.

**Tags for literature**
“personnel”

### 5.3 Student numbers (in tertiary education) (HIGH)

**Rationale**
The aim of this indicator is to show the evolution of student numbers over time in the different disciplines at a national level. The tables should include numbers of enrolled students, graduates and freshers where possible. The number of graduates should be differentiated between BA, MA and PhD level (at least since Bologna). To explain trends a contextualisation of the graphs with institutional changes like for example the creation of new curricula or even larger reforms like the Bologna process.

**Variables/Descriptor**
1) Table of student numbers per discipline and level – according to Bologna classification (BA/MA/PhD) (1945-present; timespan as available from national statistics)
   - For all levels/numbers:
     - % women (for all levels/numbers);
     - % of foreigners (for all levels/numbers).
   a) Undergraduate students (stock value);
   b) Graduate students (stock value);
   c) PhD students (stock value);
   d) number of graduates (flow value);
   e) number of freshers (flow value);
   f) gross enrolment ratio (see UNESCO definition)
   g) number of students enrolled.

**Data-gathering method**
National statistics.

**Data format**
Tables.

**Tags for literature/files in Zotero & online workspace**
“students”
5.4 Job market (LOW)

Rationale
This indicator captures the share of students recruited in (academic) research and can/could be combined with data from indicator 5.2. By taking into account larger datasets like for example census data a broader picture of the job market for students of certain disciplines should be given and possibly also changes that occurred over time should be highlighted. Furthermore, this should also show how the attractiveness of academic professions changed over time.

Variables/Descriptor
1) Cross-tables of people classified as part of a discipline according to ISCO and economic sector (as detailed as possible)
   a. % women;
   b. % born in another country.
2) What is the share of students from the disciplines recruited in research or higher education?
3) What are other professional paths taken by PhDs?
   a. Use census data, ISCO classification, IPUMS database (by coordinators) – make cross-tables of people with degrees in certain disciplines and work sectors.

Data-gathering method
Step 1: centrally gathered data by coordinators and Paris team through Eurostat and OECD statistics as well as the IPUMS database for census data comparisons. (LOW)
Step 2: partners contact national organisations that could provide for missing data. (LOW)

Templates
Data details for contacting national and European statistical associations.

Tags for literature
“personnel”

6 Public and academic evaluation and distinction

6.1 Prizes, recognition and excellence (MEDIUM)

Rationale
This indicator will report on mechanisms of evaluation and distinction for research and researchers of different disciplines in different countries. The list of awards will comprise its date of creation and the type/height of reward attached to it. For a sample of the most important awards the winners will also be listed. The second part will consist in reporting on practices concerning honorary degrees in every country and discipline. More important than
the actual number of honorary professors will be a description of the procedure leading to such an honorary degree. The third part is a report on “further” forms of evaluation/rankings etc. that exist on the national level and that help structure the hierarchy inside and in-between disciplines. Finally, the functioning of academies of science and their membership rules will be examined.

Variables/Descriptor

1) List of prizes, awards etc. for SSH researchers (and institutions?)
   a. Awards for life work (i.e. awarding the person not a specific project etc.)
      i. date created;
      ii. amount monetary reward.
   b. Awards funding research more specifically
   c. Categorization of awards according to “visibility”
      i. national (is the prize only awarded to members of the national scientific field);
      ii. international (is the prize also awarded to international members of the scientific field).
   d. List of award winners (only for a sample of the most important prizes of a discipline in a country)
   e. Link to award websites

2) Report on practices concerning the award of honorary degrees per discipline
   a. Number of honorary professors;
   b. Procedure leading to honorary degree (who decides on the award of the degree, who awards the degree etc.).

3) Report on other forms of evaluation/ranking existing on the national level that (help) structure the academic landscape of SSH disciplines
   a. ‘excellence’ vs. rest?
   b. Ranking of institutions inside a country (e.g. newspaper rankings, excellence funding schemes, etc.)

4) Functioning of academies of science
   a. % SSH members in ordinary/full-members;
   b. Report on functioning (how are members elected, what are the main tasks of the academies).

Data-gathering method

Partners gather data for the national sphere. Only very important prizes/awards are to be taken into account, i.e. not prizes for “best dissertation” at a certain University or by a professional association. Coordinators gather data for international sphere. Use interviews and METRIS reports for most important prizes.

Templates

File with examples of prizes that are relevant.

Tags for literature

“distinction”
7 Funding

7.1 Funding schemes for people and projects (timespan: today) (MEDIUM)

Rationale
The indicator captures funding schemes for university departments, (young) researchers and research projects running on the national scale. Based on existing reports like the METRIS reports the importance and availability of the different types of funding (public, private, mixed) that are available in different disciplines should be described. The underlying theoretical assumption is again that a change in the organisation of research has been taken place and that these changes have also affected the sources of funding available for research and doctoral/post-doctoral education.

