

KNOWLEDGE FOR WHOM?

PUBLIC SOCIOLOGY
IN THE MAKING

EDITED BY

CHRISTIAN FLECK
AND ANDREAS HESS

KNOWLEDGE FOR WHOM?

What's the point of sociology? Sociology has long been the most self-questioning of disciplines. Recently, such self-interrogation has involved discussion about what kinds of audiences sociology reaches and should be reaching. This book constitutes a vast leap forward in the "public sociology" debates, enriching them with historical depth, philosophical sophistication, and a truly global vision. An exceptionally attention-grabbing contribution.

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Public Intellectuals and the Sociology of Knowledge

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The sociology of knowledge has a long and distinctive history. Its function has always been that of attempting to bridge the aspirations of the discursive and institutional founding fathers of sociology with that of modern attempts to define the discipline through the study of the emergence, role and social function of ideas. However, since Mannheim first outlined his program in the 1920s, the sociology of knowledge has undergone many changes. The field has become extremely differentiated and some of its best practitioners now sail under different flags and discuss their work under different headings. This new series charts the progress that has been made in recent times – despite the different labels. Be it intellectual history Cambridge-style, the new sociology of ideas which is now gaining strength in North America, or the more European cultural analysis which is associated with the name of Bourdieu, this series aims at being inclusive while simultaneously striving for sociological insight and excellence. All too often modern attempts in the sociology of knowledge, broadly conceived, have only looked at form while they downplayed or disregarded content, substance of argument or meaning. This series will help to rectify this.

Knowledge for Whom?

Public Sociology in the Making

Edited by

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ASHGATE

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Introduction

Public Sociology in the Making

Christian Fleck and Andreas Hess

Sociology, like some of its disciplinary neighbours, seems to be often regarded by others as an unhappy endeavour. Even some of its practitioners complain occasionally about failed achievements and the lack of acknowledgement. Sociologists just do not, so the complaint goes, get the credit they deserve. Politics and the larger public do not seem to pay attention to sociology's recommendations of how best to solve the pressing social problems of our societies. Occasionally insights from sociology are even regarded to be beyond consideration. Why is it, one may ask, that other scientific disciplines are treated much more favourably? And looking at the tragic-comedy side of things, sociologists do not even seem to be able to recognize themselves in fictional figures as they appear for example in David Lodge's and Malcolm Bradbury's novels. Are we that humourless? Do we not deserve better?

When in May 2012 the Library of Congress awarded former president of Brazil Fernando Henrique Cardoso the John W. Kluge Prize for the Study of Humanity the Executive Officer of the American Sociological Association, Sally T. Hillsman, claimed that "sociologists are constantly making important contributions to society and the selection of Cardoso reinforces the significance of our efforts". What she failed to say was that Michael Burawoy's presidential address "For Public Sociology" presented at the annual meeting of the American Sociological Association made more headlines than the prize winner Cardoso, at least in the sociology community. What was even more remarkable was that Burawoy's speech actually conveyed the very same message, namely that sociology was of use and indeed contributes to society in many ways and on a regular basis. Seldom had an ASA presidential address received so much attention. Ever since Burawoy delivered his public sociology address, the discussion about sociology's role, its functions, impact and broader meaning has not abated (Burawoy 2005). The discussion peaked two years later with a full-length academic publication dedicated to the topic, including a longer version of Burawoy's speech together with responses from more than a dozen prominent American sociologists (Clawson et al. 2007).

In retrospect its success and perhaps its broad appeal at the time may also have obscured some of the 2007 collection's more problematic aspects. For example, it was almost impossible to understand Burawoy and the other discussants' contributions without knowing something about the peculiar American conditions to which most chapters referred, either directly or indirectly. To be fair, most

participants were aware of this limitation, yet, the remarkable thing was that they decided not to do much about it. Rather, the majority of contributors thought it more useful to simply appeal to the sociological community to be more inclusive, cosmopolitan and international. Enlightened attitudes, so the argument went, would, at least in the long run, help to de-provincialize American sociology departments and liberate them from their narrow national perspective and their often all-too narrow specialization. Apart from such well-intended, yet presumably hopelessly ineffective appeals, only a few attempts were made to look beyond American borders.

While Burawoy himself hinted at least at the possibility that his intervention could be seen as appealing only to American circumstances and conditions – implied here is that his discourse could also be interpreted as one that presented itself as if it were a universal problem – this suggestion was, we suggest, never picked up by the respondents. Apparently, the rest of the world was something to be referred to in passing, a complex matter that was better left to linguists, students of comparative politics or social anthropologists than to American sociologists. Globalism, yes – showing some interest in the world, maybe less so!

Burawoy's speeches have been understood, quite correctly, as a call to arms, or to put it in a less bellicose fashion, as an appeal to fellow sociologists to enrich their role set by paying more attention to their broader potential public impact. In contrast to other scholarly communities, sociologists seem to be somewhat unhappy with their own peers and become even unhappier if their attempt to reach out does not receive as much applause as they think it should. Sociological activists scorn fellow sociologists who are satisfied with a restricted reception by their own peers and limited public. To stay with our prominent example, Burawoy's four-fold table of professional, policy, critical and public sociology automatically presupposes that 'true' sociologists must reach out to extra-academic audiences, something that other social scientists would refuse to accept as part of their professional identity. Instead, for the latter such engagement is left to the self-definition of what it means to be a citizen outside the republic of knowledge. Some sociologists would call it a case of under- or better over-developed role crystallization while others might be more prone to subscribe to Erving Goffman's dry remark about some sociologists "who ... combat false consciousness and awake people to their true interests" and who, in any case, will surely have their work cut out for them "because the sleep is very deep". To this, obviously ironic remark, Goffman added that he did not see it as his task "to provide a lullaby but merely to sneak in and watch the way the people snore". It is obvious, that such a detached attitude is miles away from the *weltanschauung* of today's engaged sociologists.

Looking at some of the current debates about public sociology one can get the strong impression that institutional political amnesia seemed to have befallen its advocates and practitioners. For example, while obligatory references are made to the interventions of a Robert S. Lynd, a C. Wright Mills or an Alvin Gouldner, the discussion showed little or no awareness of some of the most important

twentieth-century experiences and related debates. Not one attempt was made to comprehensively contextualize sociological debates about public sociology. This is even odder when we consider that the declared aim of the advocates of public sociology is to reach out beyond the academic milieu and disciplinary boundaries. Granted, the usual suspects, Pierre Bourdieu and Jürgen Habermas for example, were briefly mentioned – we presume mainly because of their attempt to analyse the public and its structures and the role that the reception of enlightened ideas plays in their work. But Burawoy and almost all of the other discussants totally missed out on any serious discussion about the more socially ambiguous and historically complex dimension of the relationship between intellectuals, power and the public sphere from the past. Relevant contributions that discussed the public role of intellectuals in more critical terms, particularly those stemming from the European sociological tradition, were totally ignored. It was as if Max Weber, Raymond Aron, Ralf Dahrendorf and Wolf Lepenies had never existed.

But it was not only the case that any non-American debates were obliterated, the omission of references to any past American debates that reached beyond politically correct left-wing causes must give cause for concern. True, Thorstein Veblen, John Dewey, C. Wright Mills and Lewis A. Coser were seen as being worth a passing remark, however, as significant scholars they were sold short, their names serving only as keyword prompters for the radical public sociology agenda. Others like Talcott Parsons, Edward Shils, Daniel Bell, Jeffrey C. Goldfarb and Jeffrey Alexander, who have all discussed the complex connection between ideas, power and society but who would obviously not be allowed to ride on the left-wing ticket, were equally blanked out, not to mention the numerous other contributions that would fill a small library: about the nature of totalitarianism, the Cold War, imperialism and decolonization and the Fall of the Berlin Wall and how intellectuals (including sociologists) reacted to each of these events or historical constellations. The obliteration of these experiences in a debate about public sociology suggests that not a few of its most prominent advocates not only seemed oblivious to some of the most important twentieth-century debates but also of the history of their own discipline. This does not bode well for any future debate about sociology and its publics.

One may speculate on why the discussion of the nature of public sociology contained no convincing concrete examples or why there was no reference whatsoever to historical and sociological cases that would have helped the interested reader to understand some of the complexities involved. To put it bluntly, the public sociology discussion lacked sociological imagination. It never explained or elaborated on how exactly a sociologist can become a major intellectual or public sociologist; there was nothing or very little about reputation building, nor about the finer points of the sociology of ideas or how men and women of ideas communicate with different publics. No example was given of how sociological discourse has impacted on the functioning of social institutions and local and state governments. The absence of any historical references and the lack of any detailed discussion of the complexities and contradictions involved

made the discussion about public sociology a somewhat sterile and problem-free exercise. To paraphrase Robert K. Merton, it made society appear as if it were a body without an appendix, and it turned sociology, a discipline that claims to study societal relations, into little more than an ambitious yet in the end failed public relations exercise.

Was it a mission impossible? That one could do better than Burawoy and his sympathetic discussants is demonstrated by Robert S. Lynd, author (together with his wife Helen Lynd) of the famous Middletown studies. On the death of C. Wright Mills (one of Michael Burawoy's heroes), Lynd, a colleague of Mills at Columbia, warned the sociological community of the dangers of selling the publicly engaged sociologist short. He argued that it would be a serious mistake to portray C. Wright Mills solely as a sociological muckraker and radical Texan but not pay respect to the serious sociologist and intellectual that he also was. It seemed almost as if colleagues were only perceptive of Mills' political and public interventions, particularly his media appearances, the Cuba book *Listen Yankee!* or his *The Causes of World War III*, but not the subversive and enlightening quality of his many other academic works, such as his essays on the sociology of knowledge or *White Collar*. Indeed, if there is one thing that characterizes the trials and tribulations of C. Wright Mills, it was his attempt to identify the larger tendencies in society – tendencies which he attempted to understand in order to change them. How successful he was in his undertaking is open to debate; however, it would not be unfair to the late Mills to say that he had put the will to change and influence society before the attempt to fully understand it.

From Lynd's warning not to sell Mills short to the contemporary debate about public sociology is but a small step: Indeed, it appears as if the current call for a new public sociology is caught in exactly the same trap that Lynd warned against. There is of course nothing wrong with the attempt at making sociology more relevant by catering to the public's interests or by producing and offering more 'useful' knowledge. However, and this may be the lasting legacy and importance of Lynd's message, we should always bear in mind that 'the cause' should not be allowed to become more important than the explanation.

Indeed, we could argue that Lynd was onto something. Much earlier, actually almost 23 years before Mills' death provoked the comments referred to above, Lynd had given a series of talks at Princeton University, which were later published as *Knowledge for What?: The Place of Social Science in American Culture*. Its author addressed the complications and potential risks that a publically engaged social scientist was facing, particularly when confronted with a systemic crisis (Lynd wrote his book on the eve of the Second World War and at a time when the effects of the Depression could still be felt). Lynd regarded social scientists as trustees who were part of the culture they were studying. Consequentially "(t)he social scientist finds himself caught ... between the rival demands for straight, incisive, and, if need be, radically divergent thinking, and the growingly insistent demand that his thinking should not be subversive" (Lynd [1939] 1986, 7). Lynd concluded that, stuck between the demands of the well-being of a social institution – in the

social scientist's case the university – that appears to be increasingly controlled by special business interests and by ideologies concerning the greater good of society, it had indeed “manifest disadvantages” for the social scientist to put “one’s head into a lion’s mouth to operate on a sore tooth” (ibid.: 8).

Put differently, the social scientist is found in a dilemma. Knowledge, morals and interests appeared to be connected. But how exactly? Lynd drew attention to the fact that the social scientist had to make sense of that web called culture of which the social scientist was also a part. According to Lynd, the twentieth century social scientist faced an even more challenging task in that this culture was driven by specialization and marked by an extremely sophisticated division of labour. If that was true and if this also applied to academia we were unlikely to get a comprehensive answer by solely looking at one particular discipline, one specific political, social or economic problem. In contrast, Lynd conceived an enlightened social science as one which was aware of disciplinary limitations and one that also attempted to address the common good. For Lynd the answer lay obviously not in an ever-increasing division of labour but in attempting to understand the entire society – an impossible task if you just look at its constituent parts. We must, argued Lynd, break with our specialized habits and attempt instead at being more comprehensive: “Specialization and precise meaning”, he noted, “must continue, for without them science cannot grow. But if human institutions form a continuation of sorts, all parts of which are interacting all the time, and if specialization and the refinement of measurement are not to continue to operate in effect to prompt us to ignore these vital continuities, there is need for an inclusive frame of reference for all the social sciences. Each specialist would then state his problems with reference to the inclusive totality in which they operate. This totality is nothing less than the entire culture” (ibid.: 19). Lynd, so it seems, had a very modern notion of culture. For him, culture was not a specific realm in the way we see for example art and literature but “all things that a group of people inhabiting a common geographical area... do, the way they do things and the ways they think and feel about things, their material tools and their values and symbols” (ibid.). Culture, Lynd argued, was not a separate sphere on top of the political, economic or social spheres, but something that ran through all aspects of life. It is this modern notion of culture that allowed Lynd to see the wood for the trees.

Contemporary advocates of public sociology would be well advised to take Lynd’s insights seriously. As it currently stands, the notion of public sociology appears to be of a rather instrumentalist kind with little or no appeal beyond the discipline. This does not make for good advertising. The attempt to reach out and distribute knowledge seems to resemble the mechanical way in which water is distributed from a water sprinkler. It makes sociology look narcissistic and as being the one discipline which knows ‘the truth’. It presupposes that sociologists have the knowledge but only lack the means or access of distributing it more widely. It makes sociology look desperate, insecure and anxious about its status. It is a discipline that seems to be apparently unaware of its cultural dimensions in the way Lynd talks about it.

It is but a small step from Robert S. Lynd to Lewis A. Coser, a sociologist and radical German-Jewish exile who had not only a good knowledge of the American and European sociological publics but whose own life was also marked by the ideological wars of the twentieth century. In a foreword to the 1986 edition of Lynd's *Knowledge for What?* Coser noted that its author appeared as somebody who had come too early and was therefore punished with obliteration – “the penalty for taking the lead” (ibid.: xii). It should not come as a total surprise to the reader to learn that only a few years before Coser wrote the lines just quoted he himself had tried to map the relationship between intellectuals and publics. Coser's *Men of Ideas: A Sociologist's View* was the attempt to make sense of the plurality of contexts in which intellectuals (and sociologists) operate (Coser 1965). While some of the conditions have obviously changed since he first formulated his ideas, many descriptions still ring true today. Coser observed for example “(u)niversities have been a haven for intellectuals over the last few centuries to the extent that they allowed them to one degree or another to stand apart from the world of everyday affairs. They protected men of ideas from the insistent pressures of the market place and the political arena” (ibid.: xvii). Coser warned also “if the boundaries that in the past separated the world of the university from the world at large are broken down, if the university can no longer provide a shield protecting its members, the life of the mind in America will be in mortal peril”. With special reference to the 1960s student movement he noted critically “a politicised university ... cannot provide the environment in which intellect flowers” (both quotes ibid.: xvii). Much of Coser's comments were indeed directed against a somewhat naïve conceptualization of the relationship that existed between intellectuals and their publics. Coser also alerted fellow sociologists to be careful about what one wishes for: naïvely calling for a new public sociology without taking specific historical and social contexts into account was, in Coser's view, not only careless but could, on occasion, turn out to become a tunnel effect with detrimental consequences for the discipline itself.

Like Lynd, Coser came too early and as we know by now, the ‘punishment’ for this is obliteration. Today *Men of Ideas* is almost forgotten. This is regrettable because Coser was in an almost ideal position to explain to us why the American context differed from that of Europe. In the US, modern universities no longer just catered to an elite but had to attempt to educate a mass of students. With increased size came increased differentiation, leading American universities to fulfil ever more specialized functions. The academization of the intellect was a direct outcome: quite a few intellectuals who before then had operated outside the university environment were now drawn into academia. As Coser puts it, “There are few major university departments in the social sciences or the humanities in which we do not find radicals or ex-radicals who at one time attempted to make livings as unattached intellectuals in the interstices of official universities (ibid.: 267)”. According to Coser, this shift could not only be observed in universities but also in government and the media. In such a situation not all the people dealing with ideas could be identified as fulfilling *per definitionem* the role and function

of intellectuals. The net result was that “today intellectuals may play a role within the university, they may benefit from affiliation with it, but they can no longer be the university” (ibid.: 280).

To be sure, there have been some considerable changes since Coser identified some of the major patterns in modern higher education. The need for extra legitimation *vis-à-vis* the taxpayer has perhaps increased, performance indicators have put on extra pressures, and impact factors now try to measure the output and reception of ideas. But overall Coser’s observation describes drifts that can still be observed today. The compartmentalization of knowledge continues. But it seems now as if modern higher education wants to have its cake and eat it as well. The deeper irony in all of this is that the call for public sociology does not appear to be that far away from what it criticizes. In terms of diagnosis it ironically resembles Veblen’s ‘captains of higher learning’. The difference is only that the captains are in power and steer the ship while the others receive the orders. If there is anything new at all, it is perhaps that the balance has clearly tipped towards more bureaucratization and mindless pseudo-academic exercises. Control and fear have now become major driving factors. Whether a new public sociology that deserves its name can provide protection against such developments remains to be seen.

So far we have argued that some of Mills’, Lynd’s and Coser’s structural arguments, particularly their insights as to the broader context and culture, still provide food for thought. However, evoking Mills, Lynd and Coser, we have also argued for the importance of paying attention to changing contexts. Thus, we do not believe in the return to yesterday; rather, we insist in zeroing in on the many contexts and constellations and their changing meaning.

What changes in particular do we have in mind? First and foremost we would argue one has to take into consideration some radical demographic changes. The worldwide expansion of the universities and the establishing of modern mass universities had consequences both for the professoriate and its standing within the public. The growth of the numbers of professors, students, and the related explosion of the role of scientific papers and scholarly books, has challenged the traditional understanding of the role of the professional academic. The changes happened in several parts of the world but came about in a staggered way. The expansion of the system of higher education started first in the United States, followed by the Soviet Union and its satellites and occurred in Western Europe much later. To some degree it has been spurred by the so called Sputnik Crisis, when the Soviets launched the first Earth satellite in October 1957, very much to the complete surprise of the Western world (the next time the West was caught on the wrong foot was when the Soviet empire imploded). Immediately the American government reacted with an increase in the budget for research and initiatives to expand the country’s university programs.

However, the system of channelling government money into academic research had actually started much earlier as a reaction towards the supposed superiority of a military enemy. America’s World War II involvement was accompanied by an awareness of having been surprised and accordingly the US entered the

war somewhat unprepared. To avoid such a crisis ever happening again the US government increased the federal budget for research, which was distributed mainly via the Department of War and its branches. However, the Army, Air Force and Navy did not only spend huge amounts on militarily relevant research. Nothing could illustrate this better than a footnote of one of the earliest papers of that well-known critic Noam Chomsky: In 1955 he acknowledged the support of the Army's Signal Corps, the Air Force's Office of Scientific Research and Air Research and Development Command, as well as the Navy Office of Naval Research. All of the mentioned and the Eastman Kodak Company gave Chomsky, then affiliated with MIT's Department of Modern Language and the Research Laboratory of Electronics, money for his study "Three Models for the Description of Language". In Chomsky's case the funding by the "military-industrial complex", which President Eisenhower had warned of in his farewell address in 1961, did not silence the author. Rather Chomsky became inclined to expose the beast that had once fed him. Others, less convinced persons might have subordinated themselves more.

Another example of the problematic relationship between academics and their publics took the form of incorporation by former freethinkers usually by means of appointment or promotion, two possible pathways by which independent intellectuals became university professors. From the 1950s onwards writers, who had made their living earlier in their careers by contributing to some magazine or journal, were hired by the newly established universities. Changing places might not have led necessarily to a change in attitudes but the disappearing world of the little magazines definitely closed that channel for the next generation. One of the consequences was that young graduates were no longer obliged to spend some time in jobs outside academia but instead continued to live inside the ivory tower, if only on the ground floor. The new academic cohort did not have to go any more through a stage of life where they had to reach out to a wider, less academically educated audience. Instead, they could just produce texts for readers like themselves.

While a new cohort went through normal academic career paths, some established members of the professoriate followed a different route by starting their career outside the campus, for example as experts in governmental advisory groups. The new differentiation processes led to a re-definition of roles and agendas. Seen from an international perspective, the changes in the higher education sector did not all occur simultaneously. For example, the British university system remained up to the middle of the 1960s unchanged, whereas in the US a new hierarchy of colleges, universities and research universities has been established much earlier, not least due to changed admission policies like the G.I. Bill.

The knowledge production inside academia became affected by what has been called "scientification". Whatever social scientists produced was now evaluated according to what were assumed to be the standards of international scholarship. What the public thought no longer played any important role. The natural sciences increasingly influenced knowledge production, at times parodying the physical sciences to a point of utter absurdity. Carving out big theories lost its appeal and

testing clear-cut hypotheses became standard. Increasing competition between scholars was accompanied by new funding regimes that generally encouraged short-term deliveries of results. Both led to the slicing of the findings into the smallest publishable units, submitted to a growing number of highly specialized scholarly journals. An assault on the learned book was the inevitable result.

In the US, McCarthyism, the hysterical prosecution and expulsion of purported communists caused a climate of apprehension, where professors worried about exposing students to ‘critical’ texts. In Europe, perhaps even more so than in the US, the Cold War reached a peak when the Congress for Cultural Freedom organized public events in which disillusioned ex-communists fought Stalinist expansionism. The irony of it all was that Arthur Koestler and his compatriots did not get much support from university professors who preferred to remain apolitical. Actually, the deployment of atomic bombs at the end of World War II in Hiroshima and Nagasaki mobilized many more physicists and other scientists. In contrast, only a handful of social scientists joined the so-called Pugwash movement, named after a gathering in a small Canadian village, in the aftermath of the Russell-Einstein Manifesto (1955), which called upon scientists to assess the dangers of weapons of mass destruction.

During the 1950s and early 1960s European intellectuals either sided with the Communist Parties or the Congress for Cultural Freedom; in-between the space narrowed. In particular French and Italian intellectuals seemed to have been affected by this civil-war like positioning. Perhaps Raymond Aron’s *L’opium des intellectuels* has been the most outstanding contribution from a sociological viewpoint. The other academics who were unwilling to join the heated debate secured a niche, usually by turning into experts for applied social problems. Their special competence ranged from the sexual behaviour – Alfred Kinsley comes to mind here – to the supposed devastating consequences of the new mass media, associated with the name of Paul Lazarsfeld and his team. Whereas Kinsey filled football stadiums and appeared on the cover of *Time*, researchers of the Lazarsfeld type catered to media networks, the government or local administrations by providing project reports about whatever else clients were asking for.

By way of the cunning reason of history both Kinsey’s and Lazarsfeld’s approaches came to symbolize what would eventually be called “the average American”. It seemed as if proclaiming a distribution of particular sexual practices affected ordinary people the same way as the announcement of voting preferences before an election. Many years before, at the beginning of the twentieth century Georg Jellinek, a legal scholar from Germany, had coined the formula of the “normative power of the factual”. Jellinek thought about it in the context of law and its effects. However, from the 1950s onwards the normative force of the factual increasingly applied to other realms, like the sexual behaviour or media consumption patterns. Visions of the good life became eclipsed by reports on the distribution of habits, preferences, etc. As a consequence the space for critical commentary shrank. Theodor W. Adorno lamented this new conformist mood by proclaiming that “there could be no good life in one that is

false”, a statement which became a slogan for the emerging youth and student movement.

The Sixties saw a huge number of sociologists entering academia, perhaps more so in Europe than in America. In the US the expansion of the higher education sector had started much earlier and had almost come to an end when the cohorts of the disobedient generation entered professional life. The contrast manifested itself in throughout academia and the distinct development of sociology was no exception to this rule. The segregation and isolation of academia was less pronounced in Europe. In the old Continent sociologists got much more attention outside the universities than in the US. Ralf Dahrendorf, Jürgen Habermas, Raymond Aron, Michel Foucault, Pierre Bourdieu and Anthony Giddens, to name just a few, became public intellectuals whereas very few American sociologists managed to reach a wider public. Especially from the 1970s onwards Europe produced more influential thinkers with a sociology background than their counterparts across the Atlantic. A part of American sociology’s current unhappiness seems to be rooted in the noticeable decline of public figures in the discipline. Our suspicion is that some of the broader resonance of European public intellectuals can be explained by access to various media. The European intellectual uses public appearances in newspapers, radio broadcasts and TV talk shows, but remains, despite all this, primarily a writer. This has changed only in recent years. Today members of the chattering class seldom come from an academic background. Authors like Stéphane Hessel with his *Indignez-Vous!* appeal have come to occupy the public spaces formerly populated by authors of an academic background. The irony seems to be that some international convergence seems to have taken place because the same observation could be made with regard to the American Occupy Wall Street movement.

The purpose of this volume is to do what sociologists do best: not drawing up imaginary publics but analysing those publics and concrete contexts and specific meanings that do exist and that are of relevance to our work. We need to know more about the discipline but from a perspective of a public sociology that has no ‘imperialist’ notions or tendencies and that avoids the pitfalls discussed above. We need to get a more rounded picture of how sociological ideas and publics work in different contexts around the world. Of course, this volume cannot cover all aspects, nor can it analyse what is going on in all parts of the world. What it can do, however, is to look at a few examples that highlight some of the tensions and contradictions discussed above.

Our title evokes Lynd’s discussion of the usefulness of knowledge. However, we give it a slightly different direction by asking *Knowledge for Whom?* instead of *Knowledge for What?* The qualification in the subtitle *Public Sociology in the Making* makes clear that we have no quarrel with a new project that favours more productive encounters between academics, ideas and various publics. However, in contrast to Burawoy and other advocates of public sociology we hypothesise that it might be helpful to employ the tools of the sociology of ideas in a wider and deeper sense. We would like to understand public sociology as a delicate undertaking and achievement, full of the contradictions and tensions that Lynd and Coser alerted

us to. More specifically, we are guided by three major questions: (1) How does one become a public sociologist and prominent intellectual in the first place, and can one think about prominent examples and eminent scholars, perhaps by going beyond the traditional sociology of knowledge approach? (2) How complex and complicated do the stories of institutions and professional associations become when they take on a public role or tackle a major social or political problem? (3) How can one investigate the relationship between individual sociologists and intellectuals and their various publics without falling into the traps of uni-linear narratives like that of Burawoy?

Accordingly our book is divided up into three parts. In the first part, “Public Intellectuals and their Afterlives: Biographies, Reputation Building and Academic Disciplines”, Marcel Fournier addresses the question of how difficult is it to write a biography in social sciences by discussing the cases of Durkheim and Mauss. Having written the biographies of both, Fournier was faced with a number of difficulties, which he discusses in greater detail in this chapter: the marginal status of biography in the field of social sciences, the relation between the study of a life and the study of a work, and the theoretical perspective which often supports descriptive presentation of life and work of the authors in question. Fournier also asks whether there is something that can be said about the relative roles the private and the public play in the life of a scholar? What exactly is a work (*oeuvre*) in the social sciences? Are we looking at coherence from beginning to end or should we stress oppositions or contradictions?

Andreas Hess elaborates on the argument about biography and looks at the new sociology of ideas which is trying to distance itself from other attempts that have tried to address the complex relationship between ideas and individual life stories. But how successful is this new approach, both in theory and in practice, when compared to the more traditional sociology of knowledge and the intellectual history approach? Hess argues that the sociology of knowledge, intellectual history and the new sociology of ideas have all tried to find answers to the challenge of finding a plausible way through the complex constellation of social environment, the making of ideas and that intellectual ‘surplus’ that is generated through individual life trajectories. Yet, despite all theoretical sophistication, these attempts have remained somewhat incomplete. This incompleteness, he concludes, is not due to the lack of theoretical awareness or sophistication but can be explained by looking at the complex ways in which individual creativity plays out under often challenging social conditions.

Daniel R. Huebner investigates scholarly publishing projects in the Great Depression, projects which he treats as cases of the economic structuring of knowledge. Huebner has some doubts about previous research results, which documented the impact of economic downturns on scholarly publication, most often by demonstrating the overall decline in books and journals sold and produced during such periods. While such research highlights the large amount of competent scholarship that goes unpublished in times of economic hardship, it had little to say about what impact, if any, downturns have on the content of the works that do

manage to get published under such circumstances. In order to assess whether this claim is actually true, he selects as case studies two series of proposed monographs that were under consideration at American scholarly publishing houses during the Great Depression, the so-called “Payne Fund” studies at Macmillan, and the “Works of George H. Mead” at the University of Chicago Press. Huebner finds that in both cases the order of publication of the series volumes was determined in part by estimates of sales potential and that there was pressure to reduce and reformulate the text of the volumes in order to ensure publication and sales. These decisions, made under especially pressing conditions, affected the subsequent use made of the volumes. In particular, the order and content of the Payne Fund studies had a decisive impact on film censorship debates in the United States and helped popularize social attitude survey methods. Equally, the order and content of the Mead works popularized a particular understanding of his thinking that became influential in the social sciences.

Marcia Cristina Consolim looks at developments in France but does so as a Brazilian sociologist who is interested in the history of European sociology and particularly French sociology. Her chapter aims at contributing to a better understanding of standpoints taken by the *Revue Internationale de Sociologie* in the first 20 years of its existence (1893–1912). The journal aimed both at disseminating the social sciences and legitimizing a certain view of these subjects and their relationship with sociology. Consolim shows that the journal’s principal contributors and editors belonged to two identifiable groups: law and economics on one side and pedagogy and teacher training for secondary schools on the other. Despite the official rhetoric which supported sociology, in practice the emerging discipline and some of its exponents were regarded with suspicion. More specifically, Consolim argues that the struggle between collective psychology and sociology for hegemony explains much of the standpoints the *Revue* took. Despite the ‘organic’ defense of sociology, the work of Gabriel Tarde was used to position the journal against Émile Durkheim and his journal *L’Année Sociologique*.

Jarosław Kiliński discusses Czech and Polish narratives and what they tell us about the construction of sociology’s past. Kiliński points out that the texts that he discusses were actually not written by historians but by sociological theorists without any historical training, and, in one case, a renowned historian of ideas. However, this apparently did not influence the validity of the argument in any significant way. More surprising is perhaps that the narrative structure of all four books under consideration was rather loose; none of them exhibits narrative patterns typical for historical narratives. According to Kiliński, such a development can be explained not only by the growing time distance from the described phenomena, but also by the formation of classical sociology as an autonomous sub-discipline of sociological theory.

The second part of our book deals with the question that any public sociology faces, “Serving the Public or the State?” This part opens with Daniel Gordon who takes a closer look at some of the fundamental tensions that have emerged in universities, especially in the United States since 1945. While recognizing the

often discussed dilemmas of teaching versus research and general education versus specialized education, he focuses on the contradiction between discipline formation on the one hand, and a democratic service ethos that tends to morph over time into consumerism on the other. Gordon offers us some critical thoughts on how the trends described in his piece impact on the discipline of sociology.

With Barbara Hoenig's chapter our attention moves to Europe. While recent global changes in higher education and research evoke differences due to peculiar processes of institutionalization in the different nation-states and a variety of disciplines, not much research has been conducted on its impact in the European context and on sociology as a discipline in particular. Hoenig's concerns are with both the supra-national institutional framework of European science policy and the impact it has on a re-definition of the so-called European Research Area (ERA). Hoenig argues that it is highly likely that we will be faced with new inequalities and processes of monopolization in the European research system.

Sally Shortall argues that in order to understand how evidence is used to inform policy, we must critically reflect on the organizational culture of the civil service and how it differs from the academy. She examines the hierarchical rule-based structure of the civil service, where authority is linked to office. Shortall considers the role of the civil servant as a generalist, who does not have specialist knowledge of his or her policy area, but instead has specialist knowledge of the workings of the civil service and how to minimize uncertainty. Shortall also examines the culture of anonymity in the civil service. Academics who provide evidence to civil servants may have little knowledge of the structure of the civil service or how it differs from their culture. The academic is a specialist whose academic authority comes from questioning normative knowledge and publicly disputing accepted beliefs. Such an approach is anathema to the civil service. She concludes that the difference in values and ideology of the civil service and the academy has implications for how academic research is used to formulate policy and how it positions itself in knowledge power struggles.

From Europe we move to South-East Asia: Albert Tzeng takes a closer look at the emergence and state of the public sociology debate in Taiwan, Hong Kong and Singapore where the idea of 'Public Sociology' has attracted extensive theoretical debates. However, very few empirical surveys exist that look at the actual practices in these countries in a more systematic way. Starting from a critical revision of Burawoy's scheme, Tzeng develops an elaborated template which allows him to look at the targeted audience, epistemological style and the level of engagement. Based on his empirical material Tzeng offers some critical reflections regarding the notions of critical mass, intellectual traditions and political-institutional factors.

From South East Asia we make a big jump across the Pacific Ocean to Latin America. Márcio de Oliveira looks at a chapter in Brazilian sociological history that might not be known outside of Brazil: the UNESCO research about racial relations and the unexpected racism against Poles in Curitiba (Paraná). As Oliveira points out, the history of Brazilian sociology has been very influenced by UNESCO's fight against racism just after the Second World War. In Brazil

this fight culminated in a research program about racial relations, which took place between 1951 and 1952 in the cities of Recife, Salvador, Rio de Janeiro and São Paulo. UNESCO saw Brazil as a country that had a successful model of harmonic racial relations. In this sense, it would be a paradigm for other racial conflictive countries all around the world. Nevertheless, Brazilian history and society disappointed UNESCO's officials because the Brazilian research team had discovered that the model of democratic racial relations – as described by the most famous Brazilian anthropologist, Gilberto Freyre (1900–1987) – was widely overrated. So, after the initial UNESCO research, a new Brazilian team – headed by the most important sociologist of this period, Florestan Fernandes (1920–1995) – engaged in new research about racial relations in Southern Brazil. This area was left out of the first UNESCO research apparently because of the small number, or even total lack of, black people. As it turned out, in the city of Curitiba (capital of the state of Paraná) they were surprised by discovering a new type of racism: racism against white people, particularly those of Polish descent. Oliveira intends to recover the details of this unexpected discovery by taking a closer look at a number of neglected dimensions and by putting the case in the proper context of Brazilian sociological history.

The third part of the book discusses “Individual Intellectuals and their Audiences”. The first case study by Matteo Bortolini deals with the US sociologist Robert Bellah. Bellah started off in the mid-1950s as a specialist on Japanese religion and a general theorist in the sociology of religion, working squarely within the twin frameworks of structural-functionalism and modernization theory. Around 1965, however, he abandoned Parsonian jargon and championed a radical approach to the study of religion, which he termed ‘symbolic realism’. Describing his new stance as a politics of imagination and religion, Bellah wrote that the best guides might not be systematic theorists, but poets and ecstatic aphorists. In the autobiographical introduction to his first collection of essays, *Beyond Belief* (1970), Bellah explained his intellectual shift as the result of a personal *coupure*, born out of disillusionment with American political and cultural life and the influence of a counter-culture. Bortolini intends to complement Bellah's autobiographical explanation by showing that the structural and intellectual roots of ‘symbolic realism’ and its meaning lie also within a disciplinary and interdisciplinary context.

Studying open-editorial pages in two Austrian dailies, Philipp Korom attempts to determine who exactly it is who is doing the talking, who the public is and what the possible motives of each are when it comes to the relations between the two. Korom identifies the authors and their professional roles but he is also interested in establishing a debate about the deeper political, cultural and social meaning of this public deliberation process.

Ragnvald Kalleberg takes a closer look at the roles of academics and the media. Usually dissemination has the function of making specialized knowledge and insight relevant for and understandable to an interested public outside a particular research area. However, on occasion academics also take part in public discourse and contribute with specialized knowledge to democratic

discourse. How exactly is this task understood and practiced nowadays? Is it adequately institutionalized? What are its problems and prospects? Kalleberg focuses particularly on Norwegian academics and uses them as a case study in order to illustrate a more general phenomenon in modern media-dominated contemporary society.

Jonathan Roberge and Thomas Crosbie discuss the changing role of the intellectual as critic and what distinguishes old forms from new forms of intervention in the public sphere. They argue that many discourse communities gather around the thoughts and actions of social movement intellectuals, that is, individuals who are closely identified with the meaning of the community as a whole. However, new media technology has changed the communicative interaction patterns of many of these groups. Social movements have become balkanized and ever-smaller grained communities are the result of this. Skilled critics have taken the place of social movement intellectuals by defining the internal meanings of the group as well as projecting those meanings onto a broader public.

Andrew Abbott's text is an attempt to take stock. What does it mean for a social scientist to reason and to be passionate about the society he or she is a part of? That this is not just something that only American sociologists think about becomes clear once we expand our horizons and take a closer look at how other cultures and societies function and how their respective social scientists have tried to explain them. The University of Atlantis and the work of Barbara Celarent provide an excellent viewpoint to look at this aspect of world sociology in an imaginative fashion.

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PART I

Public Intellectuals and their Afterlives: Biographies, Reputation- Building and Academic Disciplines

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Chapter 1

Biography in the Social Sciences: The Case of Marcel Mauss

Marcel Fournier

The Status of Biography in the Social Sciences

Today very popular when it comes to politicians, writers, artists or even movie stars, biography remains a secondary genre in the social sciences, particularly in a Francophone environment. In the Anglophone world, biographies of pioneers are numerous: Ruth Benedict, Margaret Mead, Edward Sapir, etc.

We owe the only biography of Émile Durkheim to Steven Lukes, a professor of English origin. Things have changed a bit over the past 20 years. First there was the (re) discovery of the life story as a tool and object of research, then as a clinical intervention and a therapy. We also saw a “rehabilitation” of autobiography by some historians of the *Annales* school: Georges Duby and his *Guillaume Le Maréchal*, a knight of the Middle Ages, Jacques Le Goff and his *Saint Louis* and later his *Saint-François d'Assise*. In a similar approach, micro-history has focused on people's ordinary everyday life: Carlo Ginzburg depicted the world of a miller of the sixteenth century in *Le fromage et les vers* (The cheese and the worms) while Alain Corbin rebuilt the world of a Norman shoemaker of the nineteenth century (*Le monde retrouvé de Louis-François Pinagot*).

But concerning the history of our own discipline, biography is still viewed with suspicion. I can see several reasons for this. First of all, a certain Puritanism: in academia, gossip abounds about colleagues here and elsewhere but we do not accept that a book about a scientist, writer or artist, should disclose the details of his private life. It is always assumed that by making private information public, the greatness of the works would be undermined. Who wants to know that Marx had mistresses, that Max Weber had mental problems, that Durkheim was suffering from neurasthenia, that Beethoven had suffered from syphilis, that Althusser killed his wife or even that Foucault frequented gay bars in San Francisco? The temptation of sensationalism is present now more than ever: insatiable publishers and readers wanting more of it. On several occasions, researchers, mostly American, asked me intrusive questions about Marcel Mauss: his close relationship with his friend Henri Hubert, his relationships with women, the role of his mother, etc. ... Mauss married late, at 60, a few years after the death of his mother, a strong woman.

Secondly, the opposition that lies between science and literature. In his magisterial study on *Les trois cultures* (*Die drei Kulturen: Soziologie zwischen*

Literatur und Wissenschaft, 1985), Wolf Lepenies notes that sociology stands at the confluence of two quite different modes of thinking and writing: science and literature. Literature and literary thinking irritates and fascinates sociology. It is as if we could not stretch or pretend to the status of science unless we deny the literary aspect of a scientific activity. The biography is certainly the result of a long research and requires the collection of a considerable number of data, but it has, because it is a story (of life), a specifically literary dimension. There is obviously a chronological view; thrillers and mysteries. One not only seeks to convince by force of arguments and rigorous demonstration, but also to move and to communicate this emotion in style. Writing a biography is like writing a novel, or at least the writer is hoping that the reader will read it like a novel, on a beautiful summer day on a beach in Kent.

Thirdly, the opposition that exists between life and work. When the sociologist (it is the same for the philosopher, the economist or the physicist) discusses the history of his discipline, he focuses on the history of ideas, theoretical discussion and the rereading of texts. In the words of Dirk Kaesler, a German sociologist and author of a biography of Max Weber, “a scientist has no biography, he only has a bibliography”. Of course, one can introduce some biographical notes in a book about a particular author, sometimes even an entire chapter, but the reader or even the publisher will wait for one thing: the presentation of the scientific contribution, the ideas of the author in question. My intention was to build upon an educational and theoretical background when writing an intellectual biography, the first of Marcel Mauss. But what really is an intellectual life? Is it the writings? Thoughts? Beliefs? As good a structuralist as Claude Levi-Strauss was more discreet than that when I interviewed him, sending me a clear message that the knowledge of his life would add little to the understanding of his work. I wanted to publish his correspondences – a dozen letters written with Marcel Mauss between 1936 and 1944, from Rio or from New York – but he refused. Was it a defensive reflex or a theoretical position? Would it not be better to let the texts “talk” to each other? The premise of any socio-biography which is in fact the sociology of knowledge appears to be quite different: a text can only be understood in context.

Finally, the main opposition between the individual and the society. Sociologists are rightly wary of what Pierre Bourdieu, who was the director of my doctoral thesis, called “biographical illusion”. Bourdieu himself had warned me against “scavenging”, a trend that can be found in intellectual circles: the biographer is a kind of scavenger, who lives with corpses and finds pleasure in devouring them.

Or some sort of parasite that wants to grow up on the shoulders of the great. Any biography seen as a story has a philosophy of history implicit in the sense of a succession of historical events. But what if life had no meaning, in both senses of significance and direction? The advent of the modern novel, as noted by Alain Robbe-Grillet, smashed to pieces this vision of history and the history of life: “The reality is discontinuous, consisting of juxtaposed elements, each of which is

singular, and more elusive they arise so constantly unexpected, irrelevant, random”.¹ The sociologist is more than willing to recognize the contingent nature of social action, of any social life, and he knows the importance of structural and cyclical factors. One can easily identify the determinants that have influenced Mauss: his rabbi grandfather, a son of a family of merchants and small manufacturers, a child of Epinal in the Vosges, a nephew of Émile Durkheim (Durkheim was born in 1858 and Mauss in 1872, only 14 years apart). Mauss etymologically means “seller of books”. Jewish, provincial, and “petit-bourgeois” (petty-bourgeois): this is a position, in Sartre’s sense of the term, and a mentality that exerted a deep influence on the young Mauss. The challenge for the socio-biographer, is to reconstruct the context, the “social surface” on which the individual acts in a plurality of sectors or fields, at every moment. The use of an *prosopographic* approach and to a structural perspective analysis, for example, in terms of scope and structure of social relations, will not only help to define the terms that reflect the social positions and trajectories, but also to identify margins involved and the opportunities for innovation within the system. I have developed since the beginning of my academic career a research program that emphasizes a historical approach, which is in the fields of sociology of science, sociology of the university system and the sociology of intellectuals. My book on Marcel Mauss includes, to a large extent, collective biography: the presentation of the team members of *L’Année Sociologique*, a study of institutions of higher academic education, a development analysis of scientific disciplines (such as anthropology, sociology, history of religion, economy politics, etc.). This is even more true for the book that I have written on Durkheim and the French sociological school, whose title could have been Durkheim, Mauss and Co.

The Difficulty of the Task when it Comes to Writing a Biography of Marcel Mauss (or Émile Durkheim)

The problems are numerous. I will mention only two: the first one, broadly, refers to the (implicit) theory of action, while the second one, with a more methodological approach, refers to questions of sources and data interpretation. First problem encountered: Who is Mauss? An individual or a character? A free thinker, without ties, or a social character crushed by the weight of determinism? Is the main character of the story rational or non-rational? Is he guided by his interests or by his passions? Any biography contains an implicit theory of action. The natural tendency for the author of a biography is, as noted by historian Giovanni Levi, to draw upon a model that combines an ordered time sequence, a consistent and a steady personality, actions without inertia and decisions without uncertainties. But life is not so simple, and individuals far more complex to understand. There are

1 Robbe-Grillet, A. 1984. “Le Miroir qui revient”, in P. Bordieu (1986), *L’illusion biographique*, *Actes de la recherche en sciences sociales*, 62–3: 70.

inconsistencies, contradictions, moments of indecision ... prevarications are often more numerous than the decisions themselves: we postpone until tomorrow, we deliberate, we expect time will solve the problem, etc. Things do happen and we do not know why, but we do not take decisions. Habitus and cultural backgrounds are at play.

Between habitus and circumstance, how much room there is for the actor? Very little, and often no more than as a margin of error. One does not act as one should. While he was at it, Marcel Mauss kept going; he has researched and written hundreds of articles. His life, like any, is characterized by uncertainty and prevarication: remaining single, teaching at the *École Pratique des Hautes Études*, being a member in good standing of the Socialist Party or staying in Paris during the Second World War, etc. Mauss had surely “nice reasons” not to do this or that, but nice reasons are often rationalizations, that is to say ways of presenting as rational actions things that are not. Life is destiny. Mauss was in line with, though a little against himself, what Bruno Latour calls a “cycle of credibility”: get published, become renowned, get more grants, etc. His friends urged him to stand for a seat at *Collège de France*. Though he kept in mind that this venerable institution gave his teachers a lot of authority, he hoped to find his place in this “asylum of liberty, independence, and pure science”. The fame came on top of it, late in his life. But too late... In 1938, when in Copenhagen, he was invited as a vice-president at an international congress of anthropology; one of his friends, Paul Fauconnet, gently teases him: “You did, without trying, surely apply for fame”. I once felt like painting Mauss as a young man, drawing my inspiration from the *René* of Chateaubriand, known as the first great poet of “primitive civilization”. Mauss did read Chateaubriand. The “*mal de l’infini*” (the correlation of sadness with the feeling of the infinite) which Durkheim speaks of in *Suicide* is truly the world-weariness depicted by Chateaubriand:

I am accused of having fickle tastes, of not enjoying the long same dream, of being addicted to an imagination that is rushing to get to the bottom of my pleasures, as if she was overwhelmed by their duration; I am accused of passing the goal that I can achieve... Alas! I am only seeking unknowns goods, the instinct for which pursues me. Is it my fault if I bump into frontiers everywhere I go? What if that is finite has no value for me?²

René poured into melancholy, thinking about suicide and fled to America; “Blessed are the uncivilized!”, he exclaims. Has Mauss been fascinated by the dandy’s character? His own life was, until his late marriage, that of a bachelor, but can we really talk about Bohemia And what about the painful dialogue between the finite and the infinite? Durkheim criticized his nephew’s “moral unconsciousness” and he feared, indeed, more than any, the “domestic chaos”, deploring in *Suicide* the poor situation of a single man. What was he thinking about when he was writing

2 Chateaubriand, R. (de) 1992 [1805]. *Atala--René*. Paris: Flammarion, 155.

this to Mauss? We know that Mauss, much to the chagrin of his entourage, refused to make patterns and to set limits. Had it not been for the presence of his uncle, what would Mauss have become? Would he, like his younger brother Henry, have followed in the footsteps of his father and mother, or would he have followed his inclination for politics (he was a friend of Jean Jaurès) and tried to get elected as MP? What skills and ability to assign to the actor? The ability to overcome obstacles, to overthrow the determinism and to face the animosity?

By trying to highlight the strength of an individual, we run the risk of getting caught into the trap that awaits any biography, namely hagiography. Mauss pioneer of the humanities, Mauss founding father of modern anthropology, Mauss, another Durkheim but a “better equipped one”. When he presented Mauss’s candidacy at the College de France in 1930, Charles Andler praised his candidate as follows: a workforce of uncommon self-sacrifice, a huge range of mind, a knowledge of several languages, an extensive training as an ethnologist and a competent museographer, etc. As a counterbalance to an overly rationalist or proactive view, we can now try to show where the actor failed. At least early in his career, Mauss was a loser. His family, especially his uncle and his mother despaired of him: laziness, a position in an institution that was, under his mother, a “trap”, an uncompleted thesis, an unmarried status, low pay. Should we talk about failure or success? The whole question is how the small Jewish Mauss from Epinal, Mauss, who passed the aggregation exams without being a regular student at the École normale supérieure, Mauss the nephew of Durkheim in the shade, became the “father of modern anthropology” (Condominas 1972), thus providing a highly original contribution to the development of the humanities. The “Mauss enigma” remains unresolved.

Second problem: the sources, the data. Can we really write the life of an individual? *The Family Idiot* by Jean-Paul Sartre, probably his bedside book, remained with its three volumes, a work in progress. Its original question was: What can you tell from a man today? When he was writing the obituary of the English anthropologist James Frazer, Marcel Mauss formulated the following wish: “A work of art can only be suggestive. The story of a scientist must be true, and we must tell everything”. Tell everything? The life of an individual includes an infinity of facts. The biographer is faced with a series of problems: access to sources, the memory of those who knew him, the many interpretations given to his work. I was lucky enough – I was the first – to be granted access to the Hubert-Mauss archives that the Hubert and Mauss families have donated to the College de France: Thousands of letters, manuscripts, unpublished writings, etc. An almost inexhaustible source! But we can never claim exhaustive coverage. There are, therefore, necessarily forgetful, occasional errors, sometimes even censorship. Thus, any biography is tentative. The achievement of a biographical study requires the collaboration of so-called beneficiaries as they control access and use of family archives. Then it was obvious that before the publication of the biography of Mauss, I would have to submit the manuscript to the Mauss family. This was an implied understanding but who cannot fear the emotional response of the family?

I have not substantially modified my text, although I showed respect for some “touchiness” within the Mauss family: money matters (for example Mauss’ legacy to his brother Henry), various family stories, relationships or love life. The most difficult issue I struggled with during my work is really what happened during the Second World War: For the entire duration of the Occupation, Mauss lived in Paris. His wife was ill. Had he been protected by his connections and his reputation? Former classmates or colleagues occupied administrative functions: Jérôme Carcopino, Max Bonnafous, Hubert Lagardelle or Marcel Deat. Deat and Mauss were closed friends: both *agrégé* of philosophy, Durkheimians and veterans, they had fought together in the socialist movement. At the end of April 1939, Mauss wrote to Deat by sending him a subscription to *La Tribune de France*: “In recent times I have seen very few articles from you that I would not have approved”.³ A troubling confidence. This requires us to revisit Zeev Sternhell’s theses about fascist ideology in France. Marcel Deat will definitely commit himself to fascism in May 1940. A year before, his image of peace still prevails. Marcel Mauss, for his part, remains puzzled: “I am totally unable to predict what will happen... People change, wouldn’t you?” We can say that Mauss has always changed as we may say that he never changed. It is simply a matter of perspective. The biographer is required to determine periods (which are usually parts or chapters of the book: “The nephew”, “The clan totem-taboo”, “The heir” and “the recognition”).

The biographer also feels inclined to identify key or critical moments: for example, for Mauss, the deaths of his father and grandfather in 1896, those of his nephew André Durkheim in 1915 and uncle Émile in 1917, the death of his friend Henri Hubert in 1927. The years after the First World War are, for Mauss, the most fruitful: he teaches at the Institute of Ethnology of Paris (1925), publishes his famous “The Gift” (1925), revives *L’Année Sociologique* and begins the writing of two books, one on Bolshevism and the other one on the nation: Change or stagnation? Mauss’s biographer is faced with the question of the consistency of his life and his work. Is there a young and an old Mauss? The young Mauss felt old and already at an advanced age, he remained an “eternal student”. But we can distinguish some of the changes in his life and thought as turnovers, others as break: revolutionary in his youth, Mauss became more of a reformist as he aged. A cursory reading of his writings suggests that in his early work, he became interested in the ritual dimension of social life and gave more importance to morphological factors, which led him, subsequently, to study mythology and discover the power of the symbolic. But it is not so simple: All his life, Mauss was bound to Durkheim; first as disciple and later as heir. We can establish continuity between his early work on the sacrifice and research on exchange (such as the potlatch). It is possible – and I was tempted – to find a single principle of explanation of his work and life: The Gift (which, in life, becomes self-giving). *Central notion of the work: reciprocity*;

3 Lettre de Marcel Mauss à “Mon cher ami”, 17 avril 1939 (Archives Hubert-Mauss, IMEC-Caën).

central value of life: generosity. So everything turns, in work as in life, around Durkheim's main concern: solidarity.

In some major texts written in the mid-1920s, the scholar and the activist meet and speak as one man. "We must go back to the archaic", exclaims Mauss at the end of his essay on the gift. "We must reinvent the noble morality of spending and find the joy of giving". But there are, in various writings, nuances, inflections and there is, especially, a particular way of living his life and work that does not help a biographer to bring out a unifying principle. "I'm not interested", he says to an American colleague, in developing systematic theories ... I'm just working on my material and if, here or there, there is a valid generalization, I lay it down and turn to something else. My main concern is not to develop a large theoretical scheme that covers the whole field – that is an impossible task – but only to show that in some of the dimensions of the field we have hit margins. We know something here and there and that's all. Having worked this way, my theories are scattered and unsystematic and no one can try to summarize them There are so many things to do that seem more important than to cherish old times and look back.⁴

Mauss has warned his biographer. Even today, he stuns his readers and he manages to outwit his judges, and why not his biographer? The biography still remains the best way to approach Mauss's work as it respects the principle of analysis; he defends himself "... The facts that we are studied are all, that we are, if one may allow the expression, total social facts ... is to say they put in motion in some cases the entire society and institutions ...". All these phenomena are both legal, economic, religious, aesthetic, morphological ... and he concludes by saying that the task of the sociologist or the anthropologist is not to draw abstractions but to "observe what is given and what is given" is Rome, Athens, it is the average Frenchman, and it is the Melanesian of a particular island. The best way to approach Marcel Mauss is by writing, in the words of Georges Duby, his "total biography" and consider him as "a man of society". It closes the circle: the biographer adopts the view that imposes its purpose. The obvious risk is to identify with him. I have already received a fax at the university on behalf of Marcel Mauss that went directly to my office! In conclusion we may wonder, as Nathalie Heinich does about art, what does biography contribute to sociology? I would give a short answer: first of all, a deeper and more substantial understanding of the works, then a greater reflectiveness, albeit on a career as a sociologist but also about life itself. What's in a life? Fate, some say. A gamble, say others. Would it not be just, to borrow a phrase from Marcel Mauss, an "experience"? A very special experience, it must be admitted, because, as the life of societies, it is rarely conducted methodically, turning as it does most often in adventure, with all the *risks* that entails.

4 Entretien de Marcel Mauss avec Earle Edward Eubank, in Dirk Käsler, *Sociological Adventure, Earle Edward Eubank's Visits with European Sociologists*. New Brunswick, Transaction Publishers, 1991, 146.

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Chapter 2

Making Sense of Individual Creativity: An Attempt to Trespass the Academic Boundaries of the Sociology of Ideas and Intellectual History

Andreas Hess

Introduction

The way sociology has tried to understand intellectuals and the way ideas are produced have both been seriously challenged by the arrival of competing programmes, most prominently in the form of intellectual history (Cambridge-style), conceptual history (as promoted and practiced by the German historian Reinhart Koselleck and his colleagues) and by a successful combination of the two, as for example in the work of sociologists of ideas such as Wolf Lepenies or, more lately in a more explicit sociological fashion, Neil Gross. These exercises have all enriched the history of ideas in one way or another. However, what do not exist are any serious attempts that have tried to bring these various approaches into dialogue with each other.¹

As a sociologist interested in the history of ideas and concepts but also in the intellectual trajectory of individual thinkers I would argue that we should make a more serious effort at distinguishing between various ways of thinking about intellectuals (their history, role and social background) and the coinage and politics of intellectual ideas and how they bear or, more often than not, do not bear fruit in the social realm. As Francois Bosse has pointed out, intellectual history and the history of ideas do overlap but they are hardly the same. At present sociology is neither able to identify clearly what the two share nor say exactly how they are to be distinguished. Even worse, sociological approaches do not seem to care what's been discussed in other countries and neighbouring disciplines.² To complicate

1 There is perhaps one exception, that of Anthony Grafton. In a magisterial overview, reprinted in his essay collection *Worlds Made out of Words: Scholarship and Community in the Modern West* (2008), he has tried to make sense of the last 50 years of the ebb and flow, the pros and cons of the various attempts to conceptualise and write the history of ideas.

2 The somewhat isolationist nature of the sociological discussion about public intellectuals seems to be symptomatic in this respect. Even worse, we can encounter now a split between American and European sociological notions of what it means to be an

things even more, while sociology has had a good record in terms of coining, conceptualizing and exporting its ideas, it has been less successful in reincorporating what other disciplines or academic fields have done with sociological ideas.

While the Cambridge School of intellectual history and the German approach to conceptual history have to various degrees been very successful in studying the role of individual intellectuals and the history of ideas, none of that seems to matter to, or is seriously discussed, in sociology. This seems odd, particularly at a time when sociology and the social sciences have failed to develop a strong programme studying intellectuals and ideas. The various attempts of social scientists to take refuge in the study of the ‘discourse’ type (Edward Said or Michel Foucault might serve as examples) show only the conceptual confusion and helplessness that prevails. More often than not ‘conceptual voluntarism’ (or should we say ‘conceptual imperialism’) seems to reign over empirical evidence – even to the point of conceptual manipulation. The warning by C. Wright Mills that theory should help to discipline the empirical wealth and evidence has been mistaken to mean ‘give me a concept or idea and I will show you what I can do to manipulate the facts’ (Mills 1959). As a result, conceptual and ideological rigor is no longer sensitive to facts, and more often than not the former has begun to determine the latter. In an attempt to promote a much stronger programme of intellectual and conceptual history – the attribute ‘strong’ is meant here to symbolise a more solid relationship between theory, concept and empirical evidence – I will argue that any sociology which purports to address the historical dimensions of knowledge production and ideas, would be well advised to return to some ‘irreducible positivism’ and take historical facts, however socially constructed, more seriously – almost as if they had a veto.³

I am not so naïve as to argue for a simple return to Rankean historicism and to the idea of ‘writing history as it really happened’; I am merely arguing for more intellectual rigor and honesty in dealing with historical singularities and facts – something in which intellectual or conceptual history can help to fill some gaps. Sociology’s own attempt to find refuge in discourse analysis, social constructivism or, even worse, banal references to the social environment at large (sometimes also simply referred to as ‘sociologism’), has not helped to advance the field. To complicate matters even more, I maintain additionally that sociology has not been very successful in terms of understanding and explaining individual creativity, that is, the way intellectuals and thinkers have conceptualized and mined ideas

intellectual. While in the US and Canada talk about the ‘public’ in ‘public intellectual’ is emphatically stressed, Europeans have been much more reluctant to use the ‘public’ qualifier when they talk about intellectuals. My suspicion is that major disciplinary historical, sociological and political forces might explain the different notions.

3 I am not at all convinced that Randall Collins’s work fulfils these criteria. I maintain that his work falls under the type of work that was subject to Mills’s earlier critique. It also seems to me that Collins’s sociology of ideas suffers occasionally from illusions of sociological grandeur i.e. the wish to re-establish sociology as the master discipline.

not only for social and political purposes but also in an attempt to make sense of their own lives.⁴

In order to advance my argument, I will take a closer look at the intellectual history of the so-called Cambridge School and another attempt that seems to me to follow a slightly different path to intellectual history but which is in intention quite close to it – that of conceptual history as elaborated by the German historian Reinhart Koselleck. I will finally, by way of a short conclusion, argue that a productive synthesis is already available in the work of Wolf Lepenies and, to a lesser degree, that of Neil Gross. The latter two's publications are particularly interesting since they are the work of sociologists. Actually, the two are very rare examples of an attempt to explore topics that sociologists usually shy away from, such as the relation between individual biography and intellectual creativity. Lepenies in particular has also demonstrated an interest in the unique historical constellations that help intellectuals to push certain ideas. In doing so, individual achievement is never reduced to notions of playing merely an intellectual role. There is never just function, structure and form but also content and meaning. That this is all presented in accessible language and in a style that produces in the end also a readable narrative distinguishes Lepenies from the dry exercise that some intellectual history has become.⁵ However, before I argue for what seems to me a new achievement and something sociology should engage with, I would first like to say a few more critical words about the limits of the traditional sociological approach in studying intellectuals and the history of ideas.

4 In this context it should also be stressed that in their conceptualizations sociologists have always been somewhat prone to generalization and comparison while historians usually dealt with irreducible, proven historical singularities. Max Weber spotted this a long time ago while reflecting about concept formation in sociology in *Economy and Society*. To be sure, since Weber wrote there have been a good number of attempts at mutual understanding between history and sociology, and there have been plenty of attempts to borrow from each other's discipline. However, a century after Weber we also know that the balance sheet remains uneven. There are lots of historians who use, for example, sociological concepts of the Weberian type, but there are actually very few sociologists who take concepts from historians seriously. The current blind eye of sociologists and social scientist *vis-à-vis* intellectual historians and conceptual historians is just an indicator of the imbalance of trade in sociology (lots of export, very little import). This chapter cannot compensate for the lack of contact and perception; what I would like to do here is merely to suggest a few inroads and to identify some points of common interest which sociologists would be well advised to take on board. However, having made the point about imbalance, there are also some things which intellectual history can take on board from sociology. I will come back to this point in my concluding remarks.

5 I wish I could say the same for Gross's study, but those looking for intellectual excitement will hardly find that in his study of the making of Richard Rorty, despite the fact that the book is a great academic achievement. More about Gross can be found in the conclusion of this chapter.

Why the Traditional Sociology of Knowledge Approach Will No Longer Do

Although not the first to make the case for this particular academic branch, sociology of knowledge as a paradigm achieved prominent status mainly through the work of Karl Mannheim, particularly his *Ideology and Utopia* (1929/1985[1936]), now regarded as a modern classic in the social sciences. Mannheim attempted to find out how the very existence of ideas – he distinguished between ideologies and utopias – and the group of people who expressed these ideas could be understood and explained sociologically. In trying to distinguish between ideas and the social group which expressed them he faced a conundrum: how it is possible to say that some ideas are true or meet objective requirements, Mannheim's answer was: by studying (social) being in relation to consciousness and thought. Thus, in the first instance ideologies were conceived of in terms of how a set of ideas gave legitimacy to the established order (36ff, all references refer to the German edition). In contrast, utopias were a set of ideas which questioned, wanted to get rid of or at least radically change, the established order. Mannheim argued that ideologies and utopias were themselves socially constructed and thus reflected the interests of those who promoted them. From this it follows that the world of intellectual spirit was not free from the struggle for recognition and power. The task of Mannheim's sociology of knowledge was to identify the role of institutions, the class relations, the social system etc., which helped to trigger or promote such interests.

No idea could ever be conceived independently of these interests, or so one would think. But with modernity a new social group of thinkers appeared on the horizon – the intellectuals (12). This group was no longer cast-bound as the old type of thinkers once were; instead the new group “floated” freely in the newly developed public realm, be it as academics, philosophers or writers. Even though the politics of ideas of this new social group remained somewhat less affected by immediate class interests, they were not absolutely free in their thought and actions. Mannheim argued that only if intellectuals realised their own socially determined role self-critically and only if they developed an awareness of their own epistemological bias would they be able to be successful in their search for truth and objectivity (41ff). The sociology of knowledge was conceived as that new key (sub)-discipline that would help those thinkers to see the (relative) light of their particular social condition. Since this sociological approach reflected self-critically the social role and condition of thinkers and intellectuals it seemed to have provided a breakthrough by identifying ideologies and utopias as expressions of “not adequate” or “false” consciousness and discover, as it were, a reality gap and, potentially, the “real” meaning behind ideological and utopian thinking (83ff).

Epistemologically this approach proved to be problematic since it presupposed a vantage point so universal that it was almost incomprehensible. Enlightenment was only possible through a ‘dialectical trick’ that consisted of a magical, spiral movement of understanding (91ff). Through constant self-reflection the intellectual could rise above local conditions. How exactly this could possibly work is unfortunately obscured by Mannheim's sociological prose. It reads well, but

logically speaking it's a disaster. In the end, the spiral movement of understanding and discovery functioned rather like the legendary Baron Münchhausen who lifted himself out of the moving sandpit by pulling his own hair.

The problem of not being able to identify an epistemological vantage point – apart of course from the already mentioned magical spiral of self-awareness and critical self-reflection (in case of the famous 'last instance' Mannheim always reserved the word 'dialectical' to bridge the last epistemological gap) – was linked to Mannheim's inherent sociological relativism, which the author never managed to get rid of completely. Once introduced, this sociological relativism constituted a major problem, which runs like a thread through Mannheim's book. While 'normal' intellectual contributions and actions were always explained as being socially rooted or were explained by referring to belonging to a group or to group behaviour, the greatest achievements were seen as intellectual expressions of some kind of existential *Lebensgefühl*. It may come as a surprise to those who look for sociological insights that Nietzsche's *Übermensch* is never far away from Mannheim's analysis of intellectual achievement (Mannheim comes clean about his admiration of Nietzsche only in the penultimate page of *Ideology and Utopia*; before that the philosopher is quoted three times affirmatively by Mannheim but almost in a shy way, as if not to disturb the main flow of the argument.)

As should have become clear by now, Mannheim's sociology of knowledge remains deeply locked into relativism and the few times the sociologist manages to escape such relativism he ends up in Nietzschean assumptions of the superhuman. There is no real notion of individual intellectual contributions; in Mannheim's work they are either sociologized away by referring to interest-bound institutions and groups or to Nietzschean visions of the superhuman. Thus, the very sociology of knowledge which pretended to do more than giving relativist answers and which wanted to enlighten us about the ways intellectuals and their ideas functioned, actually helped to obscure the way we might understand them. It was almost as if modern democracy had levelled individual achievement while a few selected individuals stood above such levelling processes by virtue of expressing intellectually superhuman *Lebensgefühle*. In the end Mannheim's sociology of knowledge could not deliver. Neither did it analyse properly the meanings that intellectuals give to their actions and thoughts, nor could it conceive comprehensively of the way intellectual ideas functioned in the public realm, nor could it see the complex relations between the two. After all, interest was all there was; as to the outstanding rest, there was always Nietzsche.

Sociological Revisions

Two of the most prominent figures, Robert K. Merton and Pierre Bourdieu, have tried to overcome the birth pangs of the sociology of knowledge. While they suggested some innovative ways of thinking about intellectuals and intellectual

ideas, I maintain that they never fully managed to get over some of the earlier sociological relativism.

Robert K. Merton expanded the field by not only making concrete suggestions such as studying publication patterns, age groups and cohorts, links with professional associations or attempts at professionalization, but also by delivering a good number of empirical studies in which he actually managed to show in detail how the complex world of intellectuals and ideas – in his case mainly scientists and scientific ideas and advances – actually functioned (Merton 1968, 1979). For Merton, science had the great advantage that in the course of its history it had become somewhat cumulative although he also pointed out repeatedly that there was no straight ascending line of progress (Merton 1965, 1979). In contrast, ideas in the humanities and in the social sciences followed no clear patterns and one can certainly argue whether any true notion of cumulative progress applies in their case. The crucial distinction lies, as Weber has pointed out and as Merton knew, in the way the humanities and social sciences have to deal with an intervening variable of utmost importance for an explanation of any behaviour, a variable that is absent in the natural sciences – namely the very fact that the individuals who are being studied give meaning to their actions and ideas (Merton 1968, 2006). This makes the study of human action somewhat ‘messy’ and complex and also makes it a real obstacle to notions of progress.

As is known, Merton rejected any notions of working towards a grand theory. For him, it was far too early to conceive of a general theory in the social sciences (or the humanities for that matter). Social systems always had something similar to an appendix, an organ or a part of the body which did not function properly, hence Merton’s criticism of Parsons and his revised and softer version of functionalism, which took dysfunctions more seriously (Merton 1968). Hence also Merton’s many detours and attempts at conceptual refinement be it through the study of serendipity patterns, addressing self-fulfilling prophecies or looking into unintended consequences. What Merton did not manage to get rid of, however, was the assumption that all intellectual thought could be easily submitted to the sociological procedure without ever explaining in detail why some individual and intellectual surplus distinguished itself from some other individual effort. Linked to such relativization of individual intellectual contributions and creativity there was also a strange sense of timelessness in Merton’s work, despite his historical interests and investigations. As Merton’s famous book title reveals, any individual effort and creativity was always regarded as being possible only because it stood ‘on the shoulders of giants’ (Merton 1965). Individual merit looked like any Lego edifice – one plastic brick supported the other; and although individual components could be of different shape or colour, they all were part of the overall construction. Similarly, reading Merton’s work one always gets a sense of unit ideas that were identified and studied in history, never individuals. There was always a sense of progress, however serendipitously it worked. Merton was always keen on progressively ‘moving on’. In variation of the famous Ranke statement ‘that all periods are equally close to God’ one could say that in Merton’s case ‘all

inventions and intellectual contributions were equally close or of equal worth to the sociologist of (scientific) knowledge?.

Merton's preference of the sociology of science actually suggested that there were more fixed and steady points to deal with than in the case of the social world (Merton 1979). In a way, it seemed easier to nail down progress and argumentation in the sciences than it was in the social sciences and humanities. Yet this sense of stability or steady development in Merton can also be used as an argument against him. As Wolf Lepenies has demonstrated in his seminal book on the origins of sociology (Lepenies 1988), sociology was conceived of as a third culture, somewhere situated between the study of literature and letters (humanities) and science (evolutionary approaches in particular). These birthmarks, one could argue, have and will always accompany sociology. The attempt to 'solve' the ambiguities by getting rid either of the natural science aspects or the humanities link, or siding with one side (as in Merton's case with the sciences) disturbs the delicate equilibrium which has enriched sociology as a discipline. The consequence will always be that such repression will result in the return of the repressed side. Merton's narrowing down (a more neutral word would be 'specialization') of the sociology of knowledge to the sociology of science runs into the risks of obliterating the other side of the humanities – a potentially strange fate for a theory that was promoted by somebody whose great achievement has been to shed light on such sociological conundrums as 'obliteration by incorporation'.

After Merton it was Pierre Bourdieu who has been most prominent in describing an alternative route to the trodden path of the traditional sociology of knowledge. Bourdieu's contribution consists not only of introducing new elements and critical ideas to the field; he also conducted field research out of which a good number of publications have emerged, most relevant perhaps for intellectual history and academic knowledge production (but less so for the conceptual history of ideas), *Homo Academicus* (Bourdieu 1990). In this study academics and intellectuals are seen in relation not only to each other but in relation to other actors and fields in society. What Bourdieu's study reveals is that academic and intellectual distinctions are no less about power and power struggle than are other fields and occupations. On first sight Bourdieu appears thus to be a true follower of Mannheim; however, what distinguishes Bourdieu from Mannheim is the emphasis on how cultural capital and its related *habitus* forms work specifically, that is how the accumulation of titles and positions leads to prestige and how prestige helps to create social and cultural hierarchies. For Bourdieu, intellectuals score stronger in terms of cultural hierarchy than in terms of social position.⁶

Maybe because the academic field and the higher education environment turned out to be Bourdieu's 'home turf' he scrutinized it so rigorously by analysing and dissecting those structural conditions that help individuals ('actors' in the language of Bourdieu) to gain intellectual distinction and cultural capital: having

6 This seems to me to be another indirect reference of Bourdieu to Mannheim's conceptualization of intellectuals.

been educated in such institutions as elite schools, research institutes, particular universities, or through occupying distinguished positions or having gained certain titles, certificates or honours or having been appointed to editorial boards of prominent journals or book series. What distinguishes Bourdieu's work from that of his sociological predecessors is that this is all well-researched empirically, almost as if it were an ethnological study of the tribe of intellectuals and academics.

As in Mannheim and Merton, for Bourdieu understanding and conceptualizing the intellectual and academic world is the precondition for doing any serious sociology. However, as in the case of Mannheim and Merton, we encounter in Bourdieu the problem of an analysis that is more interested in form, function and structure than it is in understanding intention, motives, subjective/intersubjective meaning and the role that individual creativity plays. Without deciphering the meaning that intellectuals give to their thoughts and actions the whole scenario of power struggles and distinctions becomes somewhat less convincing. Everything is equal to the analyst of structural power conditions. Focussing entirely on cultural capital and distinction leads to scenarios in which individual contributions are reduced to mere power struggles and interests.⁷ How we can actually conceive of any significant intellectual change or individual creativity in thinking remains unclear.⁸ The lack of meaning also reveals the narrowness of national dimensions; without further reference to similar scenarios or comparisons sociology remains merely national in aspiration.⁹

Be it the sociology of knowledge Mannheim-style, the sociological-historical treatment of mainly scientific advances as practiced by Merton or the sociology of intellectual and academic life as pursued by Bourdieu – all these attempts saw themselves as true *avant-garde* approaches scouting out the direction for understanding knowledge production in general. Yet, since Mannheim, these sociological approaches have prioritized form over content, power over argument and structure and function over meaning. What is problematic is less the fact that form, power, function and structure are analysed but that they are analysed without the other, that is subjective motives and intention, content, argumentation and meaning – in short, anything that resembles individual creativity. While the three authors alluded to above paid lip service to substantive argumentation – Mannheim and Merton perhaps more so than Bourdieu – in the end there is no real depth in terms of being committed to a systematic attempt at understanding subjective meaning and/or individual creativity. It is for this very reason that I

7 See also Jeffrey Alexander's essay "The Failed Synthesis of Pierre Bourdieu" (in his *fin-de-siècle* social theory collection).

8 This, by the way, also applies to Bourdieu's own sociological 'autobiographical' study (Bourdieu 2008). It is an account which is not convincing; it does not help the reader to understand how Bourdieu became 'Bourdieu'.

9 Thus, we may have identified ironically some truth in the common prejudice about French intellectuals (i.e. that they are mainly concerned with themselves).

call this sociological approach of dealing with intellectuals and ideas a 'weak programme'.¹⁰

I cannot see how pursuing such a limited form of sociology can produce any new insights, particularly not when it comes to individual effort and creativity. I maintain that it is a sociological programme that has been exhausted and I doubt whether it can really be resurrected if it does not manage to overcome the sociological relativism which seems to run deep. However, in what follows I will suggest that there may be a way out. If it were the case that a new sociology of intellectual life and ideas developed that truly lived up to its name and that did not just focus solely on form, function, power and structure, and that took on board some radical criticism and accepts other ways of seeing and understanding, it just may pave the way to a more productive encounter with ideas and individual thinkers. Most crucial for such a revamped enterprise would be the integration of the criticism and methods of two schools of thought; one based in the UK (but by no means limited to it) the other one stemming from Germany. Both have emerged since the early 1970s: the so-called Cambridge School of Intellectual History that is associated with such names as John G.A. Pocock, Quentin Skinner and Stefan Collini and the so-called *Begriffsgeschichte*, or conceptual history as coined and practised by a group of scholars mainly associated with the German historian Reinhart Koselleck.

What follows is not intended to be a comprehensive account of the two historical schools. I will discuss the Cambridge School and Koselleck's programme only insofar as they can throw light on the deficits of the traditional sociological ways as criticized above. Also, I do not want to praise intellectual history or conceptual history as being flawless or perfect. There are, as I will argue in my conclusion, also a few things that historians might be able to learn from sociological conceptualizations and ways of thinking. In the conclusion I will hint at such a possibility by suggesting that a synthesis and a model of how to write intellectual history while also outreaching to somewhat broader sociological concerns is already available in the work of Wolf Lepenies and, to a lesser extent, in the work of Neil Gross.

10 In many ways my critique here repeats similar claims made by defenders of the strong programme in cultural sociology *vis-à-vis* other approaches of studying culture(s). However, my distinction here is less related to culture(s) than to intellectuals and ideas. Of course, one can speak of intellectual culture(s) and how culture(s) and the systematic production of ideas are related. There is certainly some overlapping and sharing similar concerns. Yet in my opinion cultural sociology has not reached that level of sophistication in developing conceptual approaches that would allow it to address those questions that are related to the specific historical dimensions of concepts, ideas and individual thinkers.

What Sociologists Can Learn from Intellectual Historians and From Conceptual History

Intellectual history comes in various styles and forms and is not as the label “Cambridge-style” first seems to suggest a closed shop or paradigm. The qualifier “Cambridge-style” refers here only to the place of origin, not at a current address or ‘home’. At present, most historians who work within the paradigm can be found in a number of (mainly) English-speaking countries; for example, Quentin Skinner enjoys his ‘retirement’ at Queen Mary, University of London, Stefan Collini continues to occupy a chair at Cambridge, while John G.A. Pocock has emeritus status at Johns Hopkins but continues to travel widely, addressing academic audiences around the world.

As the label suggests, the main exponents of intellectual history have their common origin and history in Cambridge where they first met and got to know each other. It is hard to come up with a definite year for when the paradigm that we now call intellectual history first made an appearance. It would be much more appropriate to suggest that the origins remain first somewhat tentatively hidden in the work of individual scholars such as Pocock and Skinner before, in the early 1970s, a string of articles and a number of individual monographs emerged that helped to define a paradigm more thoroughly. This paradigm has become even more refined as we approach the turn of the century, and in the last few years the paradigm seems to have reached a peak with multiple scholars and an equally impressive output. For my purposes here I will not deal with the milestone monographs, be it Skinner’s *Modern Foundations of Political Thought*, Pocock’s *Machiavellian Moment* or Collini’s study of British intellectuals in his magnificent *Absent Minds*, but rather with some of the more programmatic and methodological writings, as they have been re-published and are now available in two comprehensive essay collections, Skinner’s *Visions of Politics Vol. I* (2002), subtitled “Regarding Method” and Pocock’s *Political Thought and History: Essays on Theory and Method* (2008).

Quentin Skinner has made some of the most important contributions to the strong programme for intellectual history, a programme which also includes the discussion of major sociological concerns – without actually using the label ‘sociology’. In order to understand Skinner’s strong programme one has to know about his intellectual background. Since his student days Skinner has favoured the philosophy of language and in particular the contributions of Quine, Wittgenstein and Austin. The idea of *parole*, language games and illocutionary speech acts was particularly attractive to Skinner; it made him think more systematically about how rhetoric was used in particular historical settings (Skinner 2002, 2ff). While reflecting about the latter he faced a serious problem: how could one distinguish properly between systematic theory and philosophy on one side and history on the other? The answer was that in order to explain how ideas worked in historical contexts one had to investigate the social contexts and causes which were of concern to individual thinkers. What exactly were their motives? Why did they

insist on certain meanings and concepts? With whom did they argue? What did these concepts mean for the thinker/intellectual himself and for others?

Skinner also found that instead of focussing on “supposed meanings of the terms we use to express them” it turned out to be much more fruitful to ask “what can be done with them and ... examining their relationship to each other and to a broader network of beliefs” (4). What sounds and appears to be almost historicist is immediately relativized by Skinner by stressing that such an approach does have its benefits for the present social world and its concerns:

If we approach the past with a willingness to listen, with a commitment to trying to see things their way, we can hope to prevent ourselves from becoming too readily bewitched. An understanding of the past can help us to appreciate how far the values embodied in our present way of life, and our present way of thinking about those values, reflect a series of choices made at different times between different possible worlds. This awareness can help to liberate us from the grip of any one hegemonial account of those values and how they should be interpreted and understood. Equipped with a broader sense of possibility, we can stand back from the intellectual commitments we have inherited and ask ourselves in a new spirit of enquiry what we should think of them. (Skinner 2002, 6)

The intellectual historian has to avoid certain pitfalls, which used to plague sociological investigations. Particularly the search for unit ideas is always in grave danger of falling into the traps of anachronism and prolepsis. Anachronism is the attribution to a past author of concepts that could not or were not available to him or her. It is a kind of re-projection into past efforts. This is a common flaw in most sociological analyses. While it is a legitimate thing to resurrect and reconstruct past ideas, social scientists interested in the history of sociology and sociological thought should not mistake such efforts as proper intellectual history. Related to anachronism but not identical with it is prolepsis. Prolepsis refers to the treatment of an individual thinker and his ideas as prototypes. Skinner reminds us that both anachronism and prolepsis can create a problematic position not only in term of the scholar’s way of dealing with the past, something that could easily turn into “a pack of tricks...played on the dead” (65) but also for the very conceptualization of historical episodes that would have “to await the future to learn (their) meaning” (74).¹¹

As the fallacies of anachronism and prolepsis reveal, the search for unit ideas has repetitively troubled the history of sociology since its very conception. Either authors are retrospectively handpicked for some concepts which remained underdeveloped or isolated, or thinkers and ideas are picked out for not having lived up to the task of some imagined re-projected comprehensive programme, or

11 It is not too strange to see here some important parallels with some of Robert K. Merton’s arguments; however, I maintain that Skinner is much more outspoken about these dilemmas and dangers than Merton.

a “mythology of doctrines... proper to the subject”, is constructed, which might run against the very intentions and motives of the original thinker and his ideas.

To avoid such pitfalls Skinner asks those who have an interest in studying intellectuals and their ideas to make a double effort in terms of hermeneutics. It is not only crucial to understand the meaning of what was said but also how the meaning is communicated. Austin and Wittgenstein’s reflections on speech acts and illocutionary moments are never far away from Skinner’s concerns: “To understand any serious utterance, we need to grasp not merely the meaning of what is said, but at the same time the intended force with which the utterance is issued. We need, that is, to grasp not merely what people are saying but also what they are *doing in* saying it... As well as grasping the meaning of what they said, we need at the same time to understand what they meant by saying it” (82).

Pursuing a sociology of knowledge or practising intellectual history for purely ‘presentish’ demands such as finding solutions to contemporary problems or fulfilling a problematic desire for contemporary system-building or aiming at some sense of completeness is for Skinner not only a methodological fallacy but amounts to “a moral error” (89). For him, “to learn from the past ... the distinction between what is necessary and what is contingently the product of our own local arrangements is to learn one of the keys to self-awareness itself” (ibid.).

Skinner’s intellectual history is less concerned with long-term conceptual change than with certain rhetorical “techniques” (187). In a way, Skinner’s strong programme is more directed at decisive moments or, as he calls it, towards the “pointillist study of sudden conceptual shifts” (180). In contrast, John G.A. Pocock’s version of intellectual history gives more due to long-term developments. His interest in political argument and political thought has led him to look at the plurality of languages and within that, particularly the language of history and historiography (Pocock 2009: viii). In his masterpiece, *The Machiavellian Moment*, Pocock studied how concepts travelled over time and how they emerged in various places by following the republican thread and paradigm that runs from the Italian city republics through to the emergence of North Atlantic’s civil societies. Yet, even though Pocock addresses some methodological question in *The Machiavellian Moment*, over the years he has reserved most of his reflections related to theory and method of political thought for his essays (Pocock 2009).

In the preface to his latest essay collection Pocock takes the opportunity to discuss his own efforts by juxtaposing his pursuits in intellectual history to Thomas Kuhn’s study of science paradigms. While he identifies some parallels in constructing paradigms, Pocock is sceptical whether the very term makes sense when applied outside the realm of science and the scientific community. As Pocock points out, political communities are not really ‘communities of inquiry’ with a strictly controlled common language. While they may be so at the beginning, for example in identifying a common problem or establishing a common political language, over time political communities tend to become too differentiated to follow just one paradigm. Instead, “many ‘paradigms’ must co-exist” (xii) often competing with each other, not at least in terms of their different normative

horizons. These polities are also always open to “new linguistic possibilities” (xiii), something that is not often the case in the context of scientific inquiries (although it occasionally happens, particularly in the context of scientific revolutions).

New in Pocock, and here his programme differs slightly from Skinner’s, is the focus on *Rezeptionsgeschichte*. While Skinner looked at “what an author was doing”, his intentions, his social network and the common language that was used, Pocock decided to go a step further by asking “what he/she turned out to have done”. As Pocock points out, this concern led him away from speech acts and more towards common usage, or, in his own words, “from illocution to allocation” (xiv). As Pocock further explains, this “involved not only the shaping of *parole* by *langue*; one must (also) inquire how authors were understood by readers, and how the response of the latter both shaped and was shaped by the original author’s speech act” (ibid.). This last comment should attract the attention of any sociologist, particularly since the process of innovation and interpretation results not only in consequences which may have been intended but also in many non-intended consequences. In other words, the outcome reaches often way beyond the original motivation and control of the author.¹²

Pocock devotes considerable effort to distinguish further between an original intended meaning and its possible reception. As he stresses, it happens quite often that despite their original intention ideas are used, interpreted, changed, distorted, misperceived, translated or mistranslated and are used or misused in political action – in short, they can often take on a life of their own. Reflecting about the history of political thought, Pocock rightly points out that we had better become aware of the fact that intellectuals are never entirely at liberty in determining or influence those structures in which their ideas are discussed and acted out; and, if this is true, we must naturally devote some attention to “both the situation in which (the man, the woman) is placed and the tradition within which he (she) acts” (13).

The exact relationship between thought and action remains crucial to Pocock’s conceptualization of intellectual history. He insists, for example, that how ideas have been used in political and social action is one thing, how conceptualizations and the “activity of thinking” have functioned in history is quite another. The latter usually works in much more complex ways because it goes beyond a singular thinker or intellectual and often presupposes aggregates such as entire polities. Pocock knows of course that styles of thinking can vary considerably, pending often on peculiar style and forms of engagement within a given polity. To discuss Burke, a figure in which political action and political thought are closely intertwined, is one thing; studying a Hobbes or Locke who leaned much closer to the thinking side than the applied political action side is another. If this is an appropriate description of different thinkers and how they relate to their social and political context then it makes sense to presuppose a certain continuum rather than a complete juxtaposition between thought and action. However, the main

12 This is something that Robert K. Merton also took great interest in but that seems to have been totally neglected in Mannheim and Bourdieu.

point here is about larger aggregates and how thinkers relate to these aggregates. Once a society has reached a certain stability, once it has become a polity, it will develop a language and a vocabulary of its own, or maybe several languages and vocabularies, which allow it to conceptualise and validate its political culture, its politics and even its policies, something that is very often beyond the singular contribution of individual thinkers or intellectuals.

Like Mannheim, Pocock is aware of the difference between thinking and experience. But in contrast to Mannheim, Pocock never presumes that there is such a thing as ‘false consciousness’, and he never presumes that the very *raison d’être* of his analysis would be that of hinting at or correcting such false consciousness. Rather Pocock’s attempts to write intellectual history aim at inhabiting “that gap between thinking and experience” (17).

From Pocock’s conceptualization and understanding of intellectual history it is finally but a small step to the German conceptual history as coined by Reinhart Koselleck. Koselleck, a former student of Carl Schmitt, distanced himself early on from the extreme political leanings of his former teacher. Together with his collaborators Otto Brunner and Werner Conze he has taken some of Schmitt’s concerns related to etymological and conceptual distinctions on board while at the same time steering conceptual history in a more liberal direction. There is nothing in Koselleck that would remind one of illiberal notions of ‘the political’ or such dubious distinctions as that between ‘friend’ and ‘foe’. Koselleck learned from Schmitt that there is more to concepts and concept formation, particularly if one takes a closer look at their specific political and historical meanings. In a number of studies, mainly in the context of editing *Geschichtliche Grundbegriffe*, that encyclopaedic and monumental work of a lifetime, Koselleck has outlined his programme of conceptual history. These studies have now been published in three volumes, starting with *Vergangene Zukunft: Zur Semantik geschichtlicher Zeiten* (Engl.: *Futures Past*) to *Zeitschichten* (Engl.: *The Practice of Conceptual History*) and, just after Koselleck’s death in 2006, the still untranslated *Begriffsgeschichten* (Conceptual Histories). The contributions in those volumes have varied between substantial theoretical and methodological reflections on conceptual history to actually writing the conceptual histories of such terms as *Bildung* (education), *Fortschritt und Niedergang* (progress and decline), *Patriotismus* (patriotism) or *Revolution* (revolution).

Koselleck argues that it is impossible to write *histoire totale* (Koselleck 2006, 12). He maintains that we will always have to live with the tension of not being able to reproduce or write society’s or the world’s history on a one-to-one scale. We always need to abstract from reality and history and we do so by using concepts. The crucial, and similarly productive, tension consists of the difference that while conceptualization is necessary in order to understand history, history cannot be dissolved into mere conceptualization and neither can social history be reduced to just referring to the connections between social relations. Better formulated, there is no society and no history of society without language and *parole*; at the same time history cannot be totally reduced to language or *parole* (15). Furthermore, the historian will have to deal with both synchronicity, the attempt to conceptualise

present events, and with diachronic dimensions, which reach out deeper in terms of distance from the present moment. What is new about social and conceptual history is its awareness of having to address both dimensions (for example, it is always the older notion which both enables and limits the present usage of concepts) (22).

Bearing such important complications and necessary qualifications in mind, Koselleck reminds us that a dynamic tension between society and the way we conceptualise it will always remain. It is not difficult to identify here a crucial difference that distinguishes the intellectual history programme as suggested by the Cambridge School from conceptual history. As Koselleck himself has suggested,

(t)he contemporary methodological debate about intellectual history is prone to relativising the hard antithesis of reality and thought, being and consciousness, history and language. Instead, other distinctions are used, which can be related to each other much more easily, for example, *meaning and experience*, which are constituted together and explain each other mutually, or *text and context*, both of which hide linguistic and non-linguistic presuppositions. The sociology of knowledge and linguistic analysis converge if and when *meaning and experience* are related to each other. The justification of such methods is beyond doubt because every language is historically conditioned and all history is conditioned through language. Who would deny that all unique experience turns into real experience only because it is mediated through and by language and thus makes history possible in the first instance. Nevertheless, I maintain that the analytical distinction between language and history remains crucial, not at least because the two can never become totally identical. (32f; my translation; in the original passage the words marked in italics are in English)

What about the practice of conceptual history? Are there any preferences? Are some conceptualizations more important than others? Looking at Koselleck's own work it becomes clear that his conceptual history has always been keen on deciphering particularly those concepts and meanings (incl. their change) which have evolved and which were first and foremost used in political debates. The explanation for such a preference seems obvious: political debates are usually loaded with historical experiences and content which help to coin crucial concepts in the first place. In other words, the concepts refer to real social struggles and experiences. However, the very fact that concepts are related to real past events and experiences does not mean that they change as suddenly as events do. The opposite is actually the case; concepts and conceptualizations are by nature very sluggish. While there are cases of sudden change and innovation, the general trend is one of slowness. Koselleck sees here an important connection with the structural dimensions of any language: "The repetitive foundational structure of language and understanding, their very repetitive character is a condition for the expression of the new" (60). For the historian this can be tricky. Some terms might take on a new meaning while other, related terms will not. Some terms are more prone to change while others are not. The new can

appear together with the old and vice versa. While the historian will try to follow and identify those complications, Koselleck reminds us that we should never lose sight of the simple fact that there are no concepts without experience and that there is no experience without conceptualization; conceptualization and experience relate to each other but they do so in complex ways and almost never, as pointed out before, on a one-to-one scale (62).

Apart from political debates, most interesting and certainly of the first order to the conceptual historian are those terms that directly relate to historical times and periodization. Here the historian looks, so to speak, into the engine room of historical understanding. This perspective might also explain why so much of Koselleck's own work deals with the various conceptualizations of historical time and political-conceptual – "isms", that is concepts which he calls *Bewegungsbegriffe*, 'concepts of movements' – but also 'moving concepts'. Both notions form a kind of king's highway to conceptual history, the first one because it shows temporalization and its popular perception 'at work', the second notion because it refers to crucial collective experiences and how they were understood. For both notions intellectuals and their ideas remain obviously crucial. However, the most important fact about conceptual history is that we get a sense of both structural conditioning *and* enabling capacities. Reminding us of these two conditions Koselleck has broadened and self-reflexively limited the range of intellectual history and the history of ideas.

While there remain major differences, intellectual history Cambridge-style and Koselleck's conceptual history share many concerns that distinguish both from sociological approaches, particularly those of the discourse type. Both care deeply for substantive argumentation, content, meaning, intention and experience. This seems to me in stark contrast to the structure-only, power-obsessed sociological approaches.

Trespassing Academic Boundaries

I would like to finish this chapter on a positive note by referring to at least two instances that demonstrate how one might successfully think about intellectuals and their ideas, and it might not be by chance that my positive example comes from two sociologists.

Since his book on the origins of sociology, *Between Literature and Science* (1988), Wolf Lepenies has practised both intellectual history and the history of ideas in a balanced way that would take in much of what has been suggested so far. Unfortunately, while practising intellectual history and the history of ideas, Lepenies has rarely given us an insight into his own workshop. Be it as it may, his last two studies stand out in terms of good practice and they reveal maybe more so than any theoretical reflection what can be achieved. One is an (untranslated) intellectual biography of the French literary critic Sainte-Beuve (1997); the other study, entitled *The Seduction of Culture* (2006) leans more towards the conceptual

history side, although it combines very well with intellectual history as suggested by Skinner and Pocock.¹³ In these studies Lepenies has very much practised what the high priests of intellectual history have preached. His *Sainte-Beuve* is a masterpiece which never forgets about the individual and creative dimensions of the thinker. At the same time it is a portrait of Paris and French intellectual life in the middle of the 19th century.

In contrast to *Sainte-Beuve*, *The Seduction of Culture* is very different in aim and scope. Since it deals with a complex polity (Germany in the late 20th century) and with how concepts and ideas have travelled (for example, into American exile and back to Europe) it is more Pocock than Koselleck-style in orientation. Having said that, the story of *The Seduction* also has its own intellectual hero – the writer Thomas Mann. Lepenies uses Mann as a prism or looking glass, thereby managing to address larger cultural ideas, how they have changed over the years and how cultural rhetoric became, after a complex and complicated learning curve, finally reconciled with modern democracy. Taken together, the two studies reveal that it is indeed possible to take on board some of the criticism from intellectual historians and practice intellectual history while remaining a sociologist.

Indirectly Lepenies's historical-sociological studies also reveal and point towards the omissions or fallacies of historians of ideas and concepts. All too often intellectual and conceptual history appears to be too keen on academic border maintenance, reserving the strict conceptual application to what they define as their territory. While this makes for a good disciplinary record, it is very often extremely dry in its execution. What is often missing is a lively description of the life-world and a sense of historical possibilities, something that goes beyond ticking conceptual and intellectual history tool boxes. In contrast, with Lepenies we always get a fascinating thick description that borders on the qualities of good literature. In intellectual or conceptual history this almost never happens. Anything that smells of good writing or narratives is all too easily dismissed (maybe a Hayden White syndrome?) The naked discussion of facts always seems to win over good rhetoric and good writing style.

I began this chapter by noting how important it is that when dealing with intellectuals and their ideas that the historical-empirical, almost positivist evidence should have a kind of veto right. Not everything goes, as for example those sociologists who are keen on discourse analysis have suggested. Having discussed some aspects of contemporary intellectual and conceptual history writing and how it could potentially enlighten sociological analysis, we should always bear this in mind: while it is important to say what the historical case is we should also reflect on how we present our findings. Lepenies's work reminds those of us who are interested in intellectual and conceptual history that there is no academic monopoly just by simply claiming a stake in the field or demarcating

13 In a series of lectures, given to the Académie Française, Lepenies reflects on his lifetime preoccupation with intellectual history and the history of ideas. However, the lectures do not reveal fully what Lepenies exactly thinks he is doing.

it. Against the often clinical dry spell of intellectual and conceptual history, Lepenies's success lies in his stylish thick descriptions, which often take on the form of sociological deliberations which are not grounded purely in dry facts and meticulous historical reconstruction. Critical judgement and leaving ground for a Hirschmanian-like 'possibilism' are crucial to such work. They represent best what I mean when referring to a strong programme. Lepenies's work might just represent that necessary dose of sociological imagination and liberty that all history writing needs – and intellectual and thought-oriented historical writing perhaps even more so.

What Lepenies's writings also reveal is how one can combine the study of creativity as a means to give meaning to and to make sense of individuals' lives. Lepenies's writing is not of the type that one can encounter in old-fashioned biographies, and while Lepenies remains interested in the individual creativity process he does so without giving in to Mannheimian or Nietzschean notions of the superhuman. Neil Gross's notion and use of what he calls 'intellectual self conceptualization' are certainly of relevance here (Gross 2008: xii-xvii). However, where Neil Gross falls short himself is in the execution or application of his concept. In his intellectual biography of Richard Rorty he compensates sociologically for what seems to have been a pretty uneventful and on occasion rather dull academic life, at least until Rorty turned into an international philosophy star. My interpretation might sound somewhat adventurous at first but I maintain that it is almost as if Gross tried to outline what Lepenies is actually doing but could not conceptualise; in turn, Lepenies executed what Gross had maybe intended but had actually missed out upon in his own ambitious intellectual biography of Rorty. Sure, one could argue that Sainte-Beuve had maybe more of an exciting social life in mid-nineteenth Century Paris than the early Rorty had in East Coast and American suburbia; but here I am already entering the dangerous minefield of personal preferences and values.

To conclude, I maintain that it is important to have a sense of proportion when it comes to the history of ideas and, more specifically, intellectual biographies. I think it is plainly wrong to have an over-socialized conception of man in which everything is reduced to social and cultural circumstances. Only in stories where a life is relatively uneventful and is, for example, limited to time entirely spent in higher education or research networks might it be justified to stretch the limits of sociological explanations. The sociological-autobiographical attempt of Bourdieu, or the new sociology of ideas as practiced by Neil Gross in his Rorty biography, might serve as examples. However, when it comes to rich experience and lives that do not just have their origins solely in academia, we must always ask how individuals have actually managed to succeed, despite what might have been early hindrances, hurdles and sometimes traumatic experiences. Individual creativity, the idea of individual autonomy, the psychological capacity to be the maker of one's own fortunes are essential ideas when fighting against adverse conditions. We need to humanise biography by giving credit to such individual efforts and existentialist notions of self-creation. At the same time an intellectual biography cannot limit

itself to the view that a person is entirely self-made. While we try to make sense of the world as individuals we also learn, struggle, fight and take issue with the world and the people around us. For a portrait of a thinker and/or intellectual, it is crucial to get the proportions right. Certainly intellectual networks and influences matter but so do individual perception and the digestion of information in the light of lived experiences. Here, the insights of intellectual and conceptual history should find their appropriate use. Maybe it goes too far to demand a return to Dilthey's *Lebensphilosophie*; however, some of Dilthey's maxims still hint of what is at stake. For him the task of biography consisted not only of studying the interaction and mutual impact (*Wirkungszusammenhang*) between a thinker and his milieu; he reminded us also that one should attempt to understand other lives by employing the expressive forms of literature (*literarische Form des Verstehens von fremdem Leben*) (Dilthey 1970, 304f).

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Chapter 3

Scholarly Publishing Projects in the Great Depression: The Works of G.H. Mead and the Payne Fund Studies

Daniel R. Huebner

Publication is undoubtedly one of the major avenues by which intellectuals have an impact on one another and on public discourse. Hence, the social conditions that influence publication also influence these structures of scholarship and have lasting consequences. This fact has been long recognized in social scientific research, at least since the Great Depression, when the substantial impacts of the worldwide economic downturn on the production and distribution of printed materials in the United States became apparent (Ogburn 1934; Waples 1938; Bloch 1948; Horowitz 2011). Indeed, examinations of the state of scholarly publishing in the Great Depression have provided a particularly acute vantage point by which to assess the ways in which the shape and progress of scholarship are implicated in the broader social and economic conditions.

Works that have attempted to demonstrate how the conditions of the Great Depression (especially the drastically reduced purchasing power of individual households and educational institutions) impacted scholarship have relied primarily on aggregate measures of production and sales of books. And with good reason: at its lowest point in 1933–34 only 45 per cent as many copies per new title, 69 per cent as many new editions and reprints, and 81 per cent as many new titles were published as in 1929 (Waples 1938: 60–9).¹ These figures illustrate the massive scale of the impact on publication, and in particular the substantial contraction of new scholarship available.

1 After decades of growth, the American publishing industry as a whole peaked in terms of the number of establishments operating, number of employees, and total value of wages paid in 1929 and hit its lowest subsequent point in 1933 after which it slowly regained ground but did not again surpass the 1929 high point until after World War II (USBC 1929–1937). At their lowest point the monetary value of wages, the costs of materials, and the value added to products fell to around 50 per cent of their 1929 value, and the number of persons employed and number of publishing establishments operating fell to about 70 per cent of their peak numbers.

Previous research, however, has focused only indirectly on the impacts of the Depression on the content or quality of the scholarship published.² Put another way, previous studies have examined only the inhibitory impact of the Depression on scholarship – preventing what might otherwise have been published – and not on how those very concerns about publication can lead academics and publishers to restructure their scholarship so as to be more likely to be published and successful in such an environment. Instead of focusing on economic conditions as “forces” or “pressures” somehow directly sizing scholarship, the following analysis attempts to demonstrate how the determinative impact of socio-economic conditions can be seen to occur through the practices of key social actors as they work to understand the opportunities and constraints of their situations. In this endeavor I build on previous organizational sociology that has examined the decision-making processes of publishers (Coser et al. 1984; Powell 1985) and on the recent movements in the sociology of science to reexamine social scientific knowledge “in the making” by focusing on the actual social practices of scholarship (Camic et al. 2011).

In order to assess this question I examine two case studies of series of proposed monographs under consideration at American publishing houses during the Great Depression. I go “behind-the-scenes”, so to speak, through archival research in order to demonstrate how decisions regarding scholarship and sales were made in light of their perceptions of socio-economic conditions and possibilities. And I have chosen to trace multi-volume projects because they allow me to examine how the content and order of scholarship is negotiated over a temporal sequence of decisions in a more visible way than would be possible for individual monograph projects.

In particular I examine the “Works of George H. Mead” under consideration at the University of Chicago Press, and the so-called “Payne Fund Studies” at Macmillan Company. Both series were championed by the respective editorial staffs, both were ultimately published in some form, and both were considered as constituting single, multi-volume projects. This study focuses on the production of scholarship and will venture only a few remarks on its reception. The discussion of reception centers on how the order and structure of that publishing process affects the reception of scholarly monographs, and hence structures the state of academic knowledge. Indeed, the multi-volume projects have the added benefit of allowing a more determinate assessment of some impacts of the production of scholarship on its reception than would be possible for an individual monograph. This study is not intended as a comprehensive analysis of academic publishing in the Great Depression, but rather as a detailed examination of how, in two actual cases, the perceived economic realities

2 Douglas Waples’ foundational work combining a “sociology of the depression” with a “sociology of reading” explicitly posited that “[t]he depression affected both the number *and the character* of publications read” (emphasis added), but he was forced to admit sociologists “must usually make the best of grosser data” like “the number of books published annually in different categories” which are “[a]t the farthest remove from analyses of individual books and authors” (Waples 1938: 9, 59).

structured publishing projects and how the decisions made under those circumstances had identifiable impacts on scholarly and public discourse.

The analysis begins with the conditions of the publishing industry and the place of scholarly publishing as it entered the Great Depression. The particular circumstances of the University of Chicago Press are outlined in order to orient the discussion of the publication of a set of books as a posthumous legacy to George H. Mead. And likewise the circumstances of the Macmillan Company are used to orient the discussion of the Payne Fund Studies.

Scholarly Publishing And The Depression

The emergence of scholarly book publishing as a definite publishing category and a self-consciously organized set of institutions in the United States occurred primarily in the 1920s and 30s (Horowitz 2011; Tebbel 1978). From the first, scholarly publishing occupied a problematic position, which remains in some ways much the same in the present day. In the early twentieth century, there was increasing awareness of the divergence being formed between “scholarly” and “commercial” publishing, and a nascent distinction between “commercial” and “scholarly”, “endowed”, or “quality” publishing firms (Bean 1929; Brewer 1931; Cheney 1949; Welky 2008). But another major trend among publishing houses in those same years was the consolidation of large, diversified commercial firms with departments in scholarly and educational books, along with general trade books and other publishing categories (Lehmann-Haupt 1951: 328). Amidst increasing cost of production even prior to the shock of the stock market crash, publishers were increasingly forced to restrict the number of potentially unprofitable items on their book lists, the publication of which had previously been viewed as a professional obligation (Bean 1929: 69–71; Cheney 1930). This move affected scholarly monographs especially strongly because of their relatively high cost of production and limited sales potential (Bean 1930: 3; Marshall 1931). Thus scholarly publishing developed simultaneously in the large commercial houses and in distinction from them and was immediately confronted with problems of economic viability.

In this environment the university presses became increasingly important as publishers of non-fiction titles, because they often had the benefit of not being expected to be financially self-sustaining and because they were often explicitly oriented toward an educational “mission” that served to motivate the editors to accept good scholarship at a loss (Lane 1939: 1; Emig 1931; UTA 1930; Bean 1929: 26). Almost all university presses received important sources of support from direct or indirect subsidies from their parent institutions as well as various outside sources (Griffith 1929; Lane 1939: 81). And leaders of the several university presses stressed the obligation their organizations had to publish scholarly work as a “service” to the universities and the greater public (Couch 1934; Bean 1933; Emig 1931; UTA 1930). In part because of the unique semi-sheltered financial position they occupied, university presses rose in relative

importance against other publishers of non-fiction books, reaching a historic high of ten per cent of all non-fiction titles produced in 1935 (Lane 1939: 99). But they were not completely unaffected by financial considerations. The subsidies they received and their book sales³ were not sufficient to maintain the university presses, especially as educational budgets plunged in the 1930s. In fact, the first comprehensive study of the university presses, by University of Chicago Press manager Donald Bean, argued that the majority of books published by those presses never recovered the costs entailed in the manufacture even under much more favorable conditions (Bean 1929: 82). Given this nearly dismal prospect of breaking-even, the university presses obtained an increasingly large portion of their working capital by asking the author, society, or institution which sponsored the research that resulted in the monograph to furnish a portion of the investment necessary to publish the book or to compensate the press for the loss entailed in publication (Bean 1929: 88; Bean 1930; Lane 1939: 81).

The University Of Chicago Press

The University of Chicago Press was near the top of university press publishing throughout the period under investigation both in terms of its economic production and its prestige. In terms of number of publications the University of Chicago Press rose almost every year in the 1920s both absolutely and relative to other university presses and commercial publishing houses. By 1935 it was the largest university press in terms of publications per year, it was among the top 30 publishers overall in the United States, it held the largest cumulative number of titles of any university press, and it published by far the largest number of scholarly journals of any press in the country (Lane 1939: 47).⁴ The University of Chicago Press had also run profitably for the first time in its history in 1921–22 and continued to do so almost without fail for just over a decade (Bean 1933). The peak of sales for the University of Chicago Press occurred in the 1930–31 fiscal year, and the peak number of books published occurred in 1932–33 (Shugg 1966), three years after the corresponding high point

3 The most important book sales for university presses were not from new books, but from the so-called “backlists,” the older books still in print that continued to sell (Brandt 1945). University presses with “strong” backlists obtained two-thirds or more of their total revenue from sales of these books (Lane 1939: 75). And university presses often engaged in what was sometimes called “pooling”, in which the presence of a few popular, profitable books would go to help finance those many other “unremunerative enterprises” (Bean 1929: 95).

4 Much of the credit for the rise of the University of Chicago Press has been given to its reorganization that occurred at the end of World War I, which created a more efficient differentiation of labor and brought talented people into management positions, thus allowing for subsequent aggressive expansion (Shugg 1966; Lane 1939).

of the rest of the industry.⁵ The outstanding decade of expansion of publication and profitability that extended into the early Depression caused a change in the orientation of the University of Chicago Press that was to have decisive importance in the fate of later publications.⁶ In fact, the ability to be completely self-sustaining that came with consecutive years of profitability became increasingly an end to be pursued for its own sake and promoted the use of subventions and the publication of books with potential to contribute to profits (Bean 1933: 256). The increasing pressure from commercial demands on the scholarly and professional self-understanding of publishing was, thus, reproduced within the university press itself.

The revenue of the University of Chicago Press fell by 13 per cent in the 1931–32 fiscal year and continued to fall through 1934–35, at which time the University of Chicago Press's book sales were only 53 per cent of the volume of its peak (Shugg 1966).⁷ Instead of proposing that the deficit be made up by the University itself – the standard practice until 1921 – the press's business manager recommended severe “economies” (that is, cut-backs) of expenditure across the organization. These “economy” measures formed the basis of the University of Chicago Press's operations in the next few years, as financial conditions worsened with declining sales. Donald Bean reported that the realization that its revenue was insufficient to cover the organization's expenditures came as a shock and “[threw] the publishing program of 1932–33 into actual confusion” (Bean 1933: 256). It is in precisely this environment that the University of Chicago Press began

5 Perhaps because of its prominence and successes, the University of Chicago Press experienced an accumulation of unpublished research manuscripts piling up in their vault as their still-limited publishing resources were outstripped by submissions (Bean 1930: 1). An unprecedented grant totalling \$100,000 was given to the University of Chicago Press from 1927 to 1931 by the Laura Spellman Rockefeller Memorial Fund for the expressed purpose of bringing out the projects in that backlog, and this grant caused a further expansion of this press's publishing program even through the early period of the Depression (Shugg 1966).

6 The annual reports the Press made to the university at large (*President's Report* 1919–1920 – 1930–1931) illustrate the pride the officials felt in their historic and ever-increasing successes. Almost every year from 1919 on to the end of the *President's Reports* (1931) superlative phrases lauding the successes of the University of Chicago Press appeared: “the most successful in the history of the Press” (1919–20: 63), “the largest in its history” (1921–22: 63), “the largest in the history of the Press” (1922–23: 68), “the greatest net profit in its history” (1923–24: 69), “the largest in its history” (1924–25: 65), “the largest in the history of the press” (1925–26: 77), “the most successful in the history of the Press” (1926–27: 81), “the largest in the history of the Department” (1928–29: 102), “the largest in its history” (1929–30: 130).

7 Report by William B. Harrell, 7 April 1932 (University of Chicago Board of Trustees Committee on Press and Extension [hereafter CPE], Vol II). Harrell admitted in his report that these measures were intended to “provide a surplus of approximately \$3,216 on sales of \$300,000” but would probably “destroy a portion of the value of two years cultivation in new markets, and probably mean further recession in the volume of sales in future years”. That is, to ensure a short-term one per cent profit, long-term market cultivation and sales volume would have to be jeopardized.

to prepare to publish a series of volumes as a posthumous legacy to long-time Chicago professor of philosophy, George Herbert Mead.

The Posthumous Works Of G.H. Mead

George Herbert Mead died 26 April 1931, and by June of that year his son and daughter-in-law, Henry C.A. and Irene T. Mead, were seriously pursuing the prospect of preserving a legacy for him through publication. Mead had been a leading American pragmatist philosopher, and he came to have a major influence on American social science through the published editions of his lectures published after his death and through his students' interpretations of his teachings. Henry and Irene Mead invited several of G.H. Mead's former students to work on a number of different potential projects.⁸ The first proposed project was the preparation for publication of the Carus Lectures Mead had given in 1930 at the American Philosophical Association meeting, which were already under contract with Open Court Publishing Company. At least four other projects to edit materials were proposed, including: (1) a stenographic transcript of Mead's introductory lectures on "The Movements of Thought in the Nineteenth Century", (2) a copy of student notes from Mead's "Social Psychology" lectures, (3) several substantial unpublished manuscripts written by Mead, and (4) a volume of Mead's major published papers.⁹ These projects were discussed with the University of Chicago Press, Henry Holt, and Open Court beginning in the summer of 1931. The process by which the particular materials were collected and chosen for inclusion is examined in another article (that is, Huebner 2012) and will not be examined here.

On June 12, 1933 University of Chicago Press manager Donald Bean and former Mead student Charles Morris met and drew up a plan to publish the various projects as a single series of memorial volumes.¹⁰ The program of publication was for a "Philosophic Works of George H. Mead" including projected volumes in "Mind, Self, and Society", "Movements of Thought in the Nineteenth Century", and "The Philosophy of the Act".¹¹ A series format had the benefit, they thought, of allowing the volumes to be marketed together at a reduced price for the set,

8 The analysis herein relies heavily on a reading of the correspondence available in the George Herbert Mead Papers [hereafter GHMP] (esp. Box 2, Folder 3) and the University of Chicago Press Records [hereafter UCPR] (esp. Box 323, Folder 8).

9 Letter from C.W. Morris to I.T. Mead, 29 July 1931; I.T. Mead to C.W. Morris, 13 August 1931 (GHMP, Box 2, Folder 3).

10 Letter from D.P. Bean to C.W. Morris, 12 June 1933 (UCPR, Box 323, Folder 8).

11 Board of University Publications, unpublished minutes for 29 July 1933 (UCPR, Box 21, Folder 5). This discussion was four months after the social psychology material had been individually approved for publication under the title of "Mind, Self, and Society" by the Board of University Publications (unpublished minutes for 18 February 1933; UCPR, Box 21, Folder 5).

which more people could afford “even in these times”.¹² Given the economic conditions and the fact that they could not presuppose a market for books by G.H. Mead – who had never published a book in life – they proposed a scheme of advanced subscription to gauge interest and ensure sales. This system of financing was intended as a way to minimize the amount of money Mead’s heirs would have to pay out-of-pocket while maximizing the amount published.¹³ The rolling financial plan that was devised, by which the sales from each published volume would finance later publications, had definite effects on the whole enterprise. For one, it served to determine the order in which the volumes would appear. “Mind, Self, and Society” was seen as having the largest sales potential and the possibility of some textbook sales, so it was published first; “Movements of Thought” was seen as the second most likely to have healthy sales and so was published second, leaving “The Philosophy of the Act” as the third volume.¹⁴ The various proposals for a “published works” or other volumes fell through because of the exhaustion of financial resources.¹⁵

12 Letter from D.P. Bean to C.W. Morris, 12 June 1933 (UCPR, Box 323, Folder 8).

13 In the proposed arrangement, Henry Mead would pay the entire cost of the first volume not made up through subscriptions in the hope that the volume’s subsequent sales would finance the publication of others (R.D. Hemens to C.W. Morris, 16 March 1934; UCPR, Box 323, Folder 8). Because the series was approved by the Press “provided satisfactory financial arrangements can be made” – their phrase for the requirement of outside funding to underwrite publication – Henry Mead would have been expected to pay the total amount of all the volumes (estimated at over \$9,000 at the time) absent some self-financing scheme (D.P. Bean to H.C.A. Mead, 29 May 1934; D.P. Bean to R.D. Hemens, 18 June 1935; UCPR, Box 323, Folder 8). Adjusted for inflation, this total project would have been worth approximately \$124,000 in 2011 dollars as measured on a GDP deflation scale measure. As it turned out, the actual total expense for Henry and Irene Mead as far as I have been able to confirm it was \$1808.23, or approximately \$29,000 adjusted. It was justifiably remarked in the correspondence that “it appeared difficult for Mr. [Henry] Mead to raise the necessary funds” (R.D. Hemens to M.D. Alexander, 21 August 1934; UCPR, Box 323, Folder 8).

14 Letter from D.P. Bean to H.C.A. Mead, 29 May 1934; D.P. Bean to R.D. Hemens, 18 June 1935 (UCPR, Box 323, Folder 8).

15 The “published works” volume was seriously considered for several years, according to correspondence. It was to have been edited by Harvey J. Locke with an introduction by Ellsworth Faris (both sociologists, unlike all the other former students involved) and would have included some two dozen of Mead’s most important articles (“Request for Estimate,” 30 July 1937; UCPR, Box 323, Folder 8). This volume was approved by the Board of University Publications if 500 advanced subscriptions could be secured before its manufacture (D.P. Bean to H.J. Locke, 11 January 1938; UCPR, Box 323, Folder 8) – a benchmark which apparently was not ever reached. At least two other proposals to republish articles by G.H. Mead appear in the University of Chicago Press Records between the 1930s and the 1950s before *Selected Writings* (Mead 1964) finally appeared. A separate “history of thought” volume was championed for many years as an independent project by Charles Morris (G.J. Laing memo, 9 August 1933; UCPR, Box 323,

The financial dependence of the volumes upon one another also significantly delayed the publication of the second and third in the series. The correspondence indicates that all three of the projects subsequently published were virtually complete by the end of 1933. Yet *Mind, Self, and Society* was not published until December 1934, *Movements of Thought* until April 1936, and *The Philosophy of the Act* until May 1938. The preparation of the first volume for publication did not begin until after the solicitation of advanced subscription had yielded its apparent saturation point of two hundred subscribers.¹⁶ The sales from *Mind, Self, and Society* were modest and, in order to make possible the publication of the second volume, the press agreed to pay about half of the production costs as long as Henry Mead agreed that the sales therefrom would go to recouping that investment before production of the third was considered.¹⁷ And the third volume, which was thereby put in jeopardy, was only possible because a second change in the contract with Henry Mead was made such that he agreed to forego all royalties in perpetuity in exchange for the University agreeing to cover any deficit incurred by its publication.¹⁸ In each case, decisions were delayed to enable sales receipts from previous volumes to minimize further outlays from Mead's family or the Press and *ad hoc* solutions were found to the precariousness of the overall project. It is difficult to assess the negative effect this had on sales of the volumes, but the delays definitely came as an irritation to some subscribers.

The economic limitations did not only shape factors external to the actual content of the volumes. The early discussions regarding *Mind, Self, and Society*, for example, proposed including a large manuscript to have been entitled "Mind and Body from the Standpoint of a Pragmatist", which was not ultimately included in the volume, although it did later reappear as the 100 page-long Chapter XXI in *The Philosophy of the Act* (Morris 1938). Instead, a shorter (42-page) untitled manuscript was substituted, and it consequently comprised the first three so-called "Supplementary Essays" at the back of the published work (Morris 1934: vi). Later correspondence indicates that there was additional consideration of including some material amounting to 212 pages, but cost estimates showed it to be infeasible.¹⁹ And the fourth of the "Supplementary Essays", a ten-page essay entitled "Fragment on Ethics" that appeared at the end of the published *Mind, Self, and Society*, was severely edited down from its original form: a 243-page stenographic transcript from Mead's 1927 course in "Elementary Ethics" (Morris

Folder 8) because of the large collection of notes Morris had been able to gather on Mead's courses. This volume has never subsequently appeared because the series would have been "harder to sell".

16 Letter from M. Tyler to D.P. Bean, 13 January 1934 (UCPR, Box 323, Folder 8).

17 Letter from D.P. Bean to H.C.A. Mead, 8 July 1935 (UCPR, Box 323, Folder 8).

18 Letter from D.P. Bean to H.C.A. Mead, 13 November 1935 (UCPR, Box 323, Folder 8).

19 Letter from D.P. Bean to M.D. Alexander, 9 June 1933; "Estimate to Publication Department," 14 August 1934 (UCPR, Box 323, Folder 8).

1934, p. vii). Taken together, these decisions show both the acute restriction and non-fixity of the content of these volumes. That is, not only did financial considerations result in the determination of content negatively, by preventing much of it from publication, but it also influenced content positively, by promoting the inclusion of shorter summarized materials.

Much the same is true of the other two volumes in the Mead series. In response to being told that his edited manuscript for *Movements of Thought* was likely too long for publication, Merritt Moore admitted to doing “something by way of cutting down the appendix on French thought”. This appendix was originally a 148-page stenographic transcript from Mead’s 1928 course in “French Philosophy of the Nineteenth Century”, which was cut to 92 pages in the published version.²⁰ And when the Press was working to secure funding from the University for the publication of *The Philosophy of the Act*, Morris was informed that the administration probably would not agree to the deal “unless the number of pages is cut down or [Henry] Mead underwrites the project in some way”. Morris did cut down a section entitled “Categorical Fragments” from 103 to fewer than 50 pages and placed them as an appendix at the end of the volume.²¹ In addition, he decided against including some of Mead’s previously published material in the volume in the hope that it would appear in a subsequent “published works” volume. Indeed, the uncertainty about subsequent publications led in a number of cases to the inclusion of materials in severely fragmented form, and the inclusion of materials that were not otherwise appropriate for the themes covered in the volume. As the publication enterprise continued to shift and its financial stability progressively faltered, any clear delineation of material was forfeited to the desire to see any of it in print.

The University of Chicago Press’s posthumous Mead volumes, with their order and contents made under the pressures of the Depression, had a major impact on assessments of George Herbert Mead.²² In particular, *Mind, Self, and Society* has become the single most important source on Mead’s philosophy, constituting four-fifths of all citations to Mead in journal articles in the past half-century (Huebner 2012). Particular words and phrases from that book have come to be the definite popular representations of Mead and have influenced sociology especially through the work of symbolic interactionists. Phrases that appear nowhere else in Mead’s work, like “social behaviorism”, have become labels for his approach.

20 Letter from M.H. Moore to D.P. Bean, 28 May 1935 (UCPR, Box 323, Folder 8).

21 Letter from D.P. Bean to C.W. Morris, undated [June 1937?] (UCPR, Box 323, Folder 8); C.W. Morris to D.L. Miller, 6 August 1936 (GHMP, Box 4, Folder 5).

22 None of the three posthumous Mead volumes published in the series by the University of Chicago Press was ever significantly reedited, because the significant costs resulting from new typesetting would have made such a measure financially “impossible” (F. Wieck to M.H. Moore, 8 June 1950; UCPR, Box 323, Folder 8). Only the most significant errors were fixed in subsequent impressions, and in no case did these changes amount to the alteration of more than a single phrase.

The order of the volumes, itself, had a definite impact on perceptions of Mead. For example, Maurice Natanson, who wrote one of the earliest monograph-length interpretations of Mead's social theory, treated the order of publication of the posthumous volumes as an indication of Mead's conceptual development instead of as a consequence of their perceived sales potential (Natanson 1956). And in a similar vein, the influential critique of Mead elaborated by Jürgen Habermas in his monumental *The Theory of Communicative Action* (1984), treats the structure of *Mind, Self, and Society* as an indication of Mead's logic rather than as the result of decisions made after his death to ensure topical coherence at minimal length.

The Macmillan Company

A second case study comes from the Macmillan Company of New York, which was one of the largest publishing houses in the country at the time. It had been founded as a US office of the older British Macmillan & Co., Ltd., but from 1896 it had been managed separately. The American company continued to accrue major benefits from its relationship with its British parent company, especially through its unmatched worldwide distribution system and its permanent reciprocal contracts of agency for one another's works (Tebbel 1978: 101–3). The American company's early independent successes had been in popular fiction, including publishing works by Henry James, Jack London, Upton Sinclair, and others. But Macmillan became increasingly involved in the production of educational books, especially textbooks. This trade in textbooks accounted for half the firm's sales throughout the second decade of the twentieth century (James 2002: 178; Tebbel 1978: 102).

Macmillan was also a leader in the diversification of publishing firms, with strong departments in juvenile, religious, outdoors, educational, and medical publications as well as general trade books (Lehmann-Haupt 1951: 328). Its regional branches in Chicago, Atlanta, and elsewhere operated effectively as separate businesses with their own production and distribution capabilities (James 2002: 178; Tebbel 1978: 102). Macmillan was also consistently a leader in advertising, with over 100,000 lines of type appearing in periodicals yearly (Tebbel 1978: 452). Indeed, the firm's diversity led to it being called the "department store of publishing" in certain circles (James 2002: 178). Throughout the first half of the twentieth century, and contrary to what may be supposed by their massive success, Macmillan had a reputation as "one of the most important scholarly publishers" that steadfastly maintained a "definite standard of merit in all their general works" (Lehmann-Haupt 1951: 328). Macmillan also had a history of partnering with learned societies to publish or advertise scholarly monographs (Marshall 1931). The firm was the American agent for prestigious British scholarly presses, including Cambridge University Press (Lehmann-Haupt 1951: 328). And, in particular, its recent best-selling nonfiction titles had included H.G. Wells' *The Outline of History* (1921), Walter Lippmann's *A Preface to Morals* (1929), and Charles and Mary Beard's *The Rise of American Civilization* (1930).

In 1927 the firm had become so successful that it offered a 400 per cent dividend on its stock and raised its capital to almost \$10 million, making it the single largest publishing company in the United States (Tebbel 1978: 102–3). This phenomenal growth appears to have sheltered them from the earliest days of the Great Depression. In an interview after the stock market crash in 1929, the long-time head of Macmillan, George P. Brett, Sr., declared confidently that 1930 would be a year of increasing sales for the publishing industry (Brett 1929). And although he was wrong for the industry as a whole, Macmillan did continue to expand. The company's peak sales came in the 1930–31 fiscal year, before declining significantly over the next several years (Tebbel 1978: 536). Macmillan credited its ability to continue to pay dividends in 1931 to its strong list of “staples”: books issued in previous years that continued to sell strongly (“Staple Dividends” 1931). But in 1932, as a result of the declining budgets of school and college libraries – “the bedrock of its customer base” – and in the face of competitors’ undercutting its prices, Macmillan had a major drop in sales of almost 20 per cent (James 2002: 180–1). Despite its massive success with individual bestsellers (especially *Gone with the Wind* by Margaret Mitchell which sold 2 million copies in its first year and led to a 25 per cent increase in profits for 1936), the departments associated with scholarly publishing continued to struggle in the Depression amidst increased competition and production costs (Welky 2008: 193ff; James 2002: 182). As with the Mead volumes at Chicago, the Payne Fund Studies came up for consideration in the depth of Macmillan’s fiscal struggles.

The Payne Fund Studies

The works that became known as the “Payne Fund Studies” were a series of reports issued between 1933 and 1935 on the social and psychological effects of the motion pictures on youths issued by some of the leading social scientists of the day. By the mid-1920s there was a major public discourse about the impact of the movies, and intellectuals had begun to enter the fray. Among the most vocal were prominent clergymen and social workers, including Rev. William H. Short, who had previously been the head of the League to Enforce Peace and an important American figure in the drafting of the covenant of the League of Nations. He had become convinced by the apparent failure of reformers’ exhortations to the public that scientific studies would better bolster the efforts to reform the motion pictures (Jowett et al. 1996: 37–8). In 1928 he traveled the United States interviewing dozens of social scientists and reformers in the hopes of putting together such studies. He managed to convince The Payne Study and Experiment Fund, a newly formed philanthropic foundation, to provide funds for a series of studies to be carried out by leading social scientists over the next four years. Over two-dozen

separate projects were proposed, but only about fifteen made it past initial stages (Jowett et al. 1996: 57–66).²³

In early 1932 W.H. Short, who had become the director of the Motion Picture Research Council that organized the Payne Fund Studies, was in contact with Macmillan and other presses in order to negotiate publication.²⁴ The fact that the studies were being considered as a group and that the ultimate number and length of the studies was not yet determined (although they were planning on at least twelve completed monograph studies) apparently made his negotiations problematic. No publisher was willing to take on the risks of putting out the entire series without considerable financial subvention, nor could Short get any press to agree to pay any royalties at all for the scientific studies.²⁵ Still, he thought Macmillan made the best offer, which required the Payne Fund to provide several thousand dollars subsidy for publication and gave Macmillan the right to contract independently with a freelance writer to prepare a “popular volume” that would summarize the results of the study and market them to the general public.

The writer with whom Macmillan contracted to write the “popular volume”, Henry James Forman, agreed on the condition that he have monthly advances

23 I make no attempt in this chapter to give a full recounting of the various research studies. The studies that were published were as follows: *Motion Pictures and Youth: A Summary* by W.W. Charters bound with *Getting Ideas from the Movies* by P.W. Holaday and G.D. Stoddard (1933); *The Emotional Responses of Children to the Motion Picture Situation* by W.S. Dysinger and C.A. Ruckmick bound with *Motion Pictures and Standards of Morality* by Charles C. Peters (1933); *Motion Pictures and the Social Attitudes of Children* by R.C. Peterson and L.L. Thurstone bound with *The Social Conduct and Attitude of Movie Fans* by F.K. Shuttleworth and M.A. May (1933); *Movies and Conduct* by Herbert Blumer (1933); *Movies, Delinquency and Crime* by Herbert Blumer and P.M. Hauser (1933); *Children's Sleep* by Samuel Renshaw, V.L. Miller, and D.P. Marquis (1933); *How to Appreciate Motion Pictures* by Edgar Dale (1933); and *The Content of Motion Pictures* by Edgar Dale bound with *Children's Attendance at Motion Pictures* by Edgar Dale (1935). The most detailed behind-the-scenes examination of the Payne Fund Studies is Jowett et al. (1996) *Children and the Movies*, which is based on extensive archival research. While I draw upon their excellent documentation, their analysis of the opinions of, and relationships between, the studies' researchers is less convincing.

24 Short likely approached Macmillan in part because William M. Seabury, who participated in the early organization of the proposed studies, had published his 1926 *The Public and the Motion Picture Industry* with Macmillan. That book, along with Short's 1928 *A Generation of Motion Pictures*, was one of the motivating factors of the studies, as it explicitly called for empirical social science research into the effects of the movies.

25 Letter from W.H. Short to C.C. Gilman, 1 July 1932 (Robbins Gilman and Family Papers [hereafter RGFP], Box 71, Folder 2). In my analysis I draw upon my own archival research in the Robbins Gilman and Family Papers (RGFP) containing Catheryne Cooke Gilman's documentation from her participation on the advisory board to the Motion Picture Research Council. Gilman helped run a settlement house in Minneapolis and was a leader in a variety of social welfare projects, including the Women's Cooperative Alliance “Better Films” movement.

paid to him by the Payne Fund while he worked on the book, and the ability to write magazine articles from the research materials as kind of “advance serial publication” of the contents of the volume.²⁶ The money from the articles would go toward his writing of the popular volume. Forman had previously been a newspaper writer and an editor of several major periodicals, and he was well known at the time as a writer of travel guides and fiction novels. Through these negotiations, Short, Forman, and the Macmillan Company had effectively structured a production schedule in which the individual research monographs would have to wait for publication until a popular introduction had been separately published, which would itself have to wait until serialized articles were published, which depended in turn on the preparation of manuscripts of the research monographs for the articles’ material. These arrangements were considered necessary for the project to be financially viable in its write-up and publication. While several of the research studies were more-or-less complete by early 1932, they would not begin to appear separately in print until late 1933. And because Forman had the explicit task of preparing summary accounts under his own authorship based on the researchers’ reports before those original materials would ever come into print, the arrangement had effectively contracted away the framing and interpretation of the studies to a freelance writer responsible only to Macmillan (and not to the researchers, themselves). Indeed, when Forman’s serialized articles began appearing in *McCall’s Magazine* in September 1932 there was considerable controversy over the way the research findings were represented, and when the researchers read the manuscript of his popular book, *Our Movie-Made Children* (Forman 1933), they again protested.²⁷ In spite of additional negative reports from outside readers solicited to assess the manuscript, the volume came into print in May 1933, after being delayed long enough to address some of the more serious criticisms of the researchers (Jowett et al. 1996: 101–8, 240).

The initial records of research studies submitted to Macmillan for proposed publication listed 14 separate studies.²⁸ Of these, only 12 were ultimately

26 Letter from W.H. Short to C.C. Gilman 7/1/1932; W.H. Short to C.C. Gilman 13 October 1932 (RGFP, Box 71, Folder 2; Box 71, Folder 5).

27 Catheryne Cooke Gilman was particularly critical of the articles: “I have been in a position to hear a great many people speak of the articles and have found no one satisfied. If the same man writes the book I fear the result. He does not inspire confidence and gives one the feeling that he does not have confidence in the findings of the Motion Picture Research Council” (C.C. Gilman to W.H. Short, 3 October 1932; RGFP, Box 71, Folder 5). Short assured her of Forman’s credentials and said that the articles appeared unsatisfactory because a ghostwriter had been sent to “McCallize” Forman’s work for the magazine.

28 Initial lists included the following: “Motion Pictures and Youth, An Introduction” by W.W. Charters; “The Content of Motion Pictures” by Edgar Dale; “Motion Pictures and Mores” by Charles C. Peters; “Children’s Attendance at Motion Pictures” by Edgar Dale; “Getting Ideas from the Movies” by P.W. Holaday and George D. Stoddard; “Emotional Responses of Children to Motion Pictures” by W.W. Dysinger and Christian A. Ruckmick; “Motion Pictures and the Social Attitudes of Children” by Ruth C. Peterson and L.L.

published, and they appeared in eight volumes (four of the volumes combined two separate studies in one, likely to minimize production costs). Although I am unable to discover records of the actual negotiations, the various lists of the studies prepared demonstrate that there was considerable fluidity in the arrangement, authorship, and inclusion of the research studies occurring behind the scenes prior to publication. Some lists dropped certain studies altogether only for them to reappear later, and in some studies are listed separately that were later combined.²⁹

In the selection of materials for final publication, it appears that economic conditions had several influences. Many of the members of the research team and the executive board of the Motion Picture Research Council were feeling the Depression personally. Short reported that by October 1932 he had been without compensation from his position for a year and was finding it difficult even to afford office supplies. Catheryne Cooke Gilman had to give up her various positions on national social welfare advocacy boards in order to make ends meet. And Paul G. Cressey, who had taken over primary responsibilities on a study of “Boys, Movies, and City Streets”, which continued to be advertized as forthcoming in the Payne Fund series, was in the midst of a major financial and personal crisis that prevented him from completing his study. He was without permanent academic employment in 1934 as a result of the “radical retrenchment” of universities in the face of substantial drops in student enrollment and fees, and prolonged illnesses of his wife and himself had completely taxed all their financial reserves (Jowett et al. 1996: 127–9). After all other options were exhausted, Cressey appealed in vain to Short personally and to the Payne Fund to provide him with continued support to finish his study (Jowett et al. 1996: 129–30). The Payne Fund had become considerably more discriminating in allocating its monies to this group of researchers, rejecting several proposals including an extension of Herbert Blumer’s studies, a further outlay to support Cressey’s study, and subsidies for the publication of Frederic Thrasher’s completed study on the impact of movies on youths involved in the “Boys’ Club”.³⁰ And other

Thurstone; “Children’s Sleep” by Samuel Renshaw, Vernon L. Miller, and Dorothy P. Marquis; “The Movies and Conduct” by Herbert Blumer; “Motion Pictures, Delinquency and Crime” by Herbert Blumer; “Motion Pictures in Penal Institutions” by Philip M. Hauser; “Motion Pictures in a Delinquent Area” by Paul G. Cressey and Frederic M. Thrasher; “Teaching Children to Appreciate Motion Pictures” by Edgar Dale; and “The Social Conduct and Attitudes of Movie Fans” by Frank K. Shuttleworth and Mark A. May (RGFP, Box 71, Folder 3).

29 For example, the Shuttleworth and May study was absent from some reports, although it was later published in the same volume as the study by Peterson and Thurstone; in some lists Philip Hauser is listed as the author of a separate study, which was apparently incorporated partly into Blumer’s study on “Delinquency and Crime” and partly accepted as his University of Chicago sociology dissertation. As is discussed below, the Thrasher and Cressey volumes never appeared.

30 Blumer proposed an extension of his study to be conducted by Clifford Shaw in Fall 1931, and although the research director of the studies, W.W. Charters, and Short were supportive, they wondered where the additional money would come from, and expressed

agencies, including the Bureau of Social Hygiene that had supported Thrasher's work, had been closed down as a result of Depression conditions.

The Payne Fund apparently stopped funding new expenses to the group entirely in early 1934, as they attempted to distinguish between their support of the research (which they apparently considered concluded) and the Motion Picture Research Council's independent "propaganda" efforts on behalf of film reform.³¹ Indeed, the dismissal of certain proposals on economic grounds is difficult to distinguish from those that were politically-motivated. The leaders of the Motion Picture Research Council were less than willing to sponsor research that could not establish causal relationships between motion pictures and deviance, especially when its members was already under personal and professional economic pressures. Thus, part of the explanation of why Cressey's and Thrasher's studies never appeared is likely the critical stance they took toward the model of social influence presupposed in this discourse (Jowett et al. 1996: 86–8). Without financial support to publish materials already in preparation, let alone to write up materials already researched or prepare additional research, the remaining studies in various states of completion were excluded from publishing plans.

By the time the first research monographs were published in October 1933, Forman's *Our Movie-Made Children* was a bestseller on its third printing and had been widely advertised and reviewed in the periodical press.³² By all accounts,

doubts that the study could really "get the facts" or "establish the causal relationship between the movies and crime" (W.W. Charters to W.H. Short, 3 October 1931; W.H. Short to W.W. Charters 9 October 1931; W.W. Charters to W.H. Short 13 October 1931; W.H. Short to C.C. Gilman 7 December 1931; RGFP, Box 71, Folder 2). On Cressey's and Thrasher's studies see Jowett et al. (1996: 88, 129–30).

31 This turn of events was in large part occasioned by the aggressive move toward public advocacy on behalf of the Motion Picture Research Council by its new president, Eleanor Robson Belmont (an actress, herself) in early 1934. She asked Short to write up a statement of the historical understanding of the different roles of the Payne Fund and the MPRC. In response, he wrote that the Payne Fund agreed that the MPRC "(1) should be subsidized during the term of the research only, after which it should go to the interested public for its funds; (2) that it should operate independently of the Fund, which as an agency organized exclusively for research should not be involved in propaganda; and (3) that it should not be merely one more 'better films committee' but should be as fundamental and thoroughgoing in its application of remedies as was the Fund in its fact finding" (Memorandum "Enduring Policies of the Council" by W.H. Short, 31 March 1934; RGFP, B 72, F 8).

32 *Our Movie-Made Children* was the subject of six separate *New York Times* columns between May and December 1933, and it was mentioned in many more, including one naming it among the influential Cardinal Hayes' "white list" of best literature for the year. In its first two years, the book went through eight printings. It was the only one of the Payne Fund Studies to be advertised primarily on its own, and those ads appeared in a wide variety of popular and specialty periodicals. The book's appearance did not match the uniform series design of the other works, and it was priced separately as a general trade book (\$2.50) as opposed to the research volumes (\$1.50 each). It appears that the only research monograph from the series to sell exceptionally well was Edgar Dale's *How to Appreciate Motion Pictures*, which was

it became “*the* representation” of the Payne Fund Studies in the public’s mind (Jowett et al. 1996: 7), a fact lamented by a variety of reviewers of the scientific volumes. Kimball Young, who had been consulted early on by Short, wrote one of the most scathing reviews of Forman’s volume, calling it “pseudo-scientific”, “myth-making”, and “sensationalism” in contrast to the “excellent contribution to sociology and social psychology” of the research monographs (Young 1935: 250, 254). He concluded:

[w]hereas most of the writers of these monographs have been careful not to claim too much for their findings, the author of this popular account of their results has done them personally and the fields of psychology, education, and sociology a genuine disservice. And we are now witnessing, partly as a result of this sort of misinterpretation or partial interpretation, a wave of sentiment against the movies... (Young 1935, p. 255)

As Young’s review indicates, the Payne Fund Studies became a focus of debates on the relation between scientific research and political and ethical reform precisely because of the overshadowing of the research by its popularization. They also fostered a significant discussion on the nature and relative worth of different social scientific methodologies. The authors of the studies had innovated in a variety of ways, establishing new techniques for the quantitative assessment of social attitudes, for content analysis of visual images, for interpretation of personal documents, and for structuring interviews. There was extensive discussion of the methods of the Payne Fund Studies in several major journals, especially the *Journal of Educational Sociology*, which dedicated a whole issue to the topic (that is, December 1932, Vol. 6, No. 4).

The influence was arguably greater in the public discourse on motion pictures than in the academy. The Motion Picture Research Council that had formed to administer the studies was formally incorporated in late 1933 and cooperated in the formation of the Code of Fair Competition in the Motion Picture Industry, as part of the National Industrial Recovery Act of 1933. Although the motion picture industry had been official regulated by a self-governing production code (known as the “Hays Code”) since 1930, it had not been generally enforced until July 1934 (Doherty 1999). While the Payne Fund Studies were by no means the only source of pressure on the movie industry, they were known inside the Motion Picture Producers and Distributors of America (Hays’ organization) as the “Payneful Studies”, and several later commentators have credited their popularity with helping promote greater movie regulation (Balio 1996; Jewell 2007; Doherty

explicitly billed as “a manual of motion-picture criticism prepared for high-school students”. Macmillan’s success with *Our Movie-Made Children* likely influenced their publication of *The Movies on Trial* (Perlman 1936) with extended essays on the movies from a variety of well-known public figures, including Upton Sinclair, editor William Allen White, Archbishop John J. Cantwell, and actor Edward G. Robinson.

2007). Forman's articles and book served their public purpose of turning a set of twelve scholarly research studies – which Forman (1934) thought required a “scientifically trained reader, accustomed to laboratory practice and procedure, to graphs and statistical presentation” – into a national problem in stark, moral terms.

Conclusion

The tensions existing in the American publishing industry as a whole between economic and professional practices became drastically exacerbated as the industry experienced the Great Depression. On all measures its aggregate worth declined and its production was scaled back in several different ways. However, it is precisely the early apparent immunity of some presses, in particular the University of Chicago Press and Macmillan, that cemented the shift toward more stringent economic requirements as the effects of the Depression became internalized. It is in this environment that the projects to secure a legacy to George H. Mead came up for publication. And similarly, Macmillan's huge successes through 1931 certainly made it a premier press for publication of the Payne Fund Studies, but also led to stringent requirements perceived as necessary to hedge against unprofitable scholarly projects.

Both publishing projects were affected in some of the same ways. Perceptions of the market for scholarly book sales and the qualities of their projects led both teams to delay publications, to structure particular orders for the appearance of the volumes, to exclude materials (and whole volumes), and to promote content oriented toward more favorable reception. But particular decisions and tactics were somewhat different in each case. Where the University of Chicago Press attempted to create a market through advanced subscription lists, Macmillan advertised heavily and freely distributed review copies of its popularization. And where the lack of working capital caused the exclusion of materials in the Mead series, it was at least in part self-censorship that led to exclusion of volumes in the Payne Fund series.

This unique environment results in a particularly clear picture of the social conditions of scholarship. I have traced in some detail a part of that picture, emphasizing the relationship between the decisions of social actors and the economic conditions. Over and over the people involved oriented themselves and responded to situations in which they treated local conditions as given, looking for practical, immediate solutions. Yet their actions came to have permanent consequences in the long term as each negotiated situation was both the consequence of previous decisions and determined the course of later ones. Over the long run, the form in which the materials were published has been the taken-for-granted basis for subsequent scholarship. In both of the cases, the analysis followed the decision-making process from within and was able to trace some of the ways in which particular sequences of social actions resulted in the peculiarities of the published works that came from them. By its use of archival and primary data to illustrate how the intellectual structures of scholarship develop through historical sequences

of practical social action, it is hoped that this analysis offers a productive direction forward in the sociology of intellectuals.

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Chapter 4

Psychology and Sociology in the Late 19th
Century French Intellectual Field:
The Case of the *Revue Internationale de
Sociologie*

Marcia Cristina Consolim¹

Introduction

This chapter is part of a broader study of the social sciences in France in the late nineteenth century and investigates the disputes concerning the legitimate definition of “social sciences” and “sociology”, waged on the one hand by the *Revue internationale de sociologie* founded by René Worms and, on the other, by Durkheimian sociology. The starting point for this study was the discovery of a network of sociability surrounding the figure of Gabriel Tarde and the interest generated by his psychosociological theory among certain Parisian intellectual groups starting in 1890. Some of them were collaborators, together with Tarde, in the *Revue internationale de sociologie*, which became the international headquarters of a circle of “intellectuals” who sympathized with or were supporters of some form of psychological theory in social studies. The claim is that the formation of this “minimum consensus” was made possible by interests connected with the positions and dispositions of these groups in the intellectual field, which, in turn, is related to the social and intellectual transformations that took place in the last two decades of the century in France and which can be summarized as: 1) the growing autonomy of the university, with a moderate professionalization of teaching careers and the higher status of the university professor compared to that of the secondary school teacher and the “free intellectual”; 2) the emergence of psychology and sociology as higher education disciplines starting in the 1880s, as well as their growing scientific prestige in the intellectual field; 3) the intermediate position of the *Revue* in the field of social studies, situated as it was between journals by the heirs of Frédéric Le Play and Émile Durkheim’s *L’Année Sociologique*; and 4) the polarization between Worms’s *Revue* and the Durkheimian journal *L’Année* regarding the conception of the social sciences.

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The Intellectual Field in the Late Nineteenth Century

Starting in the second half of the nineteenth century, the French elites underwent a process of internal differentiation which resulted in the relative autonomization of the intellectual elite in contrast to the political and economic ones. This trend intensified from 1880, when the Third Republic was consolidated and initiated a range of educational reforms characterized by greater autonomy of the academic field. In the scientific areas the process of autonomy was more advanced, so that greater emphasis was put on urging the end of the traditional function of the university as a body of the State – in effect since Napoleonic times – and questions were raised about the intellectual function of the “expert”, “counselor”, or “ideologue”. In contrast, in professionalizing areas such as law and medicine, the resistance to autonomy was stronger due to the fact that recruitment for these courses took place primarily among the traditional power elites. In the fields of literature and philosophy, the process of autonomization was not fully realized during this period, which retained an intermediate position between scientific and professional courses (Charle 1983, 1987, 1994). The intellectual figures and the representations of the intellectual function itself were polarized in this sphere between a “lettered model” and a “scientific model”. On the one hand, there was the bourgeois intellectual, a salon regular who addressed his own class and flaunted his classical culture; on the other, the academic specialist, who possessed scientific knowledge and wrote for his peers (Lepenies 1996; Sapiro 2004).

In the 1890s, social studies received a lot of investment from the republican elites thanks to political circumstances strongly marked by the “social question” and the Dreyfus Affair, in the course of which new and distinct relationships were recast between intellectuals and the state. This process led to a certain differentiation between intellectual positions and to the foundation of various institutions dedicated to social studies – university and free courses, free schools, scientific societies, scientific journals and editorial collections. Such initiatives can be considered expressions of a *nébuleuse réformatrice*, characterized by the porous boundaries between the various orders of social, political, administrative and professional practice, which gathered members of the social patronage, republican politicians, high civil servants, academics, technicians and educators in the same institutions.

Characterized by the relative indistinctness between specialized knowledge and a generalist education, this movement gathered dilettantes and popularizers as well as academics and professionals (Topalov 1999). Thus, alongside a strictly academic hierarchy, informal pressure groups or “prince’s counselors” influenced the ministerial decisions, which, in turn, had the power not only to create positions and programs within the university, but also to subsidize private institutions (Karady 1979, 2001). The proposed names for posts in higher education were also filtered through the political sieve, since there was then a certain flexibility in relation to academic credentials, especially in the case of non-university institutions of higher education, such as the Collège de France, as well as the free courses created in

universities and large schools.² It was this context that gave birth to a wide range of institutions with aspirations to develop forms of the so-called “social sciences”, such as “social hygiene”, “social engineering” and “social economics”, disciplines committed to reformative actions based on technical or scientific knowledge of social problems. Such reformative initiatives took place, therefore, in a contentious scenario where social and intellectual legitimacy, respectively forms of practical and theoretical knowledge, are at stake – as reflected in attempts to establish new scientific disciplines or specialties.

Starting in the 1880s, a few disciplines in psychology and sociology were institutionalized in higher education. However, it is not possible to speak of disciplinary autonomy in relation to psychology or sociology in a period where there was no such established field of knowledge and no corresponding academic degree. Furthermore, most publications dubbed “psychological” or “sociological” at the time were written by amateurs and were aimed at a larger audience, which was the case of, for example, “crowd psychology” works (Mosbah-Nathanson 2011).³ On the other hand, while scientific psychology was quickly legitimized in intellectual and political milieus, sociology still aroused some suspicion on account of its concern with the “social question”, given the growth of socialist movements within and without the university. One of the advantages of psychology, in that sense, was the ambivalence of the subject matter, simultaneously a topic of philosophy courses and a medical specialty on its way to institutionalization in scientific laboratories. That is, it was possible to integrate it both in a scientific culture and in a literary one (Pinto 1993). “Social psychology” or “collective psychology”, an expression born as a title or subtitle for some social studies and, not coincidentally, of a journal founded, among others, by Alfred de Tarde, son of Gabriel Tarde, was not institutionalized in universities in that period. However, such expressions were subject to much controversy and constant redefinition, particularly in the debate between Tarde and Durkheim between 1893 and 1903, which was propagated and consolidated mainly in two sociology journals of that period: the *Revue internationale de sociologie*, in Tarde’s camp, and *L’Année Sociologique*, in Durkheim’s (Consolim 2010).

2 Raymond Poincaré, for example, deputy and ex-minister of Public Instruction, was avowedly one of the people responsible for the nomination of Jean Izoulet and Gabriel Tarde to the Collège de France. Ernest Lavis, member of the Conseil Supérieur de l’Instruction Publique, negotiated nominations for several “social economics” free courses with his sponsor, the Count de Chambrun, philanthropist and owner of the crystal company Baccarat.

3 Durkheim’s statement is revealing in regard to the opposition between an academic social science and a reformative one: “Your article on sociology ... will at last give the *gens du monde* a clearer idea of what this much discussed discipline is. Today, there is not a single philanthropic society president or mental institution manager who does not think he is doing social science”. Durkheim to Bouglé, August 23, 1897. Karady, Textes II, 405.