Variables/Descriptor

1) Report on funding schemes for university departments
   a. Unit of funding: (i.e. school, department, faculty, professor (Lehrstuhl))? 
   b. public vs private; 
   c. secure vs call-based; 
   d. role of public procurement; 
   e. role of evaluations of the institutions on (public) funding; 
   f. role of teaching v research in funding research institutions.

2) Report of funding schemes for doctoral students (per discipline) 
   [Comment: not a list of funding institutions/programmes]
   a. Types of funding; 
   b. sources of funding (public/private/national/international etc.).

3) Report on (third-party) project funding
   a. Public v private; 
   b. targeted funding schemes i.e. funding programmes with calls on specific thematic areas such as family, education, poverty etc.; 
   c. functioning of third party funding for basic research of the most important national funding organism (like the NSF) using the case of a standard two year project with two full-time PhD positions:
      i. Type of programmes; 
      ii. functioning of application procedure; 
      iii. functioning of decision-making procedure (who decides and on what basis).

4) Report taking into account the major events that changed the funding schemes for research on the national level.

Data-gathering method
Use METRIS data. Contact EURAXESS for doctoral/post-doc researchers.
Templates
None.

Tags for literature
“funding”

7.2 Remuneration (MEDIUM)

Rationale
This indicator captures data on the remuneration of academics (professors/researchers) in the different disciplines over time and compares these numbers average/mean incomes and the income of other leading positions in public services. The data on remuneration will not necessarily be gathered first hand, but can also consist of tables/data found in previously published reports (like for example the German Hochschullehrerstudie). Whereas data on the minimum earnings of professors/researchers should be available in many countries, information on the actual income and other/additional sources of incomes of researchers/professors will probably only be available through existing studies.

At a later stage of the project the remuneration of academics will be compared to the income of other public service officials of high rank like federal ministers or the highest civil servant in central administration.


Variables/Descriptor
1) Statistics on remuneration of academics from 1945 to the present
   a. Professors (minimum 1950, 1970, 2010);
   b. other researchers;
   c. (adapt to national groups/data available).
2) Table with comparative data
   a. Average/mean income (OECD data);
   b. income of federal ministers (minimum 1950, 1970, 2010);
   c. income of highest civil servant in central administration (e.g. ministry) (minimum 1950, 1970, 2010).

Data-gathering method
Table of average/mean income provided by coordinators. Data for professors, researchers (if available), ministers and civil servants is to be gathered by project partners.

Template
None.

Tags for literature
“remuneration”

Rationale
This indicator captures national research and development expenditures in general (i.e. not only in the social sciences and humanities) and for the social sciences and humanities in particular. For the current situation (2010-2014) it uses the reports published by the METRIS project and the references made therein. The data of this indicator will offer a broader contextualisation of the SSH specific data gathered in the other parts of the project. It will be used to form relative numbers that can then be compared between countries and disciplines. On top of that specific data about EU funding for SSH research per discipline will be analysed based on the numbers available through the European Union E-CORDA database.

Variables/Descriptor
1) OECD indicators for research and development (not SSH specific)
   a. Gross Domestic Expenditures on R&D (GERD);
   b. total R&D Personnel;
   c. women researchers;
   d. total researchers.
2) METRIS indicators on funding in the SSH
3) Table with amount of EU funding for SSH research per discipline (by university/departmental affiliation):
   a. Use E-CORDA database (get access through European Commission):
      i. Contact persons of funded projects + project details + affiliation etc.;
      ii. contact persons of non-funded projects + affiliation etc.;
      iii. call-ID of funded project/project proposals.

Data-gathering method
OECD data, E-CORDA data gathered by coordinators. OECD will be asked to disaggregate the data where possible to get specific data on the SSH where it can not be extracted from the METRIS reports.
Specific funding for SSH in the national context should be based on the METRIS reports. Some of that data will be extracted by the partners. METRIS will be contacted by the coordinators to receive access to data in tabular format instead of PDF files.

Templates
None.

Tags for literature
“funding”
8 Misc (LOW)

This last section draws attention to two further dimensions that should remain salient throughout the data-gathering process and be reported to the work package coordinators. First, any structural setting that help elites, or those already doing well, should be highlighted and existing literature included in the online repositories. Second, information on recruitment strategies gathered that could, for example, also be relevant to understand internationalization processes.

1) Are there structural settings that help those that are already doing well (Matthew effect) or are there structures supporting newcomers/change
   a) literature on the issue; report to coordinators.

2) Recruitment strategies in national universities
   a) i.e. codes of conduct; EU code:
      http://ec.europa.eu/euraxess/index.cfm/rights/codeOfConduct
      b) literature on the issue; report to coordinators

3) The academic field of SSH and their elites: Elites and counter-elites since 1945 based on:
   a) editorships of major disciplinary journals;
   b) memberships in national academies of science;
   c) membership in national encyclopaedias; biographical dictionaries etc.;
   d) counter elites (e.g. in Hungary or Argentina).