Journals in the Field of Social Studies

It was within this context of institutional transformation that new editorial collections, journals, and scientific societies proliferated, often stamping the name of a new discipline on the title, as well as diffusing distinct conceptions of the social sciences. The creation of scientific journals was in part an accreditation strategy for placement in higher education, even if they did not have the same recruitment profile or prestige (Charle 2004; Pluet-Despatin 2002). Karady's hypothesis seems particularly suited to establish some sort of distinction between those groups. Whereas journals founded by the heirs of Frédéric Le Play, *La réforme sociale* and *La science sociale*,⁴ tended to recruit their collaborators among the economic and socially dominant elites, which had links to colleges and trade schools, *L'Année Sociologique*, run by Émile Durkheim, conducted its recruitment mainly in academic circles and in faculties of letters, that is, in intellectually dominant circles (Karady 1979, 2001). In its turn, the *Revue internationale de sociologie*, founded by René Worms, held an intermediary position, even if closer to the Le Play's followers in virtue of the wide recruitment in professional schools and for welcoming "free intellectuals".⁵ Indeed, if it were possible to ascertain the profile of the sociological and intellectual recruitment of the more than 200 collaborators of the *Revue* in the first two decades of its existence, it is highly likely that it would have such a profile, even if, as we will see, part of this group was comprised of members of intellectually dominant circles.

The *Revue internationale de sociologie* is the central organ of a much larger project, formed by several institutions founded by Worms in the 1890s: two scientific journals – the *Revue internationale de sociologie* (1893) and the *Annales de l'Institut International de Sociologie* (1894) – two scientific societies – the Institut International de Sociologie (1893) and the Société de Sociologie de Paris (1895) – and a collection on social sciences at Giard & Brière, the Bibliothèque Sociologique Internationale (1896). These institutions have been often considered by scholars a forum of little importance *vis-à-vis* the further development of social theory; they have been disqualified for the lack of academic credentials of their collaborators and above all for the lack of theoretical and methodological unity within the group, factors which might explain the "failure" of the endeavor, especially when compared to the Durkheimian group surrounding *L'Année Sociologique* (Clark 1967, 1973; Geiger 1972, 1981; Karady 1976, 1979; Thiec

4 For information on the journals by the heirs of Le Play, see Kalaora and Savoye 1989. For details on their recruitment, see Karady 2001.

5 As Karady observed, *L'Année Sociologique* recruited 85 per cent of university professors, while among the *Revue*'s collaborators that figure was little more than a half. However, while an overwhelming majority in the case of *L'Année* came from colleges of letters and had a considerable amount of intellectual capital (*agrégation* titles and doctorates), in the case of the *Revue* the majority came from professional colleges and were mostly lacking in such credentials. Karady, *op. cit.*, 1976, 288.

1982; Mucchielli 1998; Mosbah-Natanson 2008). The heterogeneous character of its contributors did not prevent, however, a certain uniformity as to the conception of social sciences advocated in its pages, which emphasized the “psychological” foundation of the social sciences and was critical of Durkheimian sociology. If, on the one hand, Yvon Thiec noted the importance acquired by the theme of “collective psychology” at the *Revue*, the extension of and the reasons for this theoretical position-taking remain unknown. Roger Geiger, in turn, denied the existence of any dialogue between organic theory and Tarde’s psychology in the *Revue*, a statement that will be contested here (Geiger 1981, 357). Three factors appear to be directly linked to the formation of a “minimum consensus” around psychology: (1) the traditionalist intellectual and social dispositions of the *Revue*’s collaborators and, consequently, the ambiguous relationship they had with scientific disciplines; (2) Tarde’s rise in the Parisian intellectual field and the network of sociability and intellectual support that formed around him, and (3) the debate between Tarde and Durkheim, as well as the critiques of Worms’s collaborators made by *L’Année Sociologique*. This means, first, that one must qualify the idea that the collaborators of the *Revue* only produced “personalistic” works devoid of any doctrinal unity and, secondly, that one must question the myth of the “isolated intellectual” that developed around the figure of Tarde and of “social psychologists” in general (Consolim 2008).

Recruitment in the *Revue*

Studies which aim to characterize certain journals based on the profile of a small group always run the risk of valuing some authors to the detriment of others, that is, of giving greater importance to the most frequent and well-known to the detriment of a mass of occasional or unknown collaborators.⁶ Based on quantitative indicators on the mass of recruitment, scholars usually place the *Revue* on the juridical pole of the intellectual field (Karady 1979, 2011). Even if this is the case, both from a statistical assessment of the recruitment and from the journal’s thematic profile, the point here is to isolate a small number or the core of the most frequent collaborators, who, not coincidentally, were concerned with discussing the boundaries, the subject and the method of the social sciences. Thus, the *Revue*’s main collaborators are defined both in terms of publication percentages and in terms of their participation in the debates surrounding a legitimate definition of “social sciences”.

The nature of the recruitment has to do with the social capital Worms acquired throughout his social and intellectual trajectory, since his father was a professor of commercial law and political economy at the University of Rennes and Worms himself was a *normalien*, an *agregé* of philosophy and economics, and a doctor of law, political and economic sciences, letters, and natural sciences. In addition,

6 Karady 2001: 8–9.

Worms rose as a high-level employee in the state bureaucracy and was a professor in open courses at the colleges of law of Caen and Paris. This trajectory reveals the dual orientation of his background, but also the predominance of the juridical milieu and the state's high bureaucracy. It was not a coincidence, then, that the most active members during the first 20 years of the *Revue* had legal backgrounds.⁷ This group includes, besides Worms himself (34 articles), Raoul de la Grasserie⁸ (22 articles), Jacques Novicow⁹ (13 articles), Maxime Kovalewsky¹⁰ (12 articles), and Gabriel Tarde¹¹ (7 articles). They were authors who also had an active participation in others of Worms's organizations: they were presidents or vice-presidents of the Institut International de Sociologie and the Société de Sociologie de Paris and published several works in Worms's editorial collection, representing around 20 per cent of the total publications in the period. As for the jurist group, one must first recognize that they had very distinct trajectories. Starting with their countries of origin, this group was formed by both Russian and French "free intellectuals".

Novicow and Kovalewsky, both of whom had a liberal education, came to France, among other reasons, to escape the ideological pressure of the Russian Empire (see Gutnov 2002). Kovalewsky was a professor of comparative law at the University of Moscow and a corresponding member of the Academy of St. Petersburg, while Novicow was the son of a shipping industrialist and managed

7 According to Geiger, the participation of Worms's collaborators is very irregular, and in spite of their academic affiliations they have little in common. Some were renowned in adjacent disciplines – history, law, economics, and anthropology – while others acquired such renown through philosophy. The disciplinary criterion Geiger employs does not seem satisfactory, however, at the time there was no distinction between the several "social sciences," which renders problematic the distinction he makes between "all-out sociologists" and "authors renowned in adjacent disciplines". Geiger 1981: 349.

8 Raoul de La Grasserie (1839–1914) was born in Rennes and belonged to the ancient nobility of Brittany. His career as a magistrate did not prevent him from doing intellectual work – indeed in impressive ways, since he published more than 200 works in his life. Contemporary dictionaries present him in emblematic lines – as a psychologist, sociologist, writer, linguist, jurist, and magistrate.

9 Jacques Novicow (1849–1912) was Russian, from Odessa, and was the son of a wealthy industrialist in the shipping sector, a business Novicow also managed. He studied law at the Odessa University. He was a member of the city's Chamber of Commerce and Provincial Council. A pacifist, he became involved with the Russian liberal movement and was a supporter of the European federation.

10 Maksim Kovalewsky (1851–1916) was a Russian intellectual, a historian, a former professor of comparative public law at the University of Moscow, and a correspondence member of the Academy of St Petersburg. In France, in 1901, Kovalewsky became one of the founders and vice-president of the École Russe des Hautes Études Sociales de Paris.

11 Gabriel Tarde (1843–1904). Graduated in law and built a career as a magistrate. Originating from Dordogne, he rose in the Parisian intellectual field with the help of friends and political connections. He was a professor of the École Libre des Sciences Politiques and the École des Hautes Études Sociales. He was elected to the Collège de France and to the Académie des Sciences Morales et Politiques.

the family business. Originating from an intellectual environment that was not very professionalized, their intellectual practices were akin to those of French jurists with careers in the upper levels of the state bureaucracy who, in their position as “free intellectuals”, were unaware of the new academic rules of the French university system. Raoul de la Grasserie, Gabriel Tarde and René Worms originated from families of noteworthy locals and, despite the accumulated intellectual capital, especially by Grasserie and Worms, their careers were guided much more by social and political values than intellectual ones, which indicates the predominance of inherited or acquired social capital over other types of capital. Despite not having a doctorate, Tarde went far in his intellectual career and was the only one among them to be elected to the Académie de Sciences Morales et Politiques and to the Collège de France.¹² For this group, participation in the *Revue* represented an opportunity to present oneself as a *savant* or to accredit oneself to a position at a prestigious intellectual institution.

In turn, the group of historians and philosophers – with less participation in the *Revue* but of fundamental importance in terms of intellectual legitimacy – was composed of educators and textbook writers. The most active among them were Émile Levasseur¹³ (nine articles), Guillaume-Léonce Duprat¹⁴ (eight articles), Henri

12 Tarde had support from powerful political and intellectual groups such as Eugène Fournière, Raymond Poincaré, Ernest Lavisse and Louis Liard. See the letters from Espinas to Octave Hamelin, *Manuscrits de la Bibliothèque de la Sorbonne* (MS 356.1), the letters from Dick May to Eugène Fournière at the Institut Français d’Histoire Sociale [IFHS 181(2)], and the letters to Gabriel Tarde in the Archives d’Histoire Contemporaine at the Fondation Nationale de Sciences Politiques.

13 Émile Levasseur (1828–1911). A *normalien* and a doctor of history. He was a lyceum teacher and later a professor at the Collège de France, at the École Libre des Sciences Politiques, and at the Collège Libre des Sciences Sociales, while also being connected to the circles of liberal economists and to the Statistical Society, where he worked as a reformer of public statistics. He had a hand in reforming the teaching of geography in elementary schools, wrote handbooks for teaching geography, economics, and statistics, and was a member of the Conseil d’Éducation Nationale. Member of the Académie des Sciences Morales et Politiques.

14 Guillaume-Léonce Duprat (1872–1956) was a doctor of literature, a teacher of philosophy at the Rochefort Lyceum, and a professor at the Université Libre de Genève. Besides being a collaborator, he became assistant director and proofreader of the *Revue internationale de sociologie*, where he published articles on education, morality, and political doctrine.

Hauser¹⁵ (five articles), Alfred Fouillée¹⁶ (four articles), and Marcel Bernès¹⁷ (four articles). All of them had *agrégation* titles in philosophy or history and doctorates in letters; all except Duprat were *normaliens*. Moreover, they were members or associates of the Institut International de Sociologie and some participated in the meetings of the Société de Sociologie de Paris. Duprat and Bernès were also published by Giard & Brière, which indicates that the publisher, who brought out the work of seven of the ten main *Revue* collaborators, served as an instrument of intellectual accreditation for newcomers.¹⁸ Their interest in Worms's organizations was tied – in the case of the younger generations (Duprat, Bernès, and Hauser) – to intellectual accreditation to a university position. However, among them only Hauser went on to become a university professor in France; Duprat became a professor at the University of Geneva, and Bernès at the Lycée Louis-le-Grand in Paris. Duprat was the most assiduous member in Worms's institutions; he became not only a collaborator, but a critic and editor at the *Revue* – which is significant, as he was the only one not to have been a *normalien*, and needed therefore to accumulate other types of capital. In the case of the older and already consolidated generations (Levasseur and Fouillée), the interest was to maintain control of the reproduction of the faculty and the dominant position of philosophy and history (with their own methods and practices) in the French educational system.¹⁹

Despite the social and intellectual diversity among these two groups, there are confluences between their institutional destinations. Levasseur, Fouillée and Tarde were or became members of the Académie des Sciences Morales et Politiques; Levasseur and Tarde were professors at the Collège de France and

15 Henri Hauser (1866–1946) was Algerian, but moved to Paris in his youth. He attended the École Normale Supérieure and became a professor of history and geography at the universities of Clermont-Ferrand, Dijon, and at the Sorbonne. A historian of economics, work, and education, like Levasseur, Hauser was elected to the Sorbonne with Lavissee's support in 1919, at the age of 53.

16 Alfred Fouillée (1838–1912) had a trajectory marked by pedagogical concerns, and, like Levasseur and Hauser, a long career as a writer of school textbooks. Born in the countryside, he came to Paris with the help of teachers and politicians; with an *agrégation* title in philosophy and a doctorate in letters, he became a professor at the École Normale Supérieure, until retiring precociously and starting to live off the writing of philosophy primers for secondary school. Member of the Académie des Sciences Morales et Politiques

17 Marcel Bernès (1865–1946) is a *normalien*, the son of a *normalien*, and the grandson of Victor Cousin's successor at the Sorbonne. He built a career as a lyceum teacher. Interested in questions of pedagogy and morality, he taught at the École des Hautes Études Sociales and published works on education.

18 Fouillée and Levasseur did not publish works in Worms's collection because they were renowned writers who had more prestigious publishers at their disposal, such as Alcan and Hachette.

19 Concerning the positions of Fouillée and Levasseur in the course of the discussions on the secondary education reform, see Ringer 1992.

the *École Libre des Sciences Politiques*; and Worms, along with Kovalewsky, participated in the foundation of the *École Russe des Hautes Études Sociales*, an institution with ties to the *École des Hautes Études Sociales* founded by Dick May, an intermediary figure fundamental in the reformative networks.²⁰ Several of these authors (Bernès, Hauser, Tarde and Kovalewsky) taught or lectured at Dick May's schools, and also published by Dick May in the social sciences collection of Félix Alcan (Goulet 2008). Tarde played an important role in the relative cohesion of the Wormsian institutions, as he brought literati and jurists, free intellectuals and professors closer together. His correspondence reveals the existence of strong ties with several of the *Revue's* collaborators. He frequently corresponded with Novicow, Kovalewsky, Fouillée, Worms and Grasserie. Fouillée arranged his election to the *Académie de Sciences Morales et Politiques*; Grasserie referred to him as "master" and requested his support for the position of correspondent of the *Académie*; Novicow treated him as a "great friend".²¹ He became one of the most active and prestigious members of these institutions. What is interesting, from a doctrinal viewpoint, is that all of them were willing to make their theories converge and showed themselves not only sympathetic to Tarde's psychology but admirers of it.

During that period, such groups shared values and intellectual practices typical of reformative circles; thus it made sense to take part in Worms's organizations, whose logic was to close the gap between social science and practice. This example allows us to attest to the degree of proximity between Wormsian institutions and reformative networks: the nomination of Ferdinand Buisson and Léon Bourgeois, radical politicians, but also reformers, educators and publicists, to the presidency of the *Société de Sociologie de Paris* (and of the latter also to the vice-presidency of the *Institut International de Sociologie*) in the first decade of the twentieth century. The impetus to converge, morally speaking, the actions of educators, employees and politicians, beyond their specific professional interests, is recurrent in the pages of the *Revue*.²² There was, therefore, an inclination on the part of Wormsian institutions towards collaboration between political and intellectual

20 Gutnov draws attention to the proliferation of free schools across Europe during this period, and to the particularly ideological aspect to their foundation in Paris. Cf. Gutnov 2002.

21 The intention here is not to hypostatize such bonds, much less state that they were of an affective nature. On the contrary, it is known that René Worms himself, as Secretary General of the *Revue*, was not well-regarded by its members. Gabriel Tarde, for instance, considered him "ambitious" and of dubious character. It is hard to measure the extent to which that image, also shared by academic circles, compromised the *Revue's* projects and, in a way, disqualified its collaborators. See Salmon 2005 and Halévy 1996.

22 Worms's statement regarding the overlap of various roles in the same public figure. "[Professors] do not limit themselves to the sphere of private interests, but also willingly concern themselves with all people from their cities, from their nations, from humanity itself. This is the reason why many professors were able to play an important role in Parliament. The *Société de Sociologie de Paris* exemplifies this with its current president". Worms 1908: 204.

roles, between moral ideals and social science, summed up by the figure of the public intellectual. To celebrate such figures was also a strategy to legitimize their own intellectual position according to social demands and, in that way, respond to the attacks of the intellectual right concerning the supposed inability of the specialist to take part in public life.²³ Hence the distrust, in those circles, towards the figure of the specialized university professor, sometimes referred to as a “pedant” or “passive” for his supposedly contentedness with the *science* of social life and his unwillingness to subordinate that science to practice and to moral ideals. That view led to strife with Émile Durkheim’s practices and conceptions of social sciences, even more so from the moment that the *Revue*’s own concepts were subject to intense repudiation in the pages of *L’Année Sociologique*.

Criticism of the Members of the *Revue* by the Durkheimians

The Durkheimians offered a sizeable contribution to the disputes with the *Revue* concerning the legitimate definition of social sciences, since they decided, strategically, not to collaborate with or take part in the Wormsian institutions. In 1894, Durkheim stated in a letter to Mauss that he would never participate in a journal whose director had no academic title and who had the reputation of a fraudster to boot (Besnard and Fournier 1998: 35–6). Some of the future collaborators of *L’Année Sociologique*, such as Célestin Bouglé, François Simiand and Paul Lapie, published articles in other journals, but not in Worms’s *Revue*. None of the more active Durkheimians took part in any of Worms’ institutions, a practice of differentiation and isolation of university positions in relation to mundane institutions.²⁴ This practice is consistent with *L’Année Sociologique*’s criticism of the conception of social sciences disseminated by the *Revue*, even if there was no “school” doctrinal homogeneity among Durkheimians. Durkheim seems to be referring to Wormsians when he states, in the preface to the first issue

23 Bourgeois’s nomination is justified by Worms as follows: “In your person, precisely, we hoped to honour simultaneously the man of science and the man of action. When it comes to social science, you contributed to its promotion with your fine book about solidarity When it comes to social action, you are involved with it in all sorts of ways, all of them useful. Do you not currently preside over ten associations, dedicated to social education, to mutualism, to patronage, to the struggle against tuberculosis?”. Worms 1906: 210.

24 It is important to highlight, as shown by Besnard, that the Durkheimians were far from being a homogeneous group, unified under the doctrinal leadership of Durkheim. The letters between Durkheim, Bouglé and Lapie indeed reveal that diversity, expressed by Bouglé’s and Lapie’s interest in psychology. On the other hand, the limits of such diversity also become clear in their correspondence, as Durkheim reprehends Bouglé for regarding Tarde as a representative of a “specific sociology”, since Tarde would deny the specificity of sociology. Gaston Richard is one of the collaborators who are most critical of Durkheimian sociology and, therefore, most sympathetic to Tarde’s ideas, breaking with Durkheim in the first decade of the twentieth century. Cf. Besnard 1979: 21–3 and Karady 1979.

of *L'Année*, that “there are still many sociologists who frequently dogmatize about law, morality, and religion with casual information or even under the exclusive light of natural philosophy” (Durkheim 1898a: I). *L'Année*, unlike the *Revue*, seeks to promote the specialization of sociology, as, according to Durkheim, the primitive stage of knowledge in the field would not allow the formulation of a “general sociology”. However, this specialization should be guided by the rules of a sociological method, which would constitute the unity of the “social and moral sciences”.²⁵ Now, such guidelines opposed those of Worms’, according to whom a monograph should follow the methods specific to each discipline, leaving to a “general sociology” the task of performing a philosophical synthesis of the social sciences based on psychology.

Concerning the relationship between psychology and sociology, Durkheim’s collaborators, such as Bouglé, Lapie and Parodi, at first defended the dependency of sociology on psychological facts. Those statements were formulated based on the opposition between an “internal” and an “external” dimension to social facts, and on the idea that “external factors only become social causes when they are interpreted by consciousness” (Lapie, 1979 [1895], 35; Lapie, 1895). However, starting in the second half of the 1890s, their conceptions of sociology began to converge with Durkheim’s. This may be due to both institutional and intellectual factors. It should be kept in mind that *L'Année* was recruiting collaborators in the same period when Durkheim was defining his own position in relation to psychology. According to him, sociology could be denominated “social psychology” as long as it was understood as a *sui generis* social reality above individual consciousness; in that sense, it would be an autonomous science in relation to individual psychology and could never be identified with “social psychology” as conceived by Tarde and the *Revue*’s collaborators.²⁶ Meanwhile, Durkheim sought to reject biological explanations in the social sciences, as well as to break off the alliance between scientific psychology and social psychology that Théodule Ribot favored in the pages of the *Revue philosophique*.²⁷

25 “Methodological rules are to science what rules of law and morality are to conduct. They direct the thinking of the scientist just as the latter govern the actions of men .. But the jurist, the psychologist, the anthropologist, the economist, the statistician, the linguist, the historian – all these go about their investigations as if the various orders of facts that they are studying formed so many independent worlds. Yet in reality these facts interlock with one another at every point. Consequently, the same should hold for the corresponding sciences”. See Durkheim 1984 (1893), 303.

26 “We see no objection to calling sociology a variety of psychology, if we carefully add that social psychology has its own laws which are not those of individual psychology”. Durkheim 1951 (1897), 312. Cf. Durkheim 1898.

27 Théodule Ribot’s support for Tarde’s election to the Collège de France and the Académie des Sciences Morales et Politiques demonstrates the existence of such an alliance. Cf. Letters from Ribot to Tarde. GTA 92. Furthermore, Tarde ventured into an intellectual career through the means of Ribot’s *Revue*, a journal in which he published a great number of articles. See Consolim 2008.

Bouglé was one of the first to acknowledge and move towards Durkheimian sociology under this formulation and, as a result, to criticize the collective or social psychology promoted by the *Revue*.²⁸ Criticism by *L'Année Sociologique* followed Durkheim's guidance in attacking biological, cosmic, and psychological theses typical in such milieus, as well as in the reasons presented for that rejection, based on the supposed lack of scientific legitimacy of biological or spiritualist conceptions in social sciences and, specifically, of the theoretical and methodological eclecticism that the *Revue* practiced.²⁹ The various critical reviews can be classified according to their length and quality, since, for Durkheim, the *Revue*'s collaborators displayed very different levels of intellectual significance: short descriptive statements for works of minor importance and more extensive critical analyses in the case of others. The works of Fouillée, Bernès and Hauser were target of longer critiques, while the ones written by Worms, Novicow, Kovalevsky, Grasserie and Duprat were received with brief comments. There is no doubt that Tarde was the author Durkheim respected the most, the only one with whom he engaged in his works and articles, while the others had his attention only in *L'Année*.

The criticism, as a whole, becomes meaningful if seen from the point of view of Durkheim's wider project: it is about defending a new science and a specific method against those who sought to reconcile methods and explanations of different orders. The reviews in *L'Année* spared no ironies, branding the works of the *Revue*'s collaborators as literary, contradictory, and eclectic. In regard to philosophers and historians, the criticism was aimed mainly at their spiritualist and even metaphysical conception of social facts, as well as their "dialectical" method, that is, the attempt to explain social facts through the means of general or philosophical ideas. Fouillée's psychology has no conceptual density, and is lacking in empirical research; his collective psychology is "eclectic", "self-contradictory", "literary", "outdated", and "idealistic" – as suggested by the expressions "a people's character", "national character" and "French spirit". Bernès's *Sociologie et Morale* is a popularizing handbook, and it is "normative", since it confused the observation of facts with judging them based on a moral person. Furthermore, the "subjective" study of moral ideas by the great philosophers might have philosophical interest, but not sociological, since there is no proof that they represent collective feelings. Duprat's psychosociology is "sketchy" and does no more than indicate the "efficient causes" of social facts; the author is "dialectical"

28 "[Durkheim] does not exclude psychology completely from sociology; he demonstrates that, since the representations that comprise social life are original syntheses of individual representations, it is through a psychology, more specifically a collective one, that science should be constituted". Bouglé 1898: 155.

29 "Beyond the ideology of psychosociologists and the materialistic naturalism of socioanthropology, there is a place for a sociological naturalism which sees specific facts in social phenomena, and tries to explain them with zealous respect for their specificity". See Durkheim 1898, 302.

and does not use the proper method to uncover the social causes of the issues he wishes to study. Hauser believes that sociology should be a “philosophy of social sciences” and his conception of social science is “eclectic” and “antiscientific”, because he intends to gather and compare the methods and postulates of history, philosophy, law and economics despite the specificity of social facts. In regard to jurists or economists, the criticism was aimed mainly at their organic conception of society, that is, the explanation of social facts through the means of direct analogies to the natural body, as well as the contradiction between that postulate and the sociological individualism practiced by such authors. Novicow is “deductivist”, “metaphysical”, and “eclectic”, since one should not intermingle biology, psychology, and sociology. Grasserie’s bio-psycho-sociology is also “generic” and “eclectic”. Finally, Worms is “self-contradictory” because he mixes “organicism” with “individualism”. His most important work, *Philosophie des sciences sociales*, is merely a “good manual, an elementary popularizing work” (Lapie 1895, 1903, 1904 and 1909; Simiand 1897, 1987; Durkheim 1902; Lapie 1898 and 1902; Fauconnet 1902; Bouglé 1902, 1903, 1904 and 1907).³⁰

The critiques of the *Revue*’s collaborators by the Durkheimians are contemporaneous with the debate between Tarde and Durkheim, and they lasted around a decade. In the debate with Tarde, Durkheim attacks the emphasis on the “singularity” of the individual and of history in the name of social regularities: he criticizes the idea of individual consciousness or collective mentality in the name of the notion of “collective representation”: opposes the coercive force of institutions to Tarde’s theory of imitation. In this regard, Durkheimian sociology refused to reconcile the traditional social disciplines and to “clarify” them through psychology, as Tarde intended, and sought to reorient them in the direction of the sociological method (Lukes 1984; Besnard 1995; Consolim 2008 and 2011a). The reviews of Tarde’s works that appeared in *L’Année* were not any less forceful. Bouglé was unrelenting towards the so-called Tardean “ultra-individualism”, that is, his supposed inability to draw a distinction between social or strictly sociological relations and individual or psychological manifestations and, furthermore, to notice the difference between utopia and history, between moral ideals and social reality. Lapie also criticizes the fact that Tarde does not draw a distinction between sociology and psychology, since Tarde considered the sociological sphere “nothing more than the psychological propagated by imitation”. Fauconnet, in turn, rejects the idea that Tarde’s *psychologie des foules* could explain the origin of “collective representations”, since the mentality of the crowd would be simply a transitional phenomenon between individual consciousness and collective representations. Thus it would be “paradoxical to claim the name of collective or social psychology [for sociology] while there is a vast field of strictly collective or social phenomena” (Bouglé 1898 and 1899; Lapie 1898; Fauconnet 1900, 165).

30 The only author *L’Année* limited itself to praising was Émile Levasseur. Although Simiand states that it is a generic history work with a political and contingent framing, generally speaking he praises the quality of *Histoire de la classe ouvrière*. See Simiand 1987.

We can conclude that *L'Année's* criticism was relatively consistent, and that it pointed out theoretical or methodological issues that represented fundamental divergences in relation to Durkheimian sociology. This means that, in that period, the diversity among some of *L'Année's* main collaborators did not manage to erase the core distinction between sociology and psychology. The criticism can be summarized around three main aspects. First of all, it is a critique of eclecticism aimed at authors who intend to combine biological, cosmic and psychological factors in their explanation of social facts. Concerning strictly psychological explanations, it is a critique of the notion that collective ideas or feelings are entities autonomous from social institutions, and that their explanations are based on individual empirical consciousness or an abstract notion of human nature. Finally, it is a critique of the subordination of science to immediate practical needs, which leads to a confusion between the study of morality as a social fact, on the one hand, and the moralist judging those facts, on the other.

The Union and the Responses to the Durkheimian Group

The main theories espoused by Worms's journal were organicism, adopted by René Worms and his Russian collaborators, and the social psychology of Alfred Fouillée and Gabriel Tarde (Geiger 1981). Those were rival theories in principle, since psychologists considered society to be nothing more than the sum of its parts or a collection of individuals, while organicists postulated that a society was an organic reality above individuals. Furthermore, organicists based their sociological explanations on the notion of a universal "human nature", while Tardean psychologists highlighted the imitation process or the transmission of "beliefs" and "desires" from the socially superior to the inferior. However, an analysis of the published articles shows that the journal actually reconciled that which was theoretically irreconcilable.

In response to the attacks by *L'Année*, the *Revue's* collaborators started to highlight "psychological" positions and align themselves with Tarde's social psychology as opposed to Durkheim's.³¹ In the letters exchanged by Tarde and Novicow, there is a sort of agreement between the former's psychological or psychosocial theory and the latter's "organicism".³² In their turn, Paul de Lilienfeld

31 This constitutes a disagreement with Mucchielli, according to whom many authors of the *Revue* changed sides to support Durkheim after the congress of the Institut Internationale de Sociologie congress, in 1897, and the Congrès des sociétés savants, in 1898. See Mucchielli 1998. This was a point of view disseminated by a review of Bouglé's about the congress of the Institut in 1903 on the relationship between psychology and sociology. Bouglé 1904.

32 See the letter from Novicow to Tarde in 1900: "We are in perfect agreement: intercerebral psychology, you assert (p. 5) is certainly not the whole of sociology ... It

and Jacques Novicow, overt organicists, pressured Worms for adopt psychology as the foundation of the social sciences. While to Worms biological, cosmic, and psychological factors are still able to account for social phenomena, now psychology became the most important factor: it was up to psychology to “study the mentality of a superior individual” in order to understand how it projects and imposes itself over the rest of society, but also the “mental constitution” pertaining to certain social groups – that is, their “collective soul” (the spirit of a nation, of a race, of a family etc.). Imitation explained this process, since there could only be “individual representations”, but no “collective representations”. In 1907, Worms recognized that Tarde’s theory was very similar to his own and that the difference was really much more terminological than theoretical: the concept of “opposition” for Tarde would be equivalent to “struggle”, and the process of “imitation” was derived from what he would call “association” (Worms 1902–1907).

Kovalewsky, a critic of Tarde’s theory of imitation, according to whom social transformations occurred in virtue of an “adaptation” to institutions and to local beliefs and were not due to the imitation of a model, states that “sociology cannot have a foundation other than psychology” and that Tarde had been the author who had best understood the nature of this “collective psychology”, which is referred to as *Völkerpsychologie* by the Germans (Kovalewsky 1904).³³ Grasserie, in his turn, ascribes to sociology the study of “instinct” and “abstract facts”, while psychology is responsible for studying people and their character, mentalities and the products of thought, such as actions and intellectual creations (Grasserie 1902).

Despite the specificity of each one of these positions, it is possible to affirm that the members of the *Revue* intended to confine cultural or “mental” studies to a social or collective psychology and, therefore, to limit sociology to the study of natural (instinctive) phenomena or to the elaboration of an abstract philosophy (general philosophy of the social sciences). This strategy was intended to postulate the relative autonomy of mental or psychological phenomena in relation to institutional ones, as well as derive social institutions from beliefs and desires characteristic of a given social group.

It is not by chance that Duprat stated, in a review for the *Revue*, that Alfred Fouillée opposed Durkheimian sociology more than any other theory, since it seemed to him a sort of “moral materialism” discrediting the legitimacy of

[involves], besides intercerebral actions, all intercorporal actions ... and all of man’s actions over nature and of nature’s over man. Indeed!” CHEVS-GTA 88.

33 “I believe you will still tells us many true and useful things, and that you will end up converting all of us to your philosophy, which is so humanitarian and pacific”. Letter from Kovalewsky to Tarde, CHEVS-GTA 91. Despite all the criticism from Kovalewsky, Tarde highlights the agreement between the two of them: “You, Mr. Kovalewsky, are in perfect agreement with me in the sense that at the heart and in the origin of any evolution there is a change in the collective psychology, in the state of the soul of a people, the source of all manners of being and acting: does this not mean he recognizes that everything comes *ab interioribus ad exteriora*?” See Tarde 1904, 267.

philosophy on this issue. Fouillée published his works on the “psychology of the peoples” at the end of the century, in which he adopts the expressions “collective mentality” and “national elite” in opposition to the explanation through “social institutions” and against Durkheim’s concept of the “collective consciousness”. He argues that the “national character” should be represented by the “natural elite” of a country, whose ideas and feelings are expressed through “psychological signs” — language, religion, art, poetry and heroes — and not by the “crowd” or the “opinion of the majority”. According to him, the national character is formed by a collection of ethnical (biological and psychological heredity), geographical (physical environment) and psychological (moral) features of a people, although the last are the most important and the only ones capable of guiding a general synthesis of the social sciences. Fouillée makes wide use of Tarde’s psychology: individuals act upon and influence one another, unilaterally or reciprocally, through a process of suggestion from person to person, in such a way that a nation can be defined as a group of beings who imitate one another (Fouillée 1898).

Bernès, in turn, is indignant at the supposed Durkheimian reduction of morality to “objective” factors and defends a “general sociology” with philosophical and subjective foundations. Sociology should include as an object collective “beliefs”, “feelings”, “ideas” and “aspirations”, topics studied by idealist psychology (Bernès 1895a and 1895b). Among historians, Levasseur states that all the social sciences are tributaries to “psychology”, because “it is always the brain that conceives the directive idea and determines the action” and, thus, it is the “state of mind of a people that explains its institutions and its politics” (Levasseur 1904). Hauser, in turn, publishes in the *Revue* a classification of the social sciences where “social psychology” features as one of the disciplines, but sociology does not. In his famous book *L’Enseignement des sciences sociales*, he affirms that it is possible to do social science without “resorting to the dangerous hypothesis of a collective consciousness”, since individual consciousness is an explanatory factor of social facts. According to him, Tarde attempted to create a social science without drawing on any “mythology” or “ontology”, as was the case with Durkheim (Hauser 1902, 87). Duprat is the most ambiguous character in this group, in 1898 he showed himself to be openly favorable to Durkheim’s sociology to the detriment of Tarde’s psychology. However, he gets closer to “collective psychology” after he begins publishing at Worms’s *Revue* and, from then on, proceeds to state that one must ally psychosociology with sociological studies, since the greater part of feelings and representations have a mixed or psychosociological character. He also admits the role of “imitation” in the origin of collective feelings and mores, as well as the role of “individual invention” and of the “intellectual elite” of any country in the origin of its social progress. The concept of “social solidarity”, which he had initially borrowed from Durkheim, acquires an increasingly idealistic, individualistic, and elitist meaning (Duprat 1898, 1899, 1900, 1902, 1907).

The critical reviews of the works of Durkheim and of *L’Année Sociologique* published by the *Revue* are revealing the same impetus: they consider Durkheim’s theory incomplete and suggest its “complementation” both by the organic theory

as well as by Tarde's psychology. The image that is projected of Durkheimian sociology is that of a reductionist or speculative theory, incapable of being sufficiently scientific, like psychology, and, on the other hand, one that is reductionist with regard to the complexity of the individual's mental (and creative) life (Worms 1893, 1895 and 1898; Worms and Bochart 1898–1908).

It can therefore be concluded that, although with different dispositions regarding their professional situation, both groups of the *Revue* shared the same conception of the "social sciences" as a set of distinct disciplines and methods and, furthermore, adopted psychology as the foundation of the social sciences. This "psychological consensus" can be summarized around four main topics: 1) the adoption of some aspect of Tarde's theory of invention and imitation; 2) the defense of individual or collective psychology in the origin of social facts; 3) the emphasis on ideas (or on "collective mentality") as an explanatory factor and a factor of social solidarity; and 4) the importance of the elites (or of great men) in history as opposed to the crowd.

Conclusion

It is important to stress that this is not to affirm the existence of a supposed "Tardean school" constituted around Worms's organizations; but it is necessary to understand the impetus for a "psychological" convergence as part of a set of dispositions in each of the groups in question and, moreover, as a *position-taking* typical of an increasingly dominated *position* in the French intellectual field which sought to legitimize itself by means of a more prestigious subject than "sociology". Putting on modernist and scientific airs but in fact being deeply moralist and conservative regarding the methods and the conception of the social sciences, the supporters of a collective or social psychology became representatives of a set of traditional intellectual values and practices. Furthermore, the approach of "social psychology" was better suited to republican values and to the pedagogical role of the social sciences in "educating for democracy". In this sense, the *Revue* adopted a stratifying strategy, refractory as it was to the progressive autonomization of the many elites at the time; at the same time it fought against the division of intellectual labor on behalf of the "solitary creation" of ideas or theories and its influence in producing a social cohesion, even if their own practices and ideas were sociologically determined by pressures internal and external to the intellectual field.

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Chapter 5

From Communicative Memory to Non-History – Czech and Polish Narratives of Sociology’s Past

Jarosław Kilias

In this chapter I intend to analyse books on general history of sociological thought written in two neighbouring countries, Poland and Czechoslovakia. The histories of Polish and Czech sociology were somewhat similar, although there were important differences between them, too. Both nations lacked their own nation-states until the end of the First World War. At that time the Czech lands were well developed and urbanized, while Poland was dominated by rural population. Czech elites were mostly of petty-bourgeois origin, while the Polish were dominated by former gentry; the Polish intellectual public was (and still is) much narrower in proportion to the whole population than the Czech one. The founding father of Czech sociology is usually considered to be Tomáš Garrigue Masaryk, a professor of philosophy at the Prague University and a politician who eventually became the first president of independent Czechoslovakia. Polish sociology was short of such an important figure in the pre-war period. After the First World War both sociologies underwent ample institutionalization and both attained a good average level, although Czechs lacked a figure comparable to Florian Znaniecki, a scholar of international intellectual recognition (for more on this topic see, for example, Szacki 1995, Kraško 1996, Kilias 2000). After being suppressed during the Nazi occupation, the discipline seemed to flourish in both countries after the Second World War, only to be extinguished in the 1950s. Later on, the development of Czech and Polish social science lost its synchronicity, as in Poland sociology soon returned to universities and most of the older scholars participated in its renewal, while Czechoslovak sociology was recovering very slowly and the older non-Marxist scholars were excluded from it, except for a brief period in the late 1960s. After the suppression of the Prague Spring sociology was subjugated once more (Voříšek 2012). On the other hand, from the early 1960s Polish sociology functioned quite well, having been the only sociology in the Communist block with such extensive contact with the Western science. As a result, it attracted interest of Western scholars, while for the Czechs it functioned as a kind of “surrogate West” until the late 1960s and again in 1970s until mid-1980s (Kilias 2001). Only after 1989 did the Czechs once more rebuild their social science, while Polish sociology lost its privileged status.

More varied works on the general history of the sociological thought were published in Czechoslovakia, and later in Czech Republic, than in Poland. The first of them was Emanuel Chalupný's *Dějiny sociologie* ("History of Sociology"). A lawyer by education and a practising attorney, for a brief period an active politician, Chalupný (1879–1958) was an *enfant terrible* of Czech intellectual life, who participated in countless public arguments (Pecka 1999, Voráček 1999). Already in 1916 he had started to write a multi-volume system of sociology, and he continued to work on it until the enforced end of his academic career in early 1950s. Its second volume was to be the history of the discipline. Chalupný divided it into two parts, and while the first one had appeared already in 1922, the second one was published quarter a century later, in 1947 (Chalupný 1922, 1948). Before it was issued, another book of this kind had appeared in the late 1930s, a translation of Pitirim Sorokin's *Contemporary Sociological Theories* (Sorokin 1936, 1928).

Post-war Czechoslovak sociology did not manage to produce any similar work until the 1980s. In 1967, a Slovak translation of Polish Jan Szczepański's book appeared (which, owing to the linguistic similarity was understandable for both Czechs and Slovaks), and it served as a basic textbook for over ten years. In 1981 two books (or rather booklets) of domestic origin were published, one dealing with the Marxist, and one with the Western "bourgeois" sociology (Vaněk 1981a, 1981b). Their author was Antonín Vaněk (1932–1996), perhaps the most notorious figure of Czech sociology in the 1970s, who was at that time the head of Prague University sociology department (Petrušek 2004, Kulatý stůl 2004). His books were full of declarations of the Leninist orthodoxy; their structure was somewhat odd and the quality definitely below standard level. For example, the volume on "bourgeois" sociology contained a chapter entitled "Pitirim Sorokin's concept of the history of sociology" which summarized (or plagiarized) Sorokin's book, retaining Sorokin's original citations (Vaněk 1980b: 76–7)! Fortunately for Czech students, a translation of three worthwhile Soviet volumes on the history of sociology became available only slightly later (Konstatintinov 1981, Komarov 1982, Kon 1983).

Another Czech book on the general history of sociological thought appeared long after 1989. It was *Dějiny klasické sociologie* ("History of Classical Sociology"), written by a member of the middle generation of Czech scholars, Jan Keller (born 1955) (Keller 2004). Met with enthusiasm, Keller's book went on to become the basic Czech textbook on classical sociology. What is interesting is that, at the same time as *Dějiny klasické sociologie*, the same publishing house, Sociologické nakladatelství, issued a translation of a short French history of sociology written by Charles-Henry Cuin and François Gresle (2004). Recently Miloslav Petrušek (born 1936), an esteemed teacher of the post-transition generation of Czech sociologists, together with his postgraduates, published a concise book entitled *Dějiny sociologie* ("History of Sociology") (Petrušek 2011).

The history of Polish general histories of sociological thought started somewhat later, and the first publication of this kind, was a short, popular *Klasycy socjologii* ("Sociology Classics") written by a non-academic, Aleksander Hertz (1933). The

first fully-fledged work of this kind appeared in 1961, being republished twice, in a slightly shortened version, in 1967 and 1969. The book, entitled *Socjologia: Rozwój problematyki i metod* ("Sociology. The Development of its Issues and Methods") was written by Jan Szczepański (1913–2004), a former student of Florian Znaniecki. What is interesting is that the first version of *Socjologia* was published as early as 1953, during the time when sociology was officially ousted from universities, and was then entitled *Burżuazyjne doktryny socjologiczne 19 i 20 wieku* ("Bourgeois Sociological Doctrines of the Nineteenth and Twentieth Centuries") (Szczepański 1953). Although its title evoked Stalinist critiques of Western "bourgeois" sociology, it was a decent work which required relatively few corrections (including removal of some Stalinist phrases used merely as ornaments) when it was published eight years later.

In the same year as the Szczepański's book a curious translation appeared – the first (of the original two) volumes of Becker and Barnes's *Social Thought from Lore to Science* (Becker, Barnes 1952, 1961). One year before its official publication, a typewritten translation was circulated and used as a textbook at Warsaw University (Becker, Barnes 1960). One might wonder about the usefulness of such a crippled edition, missing its most important third volume containing information about the factual development of sociology. Anyway, owing to its politically unacceptable content, its publication would not be possible in Communist Poland.

One of persons responsible for the translation and the Polish introduction to the book was Jerzy Szacki (born 1929). Together with Leszek Kołakowski, Bronisław Baczko and Andrzej Walicki, he belonged to a group of scholars sometimes called the Warsaw School of the History of Ideas (Sitek 2000). In 1979 Szacki published an English edition of his own *History of Sociological Thought*, and then in 1981 the two-volume, extended Polish version entitled *Historia myśli socjologicznej* (Szacki 1979, 1981). The book has played an exceptional role in Polish sociology, as it is, up to this day, the valid, proper classical sociology textbook – for most younger Polish scholars the one and only source of knowledge about the history of sociology. In 2002 a new, broadened and somewhat revised edition was published (Szacki 2002). Two years later, the Polish edition of George Ritzer's *Classical Sociological Theories* appeared, which seem to be a moderate publishing success (Ritzer 2004). In addition, a local publisher from Poznań published a meagre translation of a German textbook by Hermann Korte (2003).

Although some translations played an important role both as textbooks and as models for domestic writing, my interest here is only in the latter. Here we are dealing with seven books (nine separate volumes) written by scholars belonging to different generations at various stages of the development of sociology – small wonder that their content and forms varied too. Nevertheless, despite slightly different titles, all seven works dealt mostly with a similar topic – the development of "grand theories", not with sociology in general, nor with its central problems and methods mentioned in the title of Szczepański's book. The only exception were individual chapters with slightly different content – in Chalupný's, Szczepański's and Petrusek's cases dealing with the institutional development of the discipline

in selected countries; the Petrussek's book contained also a chapter on the history of sociological research. Only two works, the first volume of Chalupný's book and Szacki's work contained any detailed description of pre-sociological social thought, while Szczepański and Keller started their stories with selected Enlightenment and pre-Comtean thinkers. On the other hand, only in the two latest Czech books was the other temporal limit of the narration set: for Keller it was the First World War, for Petrussek the early 1950s and the supposed Parsonian break; in both cases the demarcation line was identified as the end of the classical sociology period (Keller 2004, Petrussek 2010).

Narrations on the history of philosophy, sociology and other similar academic branches are usually structured by a mixture of two factors, chronology and systematics. In some instances their content is organized in a different, geographical and/or national way: theories and concepts are presented as supposed "national schools", or simply grouped by the country of origin. The highest level of structure in the books examined seemed to be basically chronology, while the level of individual parts and chapters describing various theories was dominated by systematics. Geographical or national factors as a means of arranging narration appeared only in the two of them – in a few chapters of the second volume of the Chalupný's work, and in one of the final chapters of the first edition of Szczepański's *Socjologia: Rozwój problematyki i metod* (removed from the second and the third edition of the book), dealing with the recent development of sociology in selected countries. I will return to the question of the meaning of particular narrative structures later.

A characteristic feature of most of the histories of sociological thought was gradation of the thinkers and ideas presented. Those considered the most important, mostly assumed to be the most original and influential, were usually presented in separate chapters or subchapters, while the less prominent authors were usually relegated to a paragraph of text and presented not as original thinkers "on their own", but only as representatives of broader theoretical currents. As for the number and the character of concepts presented, a comparison of the seven Polish and Czech books gives an impression of one-way although not linear development (or regression, if you wish). The number of authors and theories described decreased, and the gap between the "first class" scholars who deserved more and more elaborate descriptions and all the rest, at best condemned to brief notes, increased. Nowadays most of the former are thinkers from a limited and more or less standardized, narrow set of sociological classics (cf. Connell 1994), many of whom have been incorporated into the theoretical tradition of sociology quite recently (like de Tocqueville and to a degree Marx). For example, in the second volume of his history, while dealing with post-Comtean sociology in the nineteenth century Chalupný not only mentioned but wrote at least a few sentences about over 100 scholars. Szczepański wrote chapters or subchapters on 19 authors (or teams of authors); a dozen or so deserved at least two pages, and a several dozen others were mentioned. Although the final edition of Jerzy Szacki's book was twice as big as Szczepański's (and much broader in its scope,

covering longer periods of time and including numerous non-sociologists), only 40 scholars merited separate chapters or sub-chapters, while all the other authors mentioned merited at most a sentence or two. Jan Keller considered 12 authors worthy of separate chapters or sub-chapters, and only 13 deserved any description longer than one or two sentences.¹ For Petrušek and his co-workers, the respective numbers were 12 and over 40.

Two exceptions from the general trend were Hertz's and Vaněk's books. The former, concentrating mostly on just a few sociology classics and with only seven "first class" characters, resembles literature from much later period, while the latter seemed more similar to (plagiarized) Sorokin than to most of the contemporary literature.

There was another shift discernible in the content of the works. They were mostly composed of only loosely connected chapters on separate subjects; two basic narrative frames organizing stories they tell were nevertheless distinguishable. The first one derived from the original Comtean idea of sociology as a definitive outcome of the scientific revolution and the peak of modern science – the application of the (universal) scientific method to the most complicated of all possible study subjects: society. The history of sociology told that way was a romance (in the sense of White 1973: 8–9) of sociology growing up to be the genuine science – a variation and a component of a more general meta-narrative of human progress. Such a plot fitted well what happened from the time of birth and perhaps during the early stages of the development of the discipline, but became increasingly problematic when – despite the passage of time – the hoped-for scientific maturity still remained at best a distant goal. No wonder that since the 1960s the second, in White's terms probably ironic (*ibid.*) and somewhat relativist, narrative frame prevailed, which presented sociology as a self-reflection of the modern society. Still, in Polish and Czech histories of sociological thought, the narrative of the development of the scientific sociology survived a surprisingly long time, and even Jerzy Szacki resorted to the romance of a rationalization process leading to the development of a more scientific sociology (in fact he only presented such an idea in the introduction to his *Historia myśli socjologicznej*, not being able to utilize it as a means of description of the development of post-Comtean sociology).² As a result the only books that evoked the concept of sociology as a self-reflection of modernity were the two latest Czech books, Keller's and Petrušek's.

Basically, Polish and Czech general histories of sociological thought dealt with exactly the same concepts and theories as other books of this kind published at the same time elsewhere. Still, there was one specific problem which all authors

1 On the other hand, his *Dějiny klasické sociologie* contained an appendix consisting of brief biograms of almost 100 "sociologically relevant" thinkers.

2 The narrative structure of Vaněk's two volumes was that of the typical Marxist-Leninist story, the erroneous bourgeois story and the true Marxist one, the latter in the end winning over the former, a structure not unusual perhaps for Soviet-type interpretations but certainly not common gospel in the Czech and Polish context.

from peripheral countries writing the general history of sociological theory must face – the necessity to deal with domestic sociological heritage. Unfortunately, there were but few Polish or Czech authors who received some international acclaim. Apart from Florian Znaniecki there was only a small number of less important figures who mostly worked abroad and wrote in foreign languages. Anyway, there was no longer a place for any of them in the shrinking canon of classical sociology, and the absence of established domestic thinkers was an embarrassing fact for most of the local sociologists. There were three possible strategies of coping with this problem: separation, natural inclusion, and the one I call “but in our country”. The first, separation, resulted in writing separate histories of the general/universal and the local sociologies, or at least abstaining from describing the latter when writing about the former. The second natural-inclusion strategy was based on the incorporation of some domestic sociologists, but only where they fitted the narration and to some extent played a limited role in international sociological life. Obviously, this strategy was easier to apply to earlier histories, which dealt with many more scholars from different countries. The third, “but-in-our-country”, strategy strove to describe both the international and the local sociology, including as much domestic work as possible. There were two basic ways of doing so: either describing the local reception of foreign theories, or finding their domestic analogues. This was somewhat double-edged, because although it gave more space to the presentation of local academic work, it put it into comparative context, continuously confronting it with the central (that is, Western) science.

Most of the authors of the books analysed opted for the strategy of separation, writing only about international sociology and respectively publishing separate works on the domestic and the foreign work (which was the case of Vaněk, who also published a few books on the history of Czechoslovak sociology). Chalupný and Szczepański decided in favour of natural inclusion, while Szacki was the only one who selected the “but-in-our-country” strategy, providing information about many scholars from Poland (excluding those who definitely dropped out from the limited classical canon, such as Leon Winiarski), and stressing the Polish origins of some others (such as Ludwig Gumplowicz and Bronisław Malinowski). Szacki also described the local versions and reception of such intellectual currents as the Enlightenment, romantic historiography and Marxism. What is important is that this strategy aimed mostly at domestic readers, as some passages, including the whole chapter on Florian Znaniecki, were missing from the first English edition. The same can be said about Polish reviews of the work that expressed satisfaction at the fact that, with the publication of the English version of the book, which contained such a detailed description of Polish sociological thought, it at last received long-deserved international acclaim (Walicki 1980: 98–99, Kwaśniewicz 1982: 295). The application of such double standards, generating different communication styles for the domestic and the foreign publics, together with the longing for praise or appreciation from the more prestigious centre appears to be a

sign of the peripheral discourse (though not necessarily peripheral status) of Polish sociology (Zarycki 2009: 137).

A characteristic feature of Chalupný's, and of Petrušek's work, absent in their Polish analogues, was the presence of chapters dealing with other Central-East European sociologies. The first author presented a detailed overview of the development of social science in Europe as a whole (Chalupný 1948: 224–36, 419–30), while the second wrote a brief chapter on Polish and Russian sociology (Petrušek 2011: 194–211). This way, the Czech authors put Czech social science into the context of a whole group of more or less comparable regional social sciences. Such an unpretentious perspective was certainly a more effective means of avoiding distressing confrontation with Western science than was the highbrow attitude of Polish scholars.

In a series of papers, including 'On the Theory and Systematics of Sociological Theory' published in 1967, Robert K. Merton noted that most sociologists writing on the history of sociology confuse it with the systematics of sociological theory. One of the reasons for this was the lack of professional competence in the field of historical research (Merton 1967: 2). A possible way of answering the question of the historical character of Czech and Polish writings on the history of sociological theory is to examine the professional competence and interests of their authors. They belonged to three different categories. Three of them, Hertz, Szczepański and Petrušek were indeed sociologists (not always by their formal education) without any specific historical training or experience. Still, the rest of them had some historical education or at least practical experience. Chalupný was interested in historical issues and wrote some historical books, while Vaněk specialized in the history of sociology. Jan Keller is not only a historian by training, but even wrote his PhD thesis on Max Weber's concept of history (Jan Keller 2011). On the other hand, despite his PhD topic, his works reveal a total lack of interest in any factual history (see for example Keller 1995, 1997). Jerzy Szacki is a special case, being the only professional involved in genuine historical research – in fact he is a historian of ideas of international acclaim. As for the books analysed, all the authors except Jerzy Szacki completely disregarded theoretical and methodological issues concerning writing about the sociology's past, including the question of the selection of material. They simply wrote about what they felt to be important. A notable exception was again Jerzy Szacki, who published some papers on methodological issues in the history of sociology (for example, Szacki 1991), and whose work contained an elaborate theoretical introduction. Surprisingly, this did not seem to influence his research and literary practice. Although his *Historia myśli socjologicznej* included some reflections on the selected perspective and method and the criteria used to select the description of subjects, his solutions were more or less the same as those of his more naïve colleagues: describe anything that seems important to a contemporary sociologist (Hinkle 1979: 205).

Most of the books on the history of sociological thought were written by authors who were not professional historians and therefore lacked methodological awareness. Hence, it is probably more fruitful to examine them as a form of

disciplinary social memory than dealing with them as if they were genuine historical writings. For this purpose, I am going to turn to ideas of a German Egyptologist and a prominent student of social memory issues, Jan Assmann (1992). Dealing mostly with ancient cultures, Assmann conceived concepts that seem useful when tackling with professional memories of sociologists. Assmann distinguished two forms of collective memory – the communicative and the cultural one. The former is based on oral communication and is created, disseminated and sustained by direct interactions between individuals, being a spontaneous recollection of shared memories, and not a result of any systematic reflection. The functioning of the latter is a result of institutional practices performed by experts, in most cases serving political power. Such memory often takes the form of foundational stories, legitimizing those in power (and/or their institutions), but may also serve as an alternative history, used as an instrument for criticizing the existing institutional order. Communicative memory is a short-term one. It contains only the memories of one generation, usually covering a period of no more than 40 years, while cultural memory may deal with phenomena from the distant past, including the mythical past. As a result there are phenomena that do not belong to any collective (or individual) memory, falling into a “floating gap” between the two forms of memory (Assmann 1992: 48–56).

Assmann’s theory also enables us to address the question of historicity to be addressed. Dealing with ancient Middle Eastern cultures, the author touched upon the old distinction between the historical peoples and the people without histories. Introduced as an intellectual means of asserting European exceptionalism, for a long time it was used in a normative way as an expression of a supposed difference between the civilized West and the savages without history. Assmann resorted to its somewhat more up-to-date version – to Claude Lévi-Strauss’s distinction between two variants for dealing with the past, the “cold” and the “hot”. The former locates the absolute past outside the normal time continuum, the foundation of a supposedly eternal social order, while the latter presents it as a never-ending series of occurrences, which represent the historical change of human life forms and social institutions. Assmann’s argumentation was neither Eurocentrist nor evolutionary. He argued for the strategic use of memories which any group could opt for. Unlike the French anthropologist, he treated them not as evolutionary stages, but as possible memory strategies, and in his opinion any group could build its memory in this or that way (Assmann 1992: 66–86).

Sociologically speaking, social memory functions in a different way from, say, memory of ancient Egyptians, Jews or even nineteenth-century Poles. As an academic field, sociology is based on written records. Thus its memory simply cannot be exclusively oral, and the very existence of the communicative memory reproduced via face-to-face interactions seems questionable. However, in modern society almost all communication includes at least some writing. On the other hand, discussion on current scholarly production somewhat resembles face-to-face interactions, even if it includes a broader public and appears in part in written form. Some features specific to the cultural memory, including the fact

that it is generated and reproduced by specialized institutions, as well as used to legitimate/subvert power, are normal for sociology, divided as it is into various sub-disciplines and reproduced by formal educational institutions. Moreover, a glance at narrations on the history of sociological thought reveals that they indeed involve two different styles of describing past phenomena, which resemble Jan Assmann's communicative and cultural social memory. The floating gap between them was no less discernible.

Most of the content of the analysed books belonged to cultural memory. There were some exceptions though: the final passages of the first edition of Szczepański's *Socjologia*, some chapters of the Vaněk's books and the final parts of Szacki's work dealing with contemporary social science concerned phenomena which definitely belonged to communicative memory. Furthermore, the status of some fragments of the second volume of the Chalupný's was at best ambiguous.

There are a few distinct features to the narration on concepts and theorists who still belong to the communicative memory. Owing to its non-institutional character, it is neither standardized nor unified, and much less systematic and organized, so it is decidedly harder to organize narration about it. Phenomena belonging to the communicative memory are too near to be ordered chronologically and seldom fit established systematics, so it is easier to arrange them by the extrinsic, geographical (or national) factor. No wonder that Chalupný and Szczepański used it for chapters dealing with up-to-date, or almost up-to-date social science – and they were the only chapters of their books organized that way. Any historical description poses the issue of the selection of phenomena that are to be described: which are important enough to be described, and which should be omitted? In the case of the communicative memory, any traditional solutions of this problem are lacking, and each decision appears arbitrary. The problem of choice and anxiety connected with describing phenomena belonging to the communicative memory is clearly discernible in Szczepański's and Szacki's books, containing rather sketchy chapters about them. Anxiety involved in dealing with the "floating gap" is also easily discernible in at least some instances. The history of the subsequent editions of Szczepański's book may serve as an example: while the first one contained a chapter on contemporary sociology, it was removed from the second and the third, limiting the scope of the work to the cultural memory. A similar strategy of strict separation was also used by Hertz, Keller and Petrušek, who decided to deal with older sociological thought only, which belongs incontrovertibly and exclusively to the cultural memory. On the other hand, Vaněk and Szacki decided to continue the story up to contemporary sociology. Preparing the revised edition of his book, the Polish author even extended its scope, moving the end of his story towards the present: instead of Parsons and Lazarsfeld, the last edition of his work ends with an overview of the theories of postmodernity.

Probably the most interesting example of the "floating gap" were the two volumes of Chalupný's book, the second published after an interval of 25 years. The first one ended with Comte and it suggested that with his work was the beginning of the development of contemporary sociology. Yet when the Czech author managed

to publish his second volume, he saw things differently. In this volume, Comte was considered a scholar who merely started the preparatory period of the discipline formation, while the stage of development that deserved to be called “first building period” began only after the publication of Herbert Spencer’s *The Study of Sociology* and *The Principles of Sociology*, and the “second building period” at the beginning of the twentieth century. As Chalupný ended his story with the turn of the century, more recent sociology remained beyond his scope anyway.

Without any doubt most of the content of the books was cultural memory, which enabled twofold, “cold” or “hot” mnemonic strategies. Identification of strategies used by their authors may eventually provide some answer to the question of the historical character of stories they told. To do this, one must discern the character of the time in which the stories were embedded. Owing to the character of the literature on the general history of sociological thought, it is by no means an easy task. The narrative structure of all the seven books was extremely ill-defined, as they were fragmented into separate and only loosely connected chapters. Sometimes it is even hard to say whether they tell any story at all. As for time, it is not easy to correlate them with any specific order in time, as they mostly contained descriptions of sequences of abstract theories providing only minimal (if any) information about their historical contexts. Consequently, any answer to the question of the character of the past described in all the books analysed is going to be somewhat arbitrary and subjective to a degree.

It seems that the three earliest Czech and Polish histories of sociological thought followed rather “hot” mnemonic strategies. The first volume of Chalupný’s work told the story of the creation of sociology as a truly scientific discipline dealing with human culture and society, which started deep in ancient Asia, to be finally fulfilled in the beginning of nineteenth century by Auguste Comte.³ The story told in the second volume was less clear, but was at least framed by Chalupný’s periodization of preparatory and two building stages, which turned it into, maybe not a history, but at least a chronicle (White 1980: 9–10) of scientific progress, which appeared to continue up to the time of publication of the volume. Both the creation story and the chronicle of progress belonged to a normal time continuum, structured by and embedded in more or less specific historical time, measured by facts of intellectual history, such as progression of intellectual currents or publication of particular books. Its historical character was stressed by the fact that the chronicle of progress appeared unfinished – it ended suddenly with the end of the nineteenth century. Consequently, some important authors and concepts were left out – probably saved for the unwritten third volume. The case of Hertz’s more selective narration is less clear, but the story he told was chronologically ordered and put in the sketchy yet specific enough context of intellectual,

3 Chalupný was eager to call any thinker he described “sociologist” and his story of pre-Comtean thought was so full of fundamental discoveries that somewhat mistrustful reader might perhaps ask why the emergence of scientific sociology had taken such a long time and had required such a genius as Comte.

social and political history. Like the Chalupný's work, Szczepański's book also contained the story of the establishment of scientific sociology, although it was much shorter and dealt only with events directly preceding the establishment of Comtean sociology, presenting both social thought and its social and political context. Extra-sociological background nevertheless disappeared after sociology had been established, making the subsequent parts of narration (which was, unlike the Chalupný's story, no longer any story of progress) less embedded in a specific historical time. Still, the time perspective of his book seems historical, not absolute.

Like his three predecessors, Jerzy Szacki also evoked the foundation story of the birth of sociology as the science of society. Like Chalupný and Szczepański he appealed to an extra-intellectual context dealing with the pre-sociological era and the formation of the new science, but otherwise it was marginalized in his work, as he even omitted the usual biographical notes. On the other hand, his narration was organized by chronological order of facts, starting from the ancient Greece and ending with contemporary sociological theories, which gives the impression of historical continuity and causality in the development of sociological theory. Still, the story contains some peculiarities, such as a suggestion that at the end of the nineteenth century an "anti-positivist break" in the development of sociological theory took place, best represented by German thinkers inspired mostly by the philosopher Wilhelm Dilthey, such as Ferdinand Tönnies, Georg Simmel and Max Weber (Szacki 1981: 469–538). In fact, an anti-positivist break in sociology did indeed happen, but over a half a century later, and Jerzy Szacki himself was deeply interested in debates that surrounded it – he even published a reader, *Czy kryzys socjologii?* containing some of the most important texts on the topic (Szacki 1977). Still, attributing it to German sociology of the end of the nineteenth century was an example of the mobilization of the past to fight contemporary theoretical battles – in short, a piece of a counter-present, mythical past.

The mnemonic strategy of Antonín Vaněk, who presented an untypical, dual story of the two-fold development of Marxist and bourgeois sociology, which would one day end with the victory of the former, was not entirely clear. His narration on Western sociology was simply chaotic, while the story of the Marxist social science was more or less chronologically ordered, and contained a number of repetitions of a sequence: political blunder; Party critique; return to the Marxist (Leninist) orthodoxy. Although this model of an ever-repeating past was probably not a typical myth, it still resembled a "cold" rather than a "hot" memory strategy.

The "cold" mnemonic strategy of Vaněk was typical for the intellectual history of East European Marxism in general, and not only for sociology. As for the latter, the elements of the "cold" strategy, barely visible in Jerzy Szacki's work, dominated the two latest Czech books. Both offered some information, in Keller's case quite detailed, on the biographies of thinkers, as well as about the extra-intellectual and supposed intellectual context of the theories analysed. Despite these features, which give an impression of the embeddedness in specific, historical time, the mythical aspect dominated the stories they tell. Both narrations were placed in a

strictly demarcated period considered the domain of the classical sociology, which for Keller ended with the First World War and for Petrussek with the Parsonian-Mertonian theoretical break. As a result neither the period, nor phenomena that happened that time, were connected with the present in any direct way, being in a sense excluded from normal time continuum. Still, both authors insisted upon a universal actuality of the classical sociology – the latter simply asserting its everlasting validity, the former resorting to the refined concept of sociology as a science dealing with the transition crises of modernity.

The two authors presented the current version of sociological foundation story – the story of sociology as a self-reflection of the modern society. Petrussek resorted to the simple Nisbetian formula of the intellectual reflection of two revolutions, the political (French) and the industrial one (Petrussek 2011: 10), while Keller proposed a more elaborate concept of the reflection of the succession and transition crises between the two (of the original three) modernization phases as defined by German social theorist Peter Wagner – the restricted liberal and organized modernity (Carleheden 2010: 63). The universal actuality of classical sociology was supposed to be the result of its ability to cope with transition crises, similar to the contemporary crisis accompanying the assumed transition from the extended liberal modernity to network society. Although his analysis pretended to be sociological, the Czech author carried out only an ideological analysis in the Durkheimian sense of the word. Instead of trying to discern factual problems that might imbue individual classical sociologists, who (probably) dealt mostly with their own, considerably dissimilar societies, he substituted them with Wagner's scheme. Naturally, the idea of the ever-actual classical sociology as an intellectual reflection of the long past, yet ever-important modernization has nothing to do with any history and turns out to be a myth.

The analysis of Polish and Czech narrations on the history of sociological theory reveals three development tendencies. The first is the significant decrease in number of theories presented, the broadening of the gap between “first class” thinkers and all the rest. While the former deserved more and more thorough descriptions, the latter was becoming ever more marginal, being at best mentioned by a name only, and only as representatives of broader theoretical currents. The second is the shift from the romance of the establishment of sociology as a genuine scientific study of society to the ironical narration on sociology as self-reflection of modern society. The third is the conversion from a type of “hot” to a “cold” mnemonic strategy, which resulted in the transformation of selected facts from the history of the nineteenth century sociology (in fact, not only sociology) into a piece of a long-gone but somehow ever-present past, a source of timelessly valid classical sociological theory. The idea of sociology as an instrument of intellectual reflection of modernity turned into a sociological foundation myth. What is interesting, the transformation of content and form of narratives on the history of sociological thought was by no means gradual. All the three changes occurred at once and rather suddenly (only Szacki's book appeared to be in a sense transitory), forming an entirely new type of story,

which probably should not be called “history” any longer, but rather classical sociology. As the phenomena described by classical sociology had already belonged to the cultural memory domain, their emergence cannot be explained by growing distance to the past phenomena or the usual way of functioning of social memory.

The Polish and Czech narrations on the general history of sociological theory turned out to be exactly like the literature written abroad, without any significant local peculiarities. Their content was similar, except for the selective inclusion of the domestic theoretical heritage in some of them, and the ways in which they were told was more or less the same. The only exception was the Marxist story written and published by Vaněk, who remained an isolated scholar and did not influence the local intellectual work. Consequently, the most important local feature was a considerable delay before the new style of writing the history of sociological theory was adopted: the first full-fledged classical sociologies were probably Raymond Aron’s *Contemporary Sociological Theories* (1976, first published in 1965–1967) and – in a sense – Robert Nisbet’s *The Sociological Tradition* (1973, first published in 1967), published already in the mid-1960s. This delay is not easy to explain. Without any doubt in the case of Czech social science, political factors were important. The renewal of sociology started in 1960s, only to be stopped by the neo-Stalinist “normalization” of the 1970s, which cut off most contacts with the West and started the academic career of people like Antonín Vaněk. The case of Polish sociology is less clear. Perhaps the most probable reason why classical sociology appeared so late was, apart from the relative isolation (economical and linguistic more than political) of the Polish sociological community, the exceptional status of Jerzy Szacki as the only established sociology historian in Poland who was expected to write such a work and whom nobody could contest.

The timing and the character of the transformation of the local histories of sociological thought seem important not only for Polish and Czech sociology, but shed a light on the establishment of classical sociology in general. Raewynn Connell attributed the transformation of the attitude towards the past and the style of description to the scientific revolution that changed the leading paradigm and/or the geography of post-war sociology (Connell 1997: 1535–1539). Such an interpretation is without any doubt correct, as the functioning of social memory alone cannot explain such a thorough change. Still, Polish and Czech social sciences underwent this revolution too, but there seemed to be no urgent need also to transform its past, and the appearance of the new view on the discipline’s past was seriously delayed. Even in the West the timing is somewhat questionable. Talcott Parsons’s *The Structure of Social Action* (1968, first published in 1937) is sometimes considered the first sign of the coming change. Still, if characteristic features of classical sociology are in fact: a restricted canon of sociological classics, ironic interpretation of the history of sociology as the intellectual reflection of modernity, and “cold” mnemonic strategy, only the first one can be attributed to Parsons’s work. Another face of the scientific revolution was the newly dominant

research method – mostly in the form of opinion polls – legitimized by neo-positivist methodology. But being probably the last incarnation of the old dream about the truly scientific sociology, neo-positivism hardly conformed with any ironic vision of sociology! In fact, the interpretation of the history of sociology as the intellectual reflection of modernity was developing only gradually, and its generalized version, pretending to be the definitive interpretation of the origins of sociology, appeared perhaps in Robert Nisbet's *The Sociological Tradition*. And does the ironic view of the history of sociology not fit better the other, interpretative revolution, which started in the 1960s?

The discussion on the uses of old sociological theory and ways in which history of sociology should be written has already been going on for a few decades. What is important here is that probably every aspect of the new type of writing the history of sociological theory has been criticized, including the very idea of sociology as the intellectual reflection of modernity. The same can be said about the idea of the everlasting canon of sociological classics, as well as supposed uses of the classical theory (for instance, Jones 1983, Baehr, O'Brien 1994, Connell 1997). What is interesting, although the arguments used against them appear to be quite substantial, is that they do not seem to have influenced the classical mainstream of sociology in any way. If my interpretation of Assmann's concept is correct, they will never do so, as they are misconceived and misdirected. Classical sociology does not seem to be a bad history – it is simply not the history of historians, and those who write it and use it are not really interested in the dull and trivial past. So maybe sociology is not so different when compared to other disciplines after all?

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PART II

Serving the Public or Serving the
State?: Trials and Tribulations of
Organizational and State-related
Histories

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Chapter 6

Research For Whom?: Changing Conceptions of Disciplinarity in the American University

Daniel Gordon

Introduction: From Student to Society

Recent calls for public sociology invite a naïve question: Why is being a college professor not considered public enough?

Why is it not public to teach a large number of young people at a university? Is not an academic course a public space, in the sense defined by Habermas ([1962] 1991): a space where individuals of diverse social backgrounds convene for discussion? And, since college students are destined to become active members of their communities as professionals and policy makers, does not elevating these future leaders count as a significant public function? Disquiet about the shortage of public academics at our universities suggests that the university itself is not a wide enough context for the fulfillment of important public functions. I seek to explain this malaise through a historical overview of the transformation of the American university from a student-centered college to a research university.

The academic disciplines, until the late nineteenth century, were conceived as tools for developing the mind and character of the undergraduate student. The “faculties”, or masters of a “discipline”, existed to “discipline” the student’s mental powers, or “faculties”. The double meanings indicate that the student’s cognitive development was the reference point for articulating faculty functions. This student-centered framework dissolved in the late nineteenth and early twentieth century. The change resulted, of course, from the growing emphasis on research; but more precisely, from the concept of research as an activity designed to benefit society as a whole rather than the students residing on the campus. The problematic nature of this social ontology underlying modern research is a special focus of the present essay.

The abolition of the principle of *in loco parentis* (the principle that the university serves as a substitute for parents) is of considerable importance. It facilitated not merely the freedom of the students but the freedom of the professors to minimize their interaction with the undergraduates and to cultivate research for external constituencies. The redefinition of the professoriate in terms of socially, or externally, useful research also fatefully impacted the hierarchy of the

disciplines. In the new academic regime, the merit of a discipline depends on its value in resolving what are, or are perceived to be, practical problems for society as a whole. No longer primary is the function of a discipline in cultivating a young person's cognitive faculties.

A few disciplines of doubtful utilitarian importance, such as English and history, have managed to preserve, or even enhance, their status in the new regime. Representatives of these disciplines have succeeded by preserving some of the older educational values and portraying their fields as the supreme incarnation of student-centered learning. English and history have carved out important spaces in the curriculum, notably in general education. History, English composition, and a literature course, for example, are usually included in the American college's core requirements. Other disciplines – and sociology is one of these – are torn between the old and the new. Devoted to the study of contemporary society, sociology has naturally tried to claim a place as a useful discipline. We will see how the brightest sociological luminary of twentieth century America, Talcott Parsons, framed the discipline as a “national resource”. But sociology in the United States has had difficulty moving off the margins of public utility. While sociological research is more inscribed than the humanistic disciplines in governmental and industrial research enterprises, it has never been regarded as so vital to the national welfare as engineering, physics, chemistry, biology, management, and economics. At the same time, because sociology is a modern discipline, it lacks a tradition of being central in student-centered or liberal education. The outcry for more publicly oriented sociologists may well express the need that sociology has for a third way between the student-centered and utilitarian conceptions of the university. The following discussion is an effort to thicken the historical contours of this analysis by presenting a series of key texts illustrating the shifting principles of disciplinary in American higher education.

The key texts are:

- The Yale “Report of the Faculty on a Course of Liberal Education” (1828). This source illustrates the student-centered conception of the disciplines. It also demonstrates that the disciplines were intertwined with the concept of the student as a child. The professor functioned *in loco parentis* and superintended the development of the young person's mental powers.
- Abraham Flexner, *The American College: A Criticism* (1908). Flexner was a leading proponent of the new model of higher education associated with the founding in 1876 of Johns Hopkins, the first American research university. Yet, he highlighted the contradictions between the research endeavour and student-centered education, while attempting to promote both. His analysis of the university's internal inconsistencies, in my opinion, has not been superseded. But the intellectual history of American higher education after Flexner can be understood as a series of efforts to resolve these inconsistencies.
- Statements on academic freedom by the AAUP, 1915–1940. These

declarations by the American Association of University Professors reflect an effort to eliminate the appearance of a misfit between college teaching on the one hand and unconstrained research on the other. The AAUP harmonized what Flexner considered contradictory by redefining the student as an adult who benefits from academic freedom as much as professors do.

- Proceedings of the Annual Conference of the AAU, 1906–1920. The papers from conferences of the Association of American Universities reveal that World War I stimulated a shift in the outlook toward research, decisively favouring the production of knowledge for the benefit of society rather than scholarly investigation for its own sake. The conference papers suggest that until World War I, the AAU idealized the professor who combines research with undergraduate teaching. After the war, the concept of the research professor, relieved of teaching duties, acquired legitimacy.
- Talcott Parsons and Gerald Platt's *The American University* (1973). Parsons and Platt argued that research is the “core” function of the professor. They contributed to the effort to unify the image of the American university. Ironically, they perfected the justification of the research complex by returning to a parental conception of the college professor. The professor is the student's father, but the role of the father in industrial societies, according to Parsons and Platt, is to be professionally engaged in the external world, not child-centered.
- Michael Burawoy's 2004 presidential speech on public sociology to the American Sociological Association. This text is an outcry against the utilitarian research complex that Parsons and Platt legitimized in *The American University*. However, references to college teaching are slight in the speech. I suggest that the concept of public sociology would be stronger if it put less emphasis on making an impact outside the academy and more emphasis on establishing relevance to liberal undergraduate education.

The Yale Report (1828) On Liberal Education

Yale's “Report of the Faculty on a Course of Liberal Education” (1828) was a response to criticism of the college's ancient languages requirement. The report defends the core curriculum, with its emphasis on Greek and Latin, by putting classics in the context of the college's prime aim: to develop the mental powers of young people; “What then is the appropriate object of a college?” ... “Its object is to lay the foundation of a superior education: and this is to be done at a period of life when a substitute must be provided for *parental superintendance*” (Yale Report 1828: 6, italics in original).

The pre-adult status of the student is axiomatic. The disciplines are tools for stimulating the young person's cognitive development. Liberal education is as “a course of discipline in the arts and sciences, as is best calculated ... to strengthen

and enlarge the faculties of the mind” of the budding student (Yale Report 1828: 30). The pre-adult nature of the student also provides the rationale for the residential basis of college life; “The parental character of college government requires that the students should be so collected together, as to constitute one family; that the intercourse between them and their instructors may be frequent and familiar. This renders it necessary that suitable *buildings* be provided, for the residence of the students” (ibid.: 9, italics in original).

In the Yale report, “discipline” does not refer, as it does today, to a body of knowledge cultivated by professors that is separate from instruction. The young student may be interested in our disciplinary work today, but the existence of this work does not presuppose the student’s prior existence. Thus, professors often distinguish their “own work” from the service they provide as teachers. In contrast, the Yale report delineates an academic world in which the student is not just a physical presence in the university but is the *raison d’être* of the disciplines. Each discipline exists because it calls forth a “vigorous exercise” of the students’ faculties (Yale Report 1828: 7). The term “faculty” or “faculties” is used in the report to refer both to the professoriate and to the mental powers of the student.

In a proper education, the disciplines complement each other to develop the student’s mind as a whole:

In our arrangements for the communication of knowledge as well as in intellectual disciplines, such branches are to be taught as will produce a proper symmetry and balance of character ... As the bodily frame is brought to its highest perfection, not by one simple and uniform motion, but by a variety of exercises, and adapted to each other, by familiarity with different departments of science. (Yale Report 1828: 9)

“It is necessary that *all* the important mental faculties be brought into exercise” (ibid.: 7; italics in original). The basic faculties are reason, imagination, and memory, though the report refers in passing to other aptitudes such as “taste”, “the art of speaking”, and “the inventive powers” (ibid.: 8–9). It is not clear if these are subsumed by reason, imagination and memory or are separate faculties. But it is evident that no discipline is part of the curriculum if it does not provide vital exercise for one or more of the student’s basic mental powers.

Ultimately, the report justifies the study of Greek and Latin as meta-disciplines stimulating the development of all the faculties. In the study of ancient grammar, literature, and philosophy, “every faculty of the mind is employed, not only the memory, judgment, and reasoning powers, but the taste and fancy are occupied and improved” (Yale Report 1828: 36). From today’s standpoint, what is striking is not merely the idealization of a specific subject, classics, but the student-centered framework in which all the disciplines take on value. The report makes no reference to professors doing research for the betterment of society. This does not mean that Yale had no professors pursuing scholarship in private. But there was no disciplinary framework that gave research a public meaning apart from

teaching. The conception of the student as a child, combined with the “faculty” based psychology that informed educational theory of the time, made teaching the young, not creating new knowledge for the betterment of society, the matrix of the disciplines.

Flexner on the University’s Contradictions

Abraham Flexner (1866–1959) played a prominent role in moving American universities away from the primacy of the student-centered philosophy. Born in Kentucky, Flexner attended Johns Hopkins University at the age of seventeen and graduated in two years. Johns Hopkins, founded in 1886, was an alternative to the traditional liberal college. As Flexner stated in his autobiography, the purpose of Johns Hopkins was “to advance knowledge” (Flexner 1960: 27). The university’s first president, Daniel Coit Gilman, hired professors based on their promise as researchers. To promote the publication of new knowledge, Gilman founded the first American university press at Johns Hopkins in 1878. He also established *The American Journal of Mathematics* (1878), *The American Journal of Philology* (1880), and *Studies in Historical Political Science* (1883) (*ibid.*: 33). Johns Hopkins rapidly influenced other leading universities. By the end of the nineteenth century, most of the prestigious American universities were developing Ph.D. programs and hiring research oriented professors. Flexner reviewed the founding of Johns Hopkins with approval in his autobiography, but he expressed no nostalgia for his own education there. He simply recounts that there was little oversight of the undergraduate students (*ibid.*: 33–4). And he notes that students were allowed to graduate as soon as they passed competency tests.

Flexner went on to become a member of the Carnegie Foundation for which he composed in 1910 a celebrated report on the state of North American medical schools. The report led to the closure of numerous medical schools because Flexner condemned them for lacking research personnel and equipment. A prominent member of the Rockefeller Foundation’s General Education Board as well, Flexner was highly influential in selecting young Americans to receive extended academic training in Europe. The goal was to create a corps of American researchers in the natural sciences who would be equal to or superior to their European counterparts. Flexner also founded the Institute for Advanced Study at Princeton and served as its director from 1930 to 1939. The Institute is a refuge for top researchers in a variety of fields and frees its members from all but a few formal teaching duties.

However, there is another side to the career of this apostle of research, more tied to student-centered concerns. Prior to joining the Carnegie Commission in 1908, Flexner had a distinguished career as the principal of a college preparatory school in Louisville. Flexner based his school on small classes and personal attention to the individual student. His major at Johns Hopkins had been classics, and he was an outstanding teacher of Greek. He was in fact a fervent admirer of Matthew Arnold (Flexner 1960: 45, 54).

In 1908, Flexner published *The American College: A Critique*. He states that the American college is deficient in “pedagogical intelligence” (Flexner 1908: 11). The primary obstacle to excellence in undergraduate education is, according to Flexner, the newly emerged department system. Flexner regrets that the American college has adopted an organization consisting of “a large number of highly specialized and separate departments, each striving to be more or less completely representative of its own field” (ibid.: 29–30). With the spirit of disciplinary specialization came the decline of an overarching core curriculum for the student (ibid.: 30–1).

Flexner devoted a chapter (Chapter IV, The Elective System) to deconstructing the model of a curriculum based on a large menu of specialized courses determined by faculty research concerns. The college student is too young to create a coherent plan from such courses, and the elective curriculum itself makes no effort to instruct students in the “fundamental inter-relations” among subjects (Flexner 1908: 144–5). The professors are not able to serve as effective advisors because they must focus on research and graduate teaching. “He [the professor] is not as a rule qualified by his primary interests and concerns to do the delicate and tedious work of ‘advising’; he has not time for it. In the end it means nothing to him” (ibid.: 120). As for the quality of the courses, “An elective system in which the instructors are specialists exists for specialists. The presumption is that courses are chosen by those specially, not generally, interested in them. Of the broad fundamental concern conveyed by the term “culture”, no considerable account is taken” (ibid.: 139–40).

Flexner argued that the graduate school was exerting too much pressure on the undergraduate college. “The American college practically amalgamates the undergraduate and graduate departments!” (Flexner 1908: 162). But “despite continuity of subject matter”, there is “a decisive change of attitude” in the two levels (ibid.: 52). For the graduate program trains specialists, it is indifferent to the student as an individual; while the college teacher should view the student as “prospective man not simply professional man” (ibid.: 55–6; see also 168–9). This difference changes “the entire complexion of education” (ibid.: 56). “The graduate student is physically and mentally grown; the undergraduate is physically and mentally growing” (ibid.: 166). “Our universities have in general assumed that whatever promoted the interest of the graduate school promoted in equal measure the interest of the college. This was a dangerous assumption. It is still unproved” (ibid.: 179).

At the heart of the problem is the assumption that a single faculty can be devoted to both disciplinary research and college teaching. A single faculty “insidiously sacrifices the college” (Flexner 1908: 183). “Absorption in laborious investigation, on which the future of the instructor depends, is calculated to abate the appetite for routine college teaching” (ibid.: 183). College teaching should not be a “side issue” for professors whose passion is research (ibid.: 187). Flexner was aware of the argument that the best teachers must also be engaged in research activity. “But no one has yet explained why the minute investigations of

the modern specialist, fighting in close quarters for scientific or linguistic detail and consequent promotion, constitute him at that same moment, the best possible teacher of young students, earning and applying general principles” (ibid.: 183, note 1). The marks of a qualified college teacher are to be “broadly trained” and “broadly minded”. “His interest would usually lie in incorporating newly ascertained facts in their connections, in correlating, interpreting, and interrelating data and principles within and between various realms; scope, sympathy, an active pedagogical concern should distinguish him” (ibid.: 185–6). These are not the necessarily the traits of a strong researcher, according to Flexner, who would go on to handpick many of the beneficiaries of Carnegie and Rockefeller research grants.

Flexner critically scrutinized the value of “academic freedom”, which he called the “slogan” of the research professoriate (Flexner 1908: 189). Academic freedom permitted researchers to teach courses grounded in their specialized research rather than the intellectual needs of students (ibid.: 189–90):

The college soon becomes as “academically free” as the graduate school ... a newly-made Doctor whose personal fortune and interest lie elsewhere is appointed to teach college boys; in the name of academic freedom he is turned loose in the classroom to “apply his own theories and to follow his own bent”. He teaches what and “as he thinks fit”. As a result much of the instruction given to undergraduates is far too highly specialized in content and method of presentation to be adapted to any but expert use. This difficulty has been recognized ... it cannot be remedied except by a thoroughgoing reorganization. (ibid.: 189–90; critically quoting a Harvard report on freedom of instruction in the college)

Flexner also highlighted the “conflict of interests” between the college and the graduate school. He noted that financially the graduate school is dependent on the college, since the graduate complex is funded by undergraduate tuition. Since the resources of the college are not adequate to support both undergraduate and graduate education, the quality of undergraduate instruction suffers (ibid.: 174–5). According to Flexner, there is a need to insulate the college from the encroachment of research norms that are appropriate only in graduate school. Separating the college faculty from the departmental research faculty is a necessary step to protect the college.

Academic Freedom and the AAUP

Flexner’s analysis of the contradictions of the university was rooted in two suppositions. The first is that college students are youngsters whose instruction should conduce to intellectual and social maturation. They are not to be confused with graduate students, who are adults ready to be treated in accordance with

professional standards. The second supposition is that research professors are not well suited for undergraduate teaching.

With these suppositions in mind, we can map some of the intellectual moves that ensued in American higher education in order to eliminate the appearance of contradiction between college education and disciplinary research. The assumption that college students are children was neutralized by the affirmation of the adult status of college students. The processing of questioning *in loco parentis* is generally viewed as a legal battle that began only after 1950 (Sarabyn 2008: 49–51). But the reversal of the student's status began earlier. A series of documents from the American Association of University Professors (AAUP) intimates a relationship between the concept of faculty freedom, on the one hand, and the dissolution of the student's pre-adult status, on the other.

The AAUP was founded in 1913 in order to defend the faculty from administrators and politicians prone to firing professors for unorthodox academic views and political opinions. In 1915, the AAUP issued its Declaration of Principles on Academic Freedom and Academic Tenure. "Academic freedom in this sense comprises three elements: freedom of inquiry and research; freedom of teaching within the university or college; and freedom of extramural utterance and action" (AAUP 1915: 2). The AAUP argued that academic freedom is essential in a university because a university has three basic purposes defined as follows (*ibid.*: 5):

- a. to promote inquiry and advance the sum of human knowledge;
- b. to provide general instruction to the students; and
- c. to develop experts for various branches of the public service.

The AAUP claimed that free expression is vital for pursuing the first and third of these aims. But it conceded that within the teaching mission of the university, some limits on academic freedom are necessary. Hence, this extensive exclusion (*ibid.*: 8):

There is one case in which the academic teacher is under an obligation to observe certain special restraints – namely, the instruction of immature students. In many of our American colleges, and especially in the first two years of the course, the student's character is not yet fully formed, his mind is still relatively immature. In these circumstances it may reasonably be expected that the instructor will present scientific truth with discretion, that he will introduce the student to new conceptions gradually, with some consideration for the student's preconceptions and traditions, and with due regard to character-building. The teacher ought also to be especially on his guard against taking unfair advantage of the student's immaturity by indoctrinating him with the teacher's own opinions before the student has had an opportunity fairly to examine other opinions upon the matters in question, and before he has sufficient knowledge and ripeness of judgment to be entitled to for many definitive opinion of his own. It is not the least service which a college or university may render to those under its instruction, to

habituate them to looking not only patiently but methodically on both sides, before adopting any conclusion upon controverted issues.

In 1925, the AAUP amended its statement on academic freedom. The above exclusion for the immature undergraduate was reduced to one clause in one sentence:

A university or college may not impose any limitation upon the teacher's freedom in the exposition of his own subject in the classroom or in addresses and publications outside the college, *except in so far as the necessity of adapting instruction to the needs of immature students*, or, in the case of institutions of a denominational or partisan character, specific stipulations in advance, fully understood and accepted by both parties, limit the scope and character of instruction. (AAU (1925) 1954: 85; italics added)

In its 1940 statement on academic freedom, there is no exclusion at all for the immature student. The student is now configured as an analogue to the professor: both allegedly benefit from the maximum degree of academic freedom. "Academic freedom in its teaching aspect is fundamental for the protection of the rights of the teacher in teaching and of the student to freedom in learning" (AAUP (1940) 1954: 82). The student thus became interchangeable with the professor in the discourse of the AAUP. The AAUP statements suggest that a trend to diminish the application of *in loco parentis*, well before students protested its vestiges in the 1960s, was inscribed in an earlier trend, the professionalization of the research faculty.

The AAU on Teaching Versus Research

Flexner's critique of the American college was based on two suppositions. The first is that the student is a child. The second, that professors hired based on their research potential are not well suited for undergraduate instruction, was a major topic of discussion among university leaders in the early twentieth century. Flexner's views were unorthodox. The records of the annual conference of the Association of American Universities show that in the years running up to World War I, there was an official consensus that research enhances teaching. However, after World War I, this view began to dissolve.

In 1906, at the Seventh Annual Conference of the AAU, Stanford president David Starr Jordan delivered a lengthy paper on a panel entitled, "To What Extent Should the University Investigator Be Relieved From Teaching". The paper represents not only the speaker's viewpoint but that of numerous others, captured in a questionnaire sent by Jordan "to about a hundred leading university men" (AAU 1906: 30). Jordan states, "The American university is emphatically a teaching university" (ibid.: 24). "We cannot divide our men into research professors and teaching professors" (ibid.: 28). Teaching "clarifies the mind, broadens the view,

and saves from vagaries” (ibid.: 25). Teaching and research are “mutually helpful” (ibid.: 26). However, what Jordan regarded as “research” or “investigation” did not necessarily include publication. “We must give a broad definition for the word ‘research’. It is the appeal to first sources. Teaching from second-hand material is never good teaching” (ibid.: 23). “A voluminous bibliography does not necessarily mean a contribution to knowledge”. Jordan decries “the pressure to print” ... “Publication is a very poor test of research” (ibid.: 26).

The second paper on this panel was prepared by Yale President Arthur T. Hadley, and presented by professor Theodore S. Woolsey. This paper also affirmed the need to integrate teaching and research:

The men who are engaged in the development of new truth should be impressed with the fact that it is their duty to *teach* this truth, as well as to discover it. They should understand that research without instruction is as valueless as faith without works ... If you tell a man that he is set apart from others in a ‘research professorship,’ you encourage him to ignore these teaching duties. You seem to separate his services from those of his colleagues – to value their work for what they give to others and to value his for what it is in itself. (AAU 1906: 44)

Here again, we see that the concept of research has not yet acquired its present-day sense of work that is useful for society as a whole. For by suggesting that the professor devoted to research alone does not serve others, Woolsey implies that research is done out of intellectual curiosity and not to benefit society.

In the discussion following the panel on whether investigators should be relieved of teaching, William James observed that there was a “consensus of opinion” against relieving professors engaged in research from instruction (AAU 1906: 49). Of course, this consensus did not include Flexner. But it was the widely held view of AAU administrators.

The AAU’s 1918 conference included a session on “The Effect of the War on Education”. President Jacob Gould of Cornell spoke. “Now that peace has come I should think that our populations in all countries would realize that if pure science, if new discoveries, are valuable for war they also will be valuable for the solution of the great problem which are coming upon us and which are now upon the world in connection with peace” (AAU 1918: 62). Speaking on the same panel, James Rowland Angell, dean of the faculty at the University of Chicago, declared that the war had revealed how “the machinery of our civilization rests upon the foundations of applied science” (ibid.: 68):

The war has, in this, as in so many other matters, been a great schoolmaster. The aeroplane, the submarine, the machine gun, the heavy ordnance, the wireless, the mustard gas, the thousand and one devices for the wholesale slaughter of men, have called public attention as never before to the place occupied by science, both pure and applied, in the maintenance of the state and in the overthrow of its enemies. ...the war has been in large degree a struggle of wits... (ibid.: 68)

Angell, who in the following year became the Chairman of the National Research Council, continued:

The submarine called for scientific devices to detect its presence and destroy it ... The innumerable varieties of wound created new forms of surgical and medical treatment, many of which can happily be utilized for the relief of suffering even in times of peace. The mustard gas has elicited a neutralizing gas and the invention of still more horrible gases for attack, to say nothing of the new and terrible explosives which our chemists have devised. What science is there that has not been put under contribution at one point or another? ... the great dramatic achievements which have caught and kindled the public imagination have been those of applied physical science. In a sense, science has won the war, and many of its achievements can apparently be taken up almost unmodified and appropriated by our industrial and commercial organization for the purpose of peace. (ibid.: 68–9)

All of this cannot fail to leave natural science in apposition of educational pre-eminence such as it has never enjoyed before. (ibid.: 69)

Angell spoke in the following year, 1919, at an AAU panel on “The Organization of Research”. In this paper, Angell not only articulated the concept of research in utilitarian terms, as he had done in 1918; he also delineated research as a system, an organization whose value is distinguishable from the originality of the work done by individuals within it. Angell explains that research does not have to have a “marked originality” in order to be productive (AAU 1919: 28). He observes that there is a spectrum among researchers in a given field and at one end “the element of originality is reduced to the vanishing point” (ibid.: 28). “It would be a fundamentally wrong inference to assume that because the originality is small therefore the results which may be gained are of little value and that research of this character is to be discountenanced” (ibid.: 28). Angell states that we must look beyond individuals and think of research “as the organized technique of science itself for its own propagation” (ibid.: 28). Angell also suggested that the “the conjoining of teaching and research in American institutions is more or less a national accident” and “there is no necessary connection between the two” (ibid.: 32). He argued that researchers need more uninterrupted time.

In this way, Angell not only legitimized research, especially scientific research, as useful for society; he also legitimized *all* scientific research, regardless of its degree of originality, as part of the collective scientific enterprise that benefits society. From this perspective, society could benefit from relieving every scientific researcher from a measure of teaching. This may well be the theoretical basis for an important historical fact: the long-term trend across the twentieth century to reduce the teaching load of professors, be specially scientists, at American universities.

The impact of the war on conceptions of research is also evident in a panel devoted to the subject of “Research Professorships” at the 1920 conference of the AAU. Whereas president Jordan of Stanford, prior to the war, claimed to represent the common view that researchers should not be exempt from teaching, this panel reveals the growing attitude that certain researchers should be relieved of teaching. In an informal discussion, speakers from many universities described the kinds of professorships, exempt from teaching, that existed at their institutions. The discussion indicated, as one speaker noted, that “there are more research professors than we seem to think”. Some of the participants observed that entire departments and institutes functions with a faculty who had no teaching obligations (AAU 1920: 49–50). As David Prescott Barrows, the president of Berkeley, said, “We have several institutions which are solely research institutions. They have very few pupils” (ibid.: 50–1).

Parsons and Platt on the Research Complex

From the Yale Report of 1828 to the AAU’s meeting in 1920, a remarkable turn occurred in higher education. In the beginning, there could be no disciplines that did not serve students. By 1920, it was possible to imagine a proliferation of departments and institutes without students. The modern university thus came to house two separate structures, a college, and a research complex. However, efforts to unify the concept of the American university continued. Premier among these was the work on higher education by Talcott Parsons, much of it conducted in collaboration with Gerald M. Platt. In 1948, Parsons had submitted a report to the Social Science Research Council in response to a request by the SSRC to draft a “series of concrete proposals dealing with possible structures and procedures whereby the federal government might lend support to social science research” (SSRC committee cited by Klausner 1986: 6). The SSRC wished to convince government officials that the National Science Foundation should fund research in the social sciences, not just in the natural sciences. Parsons’ report was entitled “Social Science: A Basic National Resource”. Parsons ([1948] 1986: 81–99) dramatized how social scientists had rendered practical assistance to the government in the war and could make a vital contribution to national welfare in the post-war era.

The report was not just a plea for social science. It outlined a sociological theory of the role of the university in modern life. Parsons defined the university in terms of the production of new knowledge. Teaching, other than the training of additional researchers, did not enter into his definition of the university’s core function. “The Western World has created a powerful institutional framework for encouraging basic scientific research, training scientists, and promoting the applications of science by adequately trained personnel. The continued thriving of science and its contribution to social welfare depends on the health of the university framework” (Parsons [1948] 1986: 102). The university “provides

scientific activity with a professional framework” and confers “a status of respect and dignity on those who devote their lives the discovery of new knowledge” (ibid.: 102–3).

In the 1948 report, Parsons expressed no reservations about reducing the mission of the university to the production of socially useful knowledge. He did not consider the relationship of research to teaching in the report. However, Parsons did confront this question jointly with Gerald M. Platt, a sociologist at the University of Massachusetts Amherst, in their book, *The American University* (1973). This thought-provoking text portrays the research complex as an asset to the undergraduate college.

Continuity with the 1948 report is evident in the preface to *The American University*: “I had long decided that higher education, including the research complex, had become the most critical single feature of the developing structure of modern society” (Parsons, writing the Preface alone, in Parsons and Platt 1973: vi). The first two chapters of *The American University* amplify the ideal type of the university as the premier site of detached inquiry in the Western world: “The central core of the normative system of the university, which is embodied in its social organization, is the valuation of cognitive rationality” (Parsons and Platt 1973: 47). Research and graduate training comprise “the core sector” of the university and the “prototype of the primacy of cognitive rationality” (ibid.: 103).

The most creative component of *The American University* is its focus on the undergraduate college as an apparent anomaly, or what Parsons and Platt call a “boundary-structure of the main academic system” (Parsons and Platt 1973: 157). The college is a zone “where other values share primacy with cognitive rationality” (ibid.: 103). A socialization process, or “restructuring of commitments” (ibid.: 168), must take place because the student does not usually come to college in order to do research; the student has career goals and emotional ties to religion, political party, and other groups that are unrelated to the goals of intellectual discovery.

The key chapter addressing the tensions between the college and the research complex is Chapter 4, “General Education and Studentry Socialization”. Parsons and Platt addressed the same topic in a paper, “Higher Education and Changing Socialization”, published a year earlier, 1972. For purposes of establishing the theory of Parsons and Platt on the college’s relationship to the research complex, I will adduce both of these texts – Chapter 4 of *The American University* and the 1972 article. Parsons and Platt recognize that college students are not yet adults; hence, professors are more than researchers, they are also parental figures. Since the students do not yet have the “capacity for intellectual detachment beyond common sense in everyday life” (Parsons and Platt 1973: 141), the professors “have to socialize the students to internalize the value of cognition and to more specific disciplines and institutional values” (ibid.: 174; see also 141, 177). “Thus the college constitutes an extension to older age levels of patterns of the integration of the social system and of personality which have been found in studies of family socialization” (ibid.: 104).

Parsons and Platt thus revert to the *in loco parentis* model. But the custodial function they envision is designed to expose the students to the research complex rather than to immerse the professor in the college. The professor's primary role is always to research, "the purest embodiment of the value of cognitive rationality" (Parsons and Platt 1972: 254). Next comes graduate teaching. Undergraduate teaching ranks third (*ibid.*: 254). However, Parsons and Platt affirm, based on survey, that most researchers do not wish to give up teaching entirely; the typical researcher wishes to keep a foot in the college (*ibid.*: 254). The contribution of the professor to the undergraduate is to provide the student with a window to the research complex. Parsons and Platt view this role as sufficient based on their understanding of the specialized role of fathers in an industrial society: "The family and the college have undergone parallel types of differentiation in achieving their modern forms. In the family, the differentiation was in the form of extrusion of the economic functions from the household to the factory and the office ... The father became the wage earner ... From the point of view of the previous kinship system, he became *pater absconditus* who deserted his family ... The college teacher, qua faculty member, is also *pater absconditus*" (*ibid.*: 253–4; see also Parsons and Platt 1973, 182).

Parsons and Platt analysed student discontent in the 1960s as an immature reaction by the student against the formation a more aloof professoriate. The student disturbances are "reactions to patterns not yet fully institutionalized; once these are stabilized, the resultant disturbances may be expected to subside" (Parsons and Platt 1972: 241). This analysis turns student unrest into a transitory psychological phenomenon. The "tensions that have on occasion erupted into open conflict" are a temporary reaction by students who seek a kind of attention from the professoriate that is not, according to Parsons and Platt, in the students' best interest (Parsons and Platt 1973: 157). Parsons and Platt assess student attacks on the professoriate as "psychological infantilism" (Parsons and Platt 1973: 177–8), reflecting a search for a kind of intimacy with the faculty that is not possible or desirable. It is interesting to note that student radicals in the 1960s did complain about the remoteness of the faculty. Mario Savio, the leader of the free speech movement at Berkeley, stated (Savio 1965: 101):

They should supply us with more teachers and give them conditions under which they could teach – so they wouldn't have to be producing nonsensical publications for journals, things that should never have been written and won't be read. We have some magnificent names, all those Nobel Prize winners. Maybe a couple of times during the undergraduate years you see them 100 feet away at the front of a lecture hall in which 500 people are sitting. If you look carefully, if you bring along your opera glasses, you can see that famous profile, that great fellow. Well, yes, he gives you something that is uniquely his, but it's difficult to ask questions. It's got to be a dialogue, getting an education.

Parsons and Platt argued that the professionalized demeanor of the faculty provides the young with a resource more valuable than intimacy. According to Parsons and Platt's analysis of fatherhood in industrial society, the father acquires superior capacities by mastering industrial work: "Even unskilled factory workers developed upgraded skills, intelligence, and personal abilities in order to participate in the economy" (Parsons and Platt 1973: 183). "The unremitting hostility toward the father's absence from the family filled the novels of Kafka, Lawrence, and Proust and the psychoanalytic writings of Freud" (ibid.: 181). But the revolt is temporary: "Now that the father's occupational role external to the family has been stabilized (along with moral authority in the family linked with the occupational role) the expression of anger toward fathers has declined" (ibid.: 181). In the same way, undergraduate students benefit from being "exposed to academics who are involved in the higher reaches of the cognitive world and who act as role-models and socializing agents for these young people" (ibid.: 184).

In this way Parsons and Platt restored an appearance of consistency to the theory of the American university. By reaffirming the parental nature of professorial discipline, they revived the spirit of the early American college. But to avoid a split between the family-based conception of the college and the research complex, they argued that the professor's paternal role is fulfilled through work in the research complex.

Burawoy's Discourse of Public Sociology

Viewed in relationship to the above series of texts, the interest of Burawoy's 2004 presidential address to the American Sociological Association, "For Public Sociology", lies in the author's effort to rupture the relationship between sociology and the established research complex. At the same time, Burawoy's vision of public sociology appears to share an attribute with the object that he critiques, for he does not consider the possibility that sociology's primary service could be in serving college students. There is only one paragraph in his speech that is about teaching at all. Burawoy preserves the assumption that the discipline finds its highest meaning outside the university. Sociology should not be "hermetically sealed" and needs to expand its "extra-academic audiences" (Burawoy [2004] 2007: 33).

The quest for social impact produces a burden: to explain how a discipline can be of benefit to a public which, on the whole, is unsympathetic to the speaker's flamboyantly anti-capitalist thinking. The solution lies in denouncing society further for having "moved right" while sociology "has moved left" (Burawoy [2004] 2007: 25). This polarizing rhetoric only widens the chasm between public sociology, as Burawoy conceives it, and the empirical public. The speech becomes inflamed through the desire for a partnership between sociology and an unwilling public. Frustration gives rise to eruptions that are at times eloquent, at times intellectually suggestive, but not likely to constitute a supportive public.

Burawoy's denunciation of other academic disciplines further reduces the chances that public sociology, as he envisions it, will find allies, even among academics.

There is a pronounced "we" versus "them" feeling to Burawoy's speech. Burawoy presumes that opposition to capitalism is natural to the sociologist. He sidesteps philosophical considerations of the indeterminate relationship of every discipline, including sociology, to values. He proposes a "reflexive sociology" that promotes a "dialogue about ends" (Burawoy [2004] 2007: 34) but the anti-capitalist ends are already predetermined in his speech. Sociology, he affirms, should promote a debate about the foundations of university research programs and the basic direction of our society (ibid.: 34). Yet, if the conversation is truly open-ended, what qualifies sociology in particular to lead it? Why does critical debate about the academy's relationship to society fall under the jurisdiction of the sociologist rather than the historian, philosopher, economist, political scientist, or natural scientist?

Burawoy does not offer a voice to any of these disciplines in the conversation. He shows disdain for disciplinarity outside of sociology. The natural scientists promote "instrumental knowledge" and lack "critical reflexivity" (Burawoy [2004] 2007: 53). Economists view society in terms of "the market and its expansion". They propagate "market tyranny" (ibid.: 55). Political scientists focus on "political stability" and support "state despotism" (ibid.: 55). Only sociology views the world in terms of "civil society" and "the interests of humanity" (ibid.: 55). In reality, there are numerous critics of inequality and of capitalism spread across all the humanistic and social scientific disciplines. Civil society is not a category unique to sociology but is the common property of history, cultural studies, institutional economics, political science, and numerous other disciplines. Burawoy's insistence on the corruption of other disciplines misses an excellent opportunity: the opportunity to portray sociology as a partner with other disciplines in the humanistic enterprise of providing the best possible education for the college student.

There is just one paragraph in Burawoy's speech on students. A promising statement is, "With the aid of our grand traditions of sociology, we turn their private troubles into public issues. We do this by engaging their lives not suspending them; starting from where they are, not from where we are" (Burawoy [2004] 2007: 30-1). However, there is not enough discussion of teaching in Burawoy's speech to discern how public sociology makes a unique contribution to undergraduate education. Burawoy mentions service learning (ibid.: 31), but service learning is now a common component in many courses in a variety of disciplines, not just sociology.

In spite of this criticism, Burawoy speech is a valuable declaration of the need to restart the discussion of the purposes of higher education. If we are willing to hear Parsonian justifications of the primacy of the utilitarian research complex, why should we not consider radical criticism of it? There is a need to highlight the tensions between basic university functions, notably research and college teaching. Too much theory about the identity of disciplines is spun without

thinking about the role of disciplines in cultivating the student. The educational needs of college students, who are the largest group at every university and whose tuition payments also provide the biggest piece of funding, risk being sacrificed by the social passions of the professoriate. These passions include the passion for political struggle and social revolution as well as the passion for socially useful research. Alternating between political self-righteousness and technical expertise, the social commitments of the university risk overlooking its most important constituent, the student.

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Chapter 7

The Making of “Excellence” in the European Research Area: How Research Funding Organizations Work

Barbara Hoenig¹

Research funding organizations (RFOs) are crucial for organizing the resource flow between the state, society, and the scientific community. They perform a public role in providing for and deciding upon the distribution of financial and symbolic resources for research as in the provision of research grants. In so doing, they influence the social structure of science and contribute to the cognitive shaping of the sciences. Research grants provided by RFOs can be interpreted as symbols of scientific reward, reputation and excellence. They play a central role in the recognition of intellectual contributions in science, both in relation to the status of individual motivation of researchers and the legitimacy of the social institution of science. Moreover, grants are often a precondition of being able to undertake research at all. Our argument here is that a structural analysis that contextualizes the RFOs is crucial to understanding the recent change in the social structure of science, particularly in relation to its impact on sociology. In discussing criteria, conditions and consequences of a supranational RFO in the European context, we intend to apply Robert K. Merton’s structural analysis to the case of research funding in Europe. Situating that structure of research funding in its wider cultural and social context, we identify different groups of actors involved, their structural opportunities and constraints in making choices, the relevant mechanisms and finally the consequences of research funding for the scientific community. By applying these considerations to the case of research funding, we intend to contribute to a conceptual re-evaluation of structural analyses as the core competency of sociology and generate new knowledge relevant to an explanation of the social conditions of the discipline’s continuing intellectual activities.

The practice of evaluation and peer review of research proposals has recently gathered support within the social sciences, deriving, for example, from

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constructivist science and technology studies and proponents of a practice approach towards analyzing social knowledge (Camic et al. 2011; Lamont 2009; Lamont and Huutoniemi 2011). These approaches emphasize that making academic judgments is an at least in part subjective activity, where emotions, interaction dynamics and self-conceptions of social actors play an important role. They also assume that there are substantial field-specific differences in epistemic styles and forms of peer reviews among scientific disciplines. However, here we propose that a structural analysis of research funding can generate insights that are largely absent from these studies (ibid.). In addition to an examination of the practices of the upper stratum of the scientific elite in constructing “excellence”, it seems to us that the analysis of institutional contexts and organizational environments is crucial to developing an understanding of communal processes and outcomes in relation to the evaluation of knowledge by intellectual authorities. This also extends to the contribution to knowledge of lower level groups in the stratified scientific system. It also incorporates the scientific community’s relation to non-scientific public organizations such as RFOs that, via funding decisions, enact considerable influence on science. Our focus is restricted to the European context, which in important respects differs from other parts of the globe. In clarifying our approach we make reference to and refer to a particular RFO at *European*.

The Case of the European Research Council

“On 25 April, 5 p.m. Brussels time, the first deadline for a call for proposals for Starting grants expired and resulted in 9,167 applications. We celebrated this as a great success, a special mark of confidence in the idea of an ERC. Suddenly, from one minute to the next, the ERC was existing, was alive, and was recognized as one of the largest research councils in the world. Scientists from all around the world could apply, as long as they wanted to work in Europe. Now we have to deal with these numerous applications, on a first-class level. The review will happen in two stages, a first stage reducing this number to 500 and a second stage with a reduction to, maybe, 250” (Winnacker 2008: 128).

The European Research Council (ERC)² is the most important supranational RFO at European level and was called into life by the European Commission in 2007 (European Commission 2007a). Its central idea is to promote “groundbreaking frontier research” at the highest level of excellence in physical sciences and engineering, in life sciences, and in social sciences and humanities. Its funding structure enables individual researchers to apply for a portable long-term research grant of up to €2.5 million in order to undertake investigator-driven research for up

2 If not otherwise indicated, information on the ERC is based on communications from the European Commission (2007a, 2007b, 2007c, 2008, 2010) and on its Working Program 2012 (ERC 2010).

to five years. The grant system consists of Starting Grants particularly for young researchers up to 12 years after having received their PhD and Advanced Grants for senior researchers.³ Submitted proposals are evaluated by peer review solely on the basis of the criterion of scientific "excellence". Together with the aim of promoting groundbreaking frontier research, the shared belief in thorough and impartial peer review practices provides the grounding for much of the ERC's cultural legitimacy within the scientific community and the public at large.

According to the former ERC Secretary-General quoted above, the first ERC call in 2008 resulted in a success rate for submitted proposals of less than 3 per cent. Between 2009 and 2012 the average rate ranged from 10 to 12 per cent across all disciplines and was slightly lower in the social sciences and humanities. It is clear that the ERC selection process continues to be highly competitive. Since the beginning of the ERC more than 2,000 grants have been given to researchers all over Europe.⁴ In organizational terms, the ERC is a science-led funding body that is assured of full autonomy and integrity by the European Commission (European Commission 2007a). Its external relations are characterized by its mediating role between the European Commission and the scientific community. The ERC's political influence can be understood only in relation to that of the Commission as a political actor that delegates its competencies to it, while also codifying its tasks and roles (European Commission 2008). The ERC's economic underpinning is regulated by means of the multi-annual Seventh Framework Program for Research and Technology Development of the European Union valid from 2007 till 2013; about 15 per cent of that program or €7.5 billion is dedicated to the ERC. The distribution call budget for Starting and Advanced Grants in 2012 directed 44 per cent to physical sciences and engineering, 39 per cent to life sciences, and 17 per cent to social sciences and humanities. Accepting the allocation of the ERC grants as providing a new indicator of scientific reputation and European excellence, this raises a series of questions relating to criteria, conditions, and consequences of constructing "European excellence".

A Structural Analysis of Research Funding in Europe

Building upon the classical work of Karl Marx and Karl Mannheim, Robert K. Merton, as founding father of the sociology of science, formulated its paradigm by posing the question of how social conditions of knowledge production affect and interact with the content dimension of knowledge (Merton 1973: 12). While Merton's approach has been challenged by constructionist studies within the past

3 Since 2012 there are two additional grant types available, the Synergy Grants for groups of principal investigators, and the Proof of Concepts Grants aiming at the identification of commercialization opportunities for projects and products that have been previously funded by the ERC (ERC 2010).

4 See ERC indicative statistics on <http://erc.europa.eu>.

decades (Knorr-Cetina 1981, 1991; Latour and Woolgar 1979), it seems to us that current sociology of science might benefit from a renewed appreciation of and re-conceptualization of the significance of the institutional contexts of doing science. In the following we refer to a Mertonian structural analysis and apply it to the case of research funding in Europe, with particular attention to the role of RFOs. As formulated by Merton, such an analysis emphasizes "...structural sources and differential consequences of conflict, dysfunctions and contradictions in social structure" (1976b: 126).⁵ Apart from this focus on structural conflict and contradictions, it seems to us that some analytical strengths of a Mertonian approach include a) providing explanations rather than "black-box" interpretations of causal connections between conditions and consequences of social action, b) developing analytical links of micro-processes of social choice to its macro-conditions and consequences, c) emphasizing the central role of researchers' competition for recognition, reputation and reward in shaping the institutional processes of the scientific system while continuing to take unintended effects into account.

According to Crothers (1990: 209ff.), Merton neglected the organizational environments of scientific activity in favor of focusing on the underlying structure of science in generating recognition priorities. Nevertheless, in drawing on particular interpretations of Merton's work as a general theory of social structure as provided by Stinchcombe (1975, 1990), Crothers (1987, 1990, 1996,⁶ 2004) and Blau (1990), we suggest that a Mertonian approach can be adapted at least partially to the case of RFOs. Therefore, after introducing the example of the ERC, we outline 1) cultural goals and norms of RFOs in a given environment; 2) actors' different status positions and roles as socially structured means to achieve these goals; 3) choices undertaken by actors among structured alternatives, constrained by their position in that structure; 4) mostly unintended consequences of actors' practices, such as feedback effects on a particular organization. The underlying pattern can often be conceptualized as a self-enforcing mechanism resulting in the reproduction of that organization.

The Cultural Structure of "Integration", "Competition", and "Excellence"

In identifying the wider environment of research, we draw on Merton's distinction between cultural versus social structure outlined in his famous article "Social Structure and Anomie" (1938). The cultural structure consists of those goals,

5 As Crothers (1987: 76ff.) has pointed out, from the late 1960s onwards Merton has interpreted his functional analysis to become a structural analysis; rather than giving a detailed account and examples, Merton has formulated a programmatic framework by contrasting it with other approaches. See also Crothers 1987: 78ff. for commentaries on Merton's structural approach.

6 For a broad sociological imagery of social structure and an analytical toolkit for developing structural analyses, see Crothers (1996), in particular Chapter 5.

values, motivations and norms that social actors internalize and to which they orient their action, for example material success in a given society, in contrast to socially legitimized means provided by the social structure to achieve these goals. That conceptual distinction led him to a path-breaking explanation of deviant action in demonstrating that its formal structure can be conceived as a case of innovation, where actors accept cultural goals while using illegitimate means to achieve them. In the sociology of science, Merton's formulation of the "normative ethos of science" (1942) clarified the norms of universalism, communism, disinterestedness and organized skepticism as constitutive both for the actions and orientations of scientists and for the scientific character of knowledge.⁷ Insofar as science and research are clearly universal and international endeavors, all institutionalized forms of research have to be in accordance with that underlying structure of science. Aside from that, however, particular institutionalized structures of research such as that defined by the European Research Area (ERA) can be interpreted as a framework of orientation spelling out more specific goals and objectives for science and research in Europe, reflecting its rhetoric and terminology as well. What about the cultural structure of norms and goals in the ERA?

The ERA was endorsed at the Lisbon European Council in 2000 "to develop a genuine common research policy" (European Commission 2007b). ERA members comprise the 27 member states, associated states and candidate countries of the European Union. Its main goal is the supra-national integration of a diverse and fragmented set of national science systems in Europe. The ERA tries to overcome this assumed fragmentation by combining "a European 'internal market' for research, where researchers, technology and knowledge freely circulate; effective European-level coordination of national and regional research activities, programs and policies; and initiatives implemented and funded at European level" (ibid.). Important funding strategies at supra-national level are the multi-annual Research Framework Programs established in the mid-1980s, and the ERC research grants established in 2007. Initially the focus on scientific trans-national collaboration had been centralized in order to achieve "integration" of highly fragmented national research systems in Europe; however, the notion of integration has more and more been abandoned in favor of "excellence through competition" (Winnacker 2008) as the main objective of European science policy. Competition has been understood as being both "internal" as between individual and corporate scientific actors in the ERA and "external" as against other global knowledge societies by promoting "groundbreaking frontier research".

It can be assumed that the objectives of integration and competition are likely to generate some conflict for scientific actors, since these do not seem to be always compatible. However, it might also be that the ERA has found an effective way of resolving potential ambivalence by a policy shift in time and by providing

7 For a detailed discussion on anomalies, anomie and deviance in science, see Fleck (2000).

two parallel organizational frameworks for achieving these goals of research in Europe. As a policy-concept the ERA was designed to accompany the EU Lisbon Strategy with its ambitious objective “to become the most competitive and dynamic knowledge-based economy in the world” by 2010 (European Council 2000). While that attempt has clearly failed, its reformulations in 2005 and 2007 have not per se substituted the neo-liberal orientation of the concept. It remains important to recognize that the Lisbon Strategy provides an underlying normative guideline for ERA’s research policy that is expected to be deepened and widened “so that it fully contributes to the renewed Lisbon strategy” (European Commission 2007b). The goals of integration and excellence are defined within that context of economic competition.⁸

Merton’s article on recognition and excellence (1960) might help us to clarify their particular meanings in sociological discourse in potential contrast to those in science policy. He draws a distinction between recognition in its instrumental sense, “to find ways of detecting potential excellence and of doing so early enough to help potentiality become actuality” (Merton 1960: 419), and recognition in its honorific sense, as actual rewarding of achievement (or possibly lack of it) by appropriate public acknowledgement. Moreover, he differentiates excellence in the sense of quality, as a potential capacity, from excellence in the sense of performance, as demonstrated actual performance. In discussing possible combinations of these options, he focuses on two of them. “Recognition as instrumental, excellence as quality” lets him ask how institutions of education and research identify and reward talent and ability; he notices a clear institutional bias towards rewarding precocity, whereas “late-bloomers”, in particular from social classes with low economic or social advantages, are mostly overlooked. Concerning “recognition as instrumental, excellence as performance” he discusses the role of trainers, teachers and “catalysts” who are often socially invisible, but successful in evoking excellence in others and bringing out the best in them. According to Merton, late-bloomers in particular benefit from the role of these informal talent scouts.

We assume that the ERC did not intend to implement Merton’s early conceptual considerations, but we will try to sharpen the meaning of “excellence” with respect to these. Former ERC Secretary-General Winnacker outlines that “(w)ith competition I mean the nature of its peer-review system” (2008: 126). In Merton’s language, this is to define competitive peer-review as a means to arrive at the excellence objective, seeking to reward its actual performance based on being successful in passing highly selective peer-review. Apart from that, the ERC orientation towards “ground breaking frontier research” suggests that it also seeks instruments for identifying potential quality in content, as new and innovative knowledge. Luukkonen (2012: 54) from her interviews with 20 ERC panelists reports that these actors, in their role of deciding if a research proposal can be regarded as excellent or not, interpret the idea of excellence mostly in terms of

8 For a discussion of the changing governance of research policy in the ERA, see Edler et al. (2003).

quality requirements for research, such as its soundness, coherence, precision, rigor, and innovativeness. They seldom draw any distinction between excellent versus groundbreaking research. Moreover, she suggests that the conservative implications of the peer review process, where reviewers prefer evidence for a researcher's past performance over taking the risk of promoting potential capacity, per se make it unlikely that the ERC is always successful in achieving its goal of promoting groundbreaking research (ibid.).

Ambivalence or its Absence? Potential Conflicts in European Research Policy

Merton's concept of sociological ambivalence (1976a) refers to a situation in which scientists and scholars are subject to incompatible scientific norms, causing an "inner conflict" induced by the social structure of science itself. Examples have been scientists involved in priority struggles over discoveries who experience the tension between both the norm of originality in knowledge production and the norm of humility being aware of actually limited knowledge. According to Merton, sociological ambivalence often can be accommodated by an actors' behavior over time, just as the physician in his or her professional role oscillates between an attitude of "professional detachment" and "compassionate concern" towards the client. This opportunity for oscillating behavior, however, requires a dynamic organization of social structures where accommodations both to the normative expectation and to its counterpart are possible. Regarding the question of whether ERA's cultural goals are potential causes for scientists' ambivalence, conflict, or deviant behavior, we can analytically think of several cases. One case might be the potential for conflict between the normative ethos of science and the ERA goals in the wider context of neo-liberal capitalism, evoking a contradiction between scientific or cognitive versus economic or political norms of competition and utilization.

In discussing the role of ambivalence in the relationship between science and society, Nowotny (2011) observes that in contrast to former distinctions between industrial and academic science, today the boundaries between academic and commercial research have become blurred. According to her, the advent of entrepreneurial science might not be the sole creator of a new kind of sociological ambivalence for the scientists involved. Stabilization of ambivalence can also be accomplished by organizations setting up new intermediary units and thus providing organizational structures for the oscillation of behavior or creating new roles whose function is partially to transcend older kinds of ambivalence. Ambivalence of scientists, however, can also become structurally displaced:

The scientist-entrepreneur may have left ambivalence behind, since the new role explicitly defines and enables the co-existence of passion for science and interest in its monetary rewards. A new compatibility has been achieved, and it is used to further expand the range of the previously impossible, as when money gained is reinvested into the next research project fuelled by curiosity. But has

all ambivalence really disappeared or has it simply been displaced and relocated in different, and new hybrid structural assemblies and arrangements? (Nowotny 2011: 97)

Concerning that question, we point to empirical findings from our research on the impact of the Europeanization of science on sociology (Hoenig 2012): In interviews with sociologists in two Central European states we have found that reported ambivalence towards European research funds is highly dependent not only on sociologists' structural positions either being appointed to a university department or a research institute outside university. It also depends on their position in the academic hierarchy and on their cohort membership. While ambivalence towards EU funded research is reported by tenured sociologists appointed to a university, where other research options not based on external funds are still available, this usually is not the case for sociologists appointed to research institutes, nor for academic project assistants, where obtaining research funds is the precondition to do research at all.

The Internal Social Structure of the ERC

Both clarifying different statuses and roles of actors and investigating socially organized relationships between groups of actors are central for Merton's structural analysis. This is evident in his key concepts of status-set, status-sequence, role-set, opportunity structures, particularly as part of his theory of reference groups (1968: 279ff.).⁹ In his sociology of knowledge that structural focus is also present in discussing perspectives of insiders and outsiders (1973: 99ff.), patterns of scientists' behavior (1973: 325ff.), and the role of intellectuals in public bureaucracy (1968: 261ff.).

When applied to the case of the ERC, we describe its structured relationships between groups as characterized by a balance between mainly scientific groups, such as the Scientific Council and the Panelists, and mainly administrative groups, such as the Executive Agency. Moreover, we explicate different statuses and roles of actors involved and possible conflicts emerging from these. Although it is continuously stressed by the ERC that its scientific actors qua status position are eminent scientists themselves, as groups we classify them in relation to their actual status and role performance as defined by the internal structure of the ERC. That is, we make an analytical distinction between evaluators' status qua position in a scientific system ("eminent scientist", "professor", "post-doc researcher", "chair of science board") and their status and roles qua enacted performance in their daily working routines shaped by the internal structure of the ERC ("scientific advisor", "executive officer", "chair of panel", "applicant").

⁹ For a detailed account of social structure in Merton's writings, see Crothers (1987: 93ff.).

The ERC Scientific Council

Scientific administrators and advisors act on the basis of organizational skills, strategic abilities, and a repertoire of practices characteristic of members of the scientific "administrative elite" acting as potential gatekeepers in the science system (Cole and Cole 1973: 41). The Scientific Council can be understood as such an institutionalized group at supranational level. It consists of 22 outstanding representatives of the scientific community made known to the public via the ERC website. They are selected by a specific ERC Identification Committee, solely based on their scientific merits, not representing any particular EU member states or interest groups. The task of the Scientific Council is defining the strategic orientation, methods, quality assurance of the funding program, its annual working programs, the peer review procedure, and an ethical code for the prevention of conflicts of interest. The President of the Scientific Council is not only the formal representative of the ERC, but also an eminent scientist, and is supported by two Vice-Presidents; the first ERC President was natural scientist Fotis Kafatos, followed by social scientist Helga Nowotny.

The ERC Panelists and External Evaluators

Although peer review is a genuine part of scientific activities, researchers in their role as evaluators act on behalf of the RFO and are members of a team of panelists responsible for legitimizing scientific knowledge as "excellent" or not. While writing a research proposal and evaluating it are both part of the traditional scientific role, the latter does so with a certain mandate or contract received from the RFO, whereas the former tries to secure a contract. In practice, the fact that evaluators are researchers themselves might sometimes cause potential conflict. Being well-informed experts in possibly small circles of scientific specialties, their engagement in peer review might help them to gain advantages from this information in field-specific developments: It might well be that researchers in writing proposals anticipate this and react by including strategic vagueness at specific points of their research design (Hornbostel 2001: 147). Moreover, although the notion of peers implies a democratic equality of opportunity to all peer reviewers, evaluators are internally differentiated by their academic merits and prestige as well. Therefore, there is much room for inequality in how much peer reviews provided by particular evaluators count and their weight can often only be understood in relation to these structures of science.

Up until now, proposals submitted to the ERC have been evaluated by 20 to 25 disciplinary-specific panels, six for the social sciences and humanities, ten for physical sciences and engineering, and nine for life sciences. Panels consist of about 12 to 15 members of different disciplines and one chair. Panelists act as experts on the respective sub-fields as defined in the panel descriptors, while no specific criteria for qualification are required apart from having "the highest level of scientific and technical expertise, in areas appropriate to the call" (European

Commission 2010); in practice, ERC panelists are selected by the Scientific Council. The Review Panel that has evaluated the organization of the ERC after its first two years has criticized that procedure as lacking professionalism and has recommended the founding of a sub-committee responsible for selecting potential panelists in cooperation with the Executive Agency (2009: 16ff.). This has been accepted by the Scientific Council in 2010 specifying more, albeit diffuse requirements of qualification such as “a high level of professional experience in the public or private sector in scientific research, scholarship, or scientific management” (European Commission 2010). ERC panelists take part in both stages of the two step peer review process, while external evaluators are nominated by the panel chair for remote peer reviews at second stage.

The ERC Executive Agency

Previously represented by an implementation structure led by the Directorate-Research of the Commission, the Executive Agency was introduced only in 2009. The lateness of its establishment can be interpreted as an outcome of a reliance of the ERC on a complex system of institutions and procedures instead of a single uniform organ (Gross et al. 2010: 105). By 2012, its staff had grown to about 400 members. In order to provide the biggest share of money for the funding of research, administrative and staff costs are required to amount to only 5 per cent of the ERC budget. Located in Brussels, the Executive Agency is administered by a Steering Committee and a director that are nominated by the Commission for two to four years. The main tasks of the Executive Agency are the administrative and practical implementation of the program, particularly in relation to review procedures and selecting panelists, and the collection and evaluation of information necessary for the program implementation.

Researchers as ERC Applicants

Groupings of researchers or project teams that apply for a project usually are hierarchically structured in a manner similar to the quasi feudal hierarchy characteristic of university departments (Crothers 1991). At the top of the team we often find university professors or tenured, experienced senior researchers who within the funding context are addressed as “principal investigator”, “coordinator”, or “project leader”. Concerning the ERC grants, both advanced researchers from the top of the academic prestige ranking as well as junior researchers are eligible as principal investigators. Research teams can be assembled by staff from a single department or from several organizations collaborating as “scientific partner institutions” or “advisory boards” to the coordinating team or host institution. While in the first case the hierarchy of the team often reflects that of the department, in the second case the structure of a project partnership mostly follows from members’ positions in the hierarchy of a certain university, department, sub-discipline, research paradigm, and cohort. Project teams differ from department

staff in the definite time-framing of an activity, structured in particular phases, dedicated to research on a specific problem, with reporting obligations to the RFO.¹⁰ Differences in the hierarchical character of research are subject to significant variation across disciplines, nation states, and intra-national academic landscapes of host institutions (Galtung 1981). Apart from that, eligibility criteria defined by RFOs restrict the scope of applicants by specifying requirements such as available institutional infrastructure and a broad range of researchers’ qualifications measured against publication records, previous research funds and awards, high international mobility, affiliations to prestigious institutions, etc. These resources and actors’ control over them affect the opportunities and constraints of individual and corporate scientific actors in more or less successfully defending their knowledge claims or “jurisdiction” (Abbott 1988) against other actors in the competitive field.

Choice in RFO’s Peer Review Procedures

According to Stinchcombe’s (1975, 1990) reading of Merton’s work, social choices and decision-making are central to his theory of social structure. Stinchcombe has characterized the core process as “*the choice between socially structured alternatives*, and the core variable to be explained is *different rates of choice by people differently located in the social order*” (1990: 81, italics in original). Contrasting economic or psychological notions of choice, in Merton’s work “the utility or reinforcement of a particular alternative choice is thought of as socially established, as part of the institutional order” (Stinchcombe 1975: 12, quoted in Crothers 1987: 105).

Here we consider RFOs’ groups of actors and focus on structured conditions of the choices involved in peer-review decision making. These choices, their causes and consequences, are at the core of RFOs’ practices and are the most relevant for the scientific community. We assume that RFOs have an opportunity to influence the content dimension of research, not only by defining the scope of legitimate “foci of interest” of science and research (Merton 1973: 191ff.), but also by arriving at decisions on which research proposal shall be funded and which not. Concerning the case of ERC peer reviews, which ERC groups of actors are involved? Although criteria and procedures of peer reviews are codified by the Scientific Council, it is the Executive Agency that is responsible for implementing these decisions by a wide range of activities contributing to the process such as selecting reviewers, grouping them into panels, administrating the budget, bargaining rules and agreements with national RFOs, preparing contracts with grantees, monitoring the implementation of the project (European Commission 2007c; ERC 2010: 6). Which features of institutionalized peer-review influence actors’ structural opportunities

10 For a historical account of the emergence of the project organization of research, see Fleck (2011).

and constraints in making choices? Here we consider formal and informal criteria and different levels of visibility and ignorance as socially structured aspects in peer review decision-making.

Peer Review Criteria

Merton (1968c, 1973; Zuckerman and Merton 1971) has interpreted peer review as an institutionalized procedure of the norm of organized skepticism and as an important control mechanism operative within the scientific community. His initial analyses have been continued and complemented by his students for decades: For instance, empirical research on peer review of the US-American National Science Foundation (Cole, Rubin and Cole 1978; Cole and Cole 1981) has found that because of low levels of consensus among reviewers the proposal acceptance to a significant extent depended on chance, given by the selection of peer reviewers.

What about criteria and procedures for peer review in the ERC? Its selection criteria are oriented, on the one hand, towards the intellectual capacity, creativity, and commitment of the principal investigator, and on the other hand, towards the groundbreaking nature, potential impact, and methodology of the research project proposed; both parts are weighted equally (ERC 2010). Moreover, the formal contract between the ERC and a scientific host institution requires that this research environment provides for the necessary infrastructure, intellectual and administrative support for the project and the researcher. While these requirements seem common-sense in formal contracting of RFOs, we do not know in detail how features of host institutions are weighted in the evaluation process. From former ERC representatives we get the impression (Winnacker 2008: 126f.) that the Scientific Council's expectations in relation to host institutions' expected role in an anticipated brain-gain of talented scholars from Europe and beyond might be considerably greater than referred to in the formal guidelines.

In addition to formal criteria we assume that in peer reviews there exist a set of practical "customary rules of deliberation" (Lamont 2009, Lamont and Huutoniemi 2011). These do not inevitably invalidate the criteria mentioned above, but can be conceived as a repertoire of symbolic resources or as background knowledge on which evaluators draw when they interpret these norms in concrete situations of making choices on classification and judgment (Mulkay 1980). For instance, Luukkonen (2012: 55) mentions panelists' overarching concern with the feasibility and associated risk of the research proposed. However, risk-averse review practices inevitably introduce conservative elements in the selection process, at least in relation to controversial proposals. Moreover, these practices contradict the ERC's initial expectation of promoting innovative, controversial research. Luukkonen concludes from her study "that despite the ERC's aims, the peer review process in some ways constrains the promotion of truly innovative research"; however, she acknowledges its peer review procedure constitutes "a compromise approach that balances extreme risk-taking with a wish to support exceptional research and researchers" (2012: 59).

Ignorance and Visibility in Peer Review

Merton is one of the most important theorists focusing on the role of ignorance in social action, as shown by his articles on manifest and latent functions (1968a), unanticipated consequences of purposeful action (1936), the role of serendipity (2004) and ignorance in scientific progress (Merton 1987).¹¹ Moreover, in his reference group theory, Merton has emphasized the central role of observability and visibility as key elements of social structure (Merton 1968c: 390–410): In outlining functional requirements of effective authority, he stresses that actors in authority or those exercising social control are required to have substantial knowledge both of group norms and values and, more implicitly, the attitudes of its members.

In social relations an actor's ignorance or incomplete knowledge of others' actions and the absence of observability and visibility can often be conceived as two sides of the same coin. Merton's considerations suggest that the degree of visibility of a RFO's choice in relations to other groups is central to understanding the strategic nature of that context. Its degree of visibility then can be assumed to be not an arbitrary, but a constitutive part of the RFO's structured position and of the efficiency of its actions. On the one hand, this indicates that the RFO as administrative organization follows other norms and rules than the scientific community's norm of communalism in publishing. Consequently, "open access policy" decisions unsurprisingly refer to publication obligations of funded researchers and not to the working routines of RFO's groups. Irrespective of values of "transparency" and "participation" that promote democratic procedures and legitimize a RFO in its public and official role, we speculate that RFO actors in their professional roles are supposed to carry out their working routines with low levels of observability and visibility to others external to the organization. On the other hand, given relatively democratic relationships between the parties involved, forms of ignorance in the institutional context might be functional for universalism in how a RFO's peer review operates. Ignorance of irrelevant particularistic criteria of an applicant's race, class, gender, or age in job recruiting prevents an organization from institutional discrimination (Blau 1955, quoted in Schneider 1962: 501). Moreover, unrecognized and specified ignorance (Merton 1987) can be conceived similarly to more or less deliberate discretion. Drawing an analogy to the legal and political system, there is much discretion possible in the manner in which politicians make use of legislation to shape the policy process and how much autonomy they deliberately leave to executive bureaucrats to fill in the policy details (Huber and Shipan 2002).¹² Analogous to political procedures,

11 For a review of the role of ignorance in sociological theory, see Schneider (1962) and Gross (2007). For the role of ignorance in peer review based on a study on a Swiss RFO, see Reinhart (2012).

12 Huber and Shipan (2002) have empirically examined the variation of vagueness in state legislations that spell out the actual policy details in implementing the US-American

the opportunity for strategic voting (Lamont 2009; Lamont and Huutoniemi 2011) is relevant as well when voting decisions of panelists for or against a proposal are at stake in peer reviews.

Social Mechanisms and Unintended Consequences of RFO's Practices: A Persisting Matthew Effect?

In the analytic model provided here, choices by individuals feed back to affect long-term effects on alternative social structures, involving both organizational control and mostly unintended consequences (Stinchcombe 1975a; Crothers 1990: 220). While Stinchcombe has focused on the dynamics of effects for institutional reproduction, Crothers (1987: 85ff., 110f.) has extended these notions by their structural relation to a given institution: He draws a distinction between a) "feedback effects" of practices reinforcing an institution, such as publishing articles in journals supports the continuity of journals; b) "leakage effects" of practices mostly unintentionally affecting other institutions, such as Puritan orientations leading to the pursuit of scientific knowledge, as found by Merton; and c) the case that social practices have no effects on institutions.

Recalling Merton's differentiation between anticipated and unanticipated consequences of social action (1936) in relation to evaluation and peer review is relevant for understanding institutionalized forms of RFOs' control in the scientific system and for governance strategies in the scientific system in general. This involves asking whether the effects of evaluations are foreseen or not and whether they are controllable or not (Braun 2008). Braun suggests that the unforeseen effects case is distinguished by structurally determined interdependence and the role of cognitive-psychological factors in influencing actors' intellectual abilities. While the former refers either to uncertainty in single systems of action or to interdependence, sequence and reciprocity in multiple action systems, the latter refers to those sources previously mentioned by Merton in potentially causing unanticipated consequences of social action: ignorance, error, immediacy of interest, basic values, and self-defeating predictions. Concerning the second case of controllability or manageability, Braun distinguishes a) single effects, such as events of uncoordinated traffic transformable by a system of incentives; b) complex effects, such as prisoners' dilemma games; and c) circular movements of causality, such as Merton's self-fulfilling prophecy (Merton 1968d).

Here we consider only one analytical case in which cumulative mechanisms of a RFO have the more or less anticipated effect in maintaining and reproducing a given social structure. At the same time it is a variation of the feedback loop

federal Medicaid program, providing health care to various disadvantaged groups. While some states in their legislation constrained discretion by specifying in detail how the implementation of the program should work, other states allowed more discretion or gave enormous autonomy to policymakers.

known as the self-fulfilling prophecy. Concerning the scientific system and its mechanisms of assigning recognition for intellectual property, Merton has analysed the “Matthew effect” operative as an endogenous, self-reinforcing feedback mechanism of accumulating reputation (Merton 1968a, 1988). In Merton’s words, “(t)he Matthew effect is the accruing of large increments of peer recognition to scientists of great repute for particular contributions in contrast to the minimizing or withholding of such recognition for scientists who have not yet made their mark” (Merton 1996: 320).

We focus on a single example of an identified “monopolization mechanism” under conditions of competition with often harmful consequences for actors involved. Explicitly referring to Merton, while underemphasizing his focus on structural conflict, Muench (2007, 2008, 2010) has critically investigated practices of evaluating and ranking academic universities on the basis of scientific publications and research funds, embedded in a wider structural environment of the academic field. He stresses the self-reinforcing circle of competition among universities as reputational systems, resulting in the formation of oligarchic structures within the scientific community or even in a monopoly. According to him, at the surface of performance-based initiatives such as the German “Excellence Initiative” introduced in 2006, universities and other scientific institutions compete for symbolic and financial resources under roughly comparable conditions. However, decisions of RFOs and other scientific administrative organizations as instances for legitimizing the scientific quality of research are pre-structured by the already given in-depth structure of social stratification in science. Self-enforced procedures of evaluation enacted by these institutional instances more or less unintentionally reproduce and deepen social stratification in science.

Concluding Remarks

We have focused on the social structure of research funding in Europe and in particular on the role of research funding organizations (RFOs) taking the supranational European Research Council (ERC) as an example. Confronting empirical material with conceptual notions drawn from Merton’s theory of social structure, we have argued that a structural analysis of research funding is crucial for an understanding of the structural conditions and choices, mechanisms and consequences of establishing scientific “excellence” in Europe.

Concerning cultural goals, the full scope of the ERA policy objective seems to be inherently contradictory or indicates a shift from integration to “excellence through competition”. In Merton’s terminology, that notion of excellence involves recognizing in an honorific sense the actual performance of scientific talent rather than identifying a potential capacity. While the latter might also be characterized as “groundbreaking research” in relation to its content, its realization seems unlikely, given modes of peer review that eschew high risks in preference to remaining within the mainstream. Moreover, taking the Lisbon Strategy as an underlying

normative guideline of the ERA research policy, its excellence objective must be understood in that context of economic competition. Merton's notion of researchers' sociological ambivalence as part of the social structure of science seems to more and more eroded, when success in performance-based competition for economic resources becomes the most important criterion for being able to continue scientific research. Concerning the social structure of the ERC, we described relationships of its scientific and administrative groups of actors and explicated differences and potential conflict between status position and the performance of roles of actors involved, such as representatives of a scientific administrative elite or researchers acting as evaluators of applicants' proposals.

Regarding opportunities and constraints structuring RFO actors' choices in the peer review process, we have considered formal and informal criteria and the role of ignorance and visibility involved. Ignorance and asymmetric visibility can be assumed as socially structured and indeed constitutive aspects of the review process involved; though, that might neither correspond to RFO's official self-conceptions nor with scientific norms of publicly communicating knowledge claims. As cumulative mechanisms and mostly unintended consequence of peer review practices we have considered the case of the Matthew effect. When contextualized in a competitive scientific field of actors that is already socially stratified, we can speculate that self-reinforcing mechanisms enacted in peer review practices result in the reproduction of oligarchic or even monopolistic structures of a highly reputed elite, enforcing and deepening inequalities among scientific actors in the ERA and beyond.

Here we can only assume that irrespective of the main intentions of the ERA to promote integration and ground breaking research, institutional mechanisms of research funding create and enforce social inequalities among scientific institutions. Conceptual reflection on how to define particularism versus universalism (Cole 1992), knowledge versus ignorance (Merton 1987), empirical evidence versus counter-factual belief (Merton 1968d), might become relevant for future empirical research on the structural analysis of research funding and its more or less unintended effects of establishing excellence.

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Chapter 8

Using Scientific Knowledge in Policy Making: The Importance of Organizational Culture

Sally Shortall

In order to understand how evidence is used to inform policy, we must critically reflect on the organizational culture of the civil service and how it differs from the academy. In what follows, I will examine the hierarchical rule-based structure of the civil service, where authority is linked to office. The role of the civil servant is that of a generalist who does not have specialist knowledge in any particular policy area, but instead has specialist knowledge of the workings of the civil service and how to minimize uncertainty. In contrast, academics providing evidence to civil servants may have little knowledge of the structure of the civil service or how it differs from their culture. The academic is a specialist and academic authority comes from questioning normative knowledge and publicly disputing accepted beliefs. Such an approach is anathema to the civil service. The difference in values and ideology of the civil service and the academy has implications for how academic research is used to formulate policy and its position in knowledge power struggles.

Introduction

The study of the civil service and its role in creating social order is a well-established research topic for social scientists. It is central to political studies, social policy, the study of public administration, the study of bureaucracy (Weber 1947), and power struggles between technocracy and ideology (Habermas 1970). This research has largely focused on hierarchy, bureaucracy, how public policy is formulated and relationships between civil servants and politicians. With the current emphasis on evidence-based policy, and the need for academics to demonstrate the use value of their research, there is now a research imperative to understand how the structure of the civil service impedes or facilitates the use of evidence in formulating policy.

Academics are increasingly funded by research bodies to work in government, the private sector and the third sector to provide evidence to inform particular policy questions and problems. As a result of this an increasingly sophisticated

academic body of knowledge has developed reflecting on the complexities of evidence-based policy. This has developed from the various ways academics are now engaged in the policy process, for example; as policy advisers, (Stevens 2011; 2007), through systemic reviews of policy documents, (Monaghan 2009; 2010), through reviews of independent commissions (McLaughlin and Neal 2007), and by comparative analysis of the ways evidence and policy interact across nation states (Denzin 2009; Denzin and Giardina 2008). While evidence-based policy sounds intuitively to be a good thing (Hammersley 2005), this recent body of knowledge demonstrates the difficulties and complexities of the idea.

The British Economic and Social Research Council in particular has recently funded a number of so-called Knowledge Transfer Research Fellowships, placing academics in the civil service, industry and the community and voluntary sector, to foster the transfer of academic knowledge into these environments. This has contributed to the nuanced understanding of how evidence is used in policies. However, what is surprising is that with the exception of Stevens' (2011) eminent ethnographic study of how evidence is used to construct policy stories to support policy design and career advancement in the civil service, few other analyses or observations of the civil service environment have emerged. In this chapter I will discuss how the ethos and structure of the civil service shape how evidence is used. I will also examine how the structure of the civil service is disposed to a tense relationship with providers of research evidence. Like Stevens (2011), I argue that power dynamics are central to the use of evidence in policy. The power to 'choose' the evidence that best suits the development of policy rests with civil servants, and I maintain that their choice is in part shaped by the structure and ethos of the civil service.

This chapter begins with an overview of the structure of the civil service. Then it turns to explain participant observation. After that, I will outline how my role as a participant observer in the civil service¹ allowed me to examine the culture of the civil service and how this affects how evidence is absorbed into policy.

What We Know about the UK Civil Service

The Bureaucracy of the Civil Service

Bureaucracies are historically constituted and differ from place to place (Dahlstrom et al., 2010; Barzelay and Gallego 2010; Painter and Peters 2010). The model of the UK or Whitehall civil service is also predominantly the model of most British ex-colonies (Hardiman 2010). For almost 200 years, the need for an English civil service that is efficient, permanent, and apolitical has been accepted (Vandenabeele et al. 2006). It provides stability to parliamentary governments that change. While governments with different ideologies and values come and go, the

1 Funded by the ESRC Knowledge Transfer Fellowship ESRC RES-173-27-0096.

civil service remains intact, and impartial. It is a permanent bureaucracy separate to government that provides the main policy advice to government members and it is responsible for implementing policy (Vanderabeele et al., 2006).

The UK civil service is quintessentially Weberian in character (Chapman and O'Toole 2009). Many of the founding principles remain, that is, it is rule-based and hierarchical in structure. It is almost Victorian, in the sense that civil servants of a particular grade know 'their place' and do not question their superiors. The civil service is a 'command and control' hierarchy, where those in authority control the work load of junior colleagues (Bordua and Reiss 1966; Behn 1995). Authority over junior colleagues is rigorously established. Those in authority appraise junior staff. Officials are esteemed because of the hierarchy of offices, and the power of their office (Weber 1947). They pursue careers that bring them higher up in the hierarchy of their bureaucracies. The civil servant is an expert on the workings of the civil service. As Stevens (2011) notes, civil servants move between policy areas, they are not specialists. They are promoted to and make sideways moves to different policy areas. Sideways moves are often undertaken to develop the generic competences needed to progress to the next grade in the civil service. Civil servants are promoted on the basis of their competence in understanding how the civil service functions and their competence to solve problems within it and design policy, rather than on the basis of specialist knowledge of a particular area. Hardiman (2010) has identified the process of sideway promotions as problematic, arguing that while it was intended to widen the talent pool, it has the unintended consequence of dissipating the skills base because the specialist policy understanding built up in one departmental area does not necessarily translate to another area.

The Civil Service and Civil Servants

The civil service engages in generalist recruitment for non-specialist careers (Carmichael 2002; Hardiman 2010; Stevens 2011). Civil servants have specialist knowledge of the workings of the civil service and how to solve problems within it (Stevens 2011). It is the job of the civil servant to execute policy while it is that of government to design policy depending on ideologies and values. The civil service is organized to protect the civil servant. Initially this was developed to protect civil servants from political interference or punishment and it is achieved by protecting their anonymity, having a strong career structure, appointments on merit and the possession of general competences and secure career status (Dahlstrom et al., 2010). Kernaghan (2003) argues cogently that for the successful operation of the civil service, it is necessary that civil servants are protected in this way. In order for public servants to speak openly to politicians, their anonymity must be protected. Because they execute policy decisions loyally regardless of their personal opinions or whether they agree with the philosophy of the government in power, they enjoy security of tenure (Kernaghan 2003: 11).

Trustworthiness and integrity are values understood to underpin the civil service (Chapman and O'Toole 2009). Public servants do not express publicly their personal views on government policies or administration. Central to their job is to minimize uncertainty (Stevens 2011; Monaghan 2009). Civil servants must assess policy contexts, try to ensure stability, and aim to develop and execute policies that are favourable with the public and key interest groups (Hall 2009; Stevens 2011). What are favourable policies become embedded and reinforced over the history of the institution. This is not to overstress a structural interpretation, but rather to argue that current subjective meanings and interpretations of public policy are shaped by historical normative understandings and well-established alliances and networks with groups who share these normative assumptions. Wilkinson et al. (2010) argue that policy framing is heavily influenced by existing alliances, networks and normative understandings of social issues, and further argue that 'once policy is embedded, it can be shored up by specific forms of expertise' (p. 345). Again, this is not to present a static view of the policy environment. Change does occur, and this can be both dramatic and incremental. However, change will be shaped by the legacy of previous policy choices and normative assumptions.

Being an ESRC Knowledge Transfer Fellow and Participant Observation

Over the past number of years, the UK Economic and Social Research Council (ESRC) has co-financed so-called Knowledge Transfer Fellowships to place academics in the private, public and third sectors to provide evidence to inform particular policy questions and problems. It is also a response to the growing demand for academics to demonstrate the use-value of their research and the need for researchers to produce work that is not only 'useful' but 'useable' (Monaghan 2009). I was an ESRC Knowledge Transfer Fellow in the Rural Policy Division of the Department of Agriculture and Rural Development (DARD) in Northern Ireland for one full year. During this time, I was tasked to examine the existing rural evidence base of economic and social studies used to underpin rural policymaking in Northern Ireland and identify evidence/research gaps; to identify the priority themes and indicators for future research to address these gaps in the evidence base; and to develop models for the most efficient and effective methods of collecting, using and disseminating rural evidence and research. Ostensibly, it was a straightforward transaction of knowledge provision to develop evidence-based policy. This was how I approached the task. However as time went on, I became interested in the power struggles between different forms of knowledge, how they assert their legitimacy and which types of knowledge get used in designing and reifying policy (see Shortall 2012; 2013).

For eight consecutive months² I was based in the civil service. During this time I had office space in the Rural Policy Division. Having been trained as a qualitative researcher, I recognized that this placement afforded me an unusual opportunity to conduct participant observation. Generally it is too expensive and time consuming for academics to be placed in an environment to conduct lengthy participant observation (Gans 1999). It was the perfect opportunity for participant observation; I, the researcher, was playing an established participant role in the scene that I could study (Atkinson and Hammersley 1994), and I could observe how people behaved in their ordinary environment (Becker 1958). My 'dilemma' was that I could not see how conducting participant research, and observing what people do as opposed to what they say they do, would inform my research question. Nonetheless, I decided to conduct participant observation, and to record my observations about civil service culture. Ironically, this represents what Becker (1958) considered to be the highest form of participant observation; where evidence is gathered in an 'unthinking' fashion, when the observer records items that are not related to what they are working on, for there is less chance of bias if the observations are not linked to any wish to substantiate a particular idea (Becker 1958: 659). I did not undertake participant observation to 'prove' a hypothesis. Indeed I did not enter the civil service with a hypothesis, but rather to provide evidence. As Gans (1999) rightly notes, any instance of participant observation expresses different combinations of participation and observation. I participated fully in the policy team. I worked there full time, went for lunch with colleagues, discussed work on a daily basis and attended weekly team meetings. Nonetheless I had an outsider status, in that I was not a civil servant and I was there for a particular period of time. I discussed my observations with colleagues and they were keen to hear how I saw their world and to discuss civil service culture and know how it differed from academic culture.

Mostly I was interested in the verbal and physical expressions of status and hierarchy of roles. This became apparent on my first day when I was having my photograph taken for my identity badge, which was necessary to enter and move around the building. Each badge states the grade of the civil servant. Not being able to state my status on my identity badge caused considerable discussion and debate. It was only after some weeks that I realized the social status attached to grade and how it shaped social interaction. I was also struck quite quickly by how colleagues would tell me to go and speak to a particular civil servant, and would say 'go speak to Grade Seven John Smith'. The grade of the person was usually stated before their name. Once I was asked in a surprised fashion 'oh, you spoke to Staff Officer John Smith?' Staff officer is a more junior grade. Knowledge and understanding was attached to the grade and the role, not to the individual. Nor did I fully appreciate the control and command ethos of the civil service at that time:

2 My fellowship was for one year, but after eight months I broke my shoulder and was off work for four months. Following my return to the university, I finished my Fellowship half-time over the following eight months.

Etiquette suggested that I should speak to somebody's superior before speaking to them.

My colleagues had an air of considerably efficiency. Every time a telephone rang it was answered, even if the person being contacted was not there. All correspondence was responded to immediately. My requests for information were dealt with very promptly. Most colleagues shared offices, except Grade Sevens and above, who had their own office. Initially I shared offices, and I was basically moved to whatever space was available. Some of my colleagues were embarrassed about this. I never understood why, although now I suspect it linked to their sense that I was being afforded no status. It became clear over my time there that while I did not have a position in the civil service hierarchy, the team viewed my academic credentials as giving me status. They always used my academic title when making introductions or referring to me in documents, although I never did so. I eventually ended up in the Grade Seven office as the Grade Seven was promoted to a Grade Five and a larger office on a different floor and I had my own office for seven months of my time there. It felt to me that the rest of the team thought this best – I was different. I commented on the comfortable chair in my new office and I was told that it was a 'Grade Seven' chair. Grade Seven civil servants are entitled to more expensive office equipment. Interestingly, there is also a hierarchy to gift giving. It is usual to give the people who work for you a gift, but it is most unusual to give the person you work for a gift. At weekly meetings and in general interaction, the hierarchy of status positions were constantly reinforced. Authority was not challenged. Authority over junior colleagues was publicly displayed with public requests for tasks to be completed, requests for reports, and the return of reports with requests for corrections.

For the most part I understood my observations to be detailing classic characteristics of a Weberian bureaucracy. I observed the myriad ways in which the authority structure of the civil service was reinforced. Offices were very hierarchically organized and supervised. Skill and knowledge were attached to the grade of the individual rather than the individual. As I tried to hunt down people who had worked on rural policy initiatives I had some involvement with ten years previously, I realized that people had been promoted out of the department or to completely different areas in the department. This was evidence of promotion on generalist knowledge rather than a specialism. It also highlighted an institutional loss of memory; while there are records of previous policies there is no accumulated learning for the individuals involved of what worked well the last time round or the obstacles encountered.

I did not initially believe my observations related to my evidence-based policy position. However as I re-read my notes, and observed interaction with other evidence providers, it became clearer that the ethos and structure of the civil service shapes how evidence will be used. Differences between the organizational cultures of the civil service and the academy can cause tensions regarding the use of evidence. What follows now is a critical sociological reflection on the

relationship between the organizational cultures of the academy and research, and policy and government, and how it impacts on the use of evidence in policy.

Defining Rural Policy

In the late 1980s the European Commission undertook a major rethink of the Common Agricultural Policy (CAP), which was to have policy implications for all Member States. The idea was to develop a more general Rural Development Programme for the benefit of rural areas beyond agriculture. The European Commission was also keen that partners would be 'bought in' – that is that Member States and regional areas would be co-financing partners. This was significant because it led to devolved governance of policy, and in Northern Ireland it meant the Rural Development Programme (RDP) would be devolved to the region rather than managed from Westminster. This also meant that a policy apparatus had to exist to manage the new RDP. It was decided in 1991 that the Department of Agriculture would assume responsibility for rural development. In 1999 it was renamed the Department of Agriculture and Rural Development (DARD) and has now assumed responsibility for rural policy more generally.

The newly formed Rural Policy Unit³ in 2006 designed a rural policy programme of work that went beyond the Rural Development Programme. Rural policy is a messy concept that is contested and vague. It is generally understood to mean the economic and social sustainability of rural areas, but exactly how that is to be achieved is never clear (House of Commons Report 2008). There are also difficulties with giving one department responsibility for a policy area for which it does not have the policy instruments to deliver relevant policies (for example, DARD is not responsible for rural schools, or rural health care, or rural roads). Nonetheless, the credibility of the Rural Policy Unit demanded that rural policy be designed and implemented.

Northern Ireland, the Northern Ireland Civil Service and Rural Policy

The Labour Government's *Modernising Government* Report (1999), not only established the need for an evidence base to inform policy, it also called for increased innovation and leadership in the civil service, and a move away from the risk-averse culture inherent in government (Chapman and O'Toole 2009). While The Labour Government was speaking more immediately about the British Civil Service, these issues were even more pronounced in the Northern Ireland Civil Service (NICS). For many years the NICS 'washed their hands' of responsibility for policy formation, and instead argued they simply implemented Westminster and European policies (McLaughlin and Quirk 1996). The lack of policy capacity,

³ A Rural Policy Branch was formed in 2006, which became a Rural Policy Division in 2009.

policy skills and policy capability of senior civil servants received much comment after devolution (Carmichael 2002; Greer 2004; Birrell 2009). Similarly policy guides developed post devolution spoke of the need for the civil service to develop policy specifically for the region rather than primarily adapting policies developed in Whitehall, as was often the approach under direct rule (OFMDFM 2003: 2).

The political context is one where there is still a certain nervousness and reluctance to assume responsibility for designing and executing policy. This was particularly the case for the Rural Policy Division, which assumed a very nebulous policy concept in a department used to dealing with the science of primary industries such as fisheries, agriculture and forestry. What the sustainability of these industries means is clearer cut and relies primarily on interaction with natural scientists. One colleague I spoke to said that they were not really a Department of Agriculture and Rural Development, but just the Department of Agriculture that was given responsibility for rural development but was not too sure what to do with it. One of the strategies the Rural Policy Division used to come around this problem was to rely heavily on consultation with their stakeholder groups. Consultation with stakeholder groups is deeply embedded in the political culture of Northern Ireland as it was seen as a way around the democratic deficit in the region (Hasenfeld and Gidron 2005; Knox 1996). The political context is favourable to stakeholder engagement in policy formation, and experiential expertise of the impact of policy is seen as a key form of evidence informing the development of policy.

The Rural Policy Division relied heavily on its two key stakeholder groups, the Rural Community Network (RCN) and the Rural Development Council (RDC), for guidance in implementing the EU funded Rural Development Programmes over the past 20 years. The *raison d'être* for the existence of both the RCN and the RDC is to tackle rural poverty and disadvantage. The RCN's mission statement for example states that it 'articulates the voice of rural communities on issues relating to poverty, disadvantage and equality'. The experiential expertise and 'evidence' of the two stakeholder groups emphasized the importance of addressing poverty and social exclusion and the normative understanding of these as the rural policy priorities has been historically embedded since the emergence of rural policy. I will consider now how the organizational culture of the civil service is more attuned to absorbing the normative knowledge of stakeholder groups than empirical knowledge from the academy. This relates to the structure and culture of the civil service.

Evidence to Inform Policy

Remembering the Importance of Status in the Civil Service

The rural policy programme of work was premised on the normative knowledge that rural areas are disadvantaged, need special care and attention, and that a 'bottom-up' approach to the formation of policy, with the heavy participation of

stakeholders, would make for better policy. The key policy initiatives were the development of a Rural White Paper, rural champion function across government, and a rural anti-poverty programme. The two key stakeholder groups were centrally involved in providing evidence to justify the need for these policies, in the delivery of various elements of the policies, and in the drafting of policy.

When I began my Knowledge Transfer Fellowship, I established an advisory group that included a mixture of grades of civil servants, farming unions, rural stakeholder groups and academics. This group met six times over the course of the Fellowship. I also organized a 'think tank', which brought together academic providers of evidence from across the British Isles, to establish what worked well and what did not work well in terms of effectively advising government on rural policy. At this think tank, there were many senior DARD civil servants, as well as a mixture of grades from the team. This was my approach to identifying the best structures that allowed the flow of evidence to inform policy. As contracted, I was also examining what evidence DARD was currently using to inform its policies. When I examined available sources of evidence (statistics, public attitude surveys, other departmental policies) (see Shortall 2010), I could find little justification for the emphasis on rural poverty and social exclusion. There is considerable affluence in rural areas, people who are happiest with where they live are those in rural areas, and population projections for the region show that populations in accessible and non-accessible rural areas will increase because of in-migration (Shortall 2010). In other words, the evidence I could find suggests people choose to live in rural areas. While this is not to say that there is no poverty in rural areas, there is no evidence to suggest that all rural areas are poor. When I examined the evidence base the Policy Division was using, I concluded they were relying too heavily on stakeholder groups to provide evidence and this was skewing policy priorities. The stakeholder groups are funded to address poverty and disadvantage in rural areas, and therefore there is an incentive on their part to highlight poverty.

I made this point at various presentations to the advisory group, and to the think tank meeting. I did so as part of my brief was to identify and comment on existing sources of evidence used to inform policy. While I was never dismissive when doing this, I did state that the Rural Policy Unit needed to broaden its sources of evidence in order to have better informed policy. I thought little of this at the time. Academics dispute, argue, build upon the works of others, and interpret the world differently (Denzin 2009). The authority of your argument depends on the robustness of the case presented. However in these situations, I did not at the time reflect that the authority of the senior civil servants depended on their grade. Their authority is not questioned, it is assumed. While I had been asked to examine and comment on the evidence base used to create rural policy, the *process* of doing so publicly contravened the norms of civil service – I questioned authority and judgement. Public servants do not express publicly their personal views on government policies or administration. While I was not expressing a personal view, I was publicly expressing a professional view that questioned the evidence being used to inform policy. Academic authority comes from making an

argument publicly, either at conferences or through publication, and having peers review and assess the value of our work. This is very different to the civil service where authority comes from the position of the civil servant in the hierarchy and that authority is not questioned publicly.

The Generalist, the Specialist and Knowledge Power Struggles

Over the past number of years, the Sociology of Scientific Knowledge (SSK) has shown that science is as much a socio-cultural activity as a technical enterprise. The idea of the neutral, disinterested and objective expert has been dispelled through an impressive array of empirical studies of scientific controversies that illustrate the way in which scientific knowledge is not only based on objective rules of experimental procedure, but also the interpretations, actions and practices of scientists (Martins and Richards 1995). Values are deeply embedded in the practices of sciences that address complex, real world problems, and this has implications for how the scientific ideal of objectivity is construed (Alroe and Noe 2010). While these arguments abound in the academic literature, it remains the case that the 'idea of science' confers authority (Shapin 2007). I found this to be particularly the case in DARD, a government department used to dealing with natural scientists providing evidence to inform fisheries, agriculture and other policies. With rural policy the legitimacy of the source of knowledge becomes more difficult to establish. Who are the right evidence providers to inform policies to ensure the sustainability of rural society? How can the validity of social science be distinguished from that of stakeholder groups? Social science still suffers from the myth that natural science is more 'valid' (Yearley 2006). The two stakeholder groups exert the legitimacy of their evidence by claiming that they represent 'the common good', and they represent people living in rural areas who are 'rural experts' (Shortall 2012). In this context, civil servants have the power to decide which evidence will be deemed more valid, or at least which they will choose to use.

One issue about the use of academic 'evidence' by civil servants relates to the question of anonymity. Academic status comes from the authority of published work. Intellectual property rights prevent any academic passing off the work of another as their own. However, within the civil service, reports and policy documents do not have an author. It can sometimes take some time to identify who worked on a policy document, and frequently it is a number of people. This anonymity is part of the protection of individual civil servants. However, when a large part of my report appeared in a policy document, I wrote to a senior civil servant requesting that a footnote acknowledgement be added to the final document. I also encountered another case of a team of academics asking for recognition of their research in a policy document while I was based there. This is an instance of the difference between the two cultures. What made it easier for civil servants to use the evidence of stakeholders, was that stakeholder groups do not care about acknowledgement for use of their material. Stakeholder groups are trying to shape

policy so if their evidence is used to support and develop the policy position they want, then they have been successful. There is no issue for civil servants of using stakeholder material in documents.

To a generalist civil servant, experiential evidence is more accessible. While I was complimented many times on being an ‘accessible’ academic, it suggested this was not their usual experience. I did experience some evidence provided to the team by academics that would have been inaccessible to anybody but other academics. This caused some frustration all round; academics were annoyed their work was not used, and the civil servants were annoyed they did not get something they felt they could use. On the other hand, the stakeholder groups present evidence through ‘case studies’ or life stories. They recount the life situation of three rural people experiencing poverty and the subsequent difficulties they experience based on their location. This ‘evidence’ reaffirms the correctness of established policy priorities, and by doing so it reinforces civil servant position and authority by suggesting senior civil servants have designed the best policy for rural areas. The validity of this evidence is not relevant. It is argued elsewhere that validity of evidence is frequently not relevant (Stevens 2007; Monaghan 2009). What matters is that it is accessible to the generalist civil servant; it makes sense; and reaffirms their policy world-view. It is to this affirmation of the policy world-view that I now turn.

Controlling Uncertainty

One of the roles of the civil service is to minimize uncertainty. While governments change, the civil service is a constant. Designing favourable policies is central to social order. The expertise of civil servants is crucial to the development of policy. Civil servants will have experience of what has worked in the past and the most palatable way to interpret evidence to design policy. Given that government strives to control uncertainty, evidence will often be interpreted to justify existing policies (Stevens 2011; Monaghan 2009). Rational actors within a policy context will prefer stable institutional arrangements to situations of uncertainty. Change will be resisted if it is unclear how palatable changed policy alternatives might be with the public and key interest groups (Hall 2009).

The historical development of rural policy in Northern Ireland embodied a normative understanding of policy priorities as a focus on poverty and disadvantage from the outset. The values and beliefs of key players in the rural policy infrastructure established in the early 1990s all shared and reinforced this understanding of social reality. When I presented alternative evidence that suggests affluence in rural areas, and which shows that population projections indicate a growth in population for rural areas because people see rural as offering a better quality of life, this presents uncertainty. Being a rural champion, ensuring equity for rural dwellers, and tackling rural poverty and social exclusion, is an attractive mandate for any Minister of Agriculture and Rural Development. The previous DARD Minister time and time again, spoke of her awareness of

the hidden nature of poverty in rural areas and her commitment to tackling the unique challenges faced by rural dwellers.⁴ Reference was made throughout the last election campaign to the success of the ten million pound rural anti-poverty and social exclusion programme.⁵ In this case, the normative knowledge of policy priorities allows for a political programme that is premised on equality. This is attractive with the electorate and politicians naturally are in favour of policies that appeal to their constituents (Boswell 2004). For me to suggest that such an investment in rural poverty might be misguided, would be tantamount to political suicide for a DARD Minister.⁶

Rural policy is a contested and vague concept and one that a government department that previously only dealt with primary industries now has to make sense of. The Rural Policy Division has to design rural policy. Tackling rural poverty and social exclusion in rural areas is a laudable and commendable goal. It does not really matter if it is a misguided goal; it reassures the public that DARD cares about rural policy. There is considerable moral weight attached to policies that address poverty (Cao et al. 2009). It allows the Rural Policy Division to justify their existence. It is also one that their two key stakeholder groups will look on favourably, as it justifies their existence and source of income. Shared ideology is important for the survival of organizations (Hasenfeld and Gidron 2005). The shared ideology of the Rural Policy Division and the NGOs about rural poverty and disadvantage gives meaning to their activities. In addition, it makes for an electorally attractive political manifesto. But most importantly it allows for continuity and minimizes political and policy uncertainty. DARD has always had eliminating rural poverty as a policy priority. To suggest that evidence demonstrates that the extent of rural poverty is considerably less than previously believed and might not need specific policies, would lead to unknown reactions from rural voters.

Conclusion

In recent years there has been a noticeable tendency, which demands academics, including sociologists, to demonstrate the public value of their research. There is growing incentive for academics and universities to engage in this process as the academy comes under pressure to demonstrate its use-value to technology, social and economic development (Rubio and Tshipamba 2010; Monaghan 2009;

4 See for example: <http://www.theyworkforyou.com/ni/?id=2011-01-31.7.31>; <http://www.sward.org.uk/latestnews/name,3560,en.htm>; <http://www.fermanaghsouthtyronesf.com/news/18779>.

5 See footnote above.

6 Monaghan (2009) makes a similar point about politicians suggesting that increasing the presence of police officers actually has no effect on crime prevention. This is what the evidence shows, but it is very unpalatable with the public.

Oreszczyn and Carr 2008). Our ability to demonstrate ‘knowledge-transfer’ and the ‘impact’ of our research are now measures used to assess our standing as academics (Browlie 2009). I have argued that a key factor shaping how our research is used is the ethos and culture of the civil service.

While the civil service and government more generally are keen to advance evidence-based policy, I maintain that there are tensions in the relationship between academic evidence and policy formation. Both the academy and policy are keen to engage with each other. However, participant observation allowed for a more nuanced understanding of the culture of the civil service and how it differs from the culture of the academy.

While the civil servant is esteemed for generalist knowledge, the academic’s authority comes from specialist knowledge. Sources of evidence more accessible to a generalist may well be favoured to robust but inaccessible specialist evidence. In addition, how evidence is presented within a hierarchical culture where status is attached to grade and authority is not questioned needs to be considered. Evidence critical of current policy also criticizes the senior civil servants responsible for this policy area. Evidence which reinforces the validity of policy choices will be more attractive to evidence that questions the legitimacy of existing policy.

Academics present their research publicly and invite debate and comment. Other academics will argue with research findings and use and build upon research. Academics will find it difficult to have work published that is not original and contributing to our existing body of knowledge. On the other hand, one of the key tasks of the anonymous civil servant is to ensure social stability and minimize uncertainty. This demands a conservative, risk-adverse approach to policy design. How evidence will be used in designing policy will be shaped by these priorities for the civil service.

As sociologists we are well aware that the values and social norms of an organization shape how it functions and the type of social relations that develop in that organization. As we continue to endeavour that evidence is used to inform policy, we need to reflect on the organizational culture of evidence users, and consider whether that culture facilitates or impedes the use of evidence.

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Chapter 9

Public Sociology in Taiwan, Hong Kong and Singapore

Albert Tzeng¹

Public Sociology, an agenda advocated by Michael Burawoy (2005), has attracted extensive theoretical debates in the West about how sociology should be balanced between the pursuit of scientific professionalism and commitments to public causes. There have been, however, fewer systematic, empirical surveys of its actual practices – particularly in the peripheral sphere beyond the core countries in the West. This chapter maps and compares the public engagement of sociologists in Taiwan, Hong Kong and Singapore. Three critiques of Burawoy’s scheme will first be outlined in order to introduce a revised conceptual framework for sorting and analysing the empirical material. The findings in each case will be described and discussed, followed by a comparative summary.

Public Sociology

The agenda of public sociology, despite its recent origin, reflects a long-standing tension in the discipline, between its pursuit of scientific professionalism and commitment to public engagement. This dual identity of sociology can be traced back to the contrast between Durkheim, who made great effort in securing the professional status of the new discipline as a “positivistic science” (Durkheim 1938), and Marx, who passionately argued that “the point is to change [the world]” (Marx 1854). The subsequent history of (American) sociology has also been characterized by a division between a conservative mainstream camp that insists on a professional core, and a radical wing urging a more engaged practice. Mediating between the two stances, Burawoy proposed a “division of sociological labours” to grant visibility and legitimacy to its four categories,

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Table 9.1 Division of Sociological Labour

	Academic Audience	Extra-Academic Audience
Instrumental Knowledge	Professional	Policy
Reflexive Knowledge	Critical	Public

Source: Burawoy, 2005, Table 1.

divided in terms of “audience” (academic vs. extra-academic) and the “type of knowledge” (instrumental vs. reflexive): the professional, critical, policy and public sociologies (Table 9.1). These conceptual dimensions reminded us of the two questions raised by Alfred McClung (1976): “sociology for whom?” and “sociology for what?”

Practical Challenges

Three challenges emerged, however, when I applied this conceptual framework to empirical studies in Asia. First, the idea of “public” varied. In interviews about “public sociology”, there was often initial confusion as to whether “service in the government” or “expert consultancy” (which came closer to the category “policy sociology” in Burawoy’s terms) should be included. This departure from Burawoy’s original conception, I suggest, reflects the cultural bearing of the Confucian intellectual tradition, which considers “service in the government” to be a respectable way to serve the public. The second challenge, also related to the distinction between policy and public sociology, was that it confuses two conceptual dimensions: *type of knowledge* and *audience*. While Burawoy defines the distinction in terms of the former (instrumental vs. reflexive), the primary audience of the ideal typical cases of the two categories also differ (state authorities or corporate clients vs. civil public). The confusion might be attributed to the “affinity” between the two aspects. Those serving the authorities might tend to impose instrumental knowledge without challenging its basic assumptions, whereas those engaged with the civil public were more inclined to employ reflexive and critical thoughts. However, we could easily conceive opposite examples, such as political advisors who address the authorities with critiques of current policies, or those serving communities with instrumental expertise. Third, the distinction between “traditional” and “organic” public sociology, an aspect that I call *depth of engagement*, can find its counterpart on the policy side. We can consider “commissioned policy research” and “research briefing for authorities” as “traditional” policy sociology, because these modes only involve the dissemination of research findings. By contrast, service as an officer (on secondment), advisor, or board member in government or public bodies could be seen as “organic” policy sociology, since these modes involve direct extra-academic involvement.

Toward a Sorting Template

To cope with these challenges, I defined the concept “public” in a more expansive way, to include any engagement with “extra-academic” audiences, which encompassed the right half of Burawoy’s 2x2 table. The revision might depart slightly from the Western etymology of “public”, but it reflects more faithfully how the term (and its translation) is understood in some Asian contexts.² To distinguish between modes of public engagement amongst sociologists, an ideal-typical framework was devised with three binominal variables: (1) audience: powerful clients (state authorities or corporates) vs. civil society, (2) depth of engagement: the traditional, mediated dissemination of sociological knowledge or research outputs (press commentary, website) vs. the organic, direct, organized engagement (such as activism or service), and (3) type of knowledge: instrumental vs. reflexive. The three variables create eight possible combinations (see Table 9.2), each of which correspond to modes of practice that could be considered as fitting to the criteria that define each cell.

Table 9.2 Modes of Public Engagement of Sociologists

		State/Corporate Client	Civil Society
Traditional (Mediated)	Instr.	Policy Research Expert Testimony	Public Dissemination of Research Expert Account on Media
	Reflx.	Critical Policy Research Critical Letter to Authorities	Critical Writing for Public Critical Commentaries on Media
Organic. (Direct)	Instr.	Service as Seconded Officers Expert Consultancy	Service in Community Org.
	Reflx.	Service in Independent Org Service as Gov Advisor	Advocacy Group Petition and Protest

This table provides a symmetric conceptual framework with more analytical angles. But in practical coding, it was difficult to place the reviewed empirical cases on the third dimension (type of knowledge). This judgement requires closer scrutiny of the textual material of each case, and is less feasible for a comparative

2 Imposing the original, narrower definition of “public sociology” in the Asian context runs the risk of being Anglo-centric. Methodologically, maintaining its distinction from “service to authorities” fails to reflect the experiences of numerous Asian sociologists who exert their influence BOTH as a public intellectual and as a partner of the state. Ethically, excluding particular practices from the legitimate scope of “public sociology” showed insufficient respect to the subjective meaning perceived by its practitioners.

Table 9.3 **Sorting Template of Public Sociology**

Principal Categories^a	Mode of Practices	Data Availability
State/Corporate <i>Organic</i>	Service as Officers or Advisor to Gov/Public Body	Departmental Handbook, Website, Interviews
State/Corporate <i>Traditional</i>	Commissioned Policy Research	Publication List, Interview, Meta-Statistics
	Expert Testimony	Record not available X
	Critical Advice to Authorities	Discreet nature X
Civil Society <i>Traditional</i>	Books for Public Readers	Library directory, observation in bookstores
	Sociological Website	Online Directory, Searching Engine
	Media Commentaries	Interviews, Newspaper database
	Public Talks	Interviews, internet data- mining
Civil Society <i>Organic</i>	Community Service	Departmental Hand book, Website, Interviews
	Advocacy Group	Departmental Hand book, Website, Interviews
	Petition and Protest	Interviews, Internet data- mining

Note: ^a The ordering of the four categories roughly corresponds to their “distance from power”. The direct involvement in State or corporate client was placed first, followed by the traditional mode of “public dissemination” of research to both audiences. The organic engagement in the civil sphere, which might be strongly oppositional to authority, came at the end.

project of this scale. Moreover, many of the empirical cases examined employed both instrumental and reflexive knowledge, and the difference was just a matter of *degree*. Hence I dropped the third variable from the ‘Sorting Template’ ultimately adopted in the empirical study, which came up with four principal categories defined by the variables “audience” and “depth of engagement”. Each cell includes a number of conceivable modes of practice (see Table 9.3, the second column), from which, however, the relevant data might not be available (the third column, **X** indicated data unavailability).

This template was used as a guiding apparatus in my field investigation in the three locales. The data availability varied; notably I have not acquired sufficient data on the “community service” sector to draw any conclusions. Nonetheless, some significant patterns and contrasts can be confidently established.

Singapore

Singapore is a state known for its “culture of control” (Trocki 2006), “enthralled media” (Seow 1998) and questionable freedom of speech. It seems an unlikely land for public sociology (at least in Burawoy’s sense). However, when confronted with relevant questions, many informants still stressed the presence of colleagues committed to the public cause. They were just making contributions via channels that were more institutionalized and more politically-agreeable, such as commissioned policy research and expert consultancy. This is where I started to reflect on the definition of “public sociology”. On the other hand, it was generally agreed that a mechanism of self-censorship was at work and that it discouraged sociologists from engaging the civil public. Nonetheless, a few notable figures still managed to cast their influence through delicately balancing their public presence with the trust they accumulated from the authority.

Policy Research/Consultancy vs. Limited Public Dissemination

Sociology in Singapore has historically engaged closely in public issues, in particular in the 1970s and 1980s when sociological expertise was in great demand in numerous state-funded projects to meet the challenges of nation-building (Yee and Chua 1999: 229). An interviewee (SG12³) described the state through this time as a “systematic employer of sociology, and it took an applied approach. This particular variation of public sociology believes that the contribution of sociology was to shape public policy”. The research was focused on four domains⁴: industrialization, urbanization, changing demographic structure, and so-called “socio-cultural patterns” – such as national identity, ethnic relations, and multilingualism. The sociology department in the National University of Singapore (NUS), the only institutional base for the discipline until the recent decade, also emphasized its active role in providing consultancy.⁵

On the other hand, general public access to sociological expertise and knowledge was fairly limited. The books on Singapore society written by its sociologists were largely unavailable in major bookstores.⁶ It was neither a common practice for Singaporean sociologists to write commentary in newspapers, to accept media interviews, nor to address a public audience. A few sociologists were involved in

3 Informant code consists of country code (SG, HK and TW stands for Singapore, Hong Kong and Taiwan) followed by the chronological order of interview in each country.

4 NUS, Department of Sociology, Prospectus 1974, Handbook 1981.

5 NUS, Department of Sociology, Handbooks 1984–1998.

6 I sampled several sizable bookstores during my stay, but found limited writing on Singapore society. There were only two retailers more resourceful in this respect, the NUS press bookshop in the university campus and the Select bookshop inconveniently located on Tanglin Rd. The owner of the latter was alleged to “have been told not to stock certain titles (SG4)”.

advocacy groups – for instance, Vivian Wee and Nirmala Purushotam participated in the AWARE (Association of Women for Action and Research). But no serious challenge to authority was attempted until Daniel Goh, associate professor in NUS, publicly spoke for oppositional Workers' Party's candidate in a by-election rally on 23 January 2013.

"OB markers" and Self-Censorship

A persistent theme was the practice of *self-censorship*. It was generally agreed in Singapore that there were certain "OB markers" (out of bounds markers) one was not supposed to transgress. The perception of where these lines really were, and what the consequences would be, were they overstepped, were, however, varied. Some claimed certain topics were too sensitive to write about (for example, migrant workers, the integrity of the juridical system, or "issues related to the Lee Kuan-Yew family"), while others only considered "stepping out of your role as an academic (SG2)" as risky. The potential consequences were implied by citation of a few notable cases. For instance one expatriate recalled an incident in 1985 in which:

... there were two researchers who had worked for a long time on labour relations in Singapore and they gave what was supposed to be a closed-door talk on the 'history of the labour movement' in Singapore... and suddenly they were told to leave within 24 hours. (SG7)

Seeing incidents like this, the informant "... consciously chose not to write anything about Singapore". Two oft-cited cases were of Christopher Lingle and Chee Soon-Juan. Lingle, a former NUS economist, was charged with "contempt of court" for an essay he wrote for the *International Herald Tribune* in 1994, in which he accused an unnamed Asian regime of relying on a "compliant judiciary to bankrupt politicians". He flew to United States after being interrogated to avoid paying the enormous fine (Haas 1999: 32). Chee Soon-Juan, a former NUS psychologist, joined the oppositional Singaporean Democratic Party in 1992, and was fired a few months later by the Department Head (a member of parliament from the ruling People's Action Party [PAP]) for alleged "misuse" of research funds to send his wife's doctoral dissertation to the United States (Tamney 1996: 64). More recently, there have been anecdotal accounts about the departure (in some cases because of the unexpected termination of contract) of some former colleagues who happened to have written critically; these were recounted with varied interpretations, despite the lack of a demonstrably causal link between the criticism and the departures.

These stories shared the core feature that they inevitably contained known facts, subjective claims made by those involved, and speculation. There was no certain way objectively to determine the extent to which the government exerted the degree of control claimed by some observers. In other words, the perceived risk

of falling victim to state action was at least partially (and inevitably) constructed by actors within the system. This was not to say that the sense of fear was ‘fake’ – such fears are based on some facts, and any suspected exaggeration was due to the questionable transparency of, and the lack of trust in, the official account. A senior informant explained the elusive nature of the mechanism:

...The line [of OB markers] was naturally difficult to map. If you ask those who are in power, they can't neither specify where it is. Different people observed different lines; the objective line others set for you also varied. (SG10)

The uncertainty concerning the “boundaries of the speakable” however made it rational to step back in order to prevent risky consequences and this tends to create a politically conservative culture. One senior informant (SG2) observed “there are very few people in Singapore, academics included, who are really able to be seriously critical of PAP (the ruling People’s Action Party)... they think too highly of themselves, they think the government would go after them”. He believed that academic freedom in Singapore was greater than many had assumed. Moreover, the next section will show how these boundaries are not always fixed.

Trading in the Middle: The Art of being Critical

Despite the prevailing sense of ‘state control’, a few sociologists still engaged with the public often – sometimes in ways critical of the government. Chua Beng-Huat (蔡明發) was frequently the first name cited in interviews. Chua, a Singaporean, completed postgraduate studies in Canada in the 1970s, where he was exposed to critical theories. His university web profile⁷ stated that he “returned to Singapore in 1984 to take up the Director of Research post at the HDB but was fired from that job for his critical writings on Singapore politics”. He subsequently joined the NUS where he “brought Foucault and postmodernism to the department” and continued to write critically on issues like housing and the privatization of education (Khondker 2000: 116) Chua was often considered to be the target Lee Kuan-Yew had in mind when Prime Minister Lee in the 1992 Chinese New Year speech (*Strait Times*, 9 February 1992) expressed concern as to the influence of some contemporary Western thoughts on young academics. A later article by Chua (*Sunday Times*, 3 October 1993) on rising living costs in Singapore was also criticized by Lim Boon-Heng, then a Minister in charge of the Cost Review Committee, in the Parliamentary Debate. Lim chided “as a sociologist in our university, he should read the Report before he passes judgment” (Parliamentary Debates 1993: 718). The two incidents made Chua “the most publicly-scolded sociologist in the country”, and there was speculation that Chua would soon be fired (SG2). However, he stayed on, and promoted a critical angle in his *Communitarian Ideology and Democracy in Singapore*

7 <http://profile.nus.edu.sg/fass/soccbh/> HDB stood for ‘Housing Development Board’.

(1995) and *Political Legitimacy and Housing* (1997). Another figure was Kuo Kien-Wen (郭建文), whose commitment to public affairs can be traced to his earlier involvement in the 1970s student movement. Kuo returned Singapore in 1991 and continued to appear as an invited speaker on various occasions organized by various civil groups – including the radical journal *Tangent* and the independent centre for critical art *The Substation*. Meanwhile, he maintained a record of service in government-related bodies like the National Heritage Board and National Archive.

These two figures were often cited as the prime examples of the critical engagement of sociologists in Singapore, but in my view, their writings still focus on a ‘softer’ range of issues (such as specific policies, culture and consumerism) without engaging more deeply with the hard core questions that directly confronted the legitimacy of PAP rules (for example the election system, judicial system). Even on occasions where they came across more sensitive themes, the wording was fine-tuned within a range that avoided being provocative. This impression was supported by a number of informants. A senior scholar commented on Kuo and Chua as “relatively critical, but they did not touch on hard issues, which was the bottom line. That remained untouchable in Singapore. In particular, teaching in the universities meant you were considered part of the institution” (SG10). Another informant described Chua as successful in “striking a balance between criticism and involvement ...[and]... trading in the middle” (SG12).

There were some others who were of higher visibility in the mass media. They, too, demonstrated the art of balancing “criticism and involvement”. Eddie Kuo (郭振羽), now Emeritus Professor of Nanyang Technological University, had studied and taught in Taiwan and the United States before joining the NUS Sociology Department in 1973. He became the Founding Dean of the School of Communication and Information at Nanyang in 1992–2003, and subsequently served on the Council of the University. He had also chaired various government committees related to publishing regulations and media policies. His high profile in higher education and media made him a popular interviewee on issues related to the two sectors – which was occasionally critical. I was told that Eddie Kuo had in private reminded his junior colleagues that there was no guarantee that, were they to make similar comments to those he had made in public, they would be deemed acceptable. What Eddie Kuo “had done or said before”, the informant explained, “secured the qualification to say things at a certain level without getting into trouble...” (SG10). Syed Farid Alatas, who was actively involved in the statutory body Majlis Ugama Islam Singapura (MUIS, Islamic Religious Council in Singapore), often gave public lectures and press commentaries on issues related to the Muslim community. He might say things critical in the interviews, but as one observer indicated, Alatas “was not subversive and... the government know enough about [him]” (SG9).

Hong Kong

Sociologists in Hong Kong were rarely approached by the colonial government for policy consultancy, but neither did they perceive much risk when engaging the public. Many scholars who came to Hong Kong after World War II noted the clearer sense of “freedom” in the colony – especially when compared with China or Taiwan where the expression of thought was largely constrained by the ideological confrontation (for example, Yu 1998). Numerous sociologists engaged the audience beyond academia in various ways, although they remained a minority. From the mid-1990s higher education reform triggered quite complicated responses. While the post-reform cohort faced unprecedented pressure that drove them away from public engagement, the more established cohort ironically made more efforts.

Distance from Power

Contrary to the case of Singapore, sociologists in Hong Kong were never involved in the policy process to any significant degree. A senior informant explained:

...the British colonial government basically did not trust, and was not willing to commission, the domestic scholars for research. There might be certain political considerations – not being willing to disclose too much information, and reluctance to have domestic scholars involved in politics. (HK15)

The more popular practice, by contrast, was to commission scholars from the UK or other commonwealth countries to write reports based on short research visits. There had been no academic sociologists who had served any significant role within the colonial administration. After the 1997 handover to China, a few sociologists were absorbed into the institution – notably with the appointments of Lau Siu-Kai (劉兆佳) and Li Ming-Kwan (李明堃) to the Central Policy Unit. But in general the engagement of sociologists in the policy process remained limited. On the one hand, the universities were given the mission to pursue “international excellence”; on the other hand, the administration had developed its own team for policy research and evaluation.

Public Intellectuals (Senior Cohort)

There were a series of sociologists in Hong Kong who communicated beyond an academic audience. Of the senior cohort who joined the teaching force before 1980, Ambrose King (金耀基) was perhaps the best-known figure. A Chinese migrant student educated in Taiwan and the United States, King had already made his name in Taiwan with a best-seller on Chinese modernization (King 1966) before coming to Hong Kong. An article he wrote after the 1977 termination of the formal diplomatic tie between the United States and the “Republic of China”

(Taiwan) furthered his popularity as a writer on political issues in greater China (King 2001). Of King's cohort, Lau Siu-Kai and Lee Ming-Kwan have more visible public profiles on issues relating to Hong Kong. They were classmates in the newly founded Faculty of Social Science in the University of Hong (HKU) Kong in the aftermath of the 1967 left-wing riots. They both joined the HKU student publication *Undergrad* (學苑) as editors. They were both known for their writing on Hong Kong politics and society (Lau 1982; Lee 1987), and both wrote extensively for public readers. A journalist of *Wide-Angle Lens* magazine described how the writing of both "brought sociological theories to the local, lived context", and acclaimed them as "truly sociologists of Hong Kong" (Wong 1985). Their prominent public profile placed Lau and Lee on the short-list of "Hong Kong Affairs Consultants" employed by the Chinese government. After the handover, they were both appointed to the Central Policy Unit. This political move represented an attempt of the Hong Kong Special Administrative Region government to broaden its political legitimacy. The cooperation was controversial. While some saw the role as a way to make a greater contribution to society, more critical observers considered accepting such appointments as manifestations of compliance to the new regime.⁸

Public Intellectuals (Middle Cohort): Three Currents

The middle cohort contained more sociologists with higher public visibility (those who joined the teaching force between 1980 and 1995). Three approaches can be distinguished:

The first approach, represented by Lui Tak-Lok (呂大樂) and Ng Chun-Hung (吳俊雄), focused on public writing about the culture and society of Hong Kong. Lui and Ng were both born in Hong Kong and grew up in the "MacLehose Years" (1971–82) – a period in which the booming economy and thriving popular culture in Hong Kong gave rise to a sense of Hong Kong identity (Lui 2007a). They developed great interest in popular culture before entering the university and became editors of HKU *Undergrad* magazine. They went to England in the early 1980s and returned to teach after the 1984 Sino-British Joint Declaration about Hong Kong's future was announced. Lui and Ng both became widely known in Hong Kong for their public writings, news commentaries, radio talks, and Lui also chaired the Hong Kong think tank 'SynergyNet' (新力量網路).⁹ Compared with Liu SK and Lee MK, Lui TL and Ng CH paid more attention

8 The criticism was not without legitimate ground. Lau SK, after joining the government, had publicly announced his "four no principle"- no comment on Beijing's policies, no comment on other minister's words and act, no confrontation with the government, and no objection to the Hong Kong policies. See http://www.com.cuhk.edu.hk/ubest_past/031259/polotical_ppl_01.htm.

9 See the committee list on SynergyNet's website http://www.synergynet.org.hk/b5_about4.php.

to culture and its meanings (see, for instance, Lui 1983; Ng and Cheung 2002) – even in Lui’s writing about cohort structure (2007a) and social class (2004).¹⁰ In 2002–2003, Lui, Ng, and Eric Ma (馬傑偉) initiated the Chinese (Cantonese)-medium conference series “Hong Kong Culture and Society” to communicate and encourage Hong Kong studies in the local language. The triad have so far produced three edited proceedings from the conference series (Ng, Ma et al. 2005b; Ma, Ng et al. 2009; Lui, Ng et al. 2010). These publications have greatly enriched the literature of Hong Kong in the local language. Their focus on culture might be associated with the broader “cultural turn” in sociology following the 1980s, or with their British training background. But the cardinal factor should be that they grew up in an era when Hong Kong started to develop a cultural consciousness (Ng, Ma et al. 2005a: 1), and this sense of cultural particularity was found to be the only secure ground for anchoring their sense of identity when the political fate of the entire colony was in turmoil. Ng (2005, vii) recalled his return to Hong Kong in 1985:

... The China-UK Joint declaration was already settled. The Brits were retreating, the Chinese were in the future, and Hong Kong people were looking for themselves. I looked around: the labour in Hong Kong remained barely visible, the politics was staggering under the renovated colonial administration, the only thing inspiring turned out to be Anita Mui, Alan Tam, and my schoolmate Chow Yun-Fat¹¹... then I had a big fever, an obsession with the Hong Kong pop culture...

This enthusiasm with culture however was accompanied by a frustration deriving from a lack of agency in the political process towards the reunion with China. In his recent best-seller (2007b), Lui concluded the core message was “to continue voicing how we felt about Hong Kong, whether it is politically correct or not”.

The second approach, best represented by Chan Kin-Man (陳健民), takes the reunion with China as an opportunity for broader public engagement. Chan, a Yale trained political sociologist, started teaching in Chinese University of Hong Kong in 1993. His publications reflected an intellectual trajectory gradually moving from an early interest in corruption to a later concern with NGOs and the civil sphere in Chinese societies.¹² He has engaged with the public not only

10 A number of observers pointed out that Lui managed to do so by conducting a “double life”— he wrote hard core dry, empirical analysis of social class and mobility for academic journals to establish himself institutionally so that he could write inspiring, interpretive pieces for the general public (HK5, HK22).

11 Anita Mui (梅艷芳), Alan Tam (譚詠麟) and Chow Yun-Fat (周潤發) were all Hong Kong pop singers or movie stars.

12 See his publication list on <http://chankinman.wordpress.com/academic/> [Accessed 21 Sept. 2011].

through his frequent commentaries on the mixture of media,¹³ but also through direct involvement in various government committees, forums, corporations and civil groups in both Hong Kong and China.¹⁴ Notably, in 2002 he collaborated with a group of intellectuals and professionals in the founding of the “Hong Kong Democratic Development Network” (香港民主發展網路). Compared with Lui and Ng, Chan represented a deeper (or more “organic”) mode of engagement on both the government and the civil fronts, and he focused on the more hard-core issues of politics and democracy. But his concern expanded to greater China, diluting the scholarly attention he paid Hong Kong.

The third approach refers to the radical activism taken by the triad of Fred Chiu (邱延亮), Luk Tak-Chuen (陸德泉) and Leung Hon-Chu (梁漢柱). Sociologists in Hong Kong rarely become involved in activism of a more radical or confrontational nature beyond press commentaries or involvement in civil groups. There was however an exceptional period which took place in the 1990s in Hong Kong Baptist College (University since 1994, HKBU). The Baptist College was upgraded from a 2-year institution to a 3-year state-funded college in 1990. The upgrading demanded more staff. William T. Liu, a Chicago-based psychologist, was appointed as the Dean of Faculty of Social Sciences, and five members were subsequently recruited from the United States to serve the sociology department – including the critically-minded Fred Chiu, Luk TC and Leung HC. The “accidental” synergy of the three, under a supportive Dean and Department Head, was consequential. They offered courses of a more critical nature, brought students on field trips to factories, helped establish the Staff Union, contributed a current of left-wing critiques on the media, and at their height, got involved in staging the student protest against the 2005 WTO conference in Hong Kong (interviews HK5, HK6, HK22).

Managerialism and Academic Globalism: Impacts and Reactions

Since the mid-1990s, higher education institutions have undergone restructuring that has reflected a penetrating “managerialism” and “academic globalism” (Tzeng 2010). In short, reforms have aimed to motivate academics to publish more in international journals for the pursuit of “international excellence”, and this orientation has limited the space for practicing public sociology, at least for the junior sociologists. In 2008, the Hong Kong Sociological Association (HKSA) organized a seminar on “Public Sociology in Hong Kong” and invited Ng CH and Eric Ma, two speakers “experienced in practicing public sociology”, to share their views. The talk, however, turned out to be a lament for the difficulty of practicing

13 His website listed 91 newspaper commentary entries in the time frame February 2003-Aug. 2011. See <http://chankinman.wordpress.com/commentaries/> [Accessed 22 Sept. 2011].

14 http://chankinman.wordpress.com/about_me/.

public sociology within the current institutional framework in Hong Kong.¹⁵ In particular, the speakers stressed that they were at least people “on the shore” (with tenure), and that the pressure for junior staff was greater.

The three currents of public participation reviewed above had different trajectories. First, Lui and Ng, both with secure institutional positions, made great efforts to encourage (or rescue) Hong Kong studies and maintain their public dissemination. The “Hong Kong Culture and Society” conference series was one major, and to a degree successful, attempt. The HKSA ‘Public Seminar’ established in 2008 was an initiative that evolved in a similar vein. In the short run, these reactions ironically created a resurgence of scholarly interest in Hong Kong, but a pessimistic sense about the future was still evident even amongst the most devoted practitioners. Second, the northbound projection of public engagement in China represented by Chan KM was brought forward by a few younger scholars. A notable case was the series of pieces of ‘action research’ undertaken by Pun Ngai¹⁶ (潘毅) and Ku Ho-Bun (古學斌). Both Pun and Ku migrated from China in childhood; studied in School of Oriental and Africa Studies, London; taught in Hong Kong Polytechnic University (HKPU, which maintained close connections with numerous Chinese social welfare institutions); and were concerned of the underclass in China. They took the action research approach to set up a restaurant-pub in a migrant labour community in a Beijing suburb as a base for both research and labour empowerment. Third, the current of radical activism in HKBU, however, was largely extinguished. The HKBU administrative team, under the mounting pressure of assessments, replaced the Department Head to implement its policies. Eventually Fred Chiu and Luk TC both left the department.

Taiwan

Sociology was established in Taiwan by a small group of Chinese sociologists against a generally hostile political climate. The ruling Nationalist (KMT, *Kuomingtang*) government, before being defeated on the Chinese mainland, had a rough relationship with the Chinese sociologists who were in general more sympathetic to the socialist revolution (Yan 2004: 225–35). Those who came to Taiwan represented a conservative minority of the Chinese sociological community (Tang 2008: 568), yet they were still politically marginalized. Their

15 See the Seminar Transcript on http://www.hksa.ust.hk/Word/2008_Public_seminar_1.doc (in Chinese) [Accessed 21 Sept. 2011].

16 Pun won the 2005 C Wright Mills Award for her monograph about the female Chinese workers, *Made in China* (Duke University Press, 2005). She had established herself in Hong Kong University of Science and Technology before joining HKPU— a less prestigious institution that has an applied orientation and less publishing pressure. By moving to HKPU, an informant observed, “she can really focus on what she thinks is worthy” (HK10).

involvement in policy was limited and self-censorship was common. In the 1970s, a stream of press commentaries by a few sociologists educated in the United States emerged. This current of public engagement acquired much momentum in the process of political democratization in the late 1980s, and has now evolved to include a wide array of practices that range from press commentary, public talks, involvement in various civil groups and occasional confrontations with authority. Even scholarly sociological writings are accessible to public readers in the forms both of monographs and edited collections distributed to major bookstores.

Political Marginalization

Public and political engagement was integral to the pre-1949 Chinese sociological tradition. This legacy, however, was only partially brought to Taiwan as the critical wing among the Chinese sociologists tended to stay in China. Those who moved to Taiwan still possessed a conviction to contribute sociological knowledge for the public good, but their actual practices were restricted both by their conservative attitude and the political climate of suppression. An emphasis on the applied value of sociology (on social work and social welfare) was evident in the early development of sociology; however, the actual collaborations between sociologists and the government were limited. For example, the government's familiarity with the discipline remained limited when it was restored in Taiwan (Lung 1963). A mid-1980s survey asked 35 sociologists to evaluate the "government's impressions of sociology", and 17 opted for either "persistent ignorance" or "persistent misunderstanding" while only one identified "persistent emphasis" (Hsiao 1987: 368). Throughout the 1980s and 90s there were only about 100 policy research projects commissioned amongst sociologists, which was insignificant in relation to the amount of commissioned research and the size of the professional community. The number dropped further after 2000 (Wang, J.H. and Chu 2003). Through the years, only a few sociologists got involved in direct political service while retaining an academic identity.¹⁷

17 For instance, Michael Hsiao (蕭新煌) had been appointed Presidential Advisor for a decade; Yi Chin-Chun (伊慶春), a family sociologist, was a board member of the Taiwan Provincial Government; and social welfare expert Lin Wan-Yi (林萬億) served as the Deputy County Chief of Taipei County and is a main architect behind the social welfare policies of the Democracy Promotion Party (DPP). On the other hand, there were some politicians who had professional qualifications in sociology. Guo Ji (郭驥), one of the first few migrant sociologists from China, continued a career within the KMT party. Huang Da-Chou (黃大洲), a rural sociologist trained in Cornell, became Taipei city mayor. Pang Chien-Kuo (龐建國), Ting Tin-yu (丁庭宇) and demographer James Hsueh (薛承泰) all had full-time teaching jobs in NTU before pursuing a political career.

Table 9.4 Cases of Organized Public Engagement of Taiwanese Sociologists

Sociologist	Institution	PhD Year	Organization/ Activities
Chui Hei-Yuan	Academia Sinica	Indiana '79	Judicial Reform Foundation Death-penalty Abolition Coalition
Chang Ly-Yun	Academia Sinica	John Hopkins '80	Medical Reform Foundation
Chang Mao-Kuei	Academia Sinica	Purdue '84	Mainlander Taiwanese Association (ethnic reconciliation)
Ku Chung-Hua	National Chengchi University	Heidelberg '87	Citizen Congress Watch Judicial Reform Foundation
Chang Jing-Fen	Academia Sinica	Ohio '89	Awakening Foundation (women's mvt)
Chen Dong-Shen Lin Kuo-Ming Wu Jia-Ling	National Taiwan University (NTU)	Minnesota '90 Yale '97 Illinois '97	promoting deliberative democracy
Lin Duan	National Taiwan University (NTU)	Heidelberg '94	Judicial Reform Foundation
Hsia Hsiao-Chuan	Shi-Hsin University	Florida '97	Nanyang Sisters Association (female marriage immigrant rights)
Fan Yun	National Taiwan University (NTU)	Yale '00	Awakening Foundation
Tai Po-Fen	Fu Jen University	NTU '00 (Urban Planning)	Higher Education Union
Lin Chin-Ju	Kaohsiung Medical University	Essex '03	Nanyang Sisters Association Southern Aboriginal Community Reconstruction
Shen Hsiu-Hua Ke Chao-Ching	Tsing-Hua Univ Chiao-Tung Univ	Kansas '03 Tunghai '07	Awakening Foundation Taiwan Association for Truth and Reconciliation (transitional justice)
Kang Shih-Hao	National Formosa University	Warwick '08	Green Citizen Action Alliance
Tsai Pei-Hui	Shih-Hsin University	NTU '09	Taiwan Agricultural Frontline
Chiu Hua-Mei	Nat Sun Yat-Sen University	Essex, '10	Citizen of the Earth, Taiwan
Chiu Yu-Bin	Nat Pingtung University of Education	Essex, '10	involvement in various labour unions

Political Democratization and the Emerging Civil Space

Sociologists found a larger stage in the broadly conceived civil space that consisted first of a number of mainstream newspapers and magazines, and latterly of enriched possibilities of activism in various sorts of organized ways. This current of influence can be traced back to late 1960s during which time a number of scholars returning to Taiwan with American PhDs started to form a small network through their participation in *Thought and Words* (思與言), a journal of the humanities and social sciences, and the *University Magazine* (大學雜誌). In the mid-1970s, they were invited to contribute columns for *United Daily* (聯合報) and *China Times* (中國時報), two mainstream newspapers founded by intellectual entrepreneurs with a sense of humanistic idealism; subsequently they also founded the *Independent Evening News* (自立報).

This group of writers, influenced by both traditional Chinese intellectual idealism and the American idea of democracy, played a critical role in disseminating concepts such as liberty, equality, democracy and pluralism under authoritarian rule. They were therefore referred to as the “liberal scholars” (自由派學者) to signify their standing in relation to the conservative authoritarian regime, and they were considered as one constructive force toward eventual political democratization in the late 1980s (Chiu 1999). This group included a few sociologists, whose public writings were described by some junior sociologists as what inspired them to join the profession. However, writing under such a suppressive climate, those “liberal scholars” made some compromises to avoid being excessively provocative.¹⁸ Hence they were also criticized by later writers from more radical stances for “wagging in political stance” when “mediating between the KMT and the *Tang-wai*” (黨外 literally ‘out of the party’, referring to the grassroots opposition force which later became the basis for the rise of the DPP) (Fu 1995).

The abolition of Martial Law in 1987 opened up the space for public engagement amongst sociologists in a more organized way. Some younger intellectuals who were discontented with these liberal scholars founded the radical society *Taishe* (台社) and its associated journal in 1988 (Fu, *ibid.*). In 1989, 21 academics broadly identified as the “liberal scholars” formed the *Taipei Society* (澄社). The 1980s was also remembered by many sociologists of the younger cohort as a definitive period for anchoring their vocation in the discipline. An informant who attended university in the 1980s recalled why he chose sociology for a career:

It was obvious to our cohort why we should study sociology ... The student movement was rampant, and many social movements were emerging. The

18 Michael Hsiao recalled that he had to conceal writing about “indigenization” (a forbidden theme as it implies separation from China) in the disguise of “Sinicization”. The writings of these liberal scholars under the authoritarian regime often involved a delicate balance in order not to be riskily offensive, which shows some resemblance with the art of “mediating in the middle” demonstrated by the public-minded Singaporean sociologists.

Table 9.5 Public Engagement of Sociologists in Taiwan, Hong Kong and Singapore

Categories	Taiwan	Hong Kong	Singapore
Service to Government or Public Bodies	A few notable senior scholars appointed.	None in the colonial period Lau SK, Lee MK appointed to Central Policy Unit, 2002	Significant ratio of NUS faculty members (all SG national) involved in gov or public bodies. Gradually declined in the last decade.
Policy Research	Limited. Roughly a hundred projects in 80s–90s, significant decrease afterward.	Little. “They prefer inviting foreign scholars”.	Extensive initially, declining since the 1990s.
Books for Public Readers	Strong tradition of domestic publication in the Chinese language.	A few scholars form the second/third cohort. Some recent collections in Chinese on Hong Kong society.	Limited availability in Singapore bookstores.
Website by sociologists	Resource sites for teaching sociology courses. Topical sites on Gender, STS, SARS emerging in the past decade. Numerous personal sites.	Few, e.g. the personal site of Chan Kin-Man.	Sociology blog Singapore since 2008 (rather inactive since '09). No personal site found.
Media Commentary/ Public Talk	‘Liberal scholars’ invited to contribute columns in newspapers since 1970s. Press commentary a common practice to date. Frequent public talks by sociologists in events held by schools, bookshops, media, foundation	A few scholars form the second/third cohort wrote commentaries for press. HKSA public seminars since 2008. Some civil groups (e.g. HK Reader bookstore) held small scale talks	Self-Censorship at work. Chua BH, Kwok Kien-Wen, Eddie Kwok were occasionally interviewed/talked to the public.
Advocacy Group	Cases in groups advocating human right, immigrant right, medical-reform, media reform, gender issues (Table 9.4)	Chan KM on Democracy Network	AWARE (feminist group)
Confrontation (Petition and Protest)	‘Wild Lilly’ to ‘Wild Strawberry’. Frequent petition mobilization and occasional protest.	1990s, three HKBU Sociologist engaged in anti-WTO.	None that I am aware of.

Strong	Moderate	Slight	Barely Existent
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student activists of our cohort sought answers in scholarly writing. Sociology simply became popular at a time of tremendous transition... (TW10)

The sociology department of the National Taiwan University then was described as an “oasis of the student movement (TW18)”. The Wild Lilly Student Movement staged in 1990 marked a significant milestone. The demand made by the student protestors was responded to favourably by the President Lee Teng-Hui (李登輝), leading to a series of political reforms. Many sociologists had participated in, or witnessed, the incident either as students or staff, and had acquired the conviction that knowledge has the power to lead social reform.

Contemporary Practices: Dissemination and Engagement

This momentum continued in the next two decades, and the public engagement of sociologists multiplied in many areas. Chui Hei-Yuan (瞿海源), a high profile sociologist who wrote 31 press commentaries between 1979 and 1987, contributed over 600 articles on the press in the 1990s.¹⁹ He and other sociologists made a visible contribution to newspaper columns and forums.²⁰ There were 370 domestic books in sociology published in the 1990s, which counted for 56 per cent of all published titles in the second half of twentieth century (Wang, C.Z. 2002, Appendix Table 2–5). Almost all these books were written in Chinese and many were easily available in major bookstores. While there were no reliable statistics, most informants indicated that they, or their colleagues, were involved in delivering talks to non-academic audiences on various occasions organized by bookshops, media, public bodies, civil groups or high schools. With the growth of internet technology in the last decade, sociologists have become increasingly involved in establishing a number of topical websites aimed at facilitating the teaching of sociological courses, encouraging participatory dialogue around timely social issues, and disseminating sociological discourse to broader audience.²¹ Moreover, a smaller but significant number of sociologists have also become directly involved in various organizations set up to advocate and facilitate a variety of reforms. Table 9.4 lists a recent sample of sociologists

19 His personal website registered 1240 commentary articles written for a variety of presses in three decades <http://www.ios.sinica.edu.tw/hyc/> (Accessed 19 Sept 2011).

20 I searched the databases of two mainstream newspaper corps (United Daily and China Times) for contributions (2000–2004) in which the author was identified as affiliated to a sociology department or institutes. I found a steady flow around 40–50 contributions annually. This is an underestimate as there were often cases in which the departmental affiliation is not specified.

21 The NTU sociology department, for instance, set up the following sites: *Sociology Teaching Resources Site*; *Technology, Medicine and Society* (teaching material); *Technology, Democracy and Society* (deliberative democracy); *SARS Media Watch* (set up during the 2003 SARS outbreak).

involved in organized intervention on issues ranging from reform of the juridical system, medical institutions, women's movements, deliberative democracy, the labour movement, environmental issues and reconciliation following historical trauma. Sociologists also took leading roles in the founding of the Taiwan Higher Education Industrial Union in February 2012.

Many sociologists without routine engagement in NGOs were involved in other confrontational initiatives (petitions, and occasional protest) as either initiators or supporters. A significant incident was the Wild Strawberry Movement (野草莓運動) initiated by the NTU sociologist Li Ming-Tsun (李明亅) on 6 November 2008 to protest against the excessive use of police force during the visit of Chen Yun-Lin, a high-ranking officer from China, and the controversial *Parade and Assembly Law* (集會遊行法)²² that legitimated such police action. The protest led to legal charges against Li MT because of his violation of the very law against which he was protesting. This incident provoked the Taiwanese Sociological Association to issue an open statement on 19 August 2009 urging revisions to the law and suspension of related trials of its members. Between 2010 and 2011 I received a number of petitions forwarded by other sociologists on issues related to labour rights, media reform, and higher education policy. A more recent incident was an open statement signed by the heads of all major sociology departments in Taiwan on 30 March 2012 urging a review of urban regeneration policies, as a response to a violent state operation that tore down a civilian property against the will of the owner and hundreds of supporting protestors.²³

Comparative Summary

A summary is presented in Table 9.5.²⁴ Three different shadings indicate my (admittedly subjective) judgment of the relative significance of activities (dark grey for strong, grey for moderate, light grey for a slight, and white for barely existent²⁵) in each sector based on the material reviewed. Three points should be acknowledged. (1) The categories were sorted roughly in the order of their "distance from authority". So categories that are higher up in the table represent a closer affinity to government or corporate clients; those lower in the table represent the civil sphere, and are more confrontational to the government. (2) The top or the bottom of the table both represent the more 'organic' modes of participation, while the central lines

22 The *Parade and Assembly Law* required prior application for staging parade and assembly in public space. Supporters appealed to the importance of social order. Critics indicated that the law restricted freedom of expression.

23 <http://www.coolloud.org.tw/node/67578>.

24 The category 'community service' that appeared in the original sorting template was removed because of insufficient material collected on this sector.

25 Readers can read the table as my personal response on a series of four-point Likert-scale questionnaire.

correspond to more ‘traditional’ modes that involve the dissemination of sociological knowledge. (3) The table is limited in that it cannot include the historical dimension; in other words, it can only present a ‘temporally-compressed’ picture.

Overall, sociologists in Taiwan developed the strongest tradition of public engagement on the civil side, including a level of activism, since the 1970s. The collaboration with the state, by contrast, was never strong. Sociologists in Singapore, on the other hand, have historically closely associated themselves with the government as a way of contributing to the public. Engagement on the civil side is present, but was conducted with delicacy because of “self-censorship” at work. Amongst the sociologists in Hong Kong are a few notable figures devoted to making sociological insight more accessible to the public. But the mode of public sociology remained largely ‘traditional’ in scope, with the exception of the recent service of Lau SK and Lee MK in the Central Policy Unit and a short-lived current of activism in the 1990s–2005 HKBU.

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Chapter 10

A Chapter in the History of Brazilian Sociology: UNESCO Research about Race Relations and the Unexpected Prejudice against Poles in Curitiba (Paraná)

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The history of Brazilian sociology has been greatly influenced by UNESCO's fight against racism that was launched just after the Second World War. In the social sciences in Brazil, this struggle culminated in research about race relations defined by UNESCO officials in 1950, within the research program on race relations (1950).² Brazil³ was chosen as a perfect *terrain de recherche*. This was mainly because UNESCO wanted to investigate countries where “contacts between race and ethnic groups” were not conflictive, or were at least under control. It was also because of Brazil's positive perspective in terms of race relations. A final reason was the participation of Arthur Ramos (1903–1949) in UNESCO. In fact, he had become a critic of racism and his thoughts about Brazilian racial relations were not far from the Brazilian ‘harmonic racial relations’ thesis of Gilberto Freyre (1900–87). Ramos (1948) perceived discriminatory attitudes inside Brazilian society but regarded the main issue as how to integrate Afro-descendants. Both authors believed that Brazil could become a successful model of harmonious race relations and could be a paradigm for any racially conflictive country anywhere in the world.⁴

The UNESCO research took place from 1951 to 1952 in the cities of Salvador, Recife, Rio de Janeiro and São Paulo. Initially, only the first city (Salvador) was selected. It was chosen because of the academic relations, previously established, between the State of Bahia and Columbia University where the Brazilianist

1 I would like to thank the Brazilian agency, CNPq, for supporting this research.

2 The Fifth Session of the UNESCO General Conference published the document “Statement on Race” (1950). See Maurel (2007) and Maio (1999; 2001).

3 They also chose Central America, Martinique and Guadeloupe Islands.

4 In Ramos's words, Brazil has shown to the world the most scientific solution to the theme of racial mixture. See Ramos 1943: 179; Ramos (1942; 1946). See also Ramos and Freyre (1937) and Campos (2003).

Professor Charles Wagley (1913–1991) was working⁵ and where an important study about Afro-descendants had been conducted by Donald Pierson (1900–1995)⁶ since the early 1940s.

The city of Rio de Janeiro was added later at the request of Luiz de Aguiar Costa Pinto (1920–2002), the Brazilian sociologist who participated in the debate about racial relations promoted by UNESCO in December 1949. The inclusion of the city of São Paulo was the outcome of the relationship between Alfred Métraux (1902–1963), who was in charge of the research by UNESCO, and Roger Bastide (1899–1974), the French sociologist, who had prepared a large research project on the theme and who was, at this time, Professor of Sociology at São Paulo University. It was included also because Brazilian social scientists wished to study the most industrialized and urbanized city in the country. Finally, the city of Recife was included at the request of Gilberto Freyre whose fundamental prestige and works on race relations in northeast Brazilian society could not be neglected, even if Freyre himself was not to take part in the research.⁷

The UNESCO research was conducted by a team made up of Brazilian and foreign sociologists whose interest in this theme was very well known.⁸ Despite the fact that the UNESCO research program had made a large contribution to the development of Brazilian social sciences,⁹ the outcome of that specific research project was ambiguous. Maio (1999) claims that it did not match UNESCO's initial ideas about Brazil, but revealed more about the way the research had been conducted. Even Métraux (1951) was rather skeptical about the non-existence of racial prejudice in Brazilian society. The local research team – all but Ribeiro – revealed that the model of harmonious (or tolerant) race relations – as described by Freyre – was far from reality and they emphasized that it was greatly overestimated. The modernization process in Brazil apparently would not lead to a democratic and varied society, it would not incorporate the lower classes, mainly mixed and black people, into mass consumer society. The main point was that the results of the UNESCO research explain the change from a “national *ethos*” to a “national problem” in relation to race issues in Brazil.

5 See more about the importance of Wagley to Brazilian social sciences in http://www.mnsu.edu/emuseum/information/biography/uvwxyz/wagley_charles_walter.html.

6 Pierson studied at Chicago University (Department of Sociology) where he gained his PhD. in 1939. He organized and directed the Department of Sociology and Anthropology at “Escola Livre de Sociologia e Política”, in the city of São Paulo. At that time, he published “Negroes in Brazil: A study of Race contact at Bahia” (1942) and “Cruz das Almas: A Brazilian Village” (1951).

7 Despite that, Freyre named his friend, the anthropologist René Ribeiro, who worked with him but was also influenced by Melville Herskovits (Professor at Northwestern University, who visited the state of Bahia several times during the 1940's) to take part in it.

8 They include Arthur Ramos, Gilberto Freyre, Charles Wagley, Marvin Harris, Paulo Duarte, Roger Bastide, and Thales de Azevedo, among others.

9 See Maio (1997).

Consequently, after the UNESCO research, a new team of young Brazilian sociologists headed by Florestan Fernandes (1920–1996) – the early career and first generation Brazilian professor who had worked with Roger Bastide¹⁰ – engaged in new research about race relations within a very large research program about the modernization process in Southern Brazil. This new research was the direct consequence of UNESCO’s program and it was supported by the Brazilian scientific agencies INEP and CAPES¹¹ and also by Anísio Teixeira and Charles Wagley.

The preference for the region was very clear. Actually, it was omitted from the first UNESCO research, for three main reasons: 1) there was no tropical-product exportation system; 2) the slavery system was less intensive and less central to the economy; 3) the immigrant colonization created patterns of social and race relations different from those that prevailed in traditional areas of Brazil. Apart from that, there were few studies about that specific cultural area. Finally, it was a region in the process of rapid industrialization. Therefore, it would be of great interest to study how afro-descendants had been incorporated into a modern class society from one which historically included a large proportion of non-Portuguese European immigrants and which had been influenced by their culture, leaving it apparently even more racially stratified than other regions in Brazil.

This time, four new cities were selected: Porto Alegre, Florianópolis and Curitiba, the capitals of the southern Brazilian states, Rio Grande do Sul, Santa Catarina and Paraná, and the city of Pelotas, not far from Porto Alegre, also in Rio Grande do Sul. These cities were shared among the young team of sociologists, Fernando Henrique Cardoso (the later President of Brazil, 1994–2001), Renato Jardim Moreira¹² and Octávio Ianni (1926–2004). The main projects were led by Cardoso and Ianni. The first worked in Porto Alegre and Pelotas; the second in Curitiba, while both shared the city of Florianópolis.

In Cardoso’s words, their goal was to “improve the sociological knowledge about racism achieved in the works that Fernandes had already produced with Bastide” (Cardoso 2003: 25).¹³ In order to do so, Ianni arrived in Curitiba in 1955 to work on race relations. He began to share the results, for the first time,

10 Bastide apparently refused to take part in this new research because he was not interested in studying the Brazilian process of modernization. Besides, he left Brazil in 1954 and returned only twice, in 1962 and 1973 (Peixoto 2000).

11 INEP was a center of educational research and CAPES the public agency for supporting the graduation of instructors of the Brazilian Ministry of Culture and Education.

12 There is very little information about him. All we know now is that he was one of the students of Social Sciences at São Paulo University and, for a short period, occupied the chair of Sociology I, which had been occupied previously by Roger Bastide and Florestan Fernandes.

13 Despite Cardoso’s opinion, there were some differences between Bastide and Fernandes regardless racial relations in São Paulo as we can see at the book both published together. Nevertheless, they agreed with each other about the existence of discriminatory attitudes inside Brazilian society (Peixoto 2000:157–98).

with Cardoso and Moreira as co-authors, in the Second Brazilian Anthropology Meeting, held in Salvador in 1957. The title of the paper was “Sociological study of relations between Black and White in Brazil”.¹⁴ Working from oral statements as well as everyday behaviors, they analysed the impact of prejudice on the integration process of Afro-descendants into the mainstream society. To accomplish the proposed goal, they used the hypothesis about racial discrimination previously developed in the city of São Paulo. They concluded that, in general terms, prejudice in the city of Curitiba was similar to that identified in the city of São Paulo.

But something unpredictable came from the investigation held in Curitiba. It produced an ensemble of unexpected and original results – discriminatory attitudes and negative oral statements against Polish descendants – beyond the prejudice against Afro-descendants. Initially, in 1958, Ianni published in *Revista Brasileira* the article “The study of the Brazilian racial situation” in which he set out to describe the development of racial studies in Brazil. Following that, he presented, at the Fourth Brazilian Anthropology Meeting, held in Curitiba in 1959, a paper titled “*Do Polonês ao Polaco*”¹⁵ (From the Pole to the Polish), to which we will return later. At the end of the 1950s, Ianni published two other small works about Poles. The first was “The social status of Poles” and the second “The economic-social system and the racial problem in Curitiba”.¹⁶ Finally, in the 1960s, he published three books whose titles were: “Colour and social mobility in Florianópolis”,¹⁷ with Fernando H. Cardoso (1960), “Slavery’s metamorphosis”¹⁸ (1962) and “Races and Social Classes in Brazil” (1966). From then until his death, he never returned to the theme of race relations with the exception of the book “Slavery and racism” (published many years later, in 1978)¹⁹ and a few interviews.

Let us take a quick look at all of these works on the theme of race. Their general subject is race relations in Southern Brazil. The central thesis developed is about racial prejudice, even discrimination in those specific societies where slavery, both economic and social, was not as endemic as it was elsewhere in Brazil. To be precise, it concerns the claim that Afro-descendants performed secondary roles within those societies and that the process of integration and the social rise of afro-descendants was not as effective as it was among European descendants. In historical perspective, the thesis suggests that the slavery pattern remained

14 In Portuguese, “Estudo sociológico das relações entre negros e brancos no Brasil Meridional”.

15 “Polaco” is a prejudicial slang said in Portuguese, we might say.

16 When we compare the two versions, we notice many differences between them. Besides that, there is no accurate information about the year when these works were written. They were finally reviewed and republished in 1966 in the book titled “Raças e classes no Brasil”. See the references.

17 In Portuguese, “Cor e Mobilidade Social em Florianópolis”.

18 In Portuguese, “As Metamorfoses do Escravo”.

19 O Ianni. *Escravidão e racismo*. São Paulo: Hucitec, 1978.

strong in everyday social interaction, evidenced by powerful racial prejudice and obstructed upward mobility.

Both Ianni and Cardoso noted that there were many more skilled workers among Afro-descendants compared with other Brazilian cities. Consequently, the social condition of Afro-descendants was somewhat different. The reasons why were not clear. From the beginning of the nineteenth century, the slavery system did not completely dominate the economic productive system in Southern Brazil. A free labor market became an alternative for the newcomers even before the abolition of slavery in 1888.²⁰ On the other hand, many immigrants, especially from German territories, had been arriving in the state of Paraná from the middle of the nineteenth century, and gradually changed the economic production system organized around the export of mate. However, it did not change the representation of Afro-descendants. Ianni confirmed that former slaves were transformed (or “metamorphosed” as he put it in the title of one of his books) into the lower class and occupied the low-paid jobs in the new class society because of the colour (black and “brown”) of their skin (Ianni 1962).

All these statements were a step forward in the understanding of prejudice and racism in Brazil. But Ianni’s most baffling discovery was about a new type of prejudice, not necessarily linked with the old one. This time, it was against European descendants, specifically in the city of Curitiba (capital of the state of Paraná). In his own words, Ianni was astonished by the prejudiced statements against Polish immigrants and their descendants that came from the interviewees.²¹

The Immigrant Poles in Paraná: Who Were They?

The first Polish immigrant group arrived in Curitiba through a spontaneous migration in 1870. This was the time when the territory of Poland was divided and managed by the Austro-Hungarian, Russian and Prussian empires respectively. The territorial occupation lasted until the eve of the First World War, which made this the period during which the American continent (USA, Argentina and Brazil) received its largest Polish immigration. The main causes are widely known, including countryside starvation, poverty, negligible harvests and the dream of becoming a landowner. This last was especially important among those who were to migrate to South America.

When they disembarked in Brazil, most Poles had passports issued by Prussia. They would be immediately sent to the “German settling zones” at Blumenau,

20 As we know, at the colonial period, southern Brazil has not produced tropical goods to export. So, this region was not integrated to the colonial export system and needed fewer slaves than others regions.

21 We may draw a parallel with the research about Poles in America that led to the classic book “The Polish peasant in Europe and America” written by William Thomas and Florian Znaniecki.

Itajaí and Brusque, in Santa Catarina, southern Brazil. In the province of Paraná specifically, the spontaneous Polish in-migration occurred from 1869. Lamenha Lins, the president of the province of Paraná, was particularly influential in this process due to his immigration policy. During his mandate (from May of 1875 to January of 1878) Lins stood out due to his extensive knowledge of the issue. He first contrasted the “spontaneous” with the “official” types of immigration. He supported the journey that the new immigrants had to take from the ports of Paranaguá (Paraná) and São Francisco (Santa Catarina) to Curitiba (the capital city of the province of Paraná). He also stimulated the foundation of new colonies around the capital and provided them with infrastructure and transportation.

Although Lins’s immigration policy had a positive effect, the immigration continued spontaneously until the end of the Brazilian Empire (1889). The data obtained from 1870 to 1889 confirm that only 7,030 Poles settled during this period. Nevertheless, there were important consequences: the immigrants made a substantial contribution to the growth of the population and the wealth of the province – a fact which would not be forgotten (Oliveira 2007).

The years following the establishment of the Brazilian Republic overlapped the great flow of Polish migrants to Brazil. Between 1890 and 1914, 96,116 Poles disembarked at Rio de Janeiro, mainly to travel onwards to the southern provinces of Brazil. During those years, Paraná may have received close to 35,000 Poles; Rio Grande do Sul was the second recipient with 32,000 new immigrants. During the first great flow, from 1890 to 1894, also known as the “Brazilian Fever period”, several colonies arose in the surroundings of Curitiba and other regions of the state including the sizeable colonies of São Mateus (1,225 settlers), Eufrosina (1,475 settlers) and Rio Claro (3,425 settlers). During the period of the second great flow, 1900–1904, other colonies were established such as Cruz Machado (5,000 settlers), Apucarana (1,000 settlers) and the mixed colony of Nova Galícia (650 settlers and 500 settlers).²²

Despite this, the reasons for the migratory flow are not to be found within the young Republic’s policy. It is rather because during the 1890s the number of interventions from the Federal government designed to legislate for and act on immigration had greatly increased (Ramos 2004: 78). During those first years we can see a transfer of policies on immigration from the states towards the Union.

At the beginning of 1892, the President of the state of Paraná explained that the immigration was already arranged and that it would just be sufficient to “promote it by advertising in the most populous cities in Europe through leaflets and the press”. In 1901, the President provided the following data: 53,047 immigrants had arrived in Paraná from all over the world between 1889 and 1900. In consequence, according to him, the supported immigration in which the Union had intervened in 1896 would no longer be necessary. In his own words, the state of Paraná would

22 We might point out here the creation of some “mixed” colonies, occupied by Poles and Ukrainians, for example. Prudentópolis was the largest of the state, founded by 2,500 Poles and 7,500 Ukrainians.

not need it thanks to its climate conditions, fertile land and the prosperity of the already established colonies.

The great majority (95 per cent) of the Polish immigrants who arrived in Brazil were peasants. The Russian Empire, unlike Prussia, would not allow them to emigrate. To leave and eventually reach the ports at Hamburg and Bremen they would need money and a passport that could only be issued by the authorities in Prussia, which would force them to emigrate as Germans. For these reasons, money and documents were sent back to the families so that they could feed the migratory flow.²³

Letters²⁴ were one kind of document that would constantly cross the Atlantic. In his analysis of these letters Kula (1977)²⁵ says that people were generally inclined to be optimistic although the censors targeted the letters that gave good news to their recipients. Those that conveyed bad news were allowed through and delivered.²⁶ Hempel's (1973) reports tend to confirm this thesis. The testimonies he was able to collect while staying in Brazil reveal large number of issues – from mistreatment received when they arrived at Ilha das Flores²⁷ to the diseases, cholera mainly, and the high number of children who died due to terrible hygiene conditions – faced by the immigrants in their low quality shelters in Paraná and Santa Catarina. However, the optimistic letters, usually sent to family members, would start by invoking God and thanking him for good health, “I am healthy, thank God”. Occasionally, reports of priests, churches and worship can be found in the letters. The letters usually finish with the sender's address, more specifically the name of the colony in which they lived.

As to other content, the main themes refer to the land and the homes, including the abundant forests (where they could gather wood freely to build their houses), the beauty of the landscapes, the vegetation and the animals, especially the birds. The weather, especially the soft winter, had a special place in these reports. Many sections about the extent of the lands can be found, which explain that there is no

23 Some senders used to mention the difficulty they faced in sending money from Brazil to the Russian territories. The money would be seized (or just stolen) which brought up many claims from the immigrants against the Brazilian authorities. The Brazilian mail chose to cancel the mailing of money to Russia because they were forced to pay back many undelivered amounts. For further details see Kula (1977).

24 Precisely those that had been seized by the Russian government while trying to slow down the emigration.

25 The first letter is numbered 22 and is dated at 15th November 1890, and the last is numbered 82, 24th May 1891. They have been translated into Portuguese and published in the *Anais da Comunidade Brasileiro Polonesa*, vol. VIII, 1977: 21–117.

26 At the ending of many seized letters the supervisor would write the word “zadzierzat”, followed by a short plot of the letter's content. In the letter, number 40, the sender tells the receiver that if the “moscovita” allowed him he would write again.

27 It refers to the “Hospedaria de Imigrantes Ilha das Flores” (Ilha das Flores immigrant Inn) founded by the inspector of Land and Colonization in 1883. It had been functioning until 1966.

immediate requirement to discuss the terms of work and payment. Social life was not overlooked. The rule of law usually came first, followed by the harmony of social relations (including those between the immigrants and the ex-slaves), the freedom, the lack of “lords”, the respect they received from the richer people and the fact that they did not have to pay any taxes. The “equal relations” that prevailed in a country that had just left behind its slavery policy were given special recognition. To sum up, the tone of the writings was optimistic, the situation was said to be good, most of the time, and it was even better than it was back in the occupied territories, thus confirming the thesis that “optimistic” letters were seized for this reason.

Besides the letters, the period provides us with witnesses, whose memories (Saporski 1972) or reports of travels to Paran reveal the debates within the occupied territories, between supporters and opponents of immigration. Among the former are Hempel (1973) and Klobukowski (1971). They were members of the same expedition sent to Brazil and Argentina to check in detail on the living conditions of the Polish emigrants. Both reports agree that, in contrast to the USA, Brazil and especially its middle states should not be seen as a destination country only. Hempel became a supporter of emigration after seeing during his expedition the ideal conditions for the “survival of Polish life” that was, at that moment, threatened (Hempel 1973: 16). Klobukowski, an intellectual who had become interested in the emigration issue, went even further. After witnessing the number of Poles and their living conditions in Paran, he affirmed that the conditions existed for the emergence of a “neo-polish society” (Klobukowski 1971: 15).

Actually, from the second half of the nineteenth century, some Polish nationalists had begun to notice the good colonizing potential of their emigrants. “Like their kindred [Germans and Italians] they were putting their faith in agricultural colonies, at Paran above all, where a new Poland was rising, according to Josef Siemiradzki” (Gabaccia et al., 2006: 86–7). The idea of founding a colony on Brazilian territory (Nowa Polska), just as other European countries in Africa had done, was considered a real possibility. It is also possible to see this idea of a “*Nowa Polska*” in Brazil being related to the Poles’ lifestyle in Paran. It could occur because, during this time, the immigrants began to create their first types of social organization, like bilingual schools, press and civil associations. Among the latter the “*Sociedade Polono-Brasileira Tadeuz Kosciusko*”²⁸ (Tadeuz Kosciusko Polish-Brazilian Society) was founded in 1890 after it was realized that the immigrants had become too isolated from the “civilized world”. The “*Sociedade de Ginstica Falco*” (1898) (Gymnastic Falco Society), the “*Crculo da Mocidade Polonesa*” (Polish Youth Circle) (1901), a “*Sociedade Santo Estanislau*” (Saint Stanislaw’s society) (1906), a “*Sociedade Unio Polonesa*” (1920), a “*Sociedade de Educao Fsica Junak*” (1923) and the “*Associao de Estudantes Sarmatia*”

28 The “Society” founded an elementary school, a choir and a small music band. This action was of primary importance for the organizing of the “First Polish Congress”, which had the objective of gathering all Poles living in South America together, in order to start acting towards regaining the independence of Poland.

(Sarmatia Students Association) (1926) were some of the other forms of social organization. Also, between 1892 and 1914, 19 newspapers were established. Some of them did not last long, but others had greater longevity, as in the case of the “*Gazeta Polska w Brazylii*”, published from 1892 to 1941.

The social life was so intense that even the sections of the press in Poland that had been against the emigration, were forced to send correspondents to Brazil and Argentina. In spite of unfavorable reports,²⁹ the migratory flow did not diminish and the idea that the emigration might be a way to defend the country’s interests in other countries gained ground. In summary, the associations, schools and press probably did influence the immigrants’ life. By bringing immigrants into contact with each other they helped to create a new Polish identity, repairing the social breaches which had opened up due to emigration. But this was all being built up within a new lifestyle, less constrained and with fewer ties, although not free from conflicts.

Between the 1920s and 1930s (until the “nationalizing campaign” organized during the Vargas government) the Brazilian government still looked upon the immigrants as part of a colonizing project. But neither the immigrants nor the politicians of this “young” Polish nation seemed to have been warned of that. Quite the contrary, “highly unlikely as the dream of founding colonies in South America might seem, it persisted up until to the 1930s” (Gabaccia 2006: 89). If we reflect on this background, how can we understand the prejudice against the Poles?

Table 10.1 Marriage Preferences (in per cent)

	GERMAN		ITALIAN		POLE		JEW	
	Yes	No	Yes	No	Yes	No	Yes	No
Would you accept your brother marrying a ...?	70	30	84	16	45	55	41	59
Would you accept your sister marrying a ...?	68	32	82	18	48	52	35	65
Who do you intend to marry?	59	41	76	24	33	67	30	70

Source: Author’s table, based on Ianni’s interviewee responses.

The Prejudice against Poles

How can we understand the real dimensions of the discovery? As we have said, just after the UNESCO research, Fernandes struggled to prove how distant from the reality was the tolerant (even democratic) model of race relations proposed

²⁹ Among those who were unfavorable to emigration were father Z. Chelmicki and M. Glinka, who were members of the “Warsaw Agricultural Society”. Both visited Brazil in 1891 and published several articles against emigration back in Warsaw.

by Freyre in his classical “Masters and Slaves” which was accepted by a large number of Brazilians. He had already proved it within the city of São Paulo. This work, written with Bastide,³⁰ despite differences of opinion between them, had been published a few years before Ianni traveled to Curitiba. Because of that, Fernandes and his research team were convinced that they would find racial prejudice in southern Brazil. The point was to understand how it worked, the *modus operandi*. In Fernandes’ perspective, Brazilian society, at that time, was engaged in a modernization process transforming itself from a rural to a modern and industrialized nation, at least, in southeast and southern central areas. Finally, as he would later write, racial structure would affect the development of “western civilization” in Brazil. That is why racial issues were so important and why the outcome of this new research would be so significant for the future of Brazil.

Reading Ianni’s books and articles, we can see that the large number of immigrants, in selected cities, was not previously taken into account. He soon discovered that the model “White and Black producing mixtures” did not cover all situations of social conflict among immigrants groups, even if they belonged to the same social classes. Being in touch with the various immigrant groups, Ianni rapidly discovered that Poles and their descendants faced much more discriminatory attitudes from ordinary people than other groups. During his inquiry he heard repeatedly, from men and women, that they would never marry a Pole or they would never accept someone from their families who married a Polish immigrant. From the questions he asked,³¹ he generated frequency data, as Table 10.1 shows.

First, we should acknowledge that the number (2,335) of questionnaires Ianni sent is impressive. On the other hand, caution needs to be exercised because only 223 questionnaires were returned. Among them, as we can see in the table above, the answers from the descendants of immigrants (we do not know exactly who they were) contain some variations. For example, there are differences between the answers of all the immigrant groups to getting married to Germans, Italians, Poles and Jews. The descendants of Italians are at the top. Freyre used to say that Italians were the preferred group of immigrants among Brazilian society. After that, the preference turns to the descendants of Germans. The last two groups, Poles and Jews, are close, evidently the least favorite options in the “wedding market”. Finally, as we can see, the answers are similar whether the question is about accepting your brother or your sister “getting married to...”

So, if “getting married” was an important issue, Jews and Poles were located at the bottom of the social ladder. Ianni concluded that there was prejudice

30 Bastide, Roger et Fernandes, Florestan (1955).

31 The questions were: 1) would you get married to a descendant of Poles, Germans, Jews, Italians or Black people? 2) Would you accept your sister/brother get married with (as the above question). We return to these questions later in this chapter.

against Poles. He added that there was another prejudice, namely against Jews, and it was the strongest. But he declared he had no time to study Jews, and had nothing to say about them,³² limiting his analysis to Poles. We do not know the exact reasons why.

On the other hand the answers of all groups of immigrants relating to the question of getting married to Afro-descendants show few differences. Here, Ianni divided the groups but did not include them in the chart. Nevertheless, intermarriage index showed some differences between Italians, Germans and Poles: 30 per cent of the Italians, 32 per cent of the Germans and 37.5 per cent of the Poles would accept getting married to an Afro-descendant, regardless of whether he/she was black or brown. Apparently, Poles were closer – less opposed to the idea of getting married to Afro-descendants than to people from other groups. Still, the average was not conclusive.

But, here comes a striking finding. Besides the marriage preference, Ianni had heard repeatedly: “There are no Blacks in Curitiba. The Blacks of Paraná are the Poles”. This would usually be accompanied by another frequently repeated comment: “I would never marry a black or a Pole!” These statements astonished him, he declared later. He concluded that Poles were really closer to Afro-descendants than another ethnic group and it explained the prejudice they suffered from. He finished by taking an expression widely used to nominate Pole immigrants – *Polishes*³³ – to assert that it revealed discriminatory attitudes. He put it in these terms: “They were neither Poles nor Brazilians, but *Polishes*, a new social category” (Ianni 1966: 145). From situations he observed in his field work, he was persuaded that there was a particular prejudice against Poles.

What conclusion can be drawn from that? First, the empirical data about “getting married” showed that there are many discriminatory attitudes among immigrant groups. Ianni took the view that their diversity in Curitiba created many ideological stereotypes among them and he defined that particular form of social life as an *ethnically heterogeneous society*. “Each group suffers, somehow, a kind of prejudice from others” (Ianni, 196: 190–91) calling it “multiple prejudices”.

Today it is impossible to revisit the interviews collected by Ianni.³⁴ All we know about them are a few questions reproduced by the author himself in his works. It is hard to deny that the conclusions were derived from evidence and that the most important of them was that Poles apparently suffered from prejudice because they had replaced Afro-descendants at the bottom of social scale. In other words, one of the causes of Poles being considered as Blacks was the small number (which he described as “the lack”) of Afro-descendants in Curitiba. The racism suffered

32 That is very surprising because he added that the prejudice against Jews was the most important one.

33 In Portuguese, “Polacos”. See footnote 14.

34 We have written to Professor Seggato who is married to one of Ianni’s daughters and, in his reply, he informed us that they were lost, probably thrown away.

by the last in line, as his professor, Fernandes, had already discovered in São Paulo, was transferred to Polish immigrants. Even if true, this could not explain the prejudice they showed towards others, the so-called “multiple prejudice”.

Actually, it is not easy to support Ianni’s conclusions, although they were not entirely mistaken. But we think they were influenced by the circumstances of his team research. Ianni’s findings certainly suffered from the context of the scientific fight against Freyre’s ideas and from the results of the UNESCO research. His team were keen to prove that there were racially conflictive situations in southern Brazil and we cannot forget that his interviewees used to compare Poles to Afro-descendants. From the beginning, he did not take that assumption into account. But as he said later, it was not a mere racial matter as many might think. Poles had suffered prejudice from other ethnic groups, mainly the Germans whose country had invaded and had been dominating a third of Polish territory from the end of eighteenth century to the beginning of the twentieth century. Unfortunately, he did not return to the subject.

In a word, research about race relations was engaged in a dramatic fight against Freyre’s model, but it was also intended to provide arguments to build a democratic and “civilized” nation, as Fernandes himself recognized later (Fernandes, 1960).³⁵ When Cardoso and Ianni finally published “*Cor e Mobilidade Social em Florianópolis*”, Fernandes, in its foreword, said the results revealed that the social and historical factors related to race relations in southern Brazilian society were not congruent with the democratic system. But, on the other hand, they assumed that Brazilian society was tolerant and it could lead the way to overcome many historic problems of discriminatory attitudes. If he had not held these background assumptions he might have been less pragmatic and more sensitive to empirical data. We will never know!

In his field work, Ianni uncovered many references to Polish immigrants in the newspapers, literature and history of Paraná. In those references, immigrant Poles were frequently depicted as illiterate peasants, extreme Catholics and not modern. Perhaps, naively, he took them at face value without putting them into context. But in order to investigate it further and to understand what he had discovered, he decided to undertake specific research on Polish immigrants and in that connection he left us a research plan with some hypotheses and a historical analysis.³⁶ He entitled it “The social situation of Poles (a research project)”.³⁷ It deserves a closer inspection.

35 As a last factor, at this specific period, Fernandes was involved in a battle within the field of sociology to prove his scientific perspective was the best. See Bastos (1996).

36 As we have said, there are many differences inside these works. Nevertheless, for our purpose here, we will not take it into account.

37 The title is: “A situação social do Polonês. Projeto de estudo”. There is another paper which title is “A Situação social do Polonês”. Even both titles are practically the same, these papers are different. The second one has its origins in the paper “Do Polonês ao Polaco”.

The Project Plan about Polish Immigrants

The project was divided into four chapters: 1) The Pole before immigration; 2) The Polish immigrant in Curitiba; 3) The social integration of Poles; 4) Conclusion. Firstly, he positions his study in terms of the socio-cultural relationships between Poles and native Brazilians. He came to the conclusion that they were different from each other after considering how Brazilians and other groups distinguished and named them. In fact, it did not matter if they were born in Brazil or not, they were identified as “*Polishes*”. This expression, in Ianni’s terms, represented the social relations and ethnic links performed by them. It was also a kind of negative stereotype and, in his words, it displayed racism.³⁸ Finally, he stated that the attitudes and other social discriminatory practices faced by Polish immigrants were linked to their own history, a kind of specific “social problem”; a dissimilar problem, he added, compared with those we might find associated with other immigrant groups like Germans, Italians etc.³⁹

The main goal of the proposed research was to follow the integration process (sometimes he used the term of *absorption* borrowed from Eisenstadt 1954) of Polish immigrants into the particular Brazilian community they lived in. He linked this process to the social institutions of Brazilian society. He put the question in these terms: should the main factor in the explanation of integration, be investigated from within the immigrant community itself or was it related to the type of social institutions they had chosen to be part of? Ianni decided to pursue his research in both directions. Finally, Ianni took into consideration another issue; was the integration process of Poles affected by other ethnic groups living in Curitiba?

His approach was to use empirical data based on another research project that had been already published. The title was “*Do Polonês ao Polaco*” (From the Pole to the Polish), and the results had been presented at the Fourth Brazilian Anthropology Meeting, held in Curitiba in 1959. Ianni quoted several papers he had found when working at Curitiba which included references to Polish immigrants. Unfortunately, as we have said earlier, he took for granted the negative representations he found therein and used them to underline the parallels between Poles and Afro-descendants. He never accomplished the research he planned.

38 In fact, the notion of racism was integral to his thinking. We think that it was just prejudice.

39 In that relation, Ianni was totally wrong. As many social scientists have shown, in the past, there was prejudice against other immigrant groups such as Italians, Syrians, and Lebanese.

Conclusion

From today's perspective, we may agree (in fact, it is quite clear) that there were discriminatory attitudes in Brazilian society and they were not limited to Afro-descendants, as many authors described. That is Ianni's first accomplishment: although they were viewed positively by the Brazilian authorities from the beginning of immigration, White Europeans immigrants could have been suffering a special type of prejudice, linked to the social circumstances in which they lived before immigration as well as to general representations inside Brazilian society. But the main question, the question about the local and particular origins of this unexpected prejudice remained unanswered. Because Ianni worked on the hypothesis that had been already defined by Fernandes and Bastide in other research, the outcomes he produced could not have been different. He established a parallel between the racism suffered by Afro-descendants and the one he had discovered.⁴⁰

Despite these controversial conclusions, he opened up some important arguments even if he did not develop them. In fact, he discovered a different type of prejudice probably much more related to the history of immigration in Southern Brazil than to the racial attitudes that came from the history of slavery in Brazil as a whole. Consequently, they allow us to elaborate new interpretations of Southern Brazilian social history.

Studying the prejudice suffered by descendants of Poles, we discovered many representations and conflicts that came with immigrant groups. Historical conflicts between Germans and Poles in Europe before the immigration process explain the prejudice against the latter much better than slavery history. The Second World War is another main point of reference for this subject because of the political side the Brazilian government chose.

As Consolim, Kiliás and other authors in this book propose, social knowledge depends on the social and political context. After the Second World War, racism, communism and moral issues were the main discussions everyone – public intellectuals and politicians – faced. In fact they were much closer to each other than we can imagine. But it depends also on scientific issues. The practices of scientists are related to their place in society. They reveal our interests and social positions, as the French sociologist Pierre Bourdieu has frequently reminded us.

In conclusion, we can say that not all discriminatory attitudes among individuals in Brazilian society are related to slavery. It is likely that the prejudice immigrants experience still remains invisible to most parts of Brazilian society. Nowadays, prejudice and racism are classified as crimes and the Brazilian government has set out many affirmative action policies for Afro-descendant citizens. But we do not see anything comparable in relation to immigrant

40 In fact, many years later, he himself recognized it (Ianni 2004).

descendants like Poles or others. Do they deserve it? That is an open question to which public intellectuals should give an answer to society.

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PART III
Intellectuals and Their Audiences

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Chapter 11

Blurring the Boundary Line: The Origins and Fate of Robert Bellah's Symbolic Realism

Matteo Bortolini¹

Men resemble their times
more than they do their fathers
Arab proverb

Whether appreciative or not, all reviews of Robert N. Bellah's magnum opus – *Religion in Human Evolution*, an 800-page *tour de force* “from the Paleolithic to the Axial Age”, – recognize him as one of the most influential sociologists of the twentieth century (see Bellah 2011; Martin 2011; Wolfe 2011). Bellah's intellectual record was, by any measure, impressive: his first major book, *Tokugawa Religion*, is still in print after more than 50 years, and at least three of his publications – “Civil Religion in America”, “Religious Evolution”, and his co-written 1985 bestseller, *Habits of the Heart* – have become citation classics (Bellah 1957; Bellah 1964a; Bellah 1967; Bellah et al. 1985). He was also a recognized public intellectual speaking on educational, religious, and political matters, as well as an appreciated preacher (Bellah and Tipton 2006).

Still, as with any intellectual, Bellah's scholarly journey cannot be reduced to his most celebrated works. There exists at least one major phase of his career which has almost been forgotten, one in which he advanced some bold ideas about the way religion should be conceptualized and studied by the social sciences. A student of Talcott Parsons, Bellah made his debut in the mid-1950s as a general theorist and a specialist in Japanese religion working squarely within the twin frameworks of functionalism and modernization theory (Bortolini 2010). As such, he acquired a central position in the field of sociology of religion, and was generally recognized as one of its most important systematic thinkers. His

1 A first version of this chapter was written while I was a visiting fellow at the Department of Sociology at Harvard University (spring 2009). Drafts were presented at a “Social Science and Religion Network” seminar, Boston University (April 2009), at the Department of Studi Sociali e Politici, University of Milan (June 2009), and at the ISA RCHS Interim Conference in Dublin (June 2012). I would like to thank Michèle Lamont and Nancy Ammerman for their hospitality, and Jeffrey Alexander, Bob Bellah, Christian Dayé, Jennifer Downey, Julian Go, Victor Lidz, Samuel Porter, Massimo Rosati, Philip Smith, and Per Wisselgren for their comments.

academic affiliation to Harvard's Department of Social Relations was a clear sign of disciplinary eminence.

In 1967 Bellah moved to the University of California, Berkeley, and his writings took a radical turn. He abandoned Parsonian conceptual tools and started to develop what he called *symbolic realism*, a strongly hermeneutic approach to the study of religion according to which religious symbols are a *sui generis*, irreducible way of grasping the ultimate conditions of human existence. For almost a decade, Bellah championed symbolic realism in a host of publications, conferences, and debates. Then, quite unexpectedly, he almost completely stopped talking and writing about it, and focused on other strands of research, including the project which eventually produced *Habits of the Heart*. Thirty years later, it is safe to say that symbolic realism did not catch on, and certainly Bellah's unwillingness to work on it during the 1980s – when a new “culturalist” wave flooded American sociology (Alexander 2003; Friedland and Mohr 2004) – contributed to its oblivion.²

In this chapter I try to explain the origins and fate of symbolic realism using the conceptual tools of the new sociology of ideas, according to which in studying the emergence, development, and diffusion of cultural objects it is necessary to take into account both the *milieus* within which they were devised and the various audiences to which they were addressed. In other words, to understand the roots and fate of Bellah's symbolic realism one should look at the intellectual collectivities within which it emerged, the disciplinary structures and debates within which it was developed, and, last but not least, the performative aspects of the representation of ideas within different spheres of visibility (Camic and Gross 2001; Camic, Gross, and Lamont 2011).

My first target is Bellah's move from functionalism to symbolic realism. In the autobiographical introduction to his 1970 collection of essays, *Beyond Belief*, Bellah (1970a, p. xvii ff.) described his intellectual shift as the result of a personal *coupure*, born of the influence of late-1960s counterculture and of his disillusionment with American political life. I complement Bellah's account, which pointed to the impact of political and social changes on his intellectual self-concept, showing that symbolic realism also was part and parcel of a wider disciplinary *avantgarde*. In the mid-1960s, a new generation of similarly-positioned sociologists of religion – among whom Bellah, Thomas Luckmann, and Peter L. Berger were the most visible – began to seriously question the status of their discipline as a branch of scientific sociology devoted to the explanation of observable and measurable religious phenomena. Together with their contemporaries in cultural anthropology, they created a new intellectual space importing theoretical ideas and

2 See, for example, the scant attention to symbolic realism given in David Yamane's entry on “Robert Neelly Bellah” in the *Encyclopedia of Religion and Society* (Swatos 1998). *The Robert Bellah Reader* does not include anything related to symbolic realism, and whereas the editors of Bellah's Festschrift praise it, the book itself contains only one essay focusing on it (Alexander and Sherwood 2002; Madsen et al. 2002). A survey of major textbooks in the sociology of religion 1980–2010 confirms this observation.

methodological tools from the humanities, efficaciously creating an interpretive version of social science.

My second goal is to explain the fate of symbolic realism as one among different versions of the interpretive paradigm competing for visibility within the new space of discourse. Among these, Berger's phenomenological approach proved to be more palatable to sociologists of religion than Bellah's symbolic realism. Given the state and the self-understanding of American sociology of religion in the mid-1970s, Berger's position was more convincing than Bellah's on at least two counts: on the one hand, it fit better with then-reigning theories of secularization than Bellah's understanding of religion; on the other hand, it was more respectful of the hard boundary-work sociologists had made to establish a disciplinary cartography which clearly differentiated their scientific endeavor from humanistic studies of religion. In other words, Berger's approach allowed sociologists of religion to maintain their position and status *vis-à-vis* the wider sociological field, whereas Bellah's symbolic realism was too radical to be accepted. Unlike Bellah (1971a: 232), other sociologists of religion were quite afraid of "blurring boundary lines". A sketch of the state of the field in the 1950s-1960s is thus the best place to begin.

Field Analysis: American Sociology of Religion, 1920–1970

Although the most progressive currents of early American sociology had directly come from religious circles, the sociology of religion never occupied a central position within the field. In the early twentieth century it mostly interested scholars coming from mainline Protestant churches who thought that social science could be instrumental in solving their pastoral problems (Swatos 1989; Reed 1981). With the demise of the Social Gospel movement, the sociology of religion lost most of its practitioners and entered a troubled period: in spite of the many empirical projects completed by religiously-founded research institutes in the 1920s and 1930s, among secular sociologists it was considered as unduly compromised with its confessional roots (Reed 1982). As a result, in the following decades three professional societies were created to strengthen the subfield. The American Catholic Sociological Society (1938) and the Religious Research Association (1951) aimed to legitimate the practices of denominational researchers, while the Society for the Scientific Study of Religion (1949) aimed to raise the scientific standards of sociological research on religion and facilitate the encounter between academic and "religious" scholars (Reiss 1970; Hadden 1974; Newman 1974; Moberg 2000). In spite of their different starting points, the associations were destined to follow the same path as sociology at large moved toward scientism and empiricism during the postwar period.

The 1950s were a moment of major scientific development and institutional growth for the American social sciences (Smelser and Davis 1969; Abbott and Sparrow 2007). As a cause and a consequence of sociology's legitimation as a "science", a general though vague positivism became a diffuse framework and

new methods and tools were devised to make sure that the social sciences would rapidly reach the predictive power of the hard sciences (Steinmetz 2007; Isaac 2012). Analyses of sociological publications show “a preference for empirical enquiry as against theory, a strong interest in social-psychological issues, and an abiding commitment to quantification and statistical analysis” (Brown and Gilmartin 1969: 288).

This growth of sociology had mixed effects on the subfield of the sociology of religion. In a general survey published in 1970, James L. McCartney (1970: 33) noted that “at least one area, the sociology of religion, has expanded despite the low use of statistics and little support”. This success, however, was not matched by a concurrent expansion of research funding or the opening of new positions (Smelser and Davis 1969: 147–51; Lavender and Coates 1972). In fact, the sociology of religion was having a hard time projecting an appropriate image of itself: its scientific credentials were still uncertain, and the accusation of being a cover for religious proselytism hung as a sword of Damocles over its head (Wiebe 1999). This ambiguous condition pushed sociologists of religion to engage in explicit boundary-work (Gieryn 1999) on two fronts: in order to prove and sustain their credibility claims, they fully embraced scientism and positivism, eliminating any residual boundary between their sub-discipline and mainstream sociology, on the one side, and strengthening and enforcing the border between the “scientific” and the “humanistic” study of religion, on the other.

During the 1950–1970 period a remarkably common framework emerged for interpreting the past and a shared view of the sociology of religion as a truly scientific endeavor built on three main principles – empiricism, objectivism, and epistemic modesty – ensued. Sociologists of religion presented their subfield as a branch of sociology committed to the study of religious behavior according to the principles of the scientific method (Nottingham 1954; Schneider 1964). Big questions about the origins or the truth of religion should be abandoned in favor of more unpretentious and circumscribed descriptions, measurements, and, hopefully, predictions of observable religious phenomena (Hunt 1958; Demerath and Hammond 1969). The use of empirical methods was used as a crucial marker to distinguish the sociology of religion from philosophy and theology. Most scholars drew a distinction between the objective, impersonal method of a properly scientific study of religion and the “subjective”, “personal judgments” typical of the humanities: the sociologist of religion behaved “as though [he] himself did not exist”, and while this self-annihilation was recognized as a difficult goal, in principle there was no obstacle to the attainment of a fully impersonal objectivity (Yinger 1951; Scharf 1970). When confronted with the task of defining their general stance, sociologists of religion were careful to avoid sociology’s “original sin”, the imperialist claim to be able to explain away their object of study as a projection of inner feelings, tribal patterns, economic structures, or “society” tout court (Vernon 1962). This epistemic modesty would facilitate both the scientific study of religion and intellectual exchanges with other disciplines.

From Systems to Symbols: Bellah's Methodological Shift

Robert Bellah made his debut as a sociologist of religion in the mid-1950s and soon came to occupy a central position within the field, thanks to his theoretical boldness and his close relationship with Talcott Parsons, “virtually the only major sociologist to give serious attention to the problem of religion” (Lemert 1975: 98, n. 5; Johnson 1977). His 1957 book, *Tokugawa Religion*, reproduced the PhD dissertation in Sociology and Far Eastern Languages he wrote under the guidance of Parsons and John Pelzel, a work aimed to extend Weber’s Protestant ethic thesis to one of the main cases not touched upon by the German sociologist: the pre-modern roots of the distinctive institutional make-up and rapid emergence of modern Japan. As Bellah recalled some years later, his interpretive breakthrough came in the fall of 1954, when Parsons presented an early version of the four-function, or AGIL, paradigm, a systemic theoretical tool based on the primacy of functions over structures and processes (Bellah 1964b; Alexander 1987a). Bellah used a condensed version of the AGIL paradigm to compare the relationships between capitalism and Japanese “political” values, American “economic” values, and Chinese “integrative” values.

Roughly at the same time, he used the four-function paradigm to build a general analytical scheme for the systematic study of religion, aimed at orienting sociologists to collect sound and comparable empirical data. Following Parsons’s lead, Bellah (1955) considered religion as the most abstract subsystem of the cultural system, its two main functions being the maintenance of meaning systems and the management of tensions. Whereas the model was intended to be a tool for the empirical researcher and not a general account of the concrete relationships between different aspects of religious action, symbols, and institutions, Bellah’s conceptualization also aimed at re-orienting the understanding of the relationship between modern and traditional societies according to Parsons’s functionalism. Any structure or subsystem which effectively fulfilled the functions of pattern-maintenance and tension-management might work as “religion”. If traditional religions could not deliver the job, other symbols, practices, and institutions would do, provided they were elevated to a sacred status – in a sense, the theory ruled out the possibility of secularization. Other theoretical papers from this period eschewed technical jargon in favor of a more traditional language to substantively articulate the same general scheme (Bellah 1958; Bellah 1964a).

In the mid-1960s, Bellah focused on the relationship between meaning systems and their referents, his main methodological problem being how to correctly interpret religious symbols in order to understand their own “reality”. In “Transcendence in Contemporary Piety”, the analysis focused on “systems of pure terminology” that is signifiers without empirical referents like God, Being, Nothingness, or Life. According to Bellah (1969a: 202) these *ultimate symbols* should be interpreted as relational devices intended to overcome the dichotomies of ordinary conceptualization and “bring together the coherence of the whole experience”. This development in his understanding of religious symbols called

for a more integrated and flexible methodology, a deeply hermeneutic approach devoid of any disciplinary restriction. Whereas in his earlier works Bellah had supported the necessity of a dialogue between social science and theology, in 1969 he wrote that any absolute separation was “impossible”, as they were part of a “single intellectual universe” (ibid.: 206). Separating science from theology simply prevented a correct understanding of religion.

Time was ripe for a forceful public exposition of the new ideas, which had to be set forth in front of a properly constituted audience in order to reach their full impact (see Collins 1998; Cossu 2012). A joint session of the Society for the Scientific Study of Religion and the American Academy of Religion held at Sanders Theatre, Harvard University, on 25 October 1969, proved to be the right venue for Bellah’s performance. Following an invitation by Samuel Z. Klausner (1970: 100, n. 1), Bellah and Werner Stark convened to discuss the relationship between scientific and humanistic approaches to the study of religion. Stark (1970: 173) provocatively suggested that the first pre-condition for a correct understanding of religious phenomena was “a revolutionary act” against modernity’s “pan-mechanicism”. Bellah attacked both the historical realism of the seminary and the reductionistic rationalism of Enlightenment-inspired social science, which considered religion a projection of some underlying reality supposed to be “more real” than religion itself. He proposed to leave both perspectives behind and embrace a new framework, termed “symbolic realism”, which considered religious symbols as “constitutive of human personality and society” and thus “real in the fullest sense of the word”. In Bellah’s own words, “to put it bluntly”, religion was deemed to be “true”. As such, it could not be reduced to either empirical facts or social and psychological processes, and had to be studied using an analytical frame which fully recognized its *sui generis* nature. Symbolic realism was to be fruitfully used by theologians and social scientists alike, thus eliding one of the most harmful distinctions in the modern organization of scholarly disciplines (Bellah 1970b: 92–3).

To a big part of the audience Bellah’s speech sounded much like the revolutionary act Stark had asked for. Postponing the discussion about the reactions to symbolic realism to the next section, I now sketch its main points as they were developed during the early 1970s. According to Bellah, religious and metaphysical symbols were to be considered as attempts to grasp “the totality which [included] subject and object and [provided] the context in which life and action finally [had] meaning”. Inasmuch as they succeeded in evoking the totality which encompassed known and unknown, conscious and unconscious, subject and object, they had to be considered thoroughly real. The awareness of this *sui generis* reality of religious symbols needed a radical change in the epistemic stance of the individual social scientist, which Bellah saw as a shift from a reductionist approach to what he called, following Paul Ricoeur, a “second naïveté”. Only somebody who had “gone to school with the masters of suspicion” but had transcended their teaching in order to learn “that every interpretation of reality [was] finally only an interpretation, and not reality itself” could attain this new religious consciousness.

Since symbolic realism could not be simply learned from a book, “as it was”, this momentous historical-cultural shift had to be made individually by each scholar (Bellah 1975a: 17). For this reason “a continuing intense dialectic with the first and second forms of consciousness [was] necessary” in order to prevent symbolic realism from degenerating into the shallow eclecticism of much late-1960s counterculture (Bellah 1972: 15–17).

In order to accomplish this crucial task, sociologists should leave their “disciplinary ghetto” and “associate with all kinds of academic wanderers” and poets – Bellah’s list included Wallace Stevens, Anaïs Nin, Herbert Fingarette, Norman O. Brown, Michael Polanyi, and William Butler Yeats (Bellah 1969b; Bellah 1970b: 94). From such diverse intellectuals, sociologists should learn to appreciate the most paradoxical and ecstatic ways to understand the intrinsically embodied quality of religious experience, to accept the eruption of the sacred into ordinary reality, and to develop a “double vision”, a “conscious magical thinking”, or even a “new, purified animism” (see Bellah 1970a; Bellah 1969c: 214; Bellah 1971b: 453). The attainment of second *naïveté*, however, could not be left to the individual scholar; it had to be pursued within a broad disciplinary horizon which greatly exceeded the boundaries of existing sociology of religion. In a short self-presentation he had written just a handful of months before Sanders Theatre, Bellah deemed his fellow sociologists of religion as “dull” and voluntarily blind to the richness of both the humanities and sociology’s own classical heritage. Time was ripe for breaking down the walls of academic specialization and reuniting what had been unjustly “chopped up and parceled out to the various social sciences” (Bellah 1969b: 1–3). Since all forms of knowledge were equally symbolic – and, as Brown had shown in *Love’s Body*, all spoke of the same “thing”, the human condition – all were equally legitimate to share the terrain occupied by the social sciences. The symbolic resources of religious experience and reflection would thus become “vitaly available” to all, with radical and liberating consequences (Bellah 1970c: 234–5).

This theme of unity was transposed from the nature of religious symbols and the intellectual tools to study them to the wider organizational *milieu* within which the study of religion was to be pursued. Bellah’s plea for the establishment of departments of religion as legitimate organizational divisions of any university, whether public or private, was the logical consequence of his methodological reflections. The department of religion was the proper organizational body in which neither the historical realism of the seminary nor the symbolic reductionism of the Enlightenment social sciences could hinder the proper – that is, symbolic realist – understanding of religion. Bellah left no space to the imagination:

If this seems to confuse the role of theologian and scientist, of teaching religion and teaching about religion, *then so be it*. The radical split between knowledge and commitment that exists in our culture and in our universities is not ultimately tenable. Differentiation has gone about as far as it can go. *It is time for a new integration.* (Bellah 1970b, p. 96, italics are mine; see also Bellah 1970a, p. x)

As even this short summary makes clear, Bellah's argument was complex and paradoxical. As we have seen, he was strongly advocating the *sui generis* reality of religious symbols, up to the point of defining symbolic realism as "the knowledge that non-cognitive and non-scientific symbols are constitutive of human personality and society – are real in the fullest sense of the word" (Bellah 1970b: 92–3). At the same time, Bellah wrote that as humans only approach reality by means of symbols, all knowledge is somehow fictional. This led him to recognize the impossibility of differentiating between religion and the highest forms of art and, in general, to underline the multiplicity of the spheres of meaning: reality could, and should, be observed from different points of view (Bellah 1969a: 205–6). This is how he put it at Sanders Theatre:

Even a natural scientist selects those aspects of the external world for study which have an inner meaning to him, which reflect some often hidden inner conflict. But this is true of all of us. We must develop multiple schemes of interpretation with respect not only to others but ourselves. We must learn to keep the channels of communication open between the various levels of consciousness. (Bellah 1970b, p. 94)

Bellah had already made up his mind about this apparent contradiction in a short critique of Geertz's "Religion as a Cultural System" published in 1968. As it is well known, Geertz assigned a special importance to Alfred Schütz's distinction between different symbolic spheres – art, science, religion, and, above all, the everyday world of common sense. In his definition, for people who were believers, religious symbols had "such an aura of factuality that religious moods and motivations seem[ed] uniquely realistic" (Geertz 1973: 90). In his review, Bellah underlined that if this was to mean that social scientists should consider common sense as the "paramount reality", so that religious symbols and experiences happened to be less real than they seemed to believers, Geertz was plainly wrong. Humans could deal with objects only through symbols, so that no province of meaning could be considered more important or even "truer" than the others: "Religion does not 'seem'. It is. Or if it seems, then science and common sense equally seem" (Bellah 1968: 291).

Symbolic Realism as Interpretive Social Science

As coeval observers noted, Bellah's speech at Sanders Theatre "caused an enormous stir with respondents, warning in the darkest of tones of the dangers awaiting both the social sciences and religious studies if Bellah's ideas of commonality and unity were accepted" (Yates 1991: 212). Sociologists and psychologists interpreted the speech as treason on Bellah's part: "The delighted applause of the humanists and the angry sense of abandonment expressed by the social scientists suggested that all thought they had witnessed the capitulation of social science to humanistic study"

(Dittes 1971: 22). Given the positivistic bent of the sociology of religion, this reaction comes as no surprise: social scientists were expressing their indignation to a speaker who was threatening the carefully built and guarded boundaries of the scientific study of religion.

At Sanders Theatre Bellah had effectively made his debut on that epistemological level of scientific debate where the confrontation does not regard empirical or theoretical claims but “principles for the hierarchization of scientific practices” (Bourdieu 1991). He, however, was not alone: symbolic realism was just one of the many threads of a wider project aimed at steering the social sciences toward hermeneutics that was advanced by some “young” sociologists and anthropologists – Peter L. Berger, Thomas Luckmann, David M. Schneider, Clifford Geertz, and Victor Turner.³ Since the study of religion was only their starting point, in what follows I use the general phrase “interpretive social science”⁴ to collectively designate their work. Using Andrew Abbott’s model of fractal differentiation, I frame interpretive social science as a truly *collective* effort, with a proviso: in the absence of a planned scholarly or academic strategy it would be improper to call it a collaborative group or a scientific/intellectual movement; at the same time, the combination of generational experiences, intellectual interests, and positioning in overlapping academic networks made it more than a mere aggregate of individuals developing a similar response to their scholarly discontent.⁵

In *Chaos of Disciplines* Abbott (2001) explains the constitution and differentiation of scientific paradigms and disciplines as the recursive iteration of some basic oppositions at different conceptual and organizational levels. According to this intrinsically relational frame, a fractal differentiation pattern occurs when a distinction is re-entered and reproduced within itself, *ad libitum*. In the case at hand, the (typically modern) distinction between “scientific” and “humanistic” approaches to the study of religious phenomena is the key to understanding the emergence of interpretive social science from the scientific and positivistic version of the sociology of religion developed in America in the climate of the postwar social sciences. As the sociology of religion constituted itself as a self-proclaimed scientific endeavor, there emerged the possibility of creating a humanistic current within it: this is precisely what interpretive social scientists tried to do, proposing their understanding of religion *not* as a fully

3 In fact, pleas for a “new sociology of religion” came from many different quarters: see Lemert 1975 for a first analysis, which focuses precisely on metatheoretical issues.

4 The expression was coined by Paul Rabinow and William Sullivan (1979) in their attempt at consolidation and labelling.

5 For definitions see Farrell 2001, Frickel and Gross 2005, and McLaughlin 1998. In using Abbott’s model of fractal differentiation (not to be confused with his more particular “fractal cycles” theory), I am not subscribing to his self-proclaimed internalist view of disciplinary and intellectual change. At the same time, given that this chapter tries to complement Bellah’s own “externalist” explanation of his intellectual shift, my explanation naturally leans towards the internalist pole of the distinction.

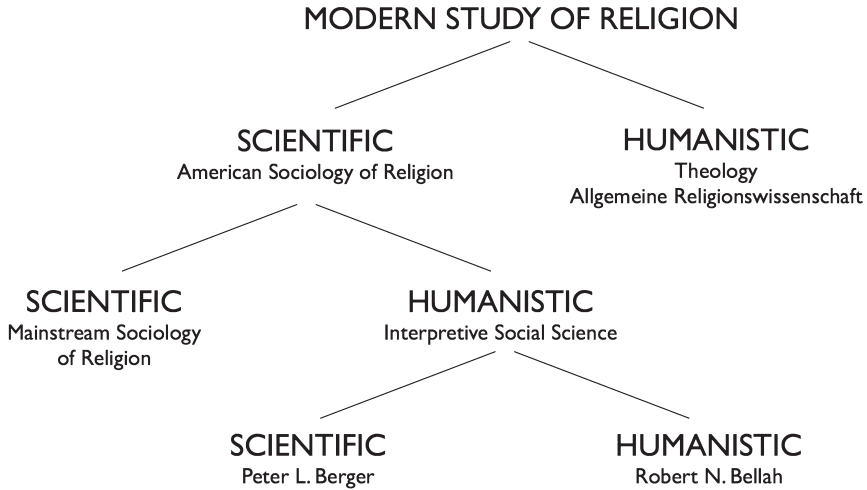


Figure 11.1 Fractal image of the main currents of thought analysed in this chapter

humanistic approach, but as *the humanistic side of positivistic social science*. Analogously, as I show in the next section, once interpretive social science was in place, two different versions of the humanistic approach became possible: a more positivistic one, proposed by Berger, and a radically humanistic (and more disturbing for positivist social scientists) one, developed by Bellah. The distinction between “scientific” and “humanistic” was thus fractally reproduced three times (see Figure 11.1).⁶

From an intellectual point of view, the main elements of interpretive social science were a strong focus on culture; a clear rejection of any attempt at reducing symbolic phenomena to social, psychological, or biological phenomena; the positing of a two-way relationship between symbolic life and practical activity, that is the reciprocal embeddedness of symbols and practices which could only be understood by pointing to the effects that, as Geertz (1973: 90 ff.) put it, the perceived reality of symbols exerted on social actors, their projects, and their lives. In Schneider’s *American Kinship*, Geertz’s “Deep Play: Notes on the Balinese Cockfight”, and Bellah’s “Civil Religion in America”, culture was “liberated” from the highly synthetic and instrumental role that Parsons had given it in his

6 In another essay, I described some of the changes in Bellah’s thinking on American religion in the early 1970s as a successful attempt to place a distance between himself and his previous image as a Parsonian disciple, a move I framed as an instance of Randall Collins’s dilemma of the gifted student (Bortolini 2010; Collins 2002). I am sure that even a cursory analysis of the properties shared by interpretive social scientists would turn our attention in the same direction. For a more nuanced understanding of this condition see Frickel and Gross 2005: 211 ff.

Table 11.1 Positional Similarities between Interpretive Social Scientists

Author (Year of birth)	PhD, year, mentor or inspirational figure	Major “interpretive” publications	Institutional affiliation (1970)
David M. Schneider (1918)	Harvard 1949 Talcott Parsons	<i>American Kinship: A Cultural Account</i> (1968)	University of Chicago
Victor Turner (1920)	Manchester 1955 Max Gluckman	<i>The Forest of Symbols</i> (1969) <i>The Ritual Process: Structure and Anti- structure</i> (1969)	University of Chicago
Clifford Geertz (1926)	Harvard 1956 Talcott Parsons	“Religion as a Cultural System” (1965) “Deep Play” (1972) <i>The Interpretation of Cultures</i> (1973)	Institute of Advanced Study, Princeton
Thomas Luckmann (1927)	New School 1956 Alfred Schütz	<i>The Social Construction of Reality</i> (1966) <i>The Invisible Religion</i> (1967)	Universität Konstanz, Germany
Robert Bellah (1927)	Harvard 1955 Talcott Parsons	“Transcendence in Contemporary Piety” (1968) “Christianity and Symbolic Realism” (1970) <i>Beyond Belief</i> (1970) <i>The Social Construction of Reality</i> (1966)	University of California, Berkeley
Peter L. Berger (1929)	New School 1954 Alfred Schütz	<i>The Sacred Canopy</i> (1967) <i>The Rumor of Angels</i> (1969)	New School (NYC) Rutgers University

analyses of social systems and societal values (see Smith 1998). Berger and Luckmann’s sociology of knowledge, for its part, depicted human societies as grandiose constructions for the stabilization of meaning, and detailed the way in which new symbols and practices emerged from the activities of human individuals and their lived experiences, established themselves as social structures, and finally confronted men and women from without in a repeated cyclic sequence of externalization, objectivation, and internalization. Their thorough analyses of the privatization of religion and the consequences of religious pluralism underlined

the importance of understanding religious meaning from the believer's perspective (Berger and Luckmann 1966; Berger 1967; Luckmann 1967).

The focus on meaning and culture *per se* pushed interpretive social scientists to reflect upon their methodological principles and this, in turn, widened their disciplinary horizon (Alexander 1987a). In a review of symbolic studies in anthropology published in 1975, Victor Turner emphasized the interdisciplinary nature of the field and its many intellectual roots:

The modern study of symbolic action and symbolic phenomena seems to be developing at the interface of hitherto unconnected or only weakly connected disciplines: social and cultural anthropology, microsociology *à la* Goffman, sociolinguistics, folklore, literary criticism (notably as practiced by Kenneth Burke), and semiotics. (Turner 1975, p. 150; see Keane 2005)

Geertz's experience in the field and his undergraduate training in the humanities made him sensitive to ordinary language and analytic philosophy, literary criticism, and semiotics, which surfaced again and again in his use of the work of Burke, Ricoeur, Langer, Ryle, and Wittgenstein (Geertz 2004). In *The Social Construction of Reality* Berger and Luckmann made heavy use of philosophical literature – mostly phenomenology but also the German philosophical anthropology of Gehlen and Plessner. Moreover, Berger explicitly adopted a substantive definition of religion taken from the history of religions, and clearly explained his rationale in the first appendix to *The Sacred Canopy*.

The similarities between the proponents of interpretive social science, however, were more than intellectual. Their professional trajectories had been quite similar and most of them also shared some positional characteristics: they had obtained their PhDs in the mid-1950s as students of big-name mentors; they were in their forties and had already gained a high standing within their respective fields; after a period during which they had published mostly mainstream work, they had started to explore new ways of thinking as an attempt to solve what they saw as pressing theoretical and methodological problems and thus to give a “course correction” to the scientific study of religion. This further differentiated their stances from both their mentors' and the center of the wider disciplinary field (see table 1).

From the standpoint of scholarly collaborations, a polarized but clearly established network of reciprocal visibility and influence began to take shape in the mid-1960s. On the one side, Schneider, Geertz, and Bellah were bonded by a scholarly and personal friendship born at Harvard's Department of Social Relations, where they had all been students of Parsons. In the 1960s Geertz, Schneider, and, later, Turner intensely collaborated in the renovation of the anthropological curriculum at the University of Chicago from which interpretive anthropology emerged as a recognizable current of thought (Geertz 1995; Kuper 2003). On the other side, Berger and Luckmann had met as undergraduates at the New School for Social Research in New York and formed a strong intellectual partnership which led them to publish *The Social Construction of Reality* in 1966. Given their

central positions within the field of religious studies, they all interacted repeatedly at conferences, seminars, and meetings of professional associations of sociologists or students of religion. One of the best known among these encounters was the “International Symposium on The Culture of Unbelief”, organized by the Vatican Secretariat Pro Non Credentibus, the Agnelli Foundation, and the University of California in Rome in March 1969, chaired by Berger and with Parsons, Luckmann, and Bellah among its key speakers; six years later, Luckmann and Geertz were among the protagonists of the second international symposium organized by the same institutions in Baden bei Wien (Caporale and Grumelli 1971; Caporale 1976).

Sturdy Boundary Lines: The Fate of Symbolic Realism Explained

In the early 1970s, the work of interpretive social scientists was regarded as a single, united front against positivism; at the same time, it was clear that each scholar was blazing a particular trail, employing different theoretical tools and strategies (Robertson 1969; Means 1970; Shepherd 1972; Karcher et al. 1981; Segal 1989). If rightly contextualized, these differences explain both the hostile reception given to symbolic realism at Sanders Theater and the theory’s failure to occupy the center of the scientific study of religion, not to mention sociology in general.

As Michèle Lamont (1987), Neil McLaughlin (1998), and Neil Gross (2008), among others, have shown, any explanation of the fate of cultural objects, authors, or intellectual movements should take into account a multidimensional array of variables: how intellectuals present their ideas, the reaction of their audiences, and the concrete alliances between authors within and without disciplines, organizations, and informal groups. I now propose four points that may help in understanding the fate of symbolic realism: the lack of an organizational base; Bellah’s failure to provide a recognizable “exemplar”; the competing rise to fame of his work on American civil religion; and his scant attention to the hard boundary-work with which sociologists of religion had attempted to mark out and legitimate their subfield as a proper scientific endeavor.

One of the conditions for success of an intellectual movement is the control over the organizational infrastructures needed to manage resources and train students (see Frickel and Gross 2005: 213 ff.). Though he held the chair of Ford Professor of Sociology and at times was chair of both the Institute for Japanese and Korean studies (1968–1974) and the Department of Sociology (1979–1985) at Berkeley, Robert Bellah always lacked the means to train young scholars according to the methodological tenets of symbolic realism. In 1973, he and Geertz devised an articulate project to create a full-fledged school of social sciences at the Institute for Advanced Study in Princeton along the lines of interpretive social science, but the attempt to appoint Bellah as a permanent member of the Institute aroused the harsh opposition of the majority of the fellows and the plan was abandoned (Bortolini 2011). At Berkeley, Bellah’s repeated attempts to establish a department of religion came to no avail, and though his continued collaboration

with the Graduate Theological Union was highly fulfilling in personal terms, his GTU students mostly pursued careers as theologians and/or clergymen (Pearson 1999; Bellah 2007). Moreover, Bellah never held editorial positions in academic journals or offices in professional and scholarly associations, the only exception being his role as a consultant for Boston's Beacon Press.⁷ In some sense, the 1976 National Endowment for the Humanities seminar on "Tradition and Interpretation: The Sociology of Culture" – which led to the publication of the *Interpretive Social Science* reader (Rabinow and Sullivan 1979) – and the 1978 research project which led to the publication of *Habits of the Heart* were his most successful attempts at creating an "interpretive" – though not "Bellahian" – school.⁸

From a more intellectual point of view, one of the most recurring criticisms leveled at symbolic realism pointed to the obscurity and vagueness of the paradigm (see Nelson 1970; Lemert 1975; Johnson 1977; Robbins et al. 1973). It is true that Bellah never worked out the methodological principles of symbolic realism in detail.⁹ In my view, however, this kind of critique would have been much milder had Bellah explicitly proposed one exemplar of what could be accomplished using his framework. As Alexander (1987b: 19) has defined them, following Thomas Kuhn, exemplars are "concrete examples of successful empirical work: examples of the kind of powerful problem-solutions which define paradigmatic fields". Geertz's essay on Balinese cockfights immediately comes to mind: when read together with "Thick Description: Toward an Interpretive Theory of Culture", its reprint in *The Interpretation of Cultures* gave Geertz's readers the opportunity to see interpretive anthropology at work (Geertz 1973; Smith 2011). In sociology, Berger and Luckmann had given *The Sacred Canopy* and *The Invisible Religion* a clear structure: the general conceptual framework of phenomenological sociology was first stated and then applied to the problem of religion in the modern world. Just like *Tokugawa Religion* had been read as an application of Parsons's AGIL scheme to a well-known body of material, these works fired the imagination of their readers because their methodological and theoretical principles were shown

7 See Beacon Press. Records, 1935–1988. Andover Seminar Archives, Andover Harvard Theological Library, boxes #1, #32, and #48.

8 Among widely known associates of Bellah, Ann Swidler, a sociologist, and Paul Rabinow, an anthropologist, also wrote dense methodological reflections which in part followed, in part rejected, the principles of symbolic realism, but surely were not recognizable as attempts to further its methodological program (see Swidler 1986; Rabinow 2007). To this day, the most conscious and successful attempt to build on symbolic realism is the "strong program" in cultural sociology developed by Jeffrey C. Alexander, himself a student of Bellah's, and Philip Smith at Yale University (see Alexander 2003).

9 In 1977 Bellah drafted the proposal of a book tentatively titled *On Religion and Social Science: A Non-Reductionistic Approach to the Study of Religion* with two of his most sympathetic critics, Dick Anthony and Thomas Robbins. In the prospectus, the authors admitted that "many of [Bellah's] assumptions [were] intuitive rather than explicit. Therefore it [was] hard for other scholars to follow his example" (Anthony et al. 1977: 5). The book, which aimed to explicitly elaborate those "intuitions", never came to light.

in actu and could thus function as models for other social scientists to follow. Without such an exemplar, symbolic realism remained without sociological gas.

Bellah's lack of interest in exemplars becomes even more puzzling when we add two details to the picture: the publication of *Beyond Belief* in 1970 and his intense work on American civil religion in the following decade. Bellah (1970a) had accurately organized the collection of his essays to mirror his intellectual development according to the cleavage between "Harvard's Bellah" (part two, "Religion in the Modernization Process") and "Berkeley's Bellah" (part three, "Religion in Modern Society"). Oddly, the building blocks of symbolic realism were listed in part three, *as if* they were an interpretation of post-modern religion and not methodological pieces aimed at changing the way in which the social sciences should study religion. On the other hand, *The Broken Covenant* implicitly is a nearly-perfect exemplar of symbolic realism (Bellah 1975b). Published in 1975, the book reproduced some lectures Bellah gave in Cincinnati in 1971, that is, in the very moment in which he was elaborating his methodological principles. And yet, there is no hint, in the book or anywhere else, of the relationship between Bellah's analyses of American myths and symbolic realism.¹⁰ The book included no methodological introduction or appendix, and its title – *The Broken Covenant. American Civil Religion in Time of Trial* – capitalized on the success of civil religion but was much less effective in promoting symbolic realism than Bellah's original title – *The Deepest Day: Studies in the Mythic Dimension of American Culture* (see Bortolini 2011: 25).

In fact, Bellah's work on American civil religion proved to be especially dangerous for the dissemination of symbolic realism. In the early 1970s, it had become the center of a wide, multi-disciplinary debate and was considered not only a thesis about "civil religion" as an empirical phenomenon, but also an instance of Bellah's critique of the theory of secularization. Again and again, critics shifted from an historical and sociological reading of the American civil religion thesis to a more theoretical one, in which they attributed to Bellah the attempt to criticize secularization theory by positing the existence of a common and legitimate value system as the foundation of even a(n apparently) secularized society like the US. This reading – which, to be sure, was perfectly consistent with both Parsons's and Bellah's understanding of the function of religious symbols in society and their idiosyncratic explanation of secularization (see Parsons 1963) – put Bellah's ideas on civil religion center stage and left symbolic realism in the background.

I am not only arguing that the sheer success of Bellah's civil religion obscured Bellah's work on symbolic realism. The point is that most sociologists identified "Bellah's theory" with his ideas on civil religion, reducing the attention given to his other lines of theoretical reasoning. This was also crucial in the "struggle"

10 In note 13 to chapter 1 Bellah cites the works of Ricoeur and Eliade as suggestive efforts to link symbol, myth, and reflective thought, and refers to his own forthcoming (but never published) *The Roots of Religious Consciousness* (Bellah 1975b: 165). This is the only reference to the universe of discourse of symbolic realism

for visibility within the sociological camp between Bellah and Peter Berger – a quarrel conducted at a distance, for the two never criticized one another explicitly or at length. Bellah’s negation of a fully secularized society was contrasted with Berger’s ideas about the disruptive consequences of pluralism for the sacralization of meaning systems, stated in *The Sacred Canopy* and *A Rumor of Angels* and, given the topology of the field, the latter were more attuned to then-reigning ideas about the irresistible spread of secularization and the privatization of religion.¹¹

This opposition between Berger and Bellah over secularization was paralleled by another, deeper contrast. In my view, the fate of symbolic realism was ultimately decided by Bellah’s neglect of the sustained and hard boundary-work that his fellow sociologists of religion had done to define their discipline. Bellah’s symbolic realism, with its (often misinterpreted) slogan “religion is true” and its plea for the blurring of boundaries between sociology and theology, can be seen as the radical humanistic wing of interpretive social science (see fig. 11.1, above). Given the topology of the sociological field, Bellah’s ideas sounded much too radical to sociological ears, an effect that, paradoxically, was *magnified* by his refusal to change his disciplinary allegiance: despite his penchant for theology and philosophy, Bellah tried to signal that he was not moving to the humanistic field. Not only did he repeatedly refuse positions in religious studies,¹² but he also insisted on the enduring importance of those very social thinkers he had accused of reductionism: Durkheim, Freud, and Weber. In “Between Religion and Social Science” Bellah had implicitly re-written Parsons’s famous essay on the development of the sociology of religion (Parsons 1944) in order to render the history of the social sciences ambiguous, self-critical, and open to paradox. Social science appeared as Janus-faced: on the one side, it was Enlightenment sociology, anti-religious and built on secularization theory; on the other, it was paradoxical and self-reflective, itself a step toward symbolic realism (Bellah 1970a: 238). In each of sociology’s patron saints Bellah found a backdoor to second naïveté. Durkheim’s collective effervescence and Weber’s charisma were seen as important ways in which something new could enter the historical process, while Freud was interpreted as the zenith and the nadir of Enlightenment, as the “very nature of the case of the unconscious proved refractory to rational analysis” (ibid.: 239–40; see also Bellah 1973). In revealing the post-modern possibilities “hidden” in the work of Weber, Freud, and Durkheim, Bellah was signaling his allegiance to the social scientific camp.

11 A rough count of citations, made with the Google Scholar Universal Gadget for Scientific Publication Citation Counting (<http://code.google.com/p/citations-gadget/>, used on August 31, 2012), leaves no doubts as to the different visibility of Berger’s and Bellah’s work: *The Sacred Canopy* has been cited around 5000 times from 1967 to 2012, while *Beyond Belief* has been cited around 1100 times from 1970 to 2012.

12 For example, in 1968 Bellah refused an offer to become the Houghton Professor of Theology and Contemporary Change at Harvard Divinity School (see letter from Robert Bellah to Talcott Parsons, 12/2/1968, Robert Bellah personal archive).

In this respect, Berger was much wiser at playing his cards. In the late-1960s he was generally recognized as one important, or even the most important, post-Parsonian theoretician in the sociology of religion, and his sociological credentials were never put into question, even by those commentators who pointed to both his training in philosophy and his religious commitment (Hammond 1969; Lemert 1975). He had stated his ideas about the relationship between sociology and theology as a refined version of “methodological atheism” in the second appendix to *The Sacred Canopy*:

Questions raised within the frame of reference of an empirical discipline ... are not susceptible of answers coming out from the frame of reference of a non-empirical and normative discipline, just as the reverse procedure is inadmissible. (Berger 1967, p. 179)

Later, Berger would reassert this position, explicitly mentioning two different modes of understanding the sociology of religion:

The scientific study of religion must bracket the ultimate truth claims implied by its subject. This is so regardless of one’s particular conceptions as to scientific methodology—for instance, as between “positivistic” or “humanistic” conceptions of science. If science means anything at all ... it means the application of logical canons of verification to empirically available phenomena. And whatever else they may be or not be, the gods are not empirically available. (Berger 1974, p. 125)

In sum, while Berger admitted his strong relationship with philosophy and adopted a substantive definition of religion that directly came from the humanistic study of religion, his clear reception of the boundary between sociology as an empirical discipline and the rest of humanistic/normative studies of religion effectively sheltered him against the accusation of “intelligence with the enemy”. As a result, his works were accepted as perfectly legitimate within the sociological camp – as some puzzled reviewers observed, *The Sacred Canopy* was at the same time “nothing new” and a milestone (see Klausner 1968; Knudten 1968; Wilson 1968). This is not to say that Berger was never criticized (Wilson 1969; Harvey 1973), but his work was not considered as “alien” or sheerly “personal” – the adjective which marked the difference between science and the humanities – as Bellah’s (see, for example, Greeley 1971; Brendle 1971).

In the face of criticism, Robert Bellah continued to develop and promote symbolic realism up to the late 1970s; then, as he had done in a similar situation (Bortolini 2012), he abandoned the problematic label and moved to a more fruitful direction, preserving the core ideas of symbolic realism. His work on the hermeneutic tradition and American morality brought him closer to practical philosophy, and Aristotle, de Tocqueville, and MacIntyre replaced Norman O. Brown and Wallace Stevens. Bellah also stopped talking of the “truth” or “reality” of religious symbols and translated his argument into a “communicative, practical,

and ethical” approach to social science *tout court* (Bellah 1982: 36; Bellah 1981). With the publication of *Habits of the Heart* (Bellah et al. 1985), he finally left the label “symbolic realism” without renouncing his most profound convictions on the methodology of social science. In a sense, he had learned his lesson: *Habits* can be read as an exemplar of an emerging paradigm of public sociology, which, since the book’s publication, has been globally discussed (Burawoy 2005). Robert Bellah has been recognized as one of public sociology’s most eminent forerunners and his short appendix to *Habits*, “Social Science as Public Philosophy” became one of his most influential works. Once again, the power of ideas only partially depends on their coherence, precision, or thoroughness – their fitness to the field of cultural production and the intellectual and material interests of its players being just as crucial.

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Chapter 12

How Spaces of Opinion Shape Public Intellectuals: A Field-based Approach to *Project Syndicate*-Op-Eds

Philipp Korom

Introduction

This chapter deals with two concepts: public intellectuals and the space of opinion. Both notions are somehow blurry and I will put much effort into delineating the contours of each concept. The compound word ‘public intellectual’ is said to have been put into circulation by Russell Jacoby (see Posner 2001: 26). For Jacoby (1987: 5) it stands for ‘writers and thinkers who address a general and educated audience’. Jacobs and Townsley (2011) were the first to establish the ‘space of opinion’ as an intelligible analytical object within the public sphere. It refers to parts of the public communicative infrastructures (for example, op-ed pages) in which members of the thinking class discuss serious matters of common concern. Already by definition the one is not thinkable without the other. On the one hand, intellectuals need a platform that makes them visible for a public. On the other hand, only actors can animate the space of opinion. Spaces of opinion correspond with intellectuals also from a historical perspective, which is easily proved by looking back at the origins of both concepts. Standard accounts trace the beginning of the bourgeois public sphere of Britain, France and Germany back to late eighteenth and the nineteenth centuries (Habermas 1989). At a time when in France the term ‘intellectuals’ gained currency, writers such as Émile Zola protested against the sentence of Alfred Dreyfus (Charle, 1990). It was from then on that intellectuals started in the first place to be normatively portrayed as ‘disturbers of the peace’ within the public sphere (Karabel 1996: 205).

Despite their close relationship, intellectuals and public opinion are mostly discussed in two different strands of literature: the sociology of the public sphere and the sociology of intellectuals. That these literatures meet only rarely in a productive synthesis strikes one as especially surprising since there are apparent communalities between recent debates on new forms of the public sphere and new characteristics of the intellectual. In both debates, the idea of plurality is key. It is widely agreed that today we face not one but multiple public spheres, and that competing views of what intellectuals are have grown. It was the observation of these common trends that motivated me to reconcile the two sociologies within one empirical study. To do so, I decided to settle on a research design that is

highly selective but keeps doors open for unexpected results. I study a small fraction of the global sphere of opinion only: op-eds written for *Project Syndicate* (ProSyn). Through ProSyn – an international, not-for-profit newspaper syndicate – intellectuals and other actors disseminate their knowledge within a supranational community. The world's largest newspaper syndicate is clearly one important venue for intellectuals to reach a huge public. I expect to generate new knowledge as I investigate the opinion space of ProSyn, without having a single conception of the public intellectual in mind. What I do, is to merely observe who articulates his opinions within a given space. After having completed this reconstructive task of establishing the space of opinion, I investigate in a second step which types of intellectuals dominate the scene. To anticipate the main result of my investigation, I find that opinion spaces lay the foundation for intellectuals. This may lead us to the speculation that the role of the intellectual mutates because of changing opportunity structures to intervene in the public sphere. At any rate, this study shows that it is worthwhile to spend time investigating the many arenas of today's public sphere when doing research on intellectuals.

The empirical study is preceded by a *tour d'horizon* of distinct approaches to intellectuals. Bearing in mind that the classics of the sociology of intellectuals (Kurzman and Owens 2002) started their analysis from different premises, I will ask which models of the intellectuals have been established so far. I will further take up the claim that a timely sociology of intellectuals should direct the focus to the 'study of intellectual spaces, their socio-cultural properties, and the multiple positions and claims that they encompass' (Eyal and Buchholz 2010: 124). Both, the review of literature on intellectuals and on intellectual spaces serve as a backdrop for interpretations of opinion-makers within ProSyn. I conclude by discussing the interrelations between public intellectuals and spaces of opinion.

Revitalizing the Classical Sociology of Intellectuals – The Problematic of Allegiance and Expertise

Anyone who reviews the chequered history of the sociology of intellectuals will notice its cyclical nature. A comparison of the various entries into the *Encyclopedia of the Social Sciences* gives a taste of the multitude of attempts to grasp central sociological features of intellectuals (Michels 1932, Shils 1968, Brym 2001). One apparent ambiguity is with respect to their place within society. While Michels (1932) adopts a class-in-itself approach, Shils puts forward a class-less approach, while Brym (2001) provides the reader with class-bound approach (see Kurzman and Owens 2002): Which means that each author defines the boundaries to other social groups differently. Zygmunt Bauman has argued that any definition of intellectuals is a self-definition and that it therefore 'makes little sense ... to ask the question "who are intellectuals" and expect in reply a set of objective measurements' (Bauman 1987: 28). Bourdieu called our attention to 'the positivist vision which, for the needs of statistics, for example, determines limits by a so-called operational

decision which arbitrarily settles in the name of science a question which is not settled in reality, that of knowing who is an intellectual and who is not, who are the real intellectuals ...' (Bourdieu 1990: 143). Imposing a concept of the intellectual thus always amounts to a self-definition that tends to frame the legitimate practice. Benda (1972), Jacoby (1989) and others bemoaned the decline of intellectuals, because they came to the conclusion that commentators in the public sphere ceased to have the stature of X, Y or Z – each standing for their favourite intellectuals. However, their concept of 'intellectual', as well as their claim of a dissolving object, is contestable – an insight that catapults us into struggles for control over the use of a particular category. Because constant shifts in its analytical framework will prevent a twenty-first-century sociology of intellectuals maturing, I propose two ways of approaching the topic. First, we can hammer out the most important parameters in classical sociology and ask which conception of the intellectual seems most significant for contemporary discussion. Second, we can define intellectuals not by individual attributes, but by systematically assessing positions within intellectual fields (Ory and Sirinelli 1996). In this section we shall distil common denominators and clear conceptual demarcations out of the classical sociology, whereas later on we shall search for new pathways.

A persistent question that emerges from the classics is whether cultural workers can or should be independent of the social forces around them (the "problematic of allegiance"). Antonio Gramsci phrased the problem in his own way: 'Are intellectuals an autonomous and independent social group, or does every social group have its own particular specialized category of intellectuals?' (Gramsci 1929–1935 [1971]: 5). He argued that every social stratum 'produces' its advocates and uses intellectuals to organize interest and gain power (the "organic intellectual"). Very likely he saw himself as an organic intellectual of the proletariat who stood in opposition to intellectuals of the bourgeoisie. What might today's organic intellectual look alike? Most likely it is an activist emerging from subaltern groupings (for example, social movements) who plays a leadership role within them. The intellectual combines knowledge and political skills in his or her struggle against some kind of hegemony (for example, neo-liberal globalization). In more abstract terms we could formulate it in the statement that today's organic intellectuals are strongly rooted in their communities and take up topics of local or global justice.

Mannheim's vision of a free-floating intellectual is diametrically opposed to any version of a socially anchored cultural worker. However, he distanced himself from Benda's founding document of the sociology of intellectuals, *The Treason of the Intellectuals* (Benda 1928). Anticipating the outbreak of World War II, Julien Benda warned that intellectuals should not endorse interests in the name of a nation, a class or a race, but stay other worldly philosopher-kings in their ivory-towers ('My kingdom is not of this world').¹ Karl Mannheim responded resolutely:

1 The *Treason of the Intellectuals* (*La trahison des clercs*) was an extremely influential jeremiad that went 'through more than 50 editions in 20 years' (Kurzman/Owens 2002: 65). Benda used a religious term to denote intellectuals – *clercs* – who he exemplified among

The traditional cult of the exclusively self-oriented, self-sufficient intelligentsia is in the process of disappearing; and let us be straight-forward about the fact that we can no longer bear this aesthetic type; we must finish with this socially aimless, socially useless thinking ... Therefore I say: join the party which is the organic expression of the class, but do not think as a functionary but a free man! ... Join the ranks, but maintain the freedom of living thought. (Mannheim 1932 [1993]: 79)

Karl Mannheim vindicated the point of view that intellectuals should be regarded as ‘members of a relatively classless stratum which is not too firmly embedded in the social order’ (Mannheim 1929 [1955]: 154). By creating the figure of the free-floating intellectual, he not only intended to revise the nullity of intellectuals within the dichotomous Marxist class theory (Pels 1993: 53) but to create also a social group that turns his ideas of a sociology of knowledge as an organ for politics as a science into practise. According to Karl Mannheim, intellectuals cannot formulate a substantially binding and indicative political knowledge, just as – to cite Mannheim’s favourite analogy – psychoanalysts cannot dispose over the knowledge of their clients or live their lives (Kettler, Meja and Stehr 1990: 123). Because of their participation in various educational milieux and their recruitment from various social classes they have, however, the capacity of viewing social problems from a distance and as a whole and to synthesize all contradictory views on the phenomenon. Who presents such a type of intellectual today? Most likely, autonomous full-time university employees who are likely to aim at a dispassionate analysis of social reality, untainted by class affiliations. There is some evidence that professors who intervene nowadays in the public sphere have become more cautious and therefore are likely to search for the ‘objective truth’. If in the twentieth century ‘a professor actually spoke to the larger audience beyond the classroom, he could be sure that his authority was never challenged ... now, they would often find themselves in the position of having to legitimize their status and their views’ (Fleck, Hess and Lyon 2009: 5).²

The second grand theme concerns the question of whether intellectuals do speak, or should speak, in the name of truth or universal reasons ‘that come necessarily before politics’. This idea of the intellectual who stands in for universal values is opposed to the conception of the intellectual who makes use of specific knowledge only (problematic of expertise). In history, the role of the universal intellectual

others by Socrates, Jesus, Spinoza and Voltaire. It’s apparent that such a being set apart must be regarded as a utopian conception.

2 Of course university professors are not free of political affiliations. However, it is common sense that one engages in science for science’s sake and that one should communicate only knowledge to the public that has some scientific backing. An empirical study on Canadian university professors concluded: ‘University professors are politically active, somewhat centrist, and at times left-leaning based on their vote and self-identification. [...] Those in liberal arts and with a leftist ideological self-placement are more likely to vote for a left party’ (Nakhaie/Adam 2008: 892).

was mostly played by writers like Jean-Paul Sartre, Susan Sontag, Günter Grass or Noam Chomsky, who were seen as moral authorities. In Jean-Paul Sartre's words, the universal intellectual is 'someone who meddles in what is not his business and claims to question both received truths and the accepted behaviour inspired by them' (Sartre 1974: 230). M. Rainer Lepsius circumscribed such an intellectual style of thought as an 'incompetent but legitimate form of criticism' (Lepsius 1964: 88). Bourdieu distanced himself from the missionary ideology of the universal intellectual by putting forward his own concept of cultural producers as bi-dimensional beings who on the one hand belong to an intellectually autonomous field and stick to the field's particular law (for example, looking for the truth), or on the other, deploy their authority in a political activity outside it. 'They must remain full-time cultural producers without becoming politicians' (Bourdieu 1989a: 99), which means walking a tightrope. The autonomy of science and the public engagement is not seen by Bourdieu as being antithetical, but rather complementary. Bourdieu portrays intellectuals as (somewhat) more than experts and scientists, insofar as the former tend to take on wider issues that open up professions to each other and to the political. In contrast to the deceased 'grand' intellectual, however, the professional intellectual remains more closely involved with partial fields of competence and their local politics of knowledge (Pels 1995: 84)

Intellectuals thus have to navigate between Scylla and Charybdis, the Scylla being the 'old-style intellectual prophesying' that is embodied best by Sartre, and 'specific intellectuals such as Foucault meant them (who limit their interventions to a particular domain of knowledge and experience)' standing for the Charybdis (Bourdieu 1989a: 108).

According to Foucault the figure of the specific intellectual has emerged since World War II:

It was the atomic scientist (in a word, or rather a name: Oppenheimer) who acted as the point of transition between the universal and the specific intellectual. It's because he had a direct and localized relation to scientific knowledge and institutions that the atomic scientist could make his intervention; but, since the nuclear threat affected the whole human race and the fate of the world, his discourse could at the same time be the discourse of the universal. (Foucault 1984: 69)

Specific intellectuals (such as nuclear scientists, computer experts) act in the first place within 'specific sectors, at the precise points where their own conditions of life or work situate them (housing, the hospital, the asylum, the laboratory, the university, family, and sexual relations' (Foucault 1984: 68). This type of intellectual "struggles against the forms of power in relation to which he is both object and instrument: within the domain of 'knowledge', 'truth', 'consciousness', 'discourse'" (Foucault and Deleuze 1972: 104). His task is to 'criticize the working of institutions which appear to be both neutral and independent; to criticize them

in such a manner that the political violence ... will be unmasked, so that one can fight them' (Foucault, cited in Chomsky and Foucault 1974: 171).

What thus distinguishes the universal from the specific intellectual is the generality of discourse and how much weight is given to 'insider knowledge' that is at the intellectual's disposal. Generalists will tend more to apply an 'incompetent but legitimate form of criticism', while sectoral specialists will be inclined to stick to their position within the order of knowledge. The first will always condemn or at least judge, while the later may also pragmatically assess options. What both, Bourdieu and Foucault have in common is the rejection of the notion of intellectuals as spokesmen.³ Of course much more is to say about what may be termed the classical sociology of intellectuals (see Kurzman and Owens 2002, Eyal and Buchholz 2010). However, the task we set for was to reconstruct only the guiding research directions.

While classical sociology is primarily concerned with possible representations of the intellectual (Said 1996), more recent investigations on interventions in the public sphere tend to depersonalize their object of analysis. The question whether intellectuals act according to an idea or representation that tells them that intellectuals should either be autonomous or advocates striving either for rational investigation or moral judgement is pushed into the background. Instead the intellectual space becomes the focus of attention. By creating vast data sets, social scientists aim at depicting the space within which opinions are articulated. Such an approach is mostly inspired by Bourdieu's field analysis as will be shown in the next section.

From Intellectuals to Intellectual Spaces – New Approaches in the Sociology of Intellectuals

Field analysis is one way of putting the sociological imagination (Mills 1961) into practise. To give one example: Studying op-eds can be a useful entry point into the public sphere. Applying field analysis to op-eds means studying systematically who enters the op-ed space, thus a space in which citizens can make arguments from a position of relative autonomy. When looking at relational distributions of op-ed authors' occupations or, in the case of academics, at their disciplines we think ourselves away from the daily routine of reading newspapers by asking what

3 Bourdieu and Foucault deal as well with the problematic of allegiance. Bourdieu argues that intellectuals compromise the dominated fraction of the dominant class that rarely acts as a collective. 'They are dominant, in so far as they hold the power and privileges conferred by the possession of cultural capital and even [...] the possession of a volume of cultural capital great enough to exercise power over cultural capital; but writers and artists are dominated in their relations with those who hold political and economic power' (Bourdieu 1990: 145). Foucault argued that people did not need a patronizing intellectual to guide them (Foucault and Deleuze 1972: 3).

the space of opinion is all about. In order to be able to point out the merits of field approaches, I will refer to some ground-breaking studies.

In *Homo Academicus*, Bourdieu's paradigmatic investigation of the French intellectual field, the author provides us with an analysis of professors according to their social origin, academic trajectories, political stances, economic and political resources and professional practices. Constructing the field brings a schismatic picture to the forth that is produced by the 'rival coexistence of several relatively independent principles of hierarchization' (Bourdieu 1988: 113). As one moves from the dominant disciplines, medicine and law to the faculties of humanities and sciences, the academic capital (control of the university's organizational instruments) decreases. The sciences are merely structured according to the scientific prestige of the academic personnel. Within the social sciences that are located between the two poles Bourdieu observed a clash of socio-political and scientific authority. By demonstrating how highly regulated academic fields are, Bourdieu suggest intellectual activities that would seem the most autonomous and detached to be preconditioned by the structure of the field in which they are embedded (Eyal and Buchholz 2010: 124).

Bourdieu conceptualizes fields as structured spaces organized around specific types of capital (Bourdieu 1989b). They are arenas of appropriations of goods and status in which actors constantly strive to accumulate different kinds of capital. We find this analytical strategy to map out fields in Medvetz's recent study on think tanks and public policy experts in the United States. Think tanks are regarded as structurally hybrid offspring of the more established institutions of academics, politics, business and journalism – "the 'parental' ties being at once material and symbolic: *material* because these anchoring institutions provide support, patronage, and personnel to think tanks, *symbolic* because the figures of the policy aide, the academic scholar, the entrepreneur, and the journalist supply the imaginary models from which policy experts draw in fashioning their mixed self-understandings" (Medvetz 2007: 35–6). Public intellectuals working for think tanks, he argues, are located within a hybrid space in which actors profit simultaneously from the authority of different fields. Think-tank-affiliated policy experts can be differentiated according to their proximity or distance to four poles. Within the space of think tanks Medvetz identifies 'hacks' (politics) and 'policy entrepreneurs' (business) that dispose of high economic and low cultural capital as well as 'wonks' (academics) and 'correspondents' (journalism) with low economic and high cultural capital.

By drawing on Bourdieu's field theory Sapiro (2003) explains why 'prophesying' predominated as a specific mode of politicization among French writers until the 1970s. "Prophetic discourse, grounded less in expert knowledge than in the aptitude for 'emotional preaching', is a particularly appropriate mode of universalization for writers, those producers of collective representations whose legitimacy rests on their charisma" (Sapiro 2003: 638). Sapiro's main argument focuses on the reconfiguration of the intellectual field, that is the emerging of professional groups of experts, that pushed the values of scientific accuracy and

technical competence. 'Prophesying was a way of compensating for the lack of professional development by redefining the social function of the writer at a time when writers were losing control of their spheres of influence' (ibid.: 640).

Fields are also addressed in the recent work of Jacobs and Townsley (2011) on the *US space of opinion* as a part of civil society that serves as a conceptual frame for my study. Media opinion and commentaries are located at the overlapping intersection of the political, journalistic and academic field. Relying on enormous samples of opinion collected from newspapers and television shows during the first years of the last two Presidential administrations, the authors sketch out the opinion space of the USA today. Media insiders like professional columnists still clearly dominate the space of opinion in the *New York Times* and *USA Today*, while the plurality of other voices populating the space of opinion is growing. By also considering TV shows like *The News Hour*, *Face the Nation*, *Crossfire* and *Hannity & Colmes*, the authors are able to demonstrate that the space of opinion became polarized over time. Political shows move closer to the political field while journalistic autonomy is reinforced in professionalized spaces like the *New York Times*. The central conclusion of the authors is thus that new formats have increased the presence of certain voices and perspectives (such as partisan hosts, binary moral narratives) while discouraging others (academics, narratives emphasizing historical complexity).

All these studies have in common that they abstract from individuals by investigating the space that is constituted by ongoing intellectual interventions. Bourdieu's field theory proves to be influential in some (Medvetz, Sapiro) and at least inspiring in other strands (Jacobs and Townsley).

The survey of literature suggests that older and more recent approaches in the sociology of intellectuals stand for distinct research programs. At first glance it seems hard to make the classical sociology of intellectuals to harmonize with more recent approaches (see Eyal and Buchholz 2010). While the classics expended much effort in defining 'intellectuals' and describing intellectual identity, contemporary studies focus on the transformation of what one can call the public arena. The unit of analysis changed. New approaches are concerned about who enters a socially delimited space of competition over intellectual authority. If the term 'intellectuals' is mentioned at all, authors try to circumvent definitional quandaries on who counts as an intellectual (Medvetz 2007). As a rule actors turn out to be more or less embedded in socio-economic networks which goes hand in hand with a more or less autonomous intellectual engagement. It is the internal structure of a field that tells us whether actors perform intellectual functions as an end or for alternative ends, for example in the service for political and economic power: Which means that the problematic of allegiance that was formulated by the classics is also taken up by the second literature. The issue of expertise is tackled too, as social scientists investigate the concrete patterns of public engagement. For example, by detailed content analysis of op-eds Jacobs and Townsley (2011) access the depth and complexity of discussions. Argument styles, for example, are telling us a lot about whether intellectuals elaborate on 'general ideas' or whether they base their arguments primarily on insider knowledge. On second glance there

thus becomes continuity apparent within the sociology of intellectuals. Even if we consider the classical writings only in light of their contribution in debates underway at the present they do give us still orientation in research. I will therefore try to bring together both literatures in my empirical study when turning to the main research object – public intellectuals within the opinion space of ProSyn.

Op-Eds in *Project Syndicate* – Who Populates the Space of Opinion?

Newspapers in the eighteenth and nineteenth centuries tended to be politically motivated organs. As modern journalistic practices started to encourage the separation of opinion from purely factual reporting, op-eds emerged as a specific space in newspapers (Kowalchuk and Laughlin 2009).⁴ Op-eds clearly contribute to the opinion formation in civil societies. ProSyn is first and foremost a source of op-ed commentaries. In June 2011 the syndicate that is located in Prague collaborated with 464 newspapers in 150 countries.⁵ Besides some financial contributions from member papers it is mainly supported from the Open Society Institute (OSI).⁶ By self-definition ProSyn is ‘committed to maintaining the broad intellectual scope and global reach that readers need to understand the issues and choices shaping their lives’. Contributors are given seven labels: ‘politicians and statesmen’ (Joschka Fischer), ‘economists’ (Jeffrey D. Sachs), ‘political scientists and philosophers’ (Joseph S. Nye), ‘global strategists’ (Richard C. Holbrooke), ‘scientists’ (Paul Berg), ‘novelists’ (Umberto Eco) and ‘activists’ (Naomi Wolf). The 800–900 word essays that authors send to the syndicate must be written in English. Accepted manuscripts are often translated into Arabic, Chinese, German, French, Russian, Spanish or other languages and distributed among national newspapers. The readers usually find the articles within the opinion-section that is clearly separated from the fact-section. At the bottom line of the op-ed *Project Syndicate* is indicated as the source.

We do not have any systematic knowledge on the impact of op-eds. Most op-ed writers usually have the experience that their op-ed does not fulfil the intended purpose of raising awareness by bringing attention to political, social, or other issues of particular importance. SynPro op-eds, however, may be published simultaneously in different countries and are often written by authors known to be an authority source on the particular issue at hand. It is this high visibility that makes them more likely to organize discussions within civil society. On the other hand, SynPro op-eds are known

4 Op-ed is an abbreviation for opposite the editorial page. The idea of a commentary page had been around since the 1920ies, but back then the page was reserved for house columnists only. In 1961 John B. Oakes, editor of the New York Herald Tribune, was the first to ask diplomats, professors and others for contributions to the editorial page (Shafer 2010).

5 http://www.project-syndicate.org/about_us/who_we_are <09.06.2011>

6 The OSI also provides administrative, financial, and technical support to other parts of the Open Society Foundations. OSF was created in 1993 by investor and philanthropist George Soros to support his foundations in Central and Eastern Europe and the former Soviet Union.

not to link to what editors call a ‘news peg’ – that is, something that happened very recently. This is why they may not receive great attention. We do have some anecdotal evidence that ProSyn op-eds from popular politicians and outstanding scholars have an important impact. The former Harvard economist Jeffrey Sachs, one of the most prominent contributors to *Project Syndicate*, might be a good example. One of his contributions, under the heading of ‘Obama Undermines the UN Climate Process’ (20 December, 2009) was circulated to 68 national newspapers. Sachs interpreted the reluctance of the US to embrace the Kyoto Protocol and its pronouncement to only accept unilateral rule-setting as a major setback.

Obama’s decision to declare a phony negotiating victory undermines the UN process by signalling that rich countries will do what they want and must no longer listen to the ‘pesky’ concerns of many smaller and poorer countries. Some will view this as pragmatic, reflecting the difficulty of getting agreement with 192 UN member states. But it is worse than that. International law, as complicated as it is, has been replaced by the insincere, inconsistent, and unconvincing word of a few powers, notably the US.

It is not easy to evaluate the debate on whether the Climate Conference in Copenhagen swept away the Kyoto architecture. We are thus very likely to accept the professional authority of accredited experts, such as Jeffrey Sachs, on the issue. Given the high circulation rate one might therefore argue that such op-eds form a somehow influential part of a global public sphere.

In what follows I will analyse who speaks in the public arena of ProSyn. I will do so by only looking at two variables: the op-ed writers’ occupations⁷ and the national base of the main institutions they belong to. I will provide a more fine-tuned analysis in the case of academics and spokespersons of think tanks. In May 2011, 1,559 contributors were registered on the ProSyn website.⁸ Academics, the largest subgroup of contributors, are most likely to come from social sciences and law or business than from sciences or humanities. The distribution confirms previous research on op-ed authors in countries like Canada (Kowalchuk and McLaughlin 2009) or Austria (Korom 2008) with one striking exception, which is the relatively high percentage of scholars from sciences. In ProSyn biologists, physicists, psychologists or psychiatrists do not only share their expert knowledge with a transnational public but use moral styles of argumentation. Such a style is illustrated by the fact that authors address their speech to the public in a way that invokes third parties (see Jacobs and Townsley 2011: 141–7). To give two examples: Henry I. Miller, a molecular biologist and fellow at Stanford’s Hoover Institution, does not only provide information on malaria or radiation to the public, but also gives policy advice to ‘reboot DDT’ as a tool for fighting malaria (Henry

7 In many cases not only one single occupation or role was stated. I was therefore confronted with the question of how to code contributors. As a rule I coded the first stated occupation while mainly using occupational categories that are given by Project Syndicate. I added, however, some categories to make the analysis more refined.

8 <http://www.project-syndicate.org/contributors> <14.05.2011>.

Table 12.1 Occupational Composition of all Contributors (Left Column) to the Project Syndicate-Space and of a Selected Subgroup (Right Columns) of Think Tank and University Affiliated Writers

All contributors	Affiliated to			
	Think Tanks	Universities		
		Professors	Fellows, lecturers, etc.	
<i>Academic Fields</i>	57.4	88.3	100.0	100.0
Social Science and Law	24.0	59.4	30.5	53.5
Humanities	5.1	0.5	11.8	11.9
Business	17.1	25.4	31.7	19.5
Science and math	11.2	3.0	26.0	15.1
<i>Activists/NGO spokesperson</i>	4.8	1.5		
<i>Authors</i>	5.6	1.0		
<i>Politicians and Statesmen</i>	23.8	6.2		
EU, IMF, UN, NATO, etc.	6.8	3.0		
President, Minister, MP, etc.	15.4	2.0		
Ambassadors	1.6	1.0		
<i>Journalists</i>	3.3	1.5		
<i>Judge/Lawyer</i>	0.6	0.5		
<i>Managers</i>	4.2	1.0		
<i>Others</i>	0.2	0.5		
<i>Total</i>	100.0% N = 1559	100.0% N = 196	100.0 % N = 502	100.0% N = 159

I. Miller, 5 May 2010) – this is a rather specific recommendation to governments. Andrew Spielman, Professor for Medicine at Harvard University, speaks more indirectly to political elites when accessing critically the applicability of a vaccine-based approach to malaria (Andrew Spielman and Awash Teklehaimanot. 21 March, 2002). In these, as in many other cases, scientists use op-eds not only as opportunity structures to leave the ivory tower of science and disseminate research findings, but rather aim at shaping public opinion and influencing opinions – a finding that is incongruent with the well-nursed picture of the purely objective scientist who is completely detached from political matters.

It is interesting to see that academics contributing to ProSyn mostly hold the academic title ‘professor’, which means that they are in the privileged position of exercising their profession in academic freedom. The second largest group of academics works for think tanks, followed by a significant fraction that is affiliated to universities as research fellows or lecturers and a negligible number of ‘economists’, ‘political scientists’ and other academic scholars who work for consulting companies and other institutions.

Contributors from academic fields compete mainly with politicians and statesmen to define questions of broad social and cultural meaning. Within this social group national politicians dominate over those who either work or have worked for

intergovernmental organizations or as ambassadors. The former German foreign minister Joschka Fischer writes for example a monthly commentary series called 'The Rebel Realist'. Denmark's Uffe Ellemann-Jensen, Vaclav Havel from the Czech Republic, or Shlomo Ben-Ami from Israel are other examples for former democratically elected political leaders who enter the public space in order to discuss matters of common concern. Even active politicians such as the Swedish Foreign Minister Carl Bildt mostly do not contribute to ProSyn in order to promote a particular policy. Bildt, for example, wants Europeans to be 'committed to an open Europe in an open world' (Carl Bildt, 1 April 2010). Such a style of argumentation indicates that politicians do not primarily use ProSyn for transmitting arguments to a transnational public that have direct implications for pressing current issues in public policy. By using the platform they rather aim to engage in general political discourses.

The proportion of all other social groups such as activists/NGO spokesperson or authors is considerably lower. Therefore it seems worthwhile to have a closer look at think tanks and universities that dominate within ProSyn. The opinion space seems to be clearly more accessible to academics who are affiliated with prestigious universities in the US, the UK, France or Germany. Posner (2001) argued that the scientific capital of public intellectuals correlates only weakly with his or her visibility in the public sphere. In the case of ProSyn we rather find a positive association at the aggregated institutional level. It seems that not only in science but also in the public space of ProSyn is structured by what Robert Merton (1968) called 'Matthew effect': Eminent scientists get disproportionate attention, while relatively unknown scientists are mostly ignored. As with universities, think tanks from the US are most strongly represented. Think tanks often gain a competitive advantage by strategically placing op-eds (Rich 2004: 207). Publishing op-eds means marketing actively one's research and thus improving one's chance of being influential (Braml 2004). It is therefore natural to assume that op-ed writers affiliated to think tanks are less autonomous speakers than others. Their contributions to ProSyn are especially characterized by kinds of argument that aim to define a legitimate vision of the social world. Bourdieu pointed out 'the imposition of a definition of the world is in itself an act of mobilization which tends to confirm or transform power relations' (Bourdieu 2005: 39). It is highly likely that op-ed contributors from think tanks are quiet conscious of the fact that they do not only create visions of the social world, but also mobilize people.

Going through the list of think tank-affiliated experts makes one realize that these op-ed writers are mostly marked by hybrid characters that traverses, links and overlaps the divergent worlds of academics, politics, business, and journalism (Medvetz 2007). This hybridity manifests itself in the writing style. We find many op-eds that contain both, the idiom of an academic scholar, but also the idiom of a policymaker, a business entrepreneur or a journalist. That in most cases one of the many expertises prevails, can be easily demonstrated by two randomly chosen examples:

Eswar Prasad, professor of economics at Cornell University and Fellow at the Brookings Institution, introduces readers first to the academic debate on capital controls – taxes or restrictions on international transactions in assets like stocks or

bonds. After having laid the groundwork for understanding the use of restrictions on capital flows he clearly rejects capital controls as a useful instrument of regulation: ‘As tempting as they are, quick fixes like capital controls merely provide a false sense of security and delay needed adjustments in an economy’ (Eswar Prasad, 2 August 2010). The author clearly does not only inform the debate with research, but fights for a particular vision of public policy.

Anders Åslund, fellow at the Peterson Institute for International Economics, sheds

Table 12.2 Per cent of Affiliation to Universities and Think Tanks

Universities		Think Tanks	
<i>US</i>	54,2	<i>US</i>	55,6
	Harvard University 8,0		Asia Society 11,7
	Yale University 3,2		Open Society Institute 5,6
	Columbia University 4,5		Brookings Institution 5,1
	New York University 3,0		Hoover Institution 4,1
	University of California, Berkeley 2,9		RAND Corporation 3,1
	Princeton University 2,4		Hudson Institute 2,0
	Stanford University 2,4		National Bureau of Economic Research 2,0
	University of Chicago 1,5		US Council on Foreign Relations 2,0
<i>UK</i>	8,6	<i>Belgium</i>	12,2
	Oxford University 2,3		Bruegel 3,1
	University of London 1,4		Carnegie Endowment for International Peace 3,1
	London School of Economics 1,2		Centre for European Policy Studies 1,5
	King’s College 0,5		European Council on Foreign Relations 0,5
<i>France</i>	3,5	<i>UK</i>	7,1
	Sciences Po 0,8		Chatham House 4,6
	EHESS 0,8		Center for European Reform 1,0
<i>Germany</i>	3,2	<i>Germany</i>	4,1
	University of Munich 0,3		German Institute for International and Security Affairs 1,5
	Freie Universität Berlin 0,3		Stiftung Wissenschaft und Politik 1,0
<i>Others</i>	30,6	<i>Others</i>	20,9
<i>Total</i>	100%	<i>Total</i>	100%
	N = 661		N = 196

light on why Putin is credited with political and economic stability. Having sketched out Putin’s achievements that non-experts might not be familiar with, he starts to offer a prognosis for the future: ‘Putin’s success may lead to his downfall [...] Because he wants to make all decisions, he replaced a strong prime minister and chief of staff with two men unable to make decisions. So rather than creating a strong vertical

command, he paralysed his government' (Åslund, 27 September 2004). Contrary to the first case, the substantive weight of own research findings is smaller, the moral dimension more important than the technical one. The propagated view enjoys in both examples clearly consensus within the policy research community. When only considering their op-eds only one would tend to classify Prasad in the first place as a scholar, while Åslund resembles more a policy expert who anticipates a hot policy issue ('Putin's last stand') before it arises. While the Peterson Institute is described as a non-partisan research institute that provides 'concrete solutions to key international economic problems', Brookings Institution is rather known be a 'university without students', where 'books are written for policy makers and read by college students' (Weaver 1989: 566). The importance of academic standards differs accordingly between both op-eds, which is clearly mirrored by apparent differences in op-ed formats and content.

Having mapped out the social space of ProSyn, I will analyse in the remainder of this chapter the role of public intellectuals. ProSyn can be regarded as one of the most influential newly emerging spaces of public intellectual activity. It seems therefore appropriate to ask who started large-scale attempts to dominate the Pro Syn scene. My starting hypothesis is that a few key figures occupied the different niches of the quiet heterogeneous space.

Public Intellectuals in *Project Syndicate* – Who are the Key Figures?

Not all contributors to ProSyn can be called public intellectuals. Although the term is quiet diffuse it its rather undisputed that it is coined for those people only who have an impact on a broad swath of society by engaging *regularly* in public debates. I will therefore turn the analysis to those intellectuals who contributed at least 20 op-eds to ProSyn. As already mentioned, ProSyn is a platform that helps intellectuals to transcend the relatively narrow confines of op-eds pages in national newspapers. Take for example the sociologist Ralf Dahrendorf, who within four years contributed 57 articles that were published 1,419 times in different languages across the globe (see appendix). The relatively high circulation rate of ProSyn enabled Dahrendorf to influence trans-national discourses and to contribute to the making of a supranational communicative space.

A list of all public intellectuals contributing to ProSyn (see appendix) gives a small picture of the whole social space. More than half of the intellectuals come from academic fields, politicians and statesmen constitute the second largest group that is fairly over-represented compared to journalists, writers, activists and others. Most intellectuals are affiliated to America's elite universities, and former national politicians dominate over those who mainly played a crucial role in world politics. There are, however, some deviations from the population sketched out in Table 12.1: The academic disciplines economics and humanities are over-represented and intellectuals from European think tanks are as frequent as those who are affiliated to American ones.

Table 12.3 Distinct Vocabulary Profiles within the group of Public Intellectuals (N = 44)

Debating Europe (I) (N = 16), Prototypes: <i>Uffe Ellemann-Jensen</i> (Denmark Foreign Minister), <i>Ian Davidson</i> (European Policy Centre)	eu [eu*], euro [euro*], Europ* , policies [policy, policy*, politic, politic*], France [France*, french, french*], government [government*], economy [econom*], America [America*], German [German*], people [people*], market [market*], member [member*], power [power*], international [international*], China [China*, chines*], war [war*], Russia [Russia*], president [president*, presidency], state, [states, state-building, state-less, statehood], growth [growth*], crisis [crisis*, crises]
Economy Debate (III) (N= 14), Prototypes: <i>Kenneth Rogoff</i> (economics, Harvard) <i>Dani Rodrik</i> (Political Economy, Harvard)	China [China*, chines*], global [global*], economy [econom*], policies [policy, policy*, politic, politic*], growth [growth*], government [government*], market [market*], bank [bank*], crisis [crisis*, crises], finance [finance*], people [people*], eu [eu*], euro [euro*], price [price*], America [America*], invest*, fiscal [fiscal*], debt [debt*], dollar [dollar*]
Debating America/Near and Middle East (II) (N = 10), Prototypes: <i>Shlomo Ben-Ami</i> (Israel, Foreign Minister) <i>Shlomo Avineri</i> (political science, Hebrew University)	Israel [Israel*], policies [policy, policy*, politic, politic*], America [America*], war [war*], president [president*, presidency], power [power*], government [government*], arab [arab*], international [international*], China [China*, chines*], Palestine [palestin*], peace [peace*], Iraq [Iraq*], people [people*], people [people*], Iran [Iran*], eu [eu*], democracy [democrac*, democrat*], state-less, statehood], region [region*]
Global Debate (IV) (N= 4), Prototypes: <i>Peter Singer</i> (ethics, Princeton) <i>Esther Dyson</i> (23andMe)	eu [eu*], policies [policy, policy*, politic, politic*], euro [euro*], China [China*, chines*], Europ* , government [government*], economy [econom*], America [America*], global [global*], people [people*], war [war*], growth [growth*], power [power*], international [international*], market [market*], president [president*, presidency], bank [bank*], crisis [crisis*, crises], Israel [Israel*].

Remarks: Yoshikoder counts the number of words from each dictionary category. Such a category, as for example ‘policies’, can consist of different wordstems (policy*, politic*) and words (policy, politic). * stands for one or more letters that are preceded by a wordstem.

Altogether this core group of contributors provided ProSyn with more than two thousands op-eds. To get to know more about their intellectual engagement I conducted a very simple word frequency analysis of massive chunks of textual data assuming that the most frequent themes in texts are the most important ones. I started by inductively creating a content analysis dictionary encompassing the most recurrent 424 keywords (words or word-stems). The software program Yoshikoder provided me with word frequency tables of all op-eds of any of the selected intellectuals according to the created dictionary. On the basis of a key-word-frequency matrix I finally clustered all intellectuals into four groups that stand for different debates. Three intellectuals (Åslund, Khrushcheva, Tharoor) proved not to fit any of the clusters. Table 12.3 provides an overview of the distinct vocabulary profile of each group of debaters.

Within the first cluster we find intellectuals affiliated to European think tanks like the German Institute for International and Security Affairs, the European Policy Centre, the Centre for European Policy Studies or Ifo Institute, economists from elite universities (Harvard, MIT, Bocconi) and former politicians who share interests in European Affairs (Ellemann-Jensen, Fischer, Patten, Rocard). The cluster is best exemplified by figures like Uffe Ellemann-Jensen and Ian Davidson whose columns always make reference to the European context. With the exception of Ralf Dahrendorf and Václav Havel, we do not find any of the usually reported ‘transnationally recognized European intellectuals’ (Outhwaite 2009, Cheneval 2010). If it comes to op-eds the European public sphere *à venire* is rather fostered by policy institutes’ affiliated thinkers (Bertram, Davidson, Gros, Krauss, Sinn).

Well-known US economists dominate the second largest debate. Within the social space of ProSyn, economists like Dani Rodrik, Kenneth Rogoff or Jeffrey D. Sachs are regarded as particularly relevant in the discussion about social problems. Contrary to the well-documented case of French public intellectuals, it is not philosophical, sociological or literary excellence that counts but a specific kind of scientific legitimacy economists are credited with, which makes them closer to Foucault’s ‘specific intellectual’ than to Bourdieu’s ‘universal intellectual’ or Sartre’s ‘total intellectual’.

A third debate circles around the Israel-Palestinian conflict, American foreign politics and the future of democracy in the Arab world. The discussion is mostly spearheaded by Israeli Arab intellectuals (Avineri, Ben-Ami, Kuttab, Yamani).

Finally, a fairly diverse and smallish group of intellectuals was identified that picks up topics of all other groups. Generalists are usually reported to come from the literary field. In the case of ProSyn intellectuals, one writer (Naomi Wolf) competes with an entrepreneur (Dyson), a historian (Król) and a philosopher (Singer).

Having mapped out in a preliminary analysis the whole space of opinion and the place of public intellectuals within that space I will now turn to the initial research question: How can we combine the study of spaces of opinion with the study of intellectuals?

Conclusion

Given the growing diversity of types of intellectuals it makes sense not to decide in advance on who counts as an ‘intellectual’, but rather to approach the topic empirically. The strategy taken here was to map out the public arena first. The social space of ProSyn proved to be a fairly good predictor for models of intellectuals to be found on the trans-national level. To put it in another way: Figures of intellectuals are to a great extent preconditioned by the structure of the field in which they are embedded. Economists from elite institutions as well as think tank-affiliated experts have not only outstanding positions within the opinion space of ProSyn: They are also to be found most often among intellectuals contributing to ProSyn, which suggests that modelling the structure of opinion spaces is a good

method for identifying niches for intellectuals. However, research on intellectuals should go beyond constructing the topology of opinion spaces. It seems essential for me to take up the questions of expertise and allegiance that were put forward in the 'old' literature on intellectuals.

First, trans-national opinion spaces seem to be marked by a clear hierarchy of academic disciplines that gives much more space for economists and political scientists than all other scholars. Only these two disciplines rival politicians and statesmen in offering appropriate view of the social world. Think-tank-associated intellectuals prove to be hybrid pivotal figures that occupy the intermediate zones between politics and the academic world. All other groups enjoy rather weak legitimacy in the public space of the trans-national press. This brings up the question of whether universal intellectuals are vanishing? A very preliminary content analysis showed that intellectuals mainly advance their professional expertise when entering the public. Further research is needed to specify what universal (Bourdieu) and specific intellectuals (Foucault) are all about in the twenty-first century.

Second, the rather strong representation of political actors and think tank policy advisers raises the question of the diminishing autonomy of today's most common figures of intellectuals. This touches a topic that was of great concern to the 'classics'. Is Mannheim's autonomous public intellectual an endangered species? Do academic professionalism, government-sponsored think tanks and a media-saturated culture make the independent social critic a thing of the past? Why, *pace* Gramsci, have intellectuals stopped to act on behalf of subordinate classes?

These are big questions that go far beyond the scope of the chosen research design. I hope, however, to have demonstrated that research on intellectuals of today can profit from a reconciliation of the new and old literature on intellectuals.

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7 Public Intellectuals participating in *Project Syndicate*

Born in	No. of Articles	No. of Publications	No. of Years	Group	Professor	Academic	Profession	Country	Main Affiliation	Nat. Affil.
1957	44	611	6	I	1	1	economics	Italy	Harvard University	USA
1956	22	293	15	/	0	1	economics	Sweden	Peterson Institute for International Economics	USA
1933	38	574	13	II	1	1	political science	Israel	Hebrew University	Israel
1943	51	950	5	II	0	0	politicians and statesmen	Israel	Toledo International Centre for Peace	Spain
1937	34	412	13	I	0	1	political science	Germany	German Institute for International and Security Affairs	Germany
1951	47	1520	4	II	1	1	political science	Netherlands	Bard College	USA
1953	32	668	7	II	0	0	politicians and statesmen	Mexico	today: New York University	USA
1929	57	1419	4	I	1	1	sociology	Germany	London School of Economics	Great Britain
	37	490	11	I	0	0	journalist	/	European Policy Centre	Great Britain
1960	111	3148	9	III	1	1	economics	USA	University of California, Berkeley	USA
1942	58	708	7	I	1	1	economics	Germany	MIT	USA
1951	30	695	3	IV	0	0	CEO	Swiss	23andMe (biotechnology company)	USA

952	28	462	3	III	1	1	economics	USA	University of California, Berkeley	USA
941	19	328	10	I	0	0	politicians and statesmen	Denmark	Former Minister of Foreign Affairs	Denmark
953	23	480	15	III	1	1	economics	China	Beijing University	China
939	34	934	3	III	1	1	economics	USA	Harvard University	USA
948	61	1942	5	I	0	0	politicians and statesmen	Germany	Former Minister of Foreign Affairs	Germany
949	36	554	6	I	1	1	economics	Italy	Bocconi University	Italy
955	20	482	8	I	0	1	political science	Germany	Centre for European Policy Studies (CEPS)	Belgium
951	35	857	11	II	0	0	politicians and statesmen	USA	Council on Foreign Relations	USA
936	35	633	15	I	0	0	politicians and statesmen	Czechoslovakia	President of the Czech Republic	Czech Republic
956	51	1574	10	III	1	1	history	Great Britain	Princeton University	USA
	40	796	13	/	1	1	humanities	Russia	New School	USA
	39	877	10	I	1	1	economics	USA	Hoover Institution	USA
944	21	213	9	IV	1	1	humanities	Poland	Warsaw University	Poland
955	25	628	8	II	1	1	journalism	Palestine	Princeton University	USA
965	71	2008	6	III	0	1	political science	Denmark	University of Copenhagen	Denmark

Continued

Born in	No. of Articles	No. of Publications	No. of Years	Group	Professor	Academic	Profession	Country	Main Affiliation	Nat. Aff.
1946	64	1695	6	I	0	0	writers	France	/	/
1937	20	303	12	II	0	0	activists	USA	Open Society Institute	USA
1937	93	2736	9	II	1	1	political science	USA	Harvard University	USA
1944	39	3848	3	I	0	0	politicians and statesmen	Great Britain	European Commission	Belgium
1930	33	/	8	I	0	0	politicians and statesmen	France	European Parliament	Belgium
1957	55	1783	13	III	1	1	political science	Turkey	Harvard University	USA
1953	75	3057	6	III	1	1	economics	USA	Harvard University	USA
1958	37	1430	4	III	1	1	economics	USA	New York University	USA
1954	174	5364	16	III	1	1	economics	USA	Columbia University	USA
1940	21	353	10	III	0	1	writers	USA	Center on US China Relations	China
1946	76	/	8	III	1	1	economics	USA	Yale	USA
1946	73	2239	10	IV	1	1	ethics	Australia	University of Melbourne	Australia

1948	36	825	6	I	1	1	economics	Germany	Ifo	Ger
1939	40	868	4	III	1	1	economics	Great Britain	Warwick University	Gre
1930	64	1556	15	I	0	0	CEO	USA	Open Society Institute	USA
1943	22	/	3	III	1	1	economics	USA	Hoover Institution	USA
1957	24	409	7	II	0	0	politicians and statesmen	Great Britain	European Parliament	Bel
1956	31	697	9	/	0	0	politicians and statesmen	India	former Indian Minister of State for External Affairs	Indi
1962	34	840	3	IV	0	0	writers	USA	/	/
1956	24	458	9	II	0	0	writers	Saudi Arabia	/	/

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Chapter 13

The Role of Public Intellectual in the Role-Set of Academics

Ragnvald Kalleberg

The expression “academic as public intellectual” is here used to refer to an academic specialist communicating his or her expert knowledge to “lay persons” outside of the research specialty. “Publics” refer to all those forums in a liberal democracy where serious discussions can take place, for instance connected to mass media, educational institutions and voluntary organizations. The word “academic” has different meanings. If we say that a person with post-secondary education is an “academic”, half and more than half of each new cohort of young people in several OECD-countries are now academics. Here the word academic is used in a much more restricted way, referring to academics employed in higher education and research institutions. The concept of “lay” people is used in a technical way, not primarily referring to those without higher education, but to all outside of a certain research specialty. In practically all specialized knowledge contexts each one of us is a lay person, an unavoidable fact of life in pluralistic knowledge societies characterized by extensive specialization. Specialists in different academic fields, then, are also lay people in relation to each other.

The word “intellectual” has been given many different meanings.¹ Here it is used to designate one of the ordinary roles in the role-set anchored in an academic position, such as a professorship in a research university.² The professor can not only be active in the role of researcher, but also in the role of teacher, of expert (for instance as a medical doctor or a consultant) and intellectual. As intellectual (s)he communicates with outside persons in their roles as citizens, not in their roles as clients. In a research university the ordinary pattern is that most of the time is used for research and teaching, much less time for being a public intellectual.

In order to ground the following analysis of academics as intellectuals in social reality, and to remind the reader how common it is to be a public intellectual, I start with three well known incidents in 2011. They all generated a demand for

1 See for instance Furedi 2004, Etzioni and Bowditsch eds. 2006, Fleck, Hess and Lyon eds. 2009.

2 My analytical framework is located within mainstream sociological role and norm theory, revised and further developed with insights from speech act theory and American pragmatism. See especially Merton 1968: 41–5, 422–38; 1976: 32–64; Habermas 1984: 75–101; 1987: 1–111; Joas 1993: 214–37; Kalleberg 2009; 2010.

specialized knowledge in broad publics and stimulated a large number and wide range of academics in their roles as public intellectuals. On 17 January an Arabic upheaval started in Tunisia, spreading to other Arab nations in the region. On 11 March a terrible earthquake and a tsunami hit Japan. On 22 July, 77 persons were killed by a lone-wolf terrorist in Norway. Mass media around the world massively reported from and commented on these events. Academic specialists of all kinds were interviewed, participated in public discussions and wrote in newspapers and magazines, using their specialized knowledge to inform and enlighten readers and listeners. They analysed and reflected on economic, societal, political, cultural and religious conditions and prospects for the future in the Arab world. They explained the causes and consequences of earthquakes and tsunamis and discussed all kind of aspects connected to the use of atomic energy, and possible alternatives. The Norwegian case generated a large number of contributions on right wing extremism, conspiracy theories, Islamophobia, the role of psychiatrists in the judicial system, psychiatric disorders and criminal responsibility.

These are examples of academics as intellectuals communicating their specialist understanding of facts, explanation of events, criticisms, recommendations and warnings, to broad publics. Obviously, several other categories of people also contributed to these public discourses with information, interpretations and evaluations, such as victims, eyewitnesses, civil servants, workers, ordinary citizens, politicians, experts from industry, military analysts, and journalists.

These events illustrated some typical conditions for academics as public intellectuals. Let us note five of them. He or she must generally be able and willing not only to *translate* scientific and scholarly knowledge to a language understandable for non-specialists (other specialists included), but also to be *short*. In most public discourses the participants will have a few minutes, or a few pages, to convey facts, interpretations and evaluations, and to answer criticism. The public intellectual must understand the larger context of communication in order to be able to make the specialized knowledge *relevant* for outside audiences. The good academic intellectual is able and willing to *communicate*, to answer questions, comments and critique, it is a dialogue, not a monologue. It is not uncommon that academic intellectuals *disagree*. There are not only disagreements between specialists from different disciplines, but also within the same discipline or field, such as specialists on the Middle East, physicists and psychiatrists. In the Norwegian case there were, for instance, intense discussions both between and within professions like law and psychiatry. Such discussions, located in broad public forums, are examples of a kind of extended peer review.

I shall discuss some descriptive-explanatory and normative-evaluative questions about academics as intellectuals, and organize the article in four sections. 1) What do we know about academics as intellectuals? How common is it to engage in such public activity? Are there differences between individuals, disciplines and institutions? There are strong opinions about what are good answers to questions like these. Distressingly often these opinions are based on vague concepts about “intellectuals”, overgeneralizations from specific cases, lack of empirical evidence

and implicit evaluations. I have often met fellow academics, and others, who have insisted that it is an unusual task, and also that it is the less productive scholars that engage in it. But is this a good description of the situation? I shall document that it is not. 2) When did the role of the academic as intellectual emerge? Is it a recent requirement, for instance connected to the PR of scientific institutions in the new media society, or is it an old one? In the contemporary world it is easy to find examples of people, arguing that it is a new role. Such a point of view is incorrect. The role emerged during the European Enlightenment, and was institutionalized in the “bundle university”. 3) There is much confusion and uncertainty concerning the role of intellectual. To get a more clear understanding we have to look at the institutional context. I try to clarify this discussion by arguing that the individual academic as intellectual is communicating in a specific context, a public forum. In such arenas communication is dialogical, not monological. All participants have generally something to learn and to contribute. 4) The article ends with a discussion of some of the challenges we confront when wanting to strengthen the role of academics as intellectuals. We then have to ask critical and constructive questions. The intellectual task has become difficult to develop also because of widespread conceptual misbehavior. The role of intellectual is today often misunderstood, also by academics, as an instrumental PR-task or as consisting in contributions to economic innovation. But the basic purpose of this task is to contribute to enlightened public discourses, an essential element in liberal, democratic societies.

What Do We Know About Academics as Public Intellectuals?

Up to now there have been few representative studies documenting and analyzing “academics as intellectuals” as defined here. We therefore have had little general knowledge about the quantity, quality, and impact of and on academics in this role. One mostly has relied on case studies, on general knowledge about national and institutional traditions, or personal knowledge about a specific research field. In my field – sociology – for instance, it is easy to point to excellent scholars who have also been influential and visible public intellectuals during long periods of time. Some examples are Jürgen Habermas, Pierre Bourdieu, Anthony Giddens, Daniel Bell and Gunnar Myrdal.³ In this article, however, I am interested in how widespread such activity is among ordinary academics.

During the last years we have got some representative studies that make it possible to give better answers to a few general questions about academics as intellectuals. We have a Norwegian series of survey studies covering all tenured faculty in its four traditional, research universities, in Oslo, Bergen, Trondheim and Tromsø. The data material is based on questionnaires sent to all academics working in these institutions. In 2001 that amounted to 3,228 persons. The level

3 On the political economist and sociologist Myrdal, see Eliaeson 2000 and Barber 2007.

of activity as intellectuals has been documented to be stable since 1980. There is a French study based on bibliometric data from 3,659 researchers working in the National Center for Scientific Research (CNRS), the largest agency for basic science in Europe. Thirdly, there is a Norwegian comparative study based on data from a random sample of academics in institutions of higher education (with BA degrees or more) in 13 countries. In the contribution I am using here, a sub-sample focused on full-time academics in research universities, is analysed. It is based on responses from 8,383 full-time academic staff members in the thirteen countries, about the three year period 2005–2007. *The Royal Society* in the UK published a study of science communication in 2006, based on data from a survey covering 1485 academics in higher education institutions, located in the hard sciences, engineering and mathematics.⁴

How many academics have been engaged in communication of scientific knowledge to general audiences, that is, to audiences outside of the disseminator's specialty? Among the French researchers, a little more than half of them had been engaged in such "popularization" in the period 2004–2006. Popularization was defined in a broad way in this study, also including researchers being interviewed on radio, television and in newspapers, giving talks in schools and to organizations, or sitting on panels in public meetings.

The Norwegian study had a much more restrictive conception of this kind of communication ("dissemination", "popularization") than the French one. Only written contributions from the academics themselves were counted, for instance articles in popular science magazines or articles to public discourses in newspapers. In the 3-year period from 1998 to 2000, half of these university academics had published at least one popular article and a third had participated in public discourse. The Norwegian academics published, on average, 2.1 popular science articles during the years 1998–2000, and 1.4 articles to public debate.⁵ The average number of scientific articles was nine, which made the average number of articles for each academic amount to 12.5.

In the comparative study one of the conclusions is that: "Over 90% of the academic staff included in the cross-country comparison had at least one scientific publication (article equivalent) during the three-year period (2005–2007), but only a third published a popular article". Regarding scientific publishing, university academics in the 13 countries published, on average, 0.6 authored books, 0.4 edited books and 7.2 articles in a book or journal. The average number of popular articles was 1.6.⁶

According to these studies between a third and a half of the academics have been active as intellectuals at least once during a 3-year-period. Based on these

4 See Kyvik 2005 on Norway; Jensen and Croissant 2007, Jensen et al. 2008 on France; Bentley and Kyvik 2011 on the comparison of 13 countries; and The Royal Society (2006) on the UK.

5 Kyvik 2005: 299.

6 Bentley and Kyvik 2011:54.

studies I find it reasonable to underline that being an intellectual is a more common activity among ordinary academics than many academics and non-academics tend to assume.

Are there differences between research fields? There are. Both in France and Norway researchers from the social sciences and the humanities were more active in dissemination than researchers from natural science, medicine and technology. The Norwegian survey documented that 70 per cent of the humanists and social scientists had published for a lay public in “contrast to about 50% of the faculty in the natural and medical sciences and technology”.⁷ Technologists and natural scientists published on average 1.1 and 1.4 popular articles during the period, scholars from the humanities 2.9 and social scientists 2.4. University academics in technological fields were the least active disseminators. The same general pattern is documented in the 13 countries in the comparative study. “Academics in the social sciences averaged significantly more popular articles than academics in the natural sciences and technology, but differences with other fields were insignificant”.⁸ The social scientists were on average the most active with 2.7 articles, academics in the technological fields the least active with 1.2 articles. The British results on academics only from the hard sciences fits into this picture, 21 per cent having given a public lecture, 15 per cent written for a non-specialist public during the last year.⁹

Are there differences between individuals? It is well known from many studies, on all kinds of research fields in different countries, that there are large differences in individual scientific productivity. Norwegian studies document that 20 per cent of the scientists and scholars in any discipline produce half of its total scientific and scholarly output.¹⁰ In the recent comparative study it is documented that 18 per cent of the academics published half of the scientific article equivalents.¹¹

The productivity pattern is even more skewed with regard to dissemination. 5 per cent of the CNSR researchers authored 30 per cent of this output.¹² In Norway half of the popular science articles were published by 6 per cent of the faculty and 4 per cent of the staff members produced half of the contributions to public discourses.¹³ In the comparative study it is shown that “half of all popular articles were published by 3% of all academic staff, with the top 1% producing 31% of all popular articles”.¹⁴

7 Kyvik 2005:299.

8 Bentley and Kyvik 2011: 55.

9 The Royal Society (2006): 26. This study also illustrates the importance of concepts for the counting. As much as 74 per cent of these British scientists had participated “in at least one science communication or public engagement activity” (p. 10). This concept also included all kinds of talks, for instance to science journalists, local journalists and school children.

10 Kyvik 1991: 102.

11 Bentley and Kyvik 2011: 55.

12 Jensen and Croissant 2007: 3–4.

13 Kyvik 2005: 299.

14 Bentley and Kyvik 2011: 55.

There are also sizeable differences between countries. “On average, academics in Hong Kong reported the greatest number of popular articles in newspapers and magazines (2.3), followed by Germany (2.0), Norway (2.0) and Argentina (1.9)”.¹⁵ On average each academic produced 1.6 such articles during the 3-year period, and 35 per cent of them contributed with at least one. We can summarize central findings about the 13 countries in the following simple ranking, with number of articles to outside publics and proportion of university academics with at least one article within parenthesis: Hong Kong (2.3, 34 per cent), Norway (2.0, 40 per cent), Germany (2.0, 35 per cent), Argentine (1.9, 49 per cent), Italy (1.8, 28 per cent), Brazil (1.7, 37 per cent), Mexico (1.7, 37 per cent), Finland (1.5, 41 per cent), Australia (1.4, 31 per cent), Canada (1.4, 42 per cent), USA 1.2, 30 per cent), UK (0.7, 26 per cent), Malaysia (0.7, 35 per cent).¹⁶

One of the results from the qualitative part of the British study, was the hypothesis that “public engagement activity was seen by peers as bad...” for the career of a scientist. It is, however, interesting to note that only one fifth of the British scientists agreed with the following claim: “Scientists who communicate a lot are not well regarded by other scientists”. Half of them disagreed.¹⁷

A further message that emerged from some of those interviewed in the British study, was that public engagement was done by those who were ‘not good enough for an academic career...’¹⁸ A version of this belief has been articulated in this theorem: “one’s popularity and celebrity with the general public ... [is] inversely proportional to the quantity and quality of real science being done”.¹⁹ The phenomenon is sometimes called the “Sagan effect”, named after the American astronomer Carl Sagan. Based on numerous interviews with insiders it has been documented that the fact that Sagan did not get a tenured position at Harvard, and was not accepted as a member of the National Academy of Science, has to be explained with reference to this theorem. However, Shermer has convincingly shown that the negative evaluation of Sagan was unsupported by facts. “Throughout his career, that began in 1957 and ended in December 1996 ... Sagan averaged a scientific peer-reviewed paper per month. The ‘Sagan Effect’, at least when applied to Sagan himself, is Chimera”.²⁰

This fits well with the general picture that has been documented with the help of the survey studies. None of the studies confirms a “Sagan effect”. In the French study the researchers “find exactly the opposite correlation: scientists engaged with society are more active than the average”. After controlling the correlation they state that “widely disseminating scientists are always, on average, academically

15 Bentley and Kyvik 2011: 55.

16 The figures are taken from Kyvik 2011.

17 The Royal Society 2006: 11, 14, 26.

18 The Royal Society 2006: 11.

19 Shermer 2002: 490.

20 Shermer 2002: 490, 493.

more active than the inactive ones”.²¹ In the comparison of 13 countries, the general finding is the same: “Academics participating in popular publishing averaged significantly more article equivalents (13.5) than those without any popular articles (9.5). The difference between these groups is significant within all countries except the USA, where it is positive but statistically insignificant”.²² There is the same general conclusion to be drawn from the Norwegian studies. The academics active in dissemination of knowledge and insight to a broader audience are the most productive in scientific publishing. One of the findings is that those “who publish popular science articles had 43 per cent more scientific and scholarly publicationsthan those who did not publish for a lay public”.²³

The Historical Emergence of the Intellectual Role within the Bundle University

The academic role of being an intellectual is old, embedded in the new type of university that emerged in Europe during the Age of Enlightenment. We often label this type of university the Humboldt University, referring to the German linguist and philosopher Wilhelm von Humboldt and the new research university he helped to establish in Berlin in 1810. Those principles and practices later influenced universities all around the world. In an excellent historical-comparative study of the German, French, British, US-American and Japanese university systems, the author underlines the importance of the German innovations two centuries ago: “In the eight centuries of university life in the Western world...no other change compares with the emergence and development of the modern research university”.²⁴

The Berlin reformers conceptualized the desirable type of university not only as a research university, but as a university where research was intertwined – bundled together – with other tasks. They spoke about four bundles (*Einheiten*, unities) in a modern university; the unity of scholarly and scientific activity with 1) teaching and study (*Einheit von Forschung und Lehre*), 2) character formation (*Bildung durch Wissenschaft*), 3) public enlightenment (*Einheit von Forschung und Aufklärung*), and 4) the unity of all scientific and scholarly work (*Einheit der Wissenschaften*). They were sure that the learning going on within a specific context could stimulate learning in other contexts, if adequately connected to each other. Consequently they insisted on the importance of designing and developing universities as bundle institutions, institutions with many tasks in combination with each other.

The German word for the intellectual task – the communication of specialized knowledge and insight to outsiders – was *Aufklärung*. The most influential

21 Jensen et al. 2008: 530–31.

22 Bentley and Kyvik 2011: 56.

23 Kyvik 2005: 304.

24 Clark 1985:1–2.

conceptualization of the idea of *Aufklärung* was not, however, formulated by the Berlin university reformers – such as Humboldt and the theologian Friedrich Schleiermacher – but by an academic in the generation before them, the philosopher and social theorist Immanuel Kant, located at the university in Königsberg. In several contributions, his great work on the *Critique of Pure Reason* included, Kant argued that academics (also) had an *exoteric* obligation, namely to communicate knowledge and insights from their specialized *esoteric* disciplines to a broader public outside of the specialized research communities.²⁵

The traditional Western research university as we know it from the last two centuries, is often called the “Humboldt-University”. This neologism from the first part of the 20th century, a century after the establishment of the university in Berlin, is historically problematic. Humboldt’s ambitions were never fully institutionalized in Berlin, not even during a short, early period of the new university’s existence.²⁶ Actually there were other and older German institutions that were more important in this respect, such as the universities in Halle and Göttingen.

The label “Humboldt-University” can also evoke a too close connection between the new Enlightenment universities and German neo-humanism. There were, however, similar reform tendencies all over the Northwestern part of Europe at the time. Scandinavia is an example of these developments outside of the German speaking world. The towering academic figure in Denmark-Norway during the first half of the 18th century was a Norwegian-born professor at the University of Copenhagen, (1684–1754). If we use contemporary terminology, Holberg was a historically-oriented sociologist and political scientist.²⁷ Holberg, like the Berlin reformers two generations afterwards, insisted on the importance of designing and developing bundle universities, multi-task organizations. He underlined the importance of having both research and teaching as main activities within universities, not locating them in separate institutions. He insisted on the instrumental importance of science – for instance in order to increase productivity in agriculture – and argued for the obligation to contribute to *opplysning*, the Scandinavian equivalent to German *Aufklärung*, the British *enlightenment* and the French *lumières*. With regard to public enlightenment he was himself a master, not only because his excellent scholarly prose, but also because he wrote comedies, novels and essays conveying the same ideas as in his scholarly contributions. One of his main themes was criticism of the discrimination against women in Europe, which he argued was both morally wrong and stimulated inefficient use of the resources in society. More than any other individual academic in Scandinavia he institutionalized the role of the academic as public intellectual. With regard to the type of universities characteristic of Denmark and Norway, and Holberg’s huge

25 See Habermas 1989: 102–17, Kalleberg 2008: 20–1.

26 See Palatschek 2002.

27 On Holberg, see Kalleberg 2008: 23–9.

influence, it would be more accurate to speak about the “Holberg university” than the “Humboldt university”.

Similar developments can easily be identified in the USA during the 19th century, for instance in connection with the establishment of the land grant universities from the 1860s. Experiences from Germany influenced the redesign of American higher education during the second half of the 19th century, for example represented by Johns Hopkins University in Baltimore in the 1870s, and the new University of Chicago from the 1890s.²⁸

A more general and appropriate label than the Humboldt university for the modern universities developing during the 18th and 19th centuries, could be *The Society Oriented Enlightenment University*. These universities were never isolated ivory towers, as a myth will have it. That also holds true for the new Berlin university in 1810. In practice it remained primarily an institution for the education of useful and obedient Prussian civil servants during the 19th century. The universities were useful for the surrounding society both with regard to the education of professionals (for instance lawyers, priests and medical doctors), and with regard to cultural development (such as historians, philologists and teachers). Generally the professional task was the primary one during the 19th century. It was first in the 20th century that research became the primary activity, and then only in an elite sector of higher education.²⁹

The American University by Parsons and Platt, is in my view the single best contribution on the contemporary Enlightenment University in its most impressive form. It is a study based on survey and case material from the leading US research universities. Here, and in several smaller contributions, Parsons insisted that universities should be designed and developed as multi-task, bundle institutions. He focused on four types of activities and their intellectual and institutional interplay: 1) research in graduate departments, 2) teaching, study and personality development in colleges, 3) education of experts and expert knowledge in professional schools and 4) an “intellectual” task. The intellectual task consisted of contributions with scholarly knowledge and insight to the public definitions of situations: “...bring cognitive resources to bear on problems of the cultural definition of the situation for the society...”³⁰

During a fateful period for Western Civilization Parsons contributed as such a public intellectual, warning the American public at an early stage about the emerging

28 Clark 1995: Ch. 4.

29 Less than 10 per cent of the institutions of higher education in Europe and North America have today a “significant research capacity” (Olsen 2007: 40).

30 Parsons and Platt 1973: 267. On the intellectual function, see Chapter 6 on “The University and the “intellectuals””. See also Parsons 1978. Talcott Parsons (1902–1979) is today unknown outside of sociology, and largely neglected also by sociologists. He was nevertheless a towering figure in 20th century sociology, and his work is of enduring interest. On his historical importance and contemporary relevance, see Habermas (1987: 199–201), and Joas and Knöbl (2009: Chs. 2, 3, 4).

danger of the German dictatorship developing after 1933. In the period 1938 to 1945 he contributed in the struggles against Nazi Germany. He did research on Germany and warned the general public about the anti-democratic threat represented by Hitler's Germany. He "wrote a large number of radio broadcasts, lectures, speeches, and memoranda for various occasions and associations". He was also an influential expert giving advice to political authorities which also included designing strategies for the democratic re-education of Germans after the war.³¹

Traditional research universities of the European and North-American type are responsible for four types of important output to society: 1) scientific publications, 2) educated students, 3) translation of scientific insights into common culture, and contributions to democratic discourses, and 4) expert contributions, such as from doctors, engineers and economists. On the level of the individual academic, these four institutional tasks are performed in a fourfold role set. Idealtypically, the academic in research universities is researcher, teacher, public intellectual and expert.³²

There are also good reasons for identifying a fifth type of disciplinary task, despite the fact that it does not result in end-products for the surrounding society. This task has to do with institutional governance, the responsibility for the design, redesign and development of adequately-functioning academic institutions. Such work is the task of all kinds of academic leaders, for instance Department Chairs and Rectors, and is a requirement for good functioning of research, teaching, dissemination and expert activity. Such academic leadership is and should also be practiced as a disciplinary activity. Maintenance and development of well-functioning academic institutions cannot be left to pure administrators and managers with little or no understanding of scholarly contents and basic academic activities in the relevant scholarly field(s). A strong tendency in the market democracies (that is, the OECD countries) during the last quarter of a century, has been to downplay the specific requirements from science-based, collegial institutions such as universities, hospitals and schools. An unbalanced belief in coordination, primarily with the help of market like inducements and hierarchical control, is widespread. In the longer run that will undermine the quality and productivity of such professional institutions. Universities, hospitals and schools are of course organizations with hierarchies, have to control their spending, and have to adjust to different types of markets. But they are primarily collegial institutions: The challenge is to find an appropriate and productive balance between hierarchies, markets and argumentation.³³

My main arguments in this section have to be idealtypical. Europe alone has almost 4,000 institutions of higher education, characterized by all kinds of differences in histories and tasks. The majority of institutions, for instance, are not

31 See Gerhardt ed. 2002. Quote from Gerhardt p. 61. See also Gerhardt 2012. Parsons studied in Heidelberg in the 1920s and was deeply influenced by German sociology, especially Max Weber.

32 Kalleberg 2005: 388, Kalleberg 2008: 22–3.

33 See Kalleberg 2011: 93–97, 117

primarily research universities, but teaching institutions. Nevertheless, knowledge and insight from research remains the primary element in all academic disciplines, the knowledge base necessary for the other tasks to be performed. When focusing on individual academics in research universities we may also ask if the different tasks typically characterize specific phases of the academic life course, for instance with more dissemination and administration at later stages of the academic life course.

Reciprocal Learning in Publics

The academic task focused in this article is conceptualized in a twofold way, as *dissemination (popularization)* and *public-discourse interventions* intended to stimulate enlightened public opinion. The task of the academic in this role is to contribute with scientific and scholarly insight to the definition of situations, for instance concerning atomic energy, political upheavals and right wing terrorism. Within what kind of forums does such communication take place? The general answer presented here is that it takes place in public. But what kind of social and cultural setting is “public”? Which are the relevant values, norms and social roles stimulating and regulating interaction in such contexts?

The primary task for academics as intellectuals, as disseminators and debaters, is not to sell something, not one-sidedly to lobby for a cause, and not to make one’s own field visible. Their role partners in interaction are not customers, clients or other users, but citizens. As Habermas observes about roles in such public arenas, in an analysis of the deterioration of quality newspapers where the institutional balance between cathedral and market is tipping in favor of the market: “Radio and television audiences are not just consumers, that is market participants, but also citizens with a right to participate in culture, observe political events and form their own opinion”.³⁴

The context of an academic intellectual is a public forum, such as a magazine read by the general public, a newspaper with sections for serious public discussions, a public broadcasting radio or TV-channel, a broader forum at a university open to contributors from different disciplines and institutions, an association in civil society, an open forum at a school or a serious discussion site on the internet. Academics as public intellectuals communicate in forums where arguments are the essential mechanism for the formation of descriptive, explanatory and normative opinions. Opinion formation in well-functioning, open forums, publics, is primarily coordinated by the force of better arguments. As far as I know, and narrowing down to the three events mentioned in the introduction, citizens around the world have had a massive experience of reasonably well-functioning publics concerning atomic energy, upheavals in the Arab world and right wing terrorism.

There are regularly some who claim that this understanding of publics is far too idealistic, too good for the capitalist-democratic world of profit, propaganda,

34 Habermas 2009: 133.

prejudice and power. Obviously, these are real phenomena. But well-functioning public forums are also real phenomena. Such critics regularly overgeneralize their point of view, implicitly presupposing that everything in the last instance is decided by power struggles. Inflated arguments about power can drive such critics into self-refuting positions, as they at the same time generally presuppose that their own opinions are true and reasonable. But how should that be possible in the dismal world they imagine? Why is it the case that they are the only ones with access to valid insights, free from illegitimate power? And if they themselves also only use power, as all others, why should we bother listening to them and each other?³⁵

Public forums more-or-less strongly regulated by the power of the better arguments, have existed for a long time in human history. Scientific communities are the best examples on the institutional level. They have existed in the real socio-cultural world for at least four centuries. Well-functioning scientific communities are argumentation collectives where only intersubjective reasons, such as documentation and explanation of socio-cultural phenomena, are allowed to influence the formation of opinions. If they do not, for instance because of political, economic or religious interference, that is then generally regarded as perversions – and rightly so. Well-functioning scientific communities are argumentation collectives, based both on an understanding of human fallibility and that the best we have to improve knowledge and insight, is free, open and rational discussion. In such collectives participants do not only have rights to free expression, but also duties to respond to criticism with cogent arguments.³⁶

More-or-less well-functioning publics do also exist in other parts of civil society, not only in scientific communities. Free and open discussions in order to develop an enlightened understanding through reciprocal learning are essential, such as among citizens in all kinds of forums in civil society or among members in organizations. In his procedural understanding of a democratic process Dahl underlines that members of the democratic *demos* should have the chance to gain “enlightened understanding”, and insists on the importance of “inquiry, discussion, and deliberation”.³⁷ The importance of discussions among free, equal and reasonable members of *demos* has become a central theme in much democratic theory, often under the heading of “deliberative democracy”.³⁸ The

35 On performative contradictions, see Kalleberg 2007: 151–2.

36 Kalleberg 2007, Kalleberg 2010. This general insight was essential in the recent revision (2004) of the article on freedom of expression in the Norwegian constitution (see Kalleberg 2013). In his modern classic Habermas (1989/1962) underestimated the centrality of the new scientific and scholarly communities as ideal types of the public forums emerging in the early modern period.

37 Dahl 1998: 39.

38 See for instance Elster ed. 1998, Parkinson and Mansbridge eds., 2012. Elster notes (p. 1) that deliberation is as old as democracy itself, referring to Pericles in the fifth century B.C.E. See also Dahl 1989: 17–18.

aim is to stimulate enlightened public understanding and contribute to solution-oriented democratic discourses.

To ensure that well-founded opinions emerge in public discourse it is crucial that opinions can be corrected and modified in confrontation with the opinions of others, similar to what goes on in scientific publics.³⁹ The general norms of argumentation in scientific communities can also be at work in democratic forums. Naturally, deliberation in science and democratic institutions takes place under different conditions. Parliaments and political-administrative institutions are not only discussion forums they are representative bodies responsible for acting on behalf of their members. Decisions based on the majority principle are legitimate in democratic systems, as an adjustment to the requirement of political equality and to the need for finishing discussions in order to implement laws and political decisions. To use the principle of majority in science in the same way would be irrational and illegitimate.

In a well-functioning public forum communication is dialogical, learning is reciprocal. That this is real in well-functioning scientific communities, where people are acting in the role of scientist, is a well-established insight within sociology of science.⁴⁰ Similar mechanisms are at work in the interplay of academic intellectuals and citizens as their interaction partners. The survey studies presented in the first section documented that the most academically productive researchers are also those most active in communication with audiences outside of their field. How can we *explain* this state of affairs? My hypothesis is that argumentative influence has flown in two directions. Academics intellectuals have also received impulses that have stimulated their scholarly productivity. They learn from framing their points of view for new audiences, from explicating the obvious, which perhaps was not that obvious anyway, from listening to unexpected questions, from eye witnesses, or from defending our own views against criticism.

It is not difficult to find examples of how academics learn from participating in public debates, for instance about difficult topics such as immigration, the quality of schools, how to counteract the tendencies to overweight and obesity in modern societies or discussions about climate change. The French study supports such an explanation, showing that research and dissemination are “mutually reinforcing”. The concept of dissemination in their study is used to denote not only popularization, but also teaching and industrial collaboration. They sum up a basic insight in this way: “Dissemination activities compel scientists to open up their horizon, to discuss with people having other points of view on their research topics, giving new insights, contacts, which could improve their academic research”.⁴¹

Researchers’ learning from dissemination and public debate is similar to learning from teaching (on all levels), participation in seminars and supervision of theses on the master and PhD level included. In several studies Burton Clark

39 Kalleberg 2010. See also Dahl 1989: 135–52.

40 Kalleberg 2007.

41 Jensen et al. 2008: 536.

did much to explain the excellence of the American University during the 20th century. He claimed that the close connection between basic research, teaching and study on all levels in the American research university, was an essential factor in the explanation of the success of these universities. Learning travels in both directions, from professors to students, from students to professors.⁴²

Learning in the role of teacher communicating with students is a common experience. One of the leading contemporary theorists of democracy, Robert Dahl, has been a teacher at Yale University for more than half a century. In the acknowledgements to his great book *Democracy and Its Critics*, he tells the reader that he has learned from his students, from the bachelor level to the PhD level: “They have stimulated me to think afresh about old problems, compelled me to deepen and clarify my ideas, and by no means infrequently have provided me with new insights”.⁴³

The American political scientist Donald Stokes articulated a similar kind of argument with regard to the stimulating relationship between research and practical developments, for example solving problems in business enterprises, developing new processes and products. Stokes chose Louis Pasteur to symbolize such mutual processes of learning in “use-inspired basic research”: “No one can doubt that Pasteur sought a fundamental understanding of the process of disease... But there is also no doubt that he sought this understanding or reach the applied goals of preventing spoilage in vinegar, beer, wine, and milk and conquering *flacherie* in silkworms, anthrax in sheep and cattle, cholera in chickens, and rabies in animals and humans”.⁴⁴

The shared learning processes between research, teaching, dissemination and expert activities confirm those theorists – from Holberg and Humboldt to Parsons and Habermas – that insist on the productivity of bundling of functions in universities and other research institutions. But these complex learning processes are still poorly documented and not well analysed. We will not only need more quantitative studies in this field, but also detailed case studies documenting and analyzing what actually goes on in the different settings of inquiry and learning.

Academic Intellectuals Contributing to Democratic and Cultural Sustainability

Cultural and social sciences are both descriptive and normative disciplines. Consequently, in these disciplines there are two basic types of legitimate research questions: descriptive-explanatory and normative-evaluative. Descriptive and

42 See Clark 1995, Chs. 2, 7, 8.

43 Dahl 1989: viii. Among the most important things he has got from his students, are the unexpected and illuminating questions (personal communication). This is a basic form of learning, as the research question is the most important element in a research design.

44 Stokes 1997: 12.

normative questions can be combined and be the basis for a third type of legitimate research questions, the constructive ones. Their elementary form is like this: What can and should the actors do to improve their situation? The words "should" and "improve" refers to the normative dimension, to what is desirable. The word "can" refers to the descriptive dimension and concerns what is feasible.⁴⁵

Social and cultural sciences are then not only descriptive-explanatory, and not only critical, they are also constructive sciences. Applied to this article we may for instance ask how common it is for an academic to be a public intellectual, as in the survey studies used here. We may also ask explanatory questions about how to explain differences between individuals, disciplines, institutions and nations, or changes over time. What is documented can also be evaluated, based on explicated value standards, for instance related to the quality of dissemination, or to its effects on scientific literacy in a community, and on the rationality of public discourses. And we can ask constructive questions about what to do with problems, how to improve existing conditions.

Contemporary threats to processes of enlightened public understanding are depressingly familiar in the OECD countries. Public space is increasingly filled with advertisements, entertainment, irrelevant news about media celebrities, infotainment, spectacular accidents, and sex. Communicating in public space is increasingly defined by communication strategies of organized interests, using professional lobbying and PR agencies. Such tendencies undermine the chances for enlightened public formation of opinions about what is actually taking place in nature and society and about what we should do as democratic collectives. Instrumental power strategies and commercialized culture undermine cultural and democratic sustainability.⁴⁶

The main strategy to counteract such tendencies is to maintain and develop publics and to contribute to a more adequate balance between markets, hierarchies and institutions in civil society. In our context, a constructive research question could be this one: What can and should academics and academic institutions do – given traditions and resources – to improve the task and role of public intellectuals? Contemporary European higher education seems primarily to be planned so as to stimulate economic growth and competitiveness. According to a recent study, reform documents within the EU "give little attention to the possible role of universities in developing democratic citizens, a humanistic culture, social cohesion and solidarity, and a vivid public sphere".⁴⁷ In order to change such a

45 On this conception of social science, see Kalleberg 2009: 262–66 and further references there.

46 Such criticism is not a repetition of the too pessimistic evaluation by Habermas in 1962, exaggerating linear developments from culture-debating to culture-consuming publics (Habermas 1989/1962). It is worth noting his self-critical revisions three decades later (Habermas 1992: 430–41). For an incisive discussion of recent literature on political communication in media society, see Habermas 2009: 138–83 and the references given there.

47 Olsen and Maassen 2007: 9.

situation, a new definition of the situation has to be agreed on, and it has to be acted on that basis.

It has also become difficult to agree on a reasonably clear definition of the situation because of widespread conceptual misbehavior in academic communities. We have seen in the foregoing sections that Norway is one of the countries where the role of academics as public intellectuals is widely practiced, and where there are strong traditions going back to the Age of Enlightenment. During the last 10–15 years there nevertheless has been much terminological and conceptual confusion concerning the character and purpose of the intellectual task. In an earlier article I documented this confusion by analyzing relevant Norwegian texts, especially from internal hearings at the University of Oslo and documents from the national level, such as official White Papers about higher education.⁴⁸ Later, internal discussions at other leading institutions and reports on dissemination from *The Norwegian Agency for Quality Assurance in Education* (NOKUT), not being able to distinguish between economic growth and public enlightenment, testifies to widespread conceptual confusion. This is worrisome because it facilitates a distorted understanding of the situation, making it difficult to change it in desirable ways.

Let me illustrate. In Norwegian law, public documents and institutional guidelines *the task focused in this article*, are named *forskningsformidling*. A direct translation into English would be “dissemination of research”, a German equivalent would be *Forschungsvermittlung*. When analyzing these different debates and texts, it becomes clear that this one word (*forskningsformidling*) refers to at least the following seven different concepts (ideas), and consequently to seven different tasks, practices and end results: 1) scholarly work resulting in peer-reviewed scientific publications; 2) teaching and supervision, resulting in graduates at different levels (“teaching is the most important type of dissemination offered by this institution”); 3) popularization of scientific knowledge and participation in public discourses with scholarly knowledge, resulting in improved cultural and political literacy; 4) expert or professional activity resulting in improvements for a client, such as advising an enterprise on more efficient leadership or improved medical treatment; 5) PR activity which is supposed to result in improved visibility, status and more money to the institution; 6) commercialization of scientific knowledge resulting in new processes, products and services, and hopefully new income for the institution; and 7) academics participating in political roles, for instance as members of social movements or participants in local community development.

To my knowledge, such confusion is also typical of contemporary discussions in other OECD nations. In the USA, for instance, it is commonly distinguished between three tasks for research universities: research, teaching and service. The understanding of “service” seems to have moved in the direction of PR for institutions, and commercialization of scientific knowledge. The American and international debates about “public sociology” are hampered by some of the same

48 Kalleberg 2004.

confusions.⁴⁹ When reading the French study presented earlier in this article, one gets the impression that they also struggle with similar problems: “Motivations [for popularization] provided by the researchers are numerous: the yearning to inform the public, to make one’s field known and encourage students to take up science, or the need to account to civil society for the funds provided for the laboratories”.⁵⁰ Naturally, it is not a problem that practices and projects fulfill several tasks, the problem is to misconceive the nature of an essential task.

Higher education is a basic cultural institution in civil society. It should be cultivated and developed as that, and not only or primarily as essential for the labor market and economic growth. The knowledge base of primary and secondary schools, the most influential cultural institutions in liberal democracies, are provided by the scientific and scholarly disciplines. The system of higher education could play a more important role in the public realm than is generally the case today, by cultivating the role of academics as public intellectuals and develop an adequate institutional infrastructure for that task. This would have to take place in cooperation with other institutions in civil society, such as schools, voluntary organizations and mass media. In a constructive perspective it can be noted that we have to do with large institutional complexes. If we just focus on the system of higher education, we have to do with an enormous system. In 2006 it was estimated that Europe with its almost 4,000 institutions, had more than 17 million students and 1.5 million staff.⁵¹ Many academics, political authorities and the public at large seem today to have forgotten this huge democratic and cultural potential.⁵²

If we focus on what it is possible and desirable to do within nation states, we have to answer by pointing to measures on different levels, such as the national, institutional and the individual ones. To avoid being too abstract I illustrate my argument with some Norwegian practices and developments.

Democratic and cultural sustainability require a desirable balance between the main institutions and institutional spheres of a modern society. Articulated on a general level, the nation states within the OECD-world are built around three societal orders – the market, the democratic state and civil society. Each one of these orders can become too dominant, and each one can distort the others. It is easy to point to examples of dysfunctional and illegitimate institutional imperialism, as when political authorities regulate what should be regulated by well-functioning markets, when market values and mechanisms distort culture and democratic deliberation, or when fundamentalist religious conceptions distort teaching of biology in schools.

49 See for instance Kalleberg 2005; and other contributions in the same issue of *The British Journal of Sociology*.

50 Jensen et al. 2008: 527.

51 Olsen and Maassen 2007: 3.

52 For a detailed argument, see Kalleberg 2011.

Constitutions should regulate essential relationships between the great institutional orders in a liberal democracy. The Norwegian constitution is the second oldest written constitution in the world still in use, installed in 1814.

Only the American constitution is older. Constitutional conservatism is strong both in the US and in Norway, but different to the American situation, the Norwegian constitution can be changed, not only by amendments. In 2004 the Norwegian Parliament accepted a new, revised article on freedom of expression, based on a proposal first presented in a public report.⁵³ There are several interesting elements in the six paragraphs of this new article, but the most relevant concerns the maintenance and development of an adequate institutional infrastructure for cultural and democratic sustainability. In the sixth and last paragraph it is demanded that: "It is the responsibility of the authorities of the State to create conditions that facilitate open and enlightened public discourse". This states the responsibility of the democratic state to ensure that individuals and groups are actually given opportunities to express their opinions. State subsidizing of the mass media, the public funding of schools and universities, public support of the arts, non-governmental organizations, and support for Norwegian and minority languages, are examples of such infrastructural support.⁵⁴

Both the institutional task of the dissemination of research and the tasks of individual academics as public intellectuals are located in this broader picture of cultural enlightenment and deliberative democracy. In the Norwegian context, institutions of higher education are regulated by a separate law. In the first article of this law it is stated that it is an institutional obligation to disseminate scientific and scholarly knowledge and facilitate participation for their staff and students so that they can contribute to public discourses with their specialized knowledge.⁵⁵ This kind of legal requirement first became part of Norwegian law during the first years after World War II. It was not, however, an invention of lawgivers, but basically a formalization of academic customs and practices stretching back to the Age of Enlightenment.⁵⁶

The connection between the new constitutional article on freedom of expression is explicitly mentioned in the Norwegian national guidelines for research ethics.⁵⁷ These guidelines have been produced within a national system of self-regulation of research, not imposed by political-administrative authorities. The guidelines were formulated by The National Committee for Research Ethics in the Social Sciences and the Humanities (NESH). The committee consists of researchers from the relevant disciplines and organizes national hearings when revising guidelines, and extensively uses its experience from the evaluation of concrete, difficult cases. The guidelines

53 See NOU 1999: 27/2005. For an English translation of the constitution, see http://www.servat.unibe.ch/icl/no00000_.html

54 In Kalleberg 2013 I discuss this constitutional article and the prospects it opens up.

55 See § 1.3 in the law. See <http://www.lovdato.no/all/tl-20050401-015-002.html#1-1>.

56 See Kalleberg 2008.

57 NESH 2006, guideline 43. The internet site contains an English translation of the guidelines.

are well known and have a high legitimacy among Norwegian researchers, basically – I think – because they are developed by experienced researchers. They are widely used also in the education of students, especially at the master and PhD levels. In this way a workable connection is made, relating constitutional requirements with the daily work of individual academics and students.

Guidelines can be interpreted as answers to constructive questions, giving advice of what to do or not to do. Within the context of this article it is interesting to note that six of the 47 Norwegian guidelines are related to academics as public intellectuals. In Guideline 42 it is stated that individuals and institutions contribute to popularization and public discourse. The reason is stated like this: “contribute to the maintenance and development of cultural traditions, to the informed formation of public opinion and to the dissemination of socially relevant knowledge”.

The most pressing problems and challenges facing us today can obviously not be intellectually mastered by only one discipline, and mastering of them also requires a high degree of democratic consensus. In order to understand the complexity of production in energy, democratization of the Arab world or different sorts of terrorism, insights from several disciplines have to be integrated. Universities can establish programs and arenas where specialists from different disciplines meet in order to document and analyse such problems. Interdisciplinary arenas are also important as a way of both cultivating scientific humility (understanding one’s own discipline’s intellectual limits) and creating a type of extended peer-review, also bringing in people with relevant experiences from other disciplines and other sectors of society.

Some have argued that as the general level of education is rising in a population, dissemination of research becomes less and less important. This view is generally based on the inadequate assumption that popularization of research is only meant for that segment of the population without higher education. When we think about democratic sustainability and the rationality of public discourses in contemporary societies, this view is utterly inadequate. All of us, including all specialized researchers, are also members of a democratic public with a constant need for mutual popularization. Multi-disciplinary forums, for instance organized by universities, offer a good platform for qualified, problem-oriented communication and mutual learning between specialists from different disciplines and for communication with a broader audience outside of the institutions.

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Chapter 14

Critics as Cultural Intermediaries

Thomas Crosbie and Jonathan Roberge

Introduction

We social scientists are still strangers to this digital age. Nowhere is this more evident than in our confusion and hesitancy concerning the impact of new media technology on public deliberation. Our professional standards and routines were fashioned in the age of print. We deliberate among ourselves through the formalized language and specialist techniques of the article, presentation and monograph. This aids us in ensuring a continuity and cumulative progression in our intellectual labor, but comes with a cost. We become less connected to the lay public to the point that we risk becoming culturally and politically irrelevant.

This rather uncomfortable position can be recognized from the impulse behind *Democracy in America*, Alexis de Tocqueville's most brilliant and important text (2000 [1835]). For better or worse, we are in yet another Tocquevillian moment. When he set out on his travels more than 150 years ago, he took on the role of a social scientist who would pass impartial judgment on the relative merits of two deliberative traditions. The first is implicit: the deliberative culture and structures of revolutionary France, still characterized by the specialist and formalist logics of the displaced aristocracy. The second is the new American model of a broad, heterogeneous polity. De Tocqueville's main concern was that "half-baked ideas" (*notions imparfaites*) would cheapen the deliberative process, posing structural and political threats (for example the election of a tyrant) as well as cultural threats (a coarsening in the quality of debate) (de Tocqueville 2000, 709). Albeit not identical or as subtle, it is this same concern that animates much of today's discussion regarding the demise of the public intellectual. What is offered is an often predictable narrative of decline that both idealizes the past – the time of Geniuses, Great Works and Grand Narratives – and is highly pessimistic for the future. People like Russell Jacoby, Éric Lott or Richard Posner, for instance, made names for themselves advancing such arguments (Jacoby 1987; Lott 2009; Posner 2004). In a similar fashion, many have predicted the "crisis of criticism", if not more simply its death (Berger 1998; Culler 1987; McDonald 2007). It is time to follow de Tocqueville's lead and consider whether these crises and supposed death knells are instead new and equally valid deliberative arrangements. Although definitely discomfiting and unfamiliar to us, these arrangements may carry their own enriching and democratizing potential.

Indeed, our research indicates that something quite different from a decline is occurring, something far more complex and puzzling. We view this as the rise of a new model of intellectuals, one based on the traditional roles critic and cultural intermediary. There is currently a shift, in other words, from intellectuals who were generalist experts and authority figures to critics who engage at smaller scales a vastly broader public that deliberates at ever finer-grained levels. Today, the field of public reasoning – so to speak – is being shaped and reshaped by the increasing balkanization of deliberative forums.

Across these fields, we repeatedly see the common trait of individuals adopting the role of critic. Like the older model of the authoritative, generalist intellectual, these critics endlessly make pronouncements, discuss and interpret possibilities, and propose alternatives related to the issues at hand. In short, they are producers of meaning.¹ However, unlike the older model, these critics get their hands dirty, occupying prominent roles within the inner communication of the given issue. They thus take part in the constant evolution of their particular sphere. As the grain becomes finer, the subject position of the commentator frequently switches from outside to inside.

In the sociological literature, there are two main understandings of cultural intermediaries. The broader understanding encompasses anyone involved in the transmission of a work of art (Bourdieu and Nice 1980; Becker 2008 [1973]). A second tradition more narrowly defines cultural intermediaries as those involved in the economic impact of the cultural product (Negus 2002; Wright 2005). Here, we would build on these and argue that cultural intermediaries are also, if not more so, engaged in symbolical transactions. Following Valentin Cornejo (2008), they would be best described as cultural mediators. The intellectuals we study are more and more complexly involved in the production and reception of culture. They blur the line between producer and consumer, as the rise of the “prosumer”, discussed below, represents. Differently put, intellectuals as critics and cultural mediators do not only transmit information, but also translate and encode it.

If the deliberative structures that surround cultural production and reception are changing, are we now finally transitioning out of a public sphere, in the sense of a domain of deliberation ruled by a common rationality? The answer, of course, depends on the model being used. Habermas (1989 [1962]), despite the fact that he acknowledged the importance of cultural criticism in the early stages of his argument, ends up contrasting reason and emotion; as a consequence, he diminishes the significance of all deliberation that lies outside the overtly political realm. By looking at intellectuals as critics and cultural intermediaries, we come to an entirely different conclusion. Discussions necessarily evolve between culture and politics, in cultural power struggle or in what Hesmondhalgh calls the “politics of aesthetics” (Hesmondhalgh 2007).

1 In this broad sense, we are following Ron Eyerman’s proposal “to view the intellectual as part of an historical process in which human actors reinvent cultural tradition in different context” (1994: 4).

The public sphere, from this perspective, could be better understood as an “aesthetic public sphere” (Jacobs 2007; Jones 2007; Roberge 2011). Democratic deliberation – the lifeblood of the democratic state – can be redefined as deliberation that deals with any production, artifact, trope or symbol whose publicity is sufficient to permit articulated dissent and advocacy. Nothing is too trivial or sacred not to be the object of criticism; today, we see spirited discussion over matters of surpassing triviality, but these discussions are threads in the tapestry of democratic deliberation. Political orientations are woven from such threads.

The rebuttal, of course, is that not all deliberative acts are created equal. Some forums are more powerful than others, some voices speak more loudly. And certainly intellectuals have traditionally been identifiable as much by the platform from which they speak as by what they speak about. For example, from a more traditional perspective, the editorialists of the *New York Times* are intellectuals, even when they write about trivial matters; but the most learned member of your book club is not, even when she talks about very consequential things. Such was the case. But today, the transformation in means of communication is in the process of equalizing these forums, to a much greater degree than ever before. The linked comments page on a blog about television may shape its readers’ voting behavior in ways that David Brooks or Paul Krugman no longer do. And this, in turn, explains why an aesthetic public sphere is today inseparable from a “virtual public sphere” (Papacharissi 2002; Dahlgren 2000; Gimpler 2001). It will come as no surprise that the Internet has revolutionized our way of deliberation and that new media technology allows for a huge increase in deliberative forums. Nevertheless, we need not succumb to technological determinism. Rather, our point is that the current conjunction of culture and technology, of intellectuals and new configurations of the public sphere, represents a development that we are yet to fully understand.

Our central claim is the following: it is the best of times and the worst of times for intellectuals as critics and cultural intermediaries. The dual opening of the public sphere toward art and culture on the one hand, and virtuality on the other, certainly represents a democratization of deliberation to the degree that it allows for more individuals with less expertise to express their interpretations and to be heard. There is an substantial gain in reflexivity, which could be seen in the degree of participation and, from there, in the new forms of public deliberation and cultural citizenship. However, there are also reasons to be worried by such balkanization occurring in the increasingly fine-grained debates on the Internet and elsewhere. These forums are not by and large concerned with the sorts of grand issues debated by the earlier style of intellectual. Rather, they are characteristically concerned with minute, particularist issues. Additionally, it is reasonable to assume that the critics’ lack of familiarity with grand debates make them more susceptible to shallow but fashionable presuppositions – the classical Tocquevillian fear of “half-baked ideas” threatening democratic culture.

The democratization underway is thus profoundly paradoxical. In order to support this claim, we divide the chapter into two empirical investigations. In the first, we analyse the evolution of music criticism as a way to make sense of both the

popular creation of complex knowledge and the struggle for recognition associated with it. From the legitimation of rock and roll to today's question regarding the globalization of techno music, embedded intellectuals have proven themselves to be important "interpretative activists" (Stamatov 2002). Their discussions about cultural drifts and trends create feedback that loops back into the trends themselves. In the second section, we consider how new media technology has transformed both television criticism and television itself, leading to a proliferation of new aesthetic and business practices and, significantly, the historic (and long prophesied) convergence of this low-art medium with high-art aesthetics. Again, we argue that the feedback caused by the fine-grain analysis of critics (made possible by, but not the direct result of, technology) has significantly encouraged these transformations. While in both cases we root our discussion in the experiences of United States, we turn in the concluding section to a transnational, multilingual model of cultural engagement.

Talking about Popular Music: From the Legitimation of an Aesthetic to an Aesthetic of Justification

To say that music is a "total social phenomenon" *à la* Mauss (2005 [1950]) verges on understatement. Music shapes individual experience, emotional connections between people and the sense of collectiveness that groups can nurture. As Frith nicely puts it, music is "a way of being in the world" (Frith 1996: 272). Nowadays, what is referred to as popular music has migrated into every aspect of mundane life – buying groceries, riding in elevators, driving to work, and so forth (Di Nora 2000). And yet despite its uniqueness, popular music has managed to retain its quasi-sacred character. It certainly retains much of its liminality, that is, its capacity to make sense of crisis or difficult times. Popular music is thus highly political; something one can see, for instance, in its many links with social movements (Eyerman 2002; Eyerman and Jamison 1998; Steinberg 2004; Street 2003). In retrospect, then, it seems obvious that the vast expansion of popular music has profoundly influenced the last 50 or more years. But this is only in retrospect. Within the process itself, this has never been self-evident. On the contrary, popular music was repeatedly dismissed as a serious form of art. Detractors came from all across the spectrum, from Marxists criticizing its alienating effect, conservatives questioning its sexual or moral depravation, and liberals refusing to compromise on the purity of *l'art pour l'art*. Theodor Adorno's article "On Popular Music" (1941), where he dismantles any possibility for rescue or even acceptability, is certainly emblematic in that regard.

Historically, the answer or the defense of popular music – the elaboration of a counter-discourse to its counter-discourse – came from within, from cultural intermediaries and critics alike. Mostly starting from the mid-60s, a gathering of formerly fragmented views about rock and roll crystallized into what Powers

calls “rock intellectualism” (Powers 2010, 535).² This is a resolutely non-academic literature, finding its expression in new kinds of journalism emerging in print, FM radio and the like – *Rolling Stone* magazine becoming iconic in that respect. What is common to all these views and discourses is that they promote rock music as a legitimate, genuine, complex and subtle artistic product. According to Regev, the period is characterized by a “discursive strategy of ‘proving’ [the music’s] artistry”, that is to say “the producers of rock meanings have formulated an interpretation of the music which applies the traditional parameters of art” (Regev 1994, 87). Other people have lately challenged Regev by saying that rock also found legitimacy in its own newness, although these differences could be considered minor (Van Venrooij and Schmutz 2010, for instance).

Most commentators agree that around these years rock and roll created a space for itself, one that would be highly mythical and one that would revolve around the ideas of authenticity and subversiveness (see also Weisethaunet and Lindberg 2010). From that point on, in other worlds, rock and roll would prove unapologetic and more and more geared towards its own development. In the language of social sciences, it would then be possible to speak of the creation of a popular genre, with all that entails in terms of connoisseurship, symbolical ownership and struggle for and around these.

What we want to stress here is that the creation of a genre, in general, and the rock and roll genre, in particular, is fundamentally a social process through which boundaries are constantly negotiated. What is identified as a legitimate aesthetic or style? Who sounds or looks like whom, and why? All of these questions are indeed “the subject of struggles for definition across the continuum from production to consumption” (Toynbee 2000, 106). Rock and roll emerges as a complex web of interpretative entrepreneurs and activists who play a powerful (because meaningful) game. Periods and values are compared and hierarchized. Some think stadially, for example the British Invasion as a golden age; other prefer to talk in terms of masterpieces, for example *Who’s Next* (1971) by The Who; or legends, for example Bob Marley. Obviously, such terms are both rock solid and shaky, but this is what makes them interesting. In yet another recent article, for instance, Powers has analysed the evolution of ‘hype’ or ‘hypefulness’ in Bruce Springsteen’s early career only to conclude that it belongs to a rhetorical and thus polysemical reality (2011). And that is the point here. It is always the ambiguity within classifications which fuels passion and, from there, impassioned and sophisticated discussions. Conflicts of interpretation reign supreme. In turn, this gives rise to what Couldry has referred to as an “emergent democratic politics” (Couldry 2006, 70) or what Atton has coined as a “democratic conversation” (Atton 2010).

2 In his own account of the history of rock criticism, Powers goes further back in the 1910s and 20s and studies a group of bohemian intellectual he claims are the ancestors of rock critics, but nevertheless describes the 60s as the “era of rock intellectualism” (Powers 2010, 540ff).

Professional critics in the press and elsewhere have been instrumental in the development of such open and dialogical space from the mid-60s on. The problem, however, is that this category of “professional” is rather unclear. There’s no diploma, no union, just journalists who like to think of themselves as having a little something extra, a real and enduring fervor for popular music. In these circumstances, where is the authority and the legitimacy? And how does this inform a necessary struggle for recognition? In a brilliant article, Bethany Klein has argued that pretty much everything in this realm revolves around alleged aesthetic connoisseurship (2005). Critics have to prove time and again that they “get it right”, that they indeed understand why this is good or bad music. It is then a question of intellectual autonomy, but one that could translate in many different strategies. As in Bourdieu’s famous discussion of orchestration versus distinction (Bourdieu 1977), rock critics can create alliances within their own group – a consensual wall, so to speak – or they can go against the grain, something that would require more symbolical capital. To this, Klein also adds the rather subtle observation that critics often try to justify their autonomy by saying that they “write for themselves” (Klein 2005, 10). In any case, what critics try to do is to secure their position against a particular kind of symbolical pollution: the accusation of being sold out to the industry. In a world of press kits and all-expenses-paid travel, intellectual probity is both a value and a luxury. The line between purity and impurity is extremely fine, the object of constant scrutiny and negotiation.

Another step in the legitimation-intellectualization of popular music was the emergence of punk and other subgenres starting in the late 70s. These developments were not against rock and roll *per se*, but highlighted some of its tendencies, among which its quest for authenticity and subversiveness.³ Concretely, what happened during this period was an important increase in publications, and especially a boom in fanzines dedicated to punk. It is not an overstatement to say that they almost universally demonstrated an “untutored enthusiasm” (Atton 2010, 519). The discourse and its many complications and justifications became deeply embedded. It was a matter of appropriation and identity, as much as a new mode of symbolic ownership of the music. From clothing shops to clubs, from music stores to independent radio station, people involved in punk created both a dense and chaotic network. It implies a community of listeners, but more than that it implies a community of performance and interpretation. Dick Hebdige’s comment remains relevant today, that most if not all of punk has always been about the “meaning of style” (Hebdige 2002 [1979]).

That said, however, it is important to acknowledge the limits of this class of subcultural theories and to argue, as Geoff Stahl has so nicely done, for a subtle renovation of its presuppositions (Stahl 2003). After all, the community in question

3 By suggesting this continuation, rather than insisting solely on the “resistance” dimension of punk à la Birmingham School, we choose to stay close to Regev’s interpretation, for instance, when he states that “punk signaled a maturing of a historical self-consciousness among rock musicians and critics regarding their art” (1994: 94).

was not strictly limited to those among England's youth who experienced the exclusion and despair of the time. What is needed, in other words, is a broader understanding of the embeddedness of legitimating discourse within semi-closed groups that would permit freer movement and association and more positive action. Nowadays, the "music scene" is one such concept as it makes sense of the fact that individuals gather, circulate and create solidary bonds in a more connexionist mode (Straw 1991, 2004; Bennett 2004; Bennett and Peterson 2004). From semi-closed groups, then, the scene perspective gives access to half-open ones where it is often the same individuals who are producers, musicians, listeners and critics, but in a faster and cleaner reversibility. In such complex webs, nonetheless, meanings are being shared and constructed as values are being put forward that are inseparably aesthetic and ethical. Elizabeth Cherry, just to give one example, has analysed the many links between veganism, animal rights and the punk scene in Southeastern U.S. only to conclude that they indeed form a significant cluster (Cherry 2006).

Yet another important moment in this short history of popular music is the surfacing of electronica or techno music in the 90s. This, too, signals the creation of a complex genre – and scene – with all that this entails in terms of legitimation and justification. As remains the case today, what is trendy depends on innovations that can vanish in a split second, as well as on innovations that can blend into other genres or, to the contrary, operate to exclude them. Anything goes as long as its meaning is believed. What came to be called "Big Beat" is a case in point. For Norman Cook – known as DJ Fatboy Slim – the genre formula "was the breakbeats of hip-hop, the energy of acid house, and the pop sensibilities of the Beatles, with a bit of punk sensibility, all rolled into one" (Matos 2011, 6). Complex indeed.

But this has not prevented the style's rapid growth to crash two or three years later, in large part because of its overexposure. Critics and other intermediaries became suddenly aware that the music was "everywhere". Its presence in television shows to movies indicated that the genre had sold-out to the industry and no longer retained any of its original edginess. An even more musically complex example within the techno genre, and an even more intellectualized discourse going alongside, could be found in Glitch, a style building on scratchy and bipy sounds of technological failure. In his analysis, Nick Prior discovered that such avant-garde practice was nurtured by a small group, at least at first, of dedicated connoisseurs (Prior 2008). The discourse was profoundly inspired by philosophy and found echoes in academic or highly sophisticated journals and magazines such as *Parachute* or *Wired*, in the UK. What the example of Glitch indicates, then, is how clear-cut connections between initiates amplifies symbolical mediation and vice versa. As Prior puts it, "in most cases, glitch's support writers are themselves directly involved in the unfolding of the style, and their intervention are either internalist in content – fulfilling aesthetic, formalist or stylistic criteria – or posit glitch as somehow outside the field through the maintenance of a cool distance from pop" (Prior 2008, 307).

Obviously, this kind of new music is inseparable from the technological revolutions of the past 20 some years and, in particular, from the rapid growth of the

Internet and, now, Web 2.0. Nothing is exactly the same nowadays, from production to distribution and consumption (Jones 2000; Granjon and Sorge 2000). If this is self-evident, however, it should not be interpreted either in terms of technological determinism or through any mythical discourses, whether they be highly optimistic or pessimistic.⁴ What is needed is a realistic approach, one that would recognize the degree to which Web 2.0 allows and disallows certain practices even as it displaces struggles for recognition, power and the like. It is about “reintermediation” that is a complete reshuffling of the cards in the hands of all cultural intermediaries (Hawkins, Mansell and Steinmueller 1998: 10).

And yet this metaphor of “cards” does not precisely render the profound impact of Web 2.0 on identity, connoisseurship, symbolical appropriation and criticism. Old limits are blurring by the day: professionals and amateurs, producers and communities of fans or performers and audiences. User Generated Content (UGC), for instance in House or Goa Trance music genres, was almost immediately hosted on such websites as Soundcloud.com, where it can be widely disseminated, and on various blogs that will spin, relay and translate their proper content. Because of Web 2.0, in other worlds, cultural artifacts of any sort – including UGC, of course – are now becoming the object of potentially endless commentaries as well as the site for more or less open challenges. In a nutshell, consumers can “talk back” to producers more than ever before. In many respect, then, we are back to Atton’s “democratic conversation”. This is not and cannot be a public sphere in the pure sense of Habermas – it remains polluted through self-promotion and degrading publicity. Nonetheless, Web. 2.0 gives rise to genuine expressions of culture and concern for culture. It is a place for the construction of meanings that changes how any given actor operates. Foxydigitalis.com, for example, hosts online criticism dedicated to electro music. The site overtly seeks out new embedded commentators: “We already know you love music, or you wouldn’t be here. But if you write, too, we could use your help”. And the same phenomenon can be found at weeklytapedeck.com: “This is our blog. We love music. We hope that you love the music that we love. If you do not love us loving your music, let us know and we will take it down”. Respect for contributors goes in many directions and proves that what could be coined as “electro intellectualism” is not devoid of values.

If electro music and Web 2.0 are so deeply intertwined, they also go hand-in-hand with the globalization of culture nowadays. This is another force to reckon with, a cultural drift that might not be yet the equivalent of a world beat, but which, nevertheless, implies displacements of gigantic proportion. In his most famous article, Will Straw talks about a “system of articulation” that links music scenes from Toyko to Berlin via New York and the like (Straw 1991). Genre, style and trend all travel wide and fast – as fast as the communication of information and meanings. What we witness, then, is a radical reorganization of the system of reference and justification. Matter at one end of this system of articulation relies on

4 As Papacharissi nicely puts it, “ultimately, it is the balance between utopian and dystopian visions that unveils the true nature of the Internet” (2002: 21).

matter at the other end and vice versa, depending on the capability of the actors to find what they need. The question of whether this creates as much exclusion as it creates inclusion obviously deserves attention. Is this, in other words, yet another case of rising “class consciousness of frequent travelers” (Calhoun 2002), or are we in the presence of a real and enduring cosmopolitanism? The question remains an open debate in which embedded intellectuals adopt myriad positions. One thing is, however, certain: what is at stake with such a globalized articulation of music is nothing less than the very identity of the art form. As Berland puts it, “the increasing mobility of music technologies ... reveals how much the ongoing (re)shaping of habits is tied to our changing sense of location: where we are, where the music can take us, where we *belong*” (1998: 133). Talking about music within the music scene is doing exactly that; it gives a sense of belonging, of being part of something meaningful, but something that will be forever mediated and at a distance.

Talking about Television: Intellectuals, Academics, Critics and Fans

In the preceding section, we describe the role played by self-proclaimed popular music critics and intermediaries in the definition, self-understanding and ultimately the meaningful content of popular music. These embedded commentators are often unaware of the way their work feeds into the political and cultural power of music, and instead present themselves as simply categorizing music by genre while demonstrating their own connoisseurship.

As we move our attention to a different entertainment medium, television, we are confronted by slightly different questions. Unlike popular music, television has not been significantly tied to social movements. Quite the opposite: for most of its history, television has almost universally been associated with implicitly supporting the status quo. There are two related reasons for this association. First, in the pre-cable era, only a few television channels competed for an enormous public. Capturing the public meant appealing to widely-shared values, and so enduring television aesthetics emerged that were oriented to very broad publics. Second, although transgressing taboos has always been a means of gaining a temporary strategic advantage in crowded marketplaces, television producers were further limited by advertisers’ fears of being tainted by such content. Accordingly, television aesthetics has traditionally been oriented to what David Thorburn calls “consensus narratives” (Thorburn 1987), which gather ideational and emotional components together to affirm in the viewers’ eyes the good of the socio-cultural order.

That television is mostly concerned with consensus narratives is rarely questioned. Rather, it is the meaning of these narratives and their effect on the social-cultural order that has been the object of dispute for intellectuals and academics. In other words, classic Tocquevillian concerns have been at the root of much of the debate about television: does the medium represent a process of democratization? If so, does this democratization threaten democratic culture?

To answer these questions, we need some clarity on who deliberates about television and how their deliberations affect television production. Fundamentally, this concerns the wider cultural drift of our times. In order to deal with this complexity, we divide the field of knowledge-meaning production about television into four categories: first, there are generalist public intellectuals; second, academics and scholars; third, professional television critics; and finally, a dynamic and emerging category of enthusiast critic. This final category is characterized by processes of knowledge-meaning production that we have seen above in the punk and techno communities, and accordingly we borrow the label from Atton's suggestion that they reflect "untutored enthusiasm" (Atton 2010, 519). However, this category is of special note in the context of television, since it is currently undergoing a deep structural transformation, blending into academic discourse to become the realm of the "aca-fan" (Jenkins 2006b).

Here, what is most striking is an absence. American intellectual discourse has largely ignored television – remarkable, given it is, by an enormous margin, the most popular entertainment medium in the United States. This is a more peculiar phenomenon that we might at first think. The comparison with film is revealing: as film came of age, public intellectuals engaged in extended deliberations to assess its relative worth and impact on the mass public. Major elite debates about *auteur* theory, for example, crossed national boundaries between France and the U.S. (Staples 1966–7) and made household names of François Truffaut, Jean-Luc Godard, Andrew Sarris and Pauline Kael – in the houses of the cultural elite, at least. Professional film critics, intellectuals and film scholars have continued to cross-pollinate their work and the boundaries between the two are quite permeable. The result has been the creation of an aestheticist tradition in discussing film (Bauman 2001).

However, while the boundaries are permeable, there is a definite hierarchy of taste-making, which has led to a de facto split in the industry between elite production and popular production. Elite taste-makers identify the appropriate aesthetic qualities for admission into festivals and, through award processes, eventual entry into the academic canon. These are the films that are interpreted in aestheticist terms.⁵ Oftentimes, at these same festivals, popular films play out of competition, to the delight of the viewing public and consternation of intellectual and professional critics (for example *Mission Impossible* at Cannes). These films are either criticized for perceived aesthetic failures or discussed in instrumental or hedonic terms. The split reminds us that the elite discourse of much film criticism signals a fairly impermeable border between, on one side, intellectuals, academics and professional critics, all of whom share an aestheticist discursive style, and, on the other, popular, enthusiast critics. From a Tocquevillian perspective, film

5 Formulated in this way, the claim is of course exaggerated: academics do write about popular films just as not all festival films share the same aesthetic qualities. However, the story holds in the main and is significant for its contrast with television.

criticism is the domain of elite deliberative processes that provide for sophisticated but not terribly democratic reflection on the medium.

In Stanley Kubrick's academically-, intellectually- and critically-lauded film *A Clockwork Orange* (1971), the cruel and callow Alex is literally forced to watch television. His eyelids are peeled back with metal hooks: he watches with increasing passivity as his mind is wiped of all revolutionary and anti-social potentiality. Corrupted and corrupting, Alex represents a manic youth culture that threatens the reproduction of the status quo. Television has long been treated with contempt by intellectuals, and Kubrick's film struck a chord with elite commentators for this very reason.⁶ From their perspective, like Alex, as it endlessly gazes at television screens, the American viewing public is lobotomized.

Norman Mailer and William F. Buckley Jr. can be taken as two such paradigmatic intellectuals (see Drezner 2008 for more on this). Both were extremely media-savvy and both made distinguished contributions to the history of the moving image. Nevertheless, both viewed television with high degrees of skepticism and concern. Buckley, for example, described television as a "time-consumer" that has led to a decline in "passive intelligence" (Buckley 1996). He argues that the televisual image is so extraordinarily powerful that the written word can simply no longer compete. Likewise, in an essay titled "Being and Nothingness", Mailer accuses television advertisements of both being form without content and of negating the content of all television programming. He notes, "every time you become interested in a narrative on television, a commercial comes on and you are jacked over abruptly from pleasure to nothingness" (Mailer 2004, 166). Mailer draws on the language of existential philosophy to convey the epic scope of his critique: advertisements on television are so completely lacking in value that they infect the medium itself. He continues:

Filling such essentially empty forms as commercials is a direct species of *nothingness*... many if not most television commercials, no matter how spiked with clash and color, give, nonetheless, little attention to the item they are there to sell... advertisers work to overcome the onus of *nothingness* that the TV commercial inserts into our nervous system. (Mailer 2004, 168)

We see this same attitude perpetuated today in the casual references to television made by our most popular public intellectuals. Both Noam Chomsky and Paul Krugman, identified in a recent poll as the most influential American intellectuals,⁷

⁶ Indeed, not watching television is often used as a mark of cultural distinction in Bourdieu's sense (1984): for example, "talking intelligently about TV, in many circles, is verboten. It is a taboo subject" (Johnson 1997). Henry Jenkins relates a similar story: "when I tell people that I teach television, they sometimes boast, 'I don't even own a TV set!' All I can say is that we inhabit different realities" (2001).

⁷ In 2005, *Foreign Policy* and *Prospect Magazine* put together one of the most popular lists of the world's public intellectuals (Drezner 2008). The list was compiled by

describe television in the narrative mode of tragedy. In a 2004 editorial titled “Triumph of the Trivial”, Krugman takes the news media to task for replacing serious content with trivia: “Somewhere along the line, TV news stopped reporting on candidates’ policies, and turned instead to trivia that supposedly reveal their personalities” (Krugman 2004). Similarly, in a 2005 interview, the year he was named the world’s most influential public intellectual, Chomsky noted, “Well, you’ve seen television ads, so I don’t have to tell you how it works. The idea is to delude and deceive people with imagery” (Chomsky 2005).

Perhaps more than any other historical or technological change, television has given rise to Tocquevillian fears of a coarsening of democratic deliberation, fears which have been presented in intellectual discourse as cause for condemnation or outright rejection. This attitude has also been adopted by many academics, perhaps most famously in the work of Theodor Adorno during the 40s and 50s, who saw television not as empty, in Mailer’s sense, but as a coercive environment with a single obvious message: buy. This line of analysis has been developed by scholars working within Frankfurt and later Birmingham School logics (Turner 2001). The Tocquevillian dilemma is solved by rejecting the democratizing potential of television and strenuously asserting its corrosive effect on democratic deliberation.

A different solution to the Tocquevillian dilemma is suggested, but not definitively explored, in a second major scholarly tradition. Marshall McLuhan’s work on popular culture in the 70s has influenced many scholars to see television as both enriching as well as constraining. McLuhan famously described the television advertisement, for example, as “cave art of the twentieth century... vortices of collective power, masks of energy” (McLuhan 1970, 7). The television commercial is a fossil of deep cultural structures, ideas and feelings, miraculously resurrected in the viewer’s mind. This may be coercive, as Adorno argues, or personally enriching and constructive of solidary bonds, as Durkheimian sociologists would argue. Nowadays, cultural sociologists have consistently moved away from Adorno and toward Durkheim. Ronald Jacobs’s research into television, for instance, suggests that these very processes of repeatedly encountering shared values and concepts helps both to generalize and to subjectively appropriate those values in a way that has the potential to vastly expand national as well as international democratic deliberation (Jacobs 2007; see also Ang 1985; and Liebs and Katz 1990).

So far, we have encountered three perspectives on television. We argue that the standard intellectual perspective is dismissive. The Adorno-esque academic tradition is both dismissive and alarmist. The McLuhan-esque academic tradition is appreciative and cautiously optimistic. Given these perspectives, how have embedded critics and cultural intermediaries, both professional and enthusiast,

votes from over 500,000 online respondents and was redone in 2008. In both years, the two top-ranking American public intellectuals were Noam Chomsky (2005: first place overall; 2008: eleventh place overall) and Paul Krugman (2005: sixth place overall; 2008: thirtieth place overall). See *Foreign Policy* (2005) and (2008).

engaged with the television medium, and what do these engagements suggest about the democratization of public participation and deliberation?

As we have suggested above, the boundary between professional film criticism and enthusiast film criticism is fairly impermeable (Holbrook 1999). For example, reception of the film *The Dark Knight* (2008) was sharply divided between enthusiasts and professionals. Professionals assessed the film in terms of genre expectation and technical proficiency, viewing it as just another film of indifferent quality.⁸ Enthusiasts wrote from within the imaginary developed by the film and found it to be exceptional.⁹ The difference can be detected in the way continuity is understood in the two communities. The professional critic Jim Emerson wrote at length on his blog about technical incompetence on the part of the editor and cinematographer.¹⁰ This held little weight with enthusiasts, who were concerned with how the film established continuity with the broader mythical Batman universe.

Because intellectuals have largely ignored television and academics only rarely address the content of television (Williams and Goulart 1981; Bielby and Bielby 2004), professional television criticism has developed with few ties to elite discourses. Consequently, it is rare to discuss television in aesthetic terms or to engage closely with its artistic values. Indeed, professional television criticism has been largely devoid of aestheticism, and instead dominated by instrumentalist and hedonic logics.

Amanda Lotz (2008) argues that critics have traditionally written with two audiences in mind, an argument that overlaps with our claims about instrumental and hedonic logics. First, critics have written for the mass public, tempting them with the pleasures or warning against the displeasures of upcoming shows. Second, they have written for television producers, whom they seek to influence.

8 Four leading professionals gave similarly mixed reviews, focusing criticism on the failure to provide coherent editing and a consistent tone. Morgenstern (2008) describes the mood as “suffocatingly dark”, the plot as “muddled” and the action sequences as “pounding but arrhythmic”. Edelstein (2008) describes the film as “noisy, jumbled, and sadistic”, characterized by a “lack of imagination”, “uncivil Shavian dialogue”, and “spectacularly incoherent” action. Hunter (2008) complains that the filmmakers “McComplicate things up all McFusingly”. Dargis (2008) describes the film as “sloppy, at times visually incoherent”.

9 One particularly intense fan reaction came from Josh Tyler, who addressed his comments to professional critics and award-givers: “It’s more than the best movie of the year, it’s one of the best movies ever made. Snub it and there will be consequences” (Tyler 2008). Fans have become so embedded in the imagined universe of the film that there are now websites dedicated to fiction written by fans concerned with elaborating the story-lines of the film (Fanfiction.net).

10 Emerson’s first blog posts (2008a, 2008b) on *The Dark Knight* noted the intensity of fan response. In the first, he notes, “two and a half weeks into its theatrical release, is it still a sacrilege to believe, for many reasons, that *The Dark Knight* is less than the greatest whatever ever?” (2008a). As his arguments continued to meet resistance, he eventually began analyzing technical flaws in the film (2009, 2011).

Lotz argues that through three phases of television history, the power of critics in relation to both of these audiences has increased consistently. In the first stage, from its origins until the introduction of previewing in the 60s, critics wrote retrospective pieces that had little impact on viewers but relatively great impact on television producers (see also Spigel 1998; and Frank 2002). In the second phase, critics gained the technology to allow them to preview episodes. Accordingly, in this phase, critical writing was of far greater interest to viewers, which in turn generated even closer ties between critics and producers. These ties often grew at the expense of journalistic integrity. This period ended in the 70s when critics reacted against industry pressures and formed a union. This prevented blackballing and hence allowed critics to write negative reviews and further gain the public's trust.

If we follow Lotz's narrative, we can interpret critics' knowledge-meaning production to have consistently democratized the medium, challenging the coercive structures identified by Mailer and Adorno (the dominance of commercial concerns) and refining the solidary structures identified by McLuhan and Jacobs. However, Lotz ends her narrative by raising penetrating questions about how new media technology, which has resulted in the exponential growth of deliberative venues, will affect professional criticism. Lotz notes that professional television criticism is in danger due to the changing print-media landscape: quite simply, reviewers are being fired and not being replaced. At the same time, she draws attention to an opposing trend. Television shows dedicated to talking about other television shows are increasingly popular (for example *Extra*, *Access Hollywood*). Other stranger forms have emerged: shows about shows about shows (for example *The Soup*, *Tosh.0*), websites about shows about shows, shows about websites... The television landscape has become massively more complex as well as extremely self-aware and self-referential.

What does this suggest about the Tocquevillian dilemma? Is the fourth phase of television criticism a democratizing of knowledge-meaning-production? Is it enriching or eroding democratic deliberation more generally? These are not idle questions. As academics and intellectuals, we may be too prone to dismiss these trends as the meaningless jabber of a lobotomized public. Put into the broader context of the medium, the rise of enthusiast criticism that is signaled in these changes is suggestive of successful and meaningful deliberation, albeit in a new and difficult-to-recognize form. What we are witnessing is the emergence of an aesthetic forum for public deliberation (Newcomb and Hirsch 1983; Macé 2005).

Glimmers of this aesthetic public sphere can be seen surrounding even the most banal televisual products. Mailer views the television commercial as epically meaningless. Adorno views it as raw coercion. An entirely different perspective on spectatorship, active audiences and interpretative activism emerges on the popular website YouTube.com, which hosts short video-clips that people can comment upon.

The minute or 30 seconds of a television commercial are not always wasted, not always what Mailer terms "nothingness". Sometimes, this is a minute of sublime cathexis for the viewer. A 60-second television commercial for Monster.

com, an employment website, is a case in point. In it, the viewer watches as a stork flies through a stormy sky carrying a bundled-up baby. The stork shades the baby in a desert, fends off wolves in a stormy canyon, and finally manages to deliver the baby to his eager new parents. Suddenly, we see a man – the baby fully grown – yawning in a drab office. The man looks out the window and sees the stork, now grown old as well; they lock eyes. A caption then plays across the screen: “Are you reaching your potential?” (YouTube.com, 2008).

The comments to this video include many examples of what McLuhan calls “vortices of collective power”. Rabadooda says, “This made me cry. I really need to get a grip”.¹¹ The shame of the emotional response indicates rabadooda’s internalization of the Adorno perspective. Arschmagnet commented, “Wow, this made me shiver”, an admission that received four thumbs-up from other users – these viewers appear less self-conscious about their emotional response. Trekkergal’s observation is particularly effusive: “This is a masterpiece. I can’t believe how much I was moved watching this. It even made me cry and reflect on my own life. I think this commercial can change people’s lives”. Trekkergal received 85 thumbs-up.

Trekkergal, Arschmagnet and Rabadooda’s comments differ in their attitude toward the acceptability of being emotionally moved, but they are all manifestly affected by the commercial. That 85 people thought Trekkergal was right, that the commercial can change people’s lives, is a statement of the medium’s power. This is why the failure of intellectuals and most academics to seriously engage with television is so important: television is not only extremely popular, but it is also extremely powerful.

Comments attached to the commercial also reveal processes of enthusiast-critical deliberation. Someone named bluehawka0, for example, quotes an earlier commenter in their response. The original comment reads as sarcastic and rather banal: “The sap doesn’t know the length the stork went through to get him safely to a family and possible life”. Bluehawka0’s response reads as entirely sincere: “Did not know... or just realizing? I thought (or like to think) the look on the stork’s face is that of a reminder, and not condemnation”. One commenter responds to another in the spirit of taking the commercial seriously as an artifact that deserves thoughtful but also flexible interpretation. This brief exchange reflects the commenter’s process of carefully viewing the commercial, reviewing earlier posted analyses, and then, finally, posting their own thoughts. What is seemingly unlocked in Bluehawka0’s mind by this process of deliberating is the decision to believe in the power of compassion and encouragement over tragedy and failure. Banal, bathetic, but loaded with meaning for the commenter.

The deliberative culture of an advertisement’s comments page is merely suggestive of broader cultural transformations. The stork commercial is a single

11 These names are the ones listed by the commenters. Their idiosyncrasies of spelling have been carried over. All comments can be found on the YouTube page (YouTube.com, 2008).

minute of video: YouTube.com claims to have eight years of content uploaded every day (YouTube.com, 2011).¹² Assuming only a small fraction of videos have comments, the amount of human reflection on minutia remains staggering. Matters of surpassing triviality are granted thoughtful analysis in a vast archipelago of micro-forums. What we are witnessing is a process of deliberative balkanization, a shifting toward ever-finer points of dispute. The stork commercial suggests that these effectively unlock the quotidian experience of watching television: a commercial or an episode of a show brings up powerful feelings for the viewer; now, these feelings can be shared and discussed with others, prompting in turn ever more reflection and analysis.

If we aggregate up from a single commercial to a television series and beyond to a television genre, we see a massive proliferation of extremely fine-grained deliberative venues of this sort. Throughout these venues, we see the dominance of the logic of fandom (Jenson 1992). Because the stakes are relatively low and the cost of entry non-existent, people can freely debate on largely emotional and aesthetic grounds. They are freed of the burden of rationality and consistency. The convention of “shipping” is an example of aesthetico-emotional debate in enthusiast or embedded critical communities. The term derives from “relationship” and indicates a fan’s identification with or desire for specific characters to enter into a relationship. How precisely this unfolds is the matter of dispute and detailed discussion – even “fan wars” (Bieibly and Bielbly 2004). The analysis is not rational, per se, but rather based on individual taste and emotional intelligence. Status hierarchies and symbolical ownership emerge through mastery of the shared object. Accordingly, what emerges is not the hierarchical aestheticism of classic elite discourses, but rather democratically-adjudicated taste. People give Bluehawk0’s comment a thumbs-up, but her position is taken even against her own rational judgment. It is an attitude that she prefers, as do others.

Henry Jenkins’s research into popular culture suggests ways that the democratic and aesthetic deliberation of fans seeps into the production of television. He pays attention to industry involvement in fan gatherings, interplay between media technologies, and the role of culture and industry in globalization processes, among other things. He theorizes these processes as aspects of “convergence culture”, which he defines as:

the flow of content across multiple media platforms, the cooperation between multiple media industries, and the migratory behavior of media audiences who would go almost anywhere in search of the kinds of entertainment experiences they wanted. (2006a: 2)

However, his work itself is a powerful example of a convergence in critical, academic and intellectual knowledge-production. On his website, *Confessions*

¹² This means that in the time it takes to watch the stork commercial, nearly 3000 minutes of new content has been uploaded.

of an *Aca/Fan*, Jenkins engages in spirited discussions with fans and academics about how their worlds intersect. Whereas in the film world, professional critics can draw on the symbolic capital of elite, aestheticist discourses as well as technical knowledge in their disputes with fans and embedded critics, television critics have very few conceptual resources at their disposal. The movement is in the opposite direction: academics like Jenkins involve themselves in enthusiast deliberative communities, bringing back to the academy new concepts and refined categorizations.

The balkanization of television's deliberative forums has allowed for the development of highly detailed but very narrow debates (for example shipping debates). This is indeed a democratizing influence to the degree that it incorporates a broad public in the deliberative process of making meanings. Academics like Jenkins should be commended for attempting to maintain such broad-based conversations while simultaneously endeavoring to grant them sophistication and commensurability. On the negative side of the ledger, there is a danger that academics will fall victim the medium's power and lose their intellectual distance even as they become embedded in television debates.¹³ Having for long decades ignored the meaning-making potentiality of television, intellectuals and academics now find themselves confronted with the possibility of massively expanding their deliberations by connecting with fan discourse – or being utterly overwhelmed by it.

Conclusion

Being an intellectual in today's digital age is not the easiest task or the most comforting of vocations. Rapid structural changes as well as an increased blurring of many boundaries make it difficult to navigate. What is thus required is a new sense of adaptation and re-questioning of who we are and how we operate. And yet, there is some *grandeur* in the current situation. Indeed, this has been our central claim throughout the chapter: today is simultaneously the best and the worst of times for intellectuals as critics and cultural intermediaries. While looking at the evolution of music for the last 50 years or so, for instance, it is difficult not to be struck by how instrumental critics and cultural intermediaries have been in cultural drifts, the evolution of genres and, more broadly, the way people talk about music. Their embeddedness makes them simultaneously audience and producers of meaning, to such an extent, in fact, that it would be profitable to see them as "meaning prosumers". Simply put, the new critics and cultural intermediaries have

13 William Julius Wilson's attitude to the HBO television series *The Wire* (2002–2008) is an example of enthusiast tendencies overwhelming critical ones. According to Wilson, this series "has done more to enhance our understandings of the challenges of urban life and urban inequality than any other media event or scholarly publication, including studies by social scientists" (quoted in Penfold-Mounce et al 2011). If scholars adopt the authority of artists over their object of study, then they effectively become embedded critics.

proved themselves to be the more influential precisely because of their deeper *in situ* understanding and more creative interpretation.

Almost the same could be said about the evolution of television and television criticism. The distance from Adorno's dark diagnosis in the 40s and 50s to today's fans and aesthetic deliberation going on all across Web 2.0 is immense. The public sphere has expanded dramatically both in direction of culture and virtuality. Individuals engage deeply with minute aspects of what they see on television, but they also talk back to television – often times via the web. Such discussions cannot be dismissed as mere epiphenomena. They operate at a very fine grain, but in doing so address dense knots of meaning. The individuals involved are not typical experts, but this is exactly what makes them such forceful hermeneutical agents.

But, of course, every coin has two faces: *grandeur* and *misère*. The democratic potential and promises are there, and yet they remain latent. Hence, this is a Tocquevilian moment. The finer grain may paradoxically lead to a coarsened debate. Among other things, the different critics and intermediaries are far from being equal in their capacities or rhetoric, and this necessarily finds an echo in the multiple discrepancies between venues. Each of them has their proper rules of engagement, level of sophistication and the like. In turn, they have a tendency to develop internally, if not autistically. This is what we mean by the balkanization of the aesthetic-virtual public sphere: as they argue at an ever-finer grain, the new intellectuals often lose the sense of what constitutes the unity of the public sphere. Where is the core, where is the margin? Who is in control, who is not? Like Monet's *The Rue Montorgueil in Paris*, the closer we are to the canvas, the harder it is to make out the whole.

If de Tocqueville is still accurate and relevant today, this means that the paradox behind the current democratization process cannot be overcome. But does this imply that it cannot be exposed? Does it mean, moreover, that it is impossible to think of ways to build on, and to try to expend the latent potential? Obviously not. A complementary approach to the widening of the public sphere in direction of culture and virtuality could be to re-interpret this sphere in terms of cultural citizenship and cultural rights. The historical problem with citizenship is that it has been understood as the allegiance to a specific nation-state. Now, by suggesting a cultural version of it, what is proposed is a commitment toward culture itself, its meaning and development. The nation-state frame has little and less to do with it then: the important thing is the engagement of different individuals, their dedication and sense of concern. Culture is interpretation. The competency to talk about it grows by talking about it; no more, no less. And for that purpose, it seems that intellectuals acting as critics and cultural intermediaries pave the way. Indeed, they could serve as an example. What these new and embedded intellectuals have to offer, in other words, is both a mediation and an encouragement. Their voices give rise to thoughts. Their actions allow others to connect and to dialogue, linking subjects to each other and to new objects of analysis. Their passionate and voluntary discussions contribute to the evolution of meanings and meaningful

linkages more than any imposed or formal structure could hope to do. And that, at least, is a hopeful sign.

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Chapter 15

World Sociology: The View from Atlantis

Andrew Abbott

There seem today to be sharp changes in what we know, in how we know, and in what we think is knowledge. Researchers find themselves spending more time searching and less time reading. Students think that knowing something means knowing a web address and that creating an argument means making a list of bullet points. In both areas, the change is striking. Moreover, substantive research corroborates these impressions. Research and knowledge practices in the humanities and the humanistic social sciences really have changed in the last 40 years. A whole view of knowledge seems to be slipping away.

But if we are losing something, what is that something? This is a question that is posed to us continuously: by the students who think our knowledge trivial and unworldly, by the neo-liberal state which treats us as assembly-line workers in a knowledge factory, by the natural scientists who think that our research procedures are unscientific and feckless. And behind all of these questions lies the deeper question of our own normative ideals: what ought knowledge to be in the humanities and the social sciences?

This question of knowledge ideals comes not only from without, but also from within. Our previous knowledge ideal seems to be in the process of self-destruction. It is increasingly clear that in sociology, at least, cumulation is not a practicable ideal. One has only to read work from 50 years ago to realize that today we are often saying the same old things with new data, new methods, and new citations – pouring old wine into new bottles.

And not only is our sociology not cumulating, it is also deeply fragmented. There are huge variations in methods, philosophical assumptions, and style across the discipline. In the old days, this variation was not worrisome. We believed in cumulativeness, and cumulativeness meant that these fragments would be sorted into the sheep and the goats, and the sheep would in turn be arranged into a cumulative flock called scientific sociology, while the goats would scatter across the intellectual hillsides as pop sociology or journalism. But if cumulation is not going to happen, then we have no easy method to tell the sheep from the goats and hence no way to bring order out of fragmentation. How then are we to deal with it? How and when can and should our different sub-disciplines and methodological paradigms and research areas combine or hybridize or indeed even converse?

I wish here to address one aspect of this fragmentation, the variation of sociology from nation to nation. Although this variation is often overlooked by those of us on the self-sufficient continent that is American sociology, it is

evident that there are sometimes striking differences among the rhetorical styles of articles from different nations and regions. More generally, there is considerable international variation in use of specialized methods and particular theories.

Of course there are those who would impose a concept of cumulation here as well. They expect the false sociologies – whatever and wherever they are – to vanish, while the true sociology emerges and coalesces across these various national voices. But if there is no cumulation, then we must confront the fragmentation just noted. How can these diverse “national sociologies” be related to one another? How should we be thinking about sociology as it becomes a more global enterprise?

Barbara Celarent Reviews

One could approach such questions in a highly deductive manner. One would treat variation in sociologies within the same framework one might use for variation by subdisciplines or methodology. But while that might further a general analysis of sociological knowledge, it might not be the most effective approach to the particular question of national differences.

I therefore proceed inductively, reporting the results of my own reading and reflection about global sociology. In particular, I report the exercise of reading a series of older works drawn from around the world. In this I am following the lead of the *American Journal of Sociology*, which has for several years been publishing in each issue a review essay on some earlier book of social analysis, a work taken from the broad heritage of sociology. Although the author of these reviews wrote at first about American works, for the past two years she has written and – apparently will hereafter write – only about works whose authors were born outside the erstwhile global metropolis of Europe and North America. She has examined writers from Latin America and Africa, from India and China, from Iran and Turkey. So the essays give us a window into a global sociology.

The author of these reviews is Professor Barbara Celarent of the University of Atlantis. Obviously, both the name and the university are fictitious: the name is a quotation from a medieval mnemonic for remembering the valid forms of syllogism, while the university takes its title from Plato’s *Timaieus*. So we can assume that the author wishes to conceal her identity and therefore we must eschew any *ad feminam* analysis. We must simply take at face value both these review essays and the texts they analyse. But this presents no difficulty. Like some of my colleagues, I have myself followed both the reviews and the texts carefully. So I thought that today I would reflect on what one learns by reading both the reviews themselves and the 18 authors and 23 works so far covered in Professor Celarent’s series. These books might be seen as part of a common heritage for a world sociology. What do we learn by reading them alongside one another and how does that bring us closer to imagining global sociology?

From the outset, we learn three lessons that are implicit in the very idea of reading old works. First, careful reading is a central – indeed an essential – part of scholarship. This is a truth no longer universally acknowledged by our students, and perhaps not even by some of us. In an age of keyword searches and research assessment exercises, we spend much less time in slow and meditative reading than we did heretofore. We search more and read less, as if we had become more certain what we could and would find in the work of others, and were simply locating it to verify that indeed it said what we expected. We have lost the desire to be surprised.

How many of us, I wonder, have taken the time, as has Professor Celarent, slowly and carefully over the course of a year to read six books from cover to cover, then to find and read for each book the dozen or so historical, critical, and biographical works necessary to place that book in context, and finally to undergo the discipline of writing 4,000 words that can capture some important intellectual lessons from that reading? I imagine some of us may so read familiar works in the course of teaching, or perhaps we so read works particularly central to our scholarly interests. But few of us deliberately read a broader selection of works in order to encounter unfamiliar social analysis from unfamiliar times and places. Celarent's first lesson is therefore that reflective, meditative reading of adventurously chosen texts remains a necessary nutrient for the scholarly life: that we will lose our way if we surrender completely to the seductions of the internet and the pressures to publish. We must take the time to think.

Celarent's second lesson is that we should read works that are old. She does not write about living authors. By such a rule she insists that old work is relevant and indeed important. She thereby denies the simple version of cumulation, the idea that all past knowledge is subsumed or otherwise contained in the writing of the present. To be sure, I do not think Professor Celarent denies all forms of cumulation. She clearly believes that knowing more simple facts is better than knowing fewer simple facts, and she often speaks of the growth of this or that short-term paradigm in sociology – new social movements theory or practice theory or subjective ethnography or network analysis. But she has been clear that she thinks that the great underlying themes of the discipline are largely constant, and it is these that she seeks to engage.

Because such a profound constancy lies behind the deceptively progressive surface of our discipline, another of Celarent's reasons for reading old work is that it teaches humility. When we read old work without the crutch of cumulation we begin to see in that work not simply those few things that we can identify as the precursors of what we currently take to be "the truth". We begin to also see the many important past concerns that we have ourselves dismissed: vocabularies of thinking that now seem wrongheaded or perverse; once-enticing paths of argument that – as we presently think – led into blind canyons of the mind. This encounter with the many lost causes of the past – causes perhaps lost only momentarily, I might add – helps us to realize that our own current knowledge must be similarly uneven and provincial. The handwriting of the future is indeed on the wall. But so also is a lot of graffiti, and like our predecessors – like Belshazzar himself –

we have difficulty telling the one from the other. To see our predecessors living this confusion may teach us to avoid that seductive but false pleasure that E.P. Thompson once called “the enormous condescension of posterity”. We often speak of old work as “outdated” or “passé”. We say that old theory is “wrongheaded” or “imprecise”. So also will our own work be labeled, soon enough.

Celarent also believes that just as we need to recognize our temporal particularity, we need also to recognize our spatial particularity, especially as encoded in the widespread assumption of dominance by the sociologies of the current metropolis. We can infer this belief from her selections of books, which have ranged around the globe.

There is finally a third lesson we can take from Celarent, beside the injunctions to think reflectively and to avoid temporal and spatial provincialism. It is Celarent’s fiction that the works she introduces will be read corporately, by the discipline as a whole, thereby providing a basis for common discussion in the corridor or the coffee shop, at the conference or the colloquium. By reading together works that are unfamiliar to nearly all, a discipline can renew its commitment to a common if immensely various enterprise. As she puts it, “. . .by reading together a series of old works, we leaven our specialization with the yeast of difference”. Imagine, indeed, if we all came to a conference not only with our own papers prepared, but also having read in common one great old work that almost none of us had ever read before! Conferencing might be different then. There would be a novelty and excitement – dare I say, a youthful surprise – that perhaps we have lost.

Having taken these first three lessons, then, let me work through Celarent’s work to this point, noting her important themes. I shall try to derive from her insights a view of how we might conceive and live a sociology that is global in its reach. I begin with her choices of subjects, trying to infer from them her model of sociological knowledge more generally. I then turn to the themes she seems to regard as central to a world sociology, focusing on her views of the problem of universal and particular knowledge.

Subjects of Reviews

The six reviews of Professor Celarent’s first year set forth her attitude towards the metropolitan traditions. In those reviews, she reflected about what is surely the dominant voice in that metropolitan tradition, sociology as practiced in the United States.

Celarent read six authors in that first year, only one of whom was actually well-known across American sociology for any length of time: Herbert Marcuse. Her other writers included English scholar and social reformer Michael Young, writer Henry David Thoreau, schoolteacher/ethnographer Frances Donovan, librarian/social scientist Bernard Berelson, and Marxist sociology professor Oliver Cromwell Cox. A diverse lot indeed to be the heritage that Celarent envisions for American sociology!

These selections constitute Celarent's first message: that American sociology has intellectual connections far beyond its normally accepted canon. I should underscore my choice of the phrase "has intellectual connections" rather than the more expected "has intellectual roots". Celarent believes that past writing should be read as part of the present, not as something that is dead and gone, the subject of merely historical work. In her view, the historical account of how sociology came to be what it is today – important as that may be – should not govern our current reading of the great works of past social analysis. Put another way, while Celarent is both careful and respectful as a reader of works in their historical context – indeed, detailed biographical and historical context marks all of her reviews – she believes that contextual reading should achieve a translation of past work into terms that make immediate sense as argument in the present. This seems an important – perhaps a challenging – message. History matters, to be sure, but it matters less than does discovering what is enduringly human in a given text.

That side point restated, the main message of Celarent's choices is clear. Even in metropolitan sociology, the heritage of the discipline involves more than just sociology professors. Oliver Cox is the only full-time, lifelong sociology professor on Celarent's first-year list. Indeed, two of them were not professors at all (Thoreau was a writer and Donovan was a high-school teacher) while Young and Marcuse held academic posts only once their intellectual reputations had already been made outside academia. Thus, in theorizing global sociology, we need to remember that even in the metropolis, sociology reaches out to a very wide set of sources. It is not simply a professional enterprise, even where it is most institutionalized.

Such an approach entails a wide definition of sociology. A narrow approach would define sociology (trivially) as only those forms of social analysis done by sociology professors. On that (strongly cumulativist) definition, the *heritage* of the discipline can include those who are not sociology professors – because earlier scholars could know many fields, while today there is too much to know. But while the *heritage* of the discipline might include people who are not sociology professors, the *present* discipline should not, on this view. Yet this approach is obviously wrong. American sociologists in recent decades have drawn on philosophers like Foucault and Rawls, political scientists like Putnam and Elster, statisticians like Cox and Rubin. So Celarent's wide definition appears to obtain in the present as well.

One notes also that Celarent's authors speak to sub-communities within American sociology, each of which links quite closely with similar sub-communities beyond that nation. For, to be sure, while these writers are to some extent unfamiliar, each has his or her particular area of visibility in contemporary American sociology. Marcuse is read by theorists and Donovan is read by students of the Chicago School. The long-ignored and politically incorrect Cox is being retrospectively repackaged as an important predecessor for today's African-American sociologists. And each of these communities – theory, Chicago School, studies of domination – reaches beyond the boundaries of the United States.

In terms of general visibility, Thoreau and Young are a different matter, of course. Thoreau is a household name, although not as a sociologist. And Young, although himself almost unknown, coined in his title a word (meritocracy) that is in the working vocabulary of most educated speakers of English. By choosing Thoreau and Young, Celarent wants to emphasize that ideas familiar in other contexts and usually limited to those other contexts can make sociology come alive when imported in novel ways. This appears again to be part of her rejection of the overarching ideology of internal cumulativity, in particular her rejection of the common argument that the cumulative increase of knowledge requires specialization to deal with overload. There is something about specialization that Celarent does not like, a point we do well to remember when we wonder whether national sociologies might not be imagined as “sub-communities” of a “comprehensive” global sociology.

By her choices of writers, Celarent also seems to be saying that “American” sociology is not really national. Of her six writers, only Donovan, Thoreau, and Berelson were Americans by birth. Young was English, Cox Trinidadian, and Marcuse German. Metropolitan sociology might therefore be less nationalistic than we sometimes think. After all, the mid-century Americans ignored their own William James, George Herbert Mead, and John Dewey to borrow their social thought from Weber and Durkheim. To be sure, the vast majority of American sociology professors are today Americans. But this American majority is steadily declining as American graduate programs and faculties absorb more and more students and scholars from abroad. So even on the narrow argument, Celarent is probably correct that metropolitan sociology is more international than we sometimes think.

In summary, we learn from Celarent’s metropolitan selections the following lessons. First, metropolitan sociology is much more than simply what metropolitan sociology professors do. Second, much of the intellectual content of metropolitan sociology comes from other intellectual venues in any case. Third, metropolitan sociology is in part constituted of sub-communities which have strong links across national boundaries. Fourth, there is something worrisome about the idea of “specialization” with its implicit claim that “sub-communities” are nested within a “larger” and possibly “national” sociology or sociological tradition.

In her second and third years, Celarent turned back – as she put it – to the rest of the world. Here her message is first and foremost one of almost overwhelming diversity.

One can see this diversity first by viewing her subjects in terms of continents and nations: South Americans Gilberto Freyre from Brazil and Domingo Faustino Sarmiento from Argentina; Caribbean Frantz Fanon from Martinique; Africans Jomo Kenyatta from Kenya and Mariama Bâ from Senegal; Asians Ziya Gökalp from Turkey, Ali Shari’ati from Iran, Govind Ghurye, Pandita Ramabai, and M. N. Srinivas from India, and Qu Tongzu and Chen Da from China. Or one could equally divide them by periods: Sarmiento and Ramabai from the nineteenth century; Gökalp from the turn of the twentieth; Freyre, Chen, Kenyatta, and Ghurye from the interwar; Qu and Fanon from the immediate postwar; and Bâ,

Shari'ati, and Srinivas from the later postwar. Or by politics and status: Freyre and Qu the aristocrats, Srinivas and Ghurye the Brahmins, Ramabai and Bâ the feminists, Shari'ati and Fanon the radicals, Gökalp and Sarmiento the middle class reformers, Kenyatta the revolutionary, Chen the scholar. As for religion, there are religious Muslims (Shari'ati and Bâ) by contrast with the secular Muslim Gökalp. There are Ghurye and Srinivas – the high-caste Hindus – by contrast with Ramabai the high-caste Hindu turned evangelical Christian. Occupationally, there are three sociology professors (Ghurye, Srinivas, and Chen), one historical sociologist (Qu), and one independent scholar (Freyre), but the other seven include a teacher/novelist (Bâ), two heads of state (Kenyatta and Sarmiento), and four writers whom we might characterize as activists and public intellectuals (Fanon, Shari'ati, Gökalp, and Ramabai). These various dimensions of difference crosscut the group in many ways. Those close in one way are separated in others.

From this wild variety we can see that Celarent imagines sociology not as a building placed in a specific intellectual location, with a specific intellectual design, ever more perfect and aspiring to a cumulative grandeur, but as a specific intellectual crossroads where ideas of varying kinds come together. There are indeed some long-term tenants of stores at this crossroads – the sociology professors – but they do much or most of their real intellectual trade with other kinds of people, who are passing through on the way to other things.

Not surprisingly for those found at a crossroads, these authors were all formidable linguists. Most of them knew three or more languages; indeed, half of them wrote books in English although it was not their birth language. Some of them were extraordinarily cosmopolitan with respect to language: Chen's footnotes range across Chinese, English, Dutch, French, German, Spanish and Japanese sources, and Ramabai knew – at a minimum – Sanskrit, Marathi, Kannada, Hindi, English, Ancient Greek, and Hebrew.

All this linguistic mastery of course bespeaks itineracy: Celarent's authors were all travelers. Some of them found education abroad: Kenyatta, Chen, Fanon, Shari'ati, Srinivas. Others went abroad through exile, like Freyre and Sarmiento. Still others sought new worlds, like Ramabai and Fanon. Of all these writers, only Bâ never spent an extended period abroad. But the multiplicity of languages and cultures was often a fact at home as well. Some of these writers grew up in communities of mixed language and ethnicity, like Gökalp in Eastern Anatolia, where he had studied Turkish, Kurdish, Arabic, Persian, and French by his mid-teens, and of course many – like Bâ – grew up in imperial settings where bilingualism in the local and imperial languages was necessary to everyday life: Kenyatta, Ghurye, Srinivas, Ramabai, and Bâ are examples.

We see then that Celarent's writers are diverse people in terms of origin, religion, period, profession, and politics. They are people who traveled much both in person and in thought. They knew difference at first hand through difference of language, most of them being multilingual almost to a fault.

They were, finally, nearly all people whose big ideas meant that they lived big lives, lives of triumph and tragedy. Of all these, only Ghurye and Srinivas had

stably unfolding careers as respected academics, and, even then, Ghurye's record in academic politics is something of a police blotter. What do I mean by triumph and tragedy? Here is the record: Freyre was early exiled and later watched his work and fortunes ebb and flow with the changes of academic and public politics. Kenyatta went from activism to scholarship to radicalism, then to a concentration camp, from which he was released to a revered presidency. Qu left his country for a successful career as a Western academic, then returned to be permanently rusticated during the Cultural Revolution. Chen fell from eminence when Marxist social science replaced Chinese sociology and was never rehabilitated. Shari'ati bounced between teaching, scholarship, radicalism, and jail, and died in mysterious circumstances, probably assassinated. Gökalp went from provincial dilettantism to Young Turk eminence to a war crimes trial, exile, and return to the provinces. Sarmiento varied between reformism, exile, political failure, and overseas triumphs, ending up as a respected president and elder statesman. Fanon escaped French racism only to find Algerians treating him as the hated imperial oppressor. Like Bâ, he died early of cancer. And Ramabai's is in many ways the most extraordinary of these stories: She began life wandering India as an itinerant reciter of the *purāṇas*. After watching her family die of starvation she wound up in Calcutta, triumphing as a prodigy of learning. Then she gradually left Hinduism for Christianity and tradition for feminism, and traveled the West raising money for relief of Hindu widows, in part by writing a book on them in English, her fifth language. On her return to India she wrote a brilliant book about the United States in Marathi, founded a series of relief institutions over strong local opposition, and finished life running those institutions and translating the Bible into Marathi from original languages, which she learned for the purpose.

These are not academic lives, and the books those lives produced were in many cases written for non-academic reasons – sometimes as propaganda exercises, sometimes as part of making a nation, sometimes as part of unmaking a nation. But Celarent sees in them all the central issues of imagining society. Although these men and women were enmeshed in active life, they were, at heart, intellectuals and writers. Their books are worth reading today because they bespeak that great imagination.

Celarent's subject choices outside the metropolis thus underscore the same lessons of her metropolitan choices. Again, sociology is more than just "what sociology professors do", although the professors' activity is again one central lineage of the sociological enterprise. Second, much of the intellectual content of sociology comes from diversity and range of life experience, not only in one's society but beyond it. One further fact about her non-metropolitan writers is that they are nearly all deeply passionate about their topics. Rereading the metropolitan list with that fact in mind, one sees that Celarent found passion there as well: Young the polymath reformer, Thoreau the ardent naturalist, Cox the angry outsider, Marcuse the theoretical revolutionary. Even the quietly feminist Donovan had the same intense commitment as did her non-metropolitan counterparts. Indeed, even Berelson and Steiner were deeply passionate – about the project of cumulative social science.

Celarent is not then a fan of dry professionalism. And she has a passion for diversity, for reading men and women from different places, different backgrounds, different occupations, different life experiences, different politics: all of whom share a passionate commitment to the imagining of social life. Celarent's own vision is a global one because it embraces such diverse particulars and seemingly embraces them for their particularity – one might even say for their peculiarity. Perhaps if we now turn to the central themes she describes in these various texts we will find whether she thinks that a global imagining of the social will or should take a view that is in some way national.

Central Themes

Celarent's essays and their subjects can be analysed under two general headings. The first of these is form: what are the forms of these books? What are their rhetorical structures? What are their devices as writing and as theory? The second heading is the relation of universal and particular: how do these works think about the differences between people? How do they conceptualize the universal? What claims do they make for the possibility of universalism? And in this vision of universal and particular, do they themselves explicitly claim or implicitly show a national style?

Form

I begin with the formal structure of these works because form relates directly to the question of cumulation already raised. If we envision works as embedded in a transnationally cumulating discipline, then we expect the emergence of common organizing principles: general theoretical works, partial empirical tests, reformulations, and so on. But if, as Celarent seems to argue, we can expect only local and temporary cumulation (which we must then read through and behind to find some deeper and quite orthogonal set of categories), then we may find different kinds of forms relating in more diverse ways. And of course, we will be particularly interested in whether there is some national or regional quality to the forms chosen by her writers.

In Celarent's reviews, the transcendent cumulative project is represented by the widely-cited Berelson and Steiner volume, entitled *Human Behavior: an Inventory of the Scientific Findings*. Published in 1964, this book imagines a truly cumulative social science, based largely on experiments and surveys, with an occasional inductive generalization from case studies. This cumulative social science progresses by inductively subsuming previous findings under more general laws, as well as by the Popperian process of conjectures and refutations. In such an analysis, whatever does not fit the proposed standards disappears from view, and indeed the interested reader will find that most of what was actually published in American sociology journals at the time does indeed disappear in

this book: neither Alvin Gouldner's article on reciprocity nor Howard Becker's on marijuana use appears in the book, although each has been cited more than 1,500 times since publication.

Celarent's review argues *per contra* that such cumulation as we do observe is local and temporary. Many or even most of Berelson and Steiner's "truths" were dismissed by later social science. Those that survived were simple – almost tautological – facts. They do not concern matters of grand human interest, but tiny fractions of human behavior; not people, but parts of people; not organizations, but particular events in particular kinds of organizations. The book attends mostly to then-dominant paradigms like psychoanalysis, modernization, and behaviorism. But while all three showed internal cumulation, all three were soon set aside for other – and equally temporary – internally cumulating paradigms.

One cannot imagine anything further from Berelson and Steiner's book than Thoreau's *Walden*. Yet Celarent argues that the Thoreau of *Walden* is a social theorist, at once analyzing both individual and society as a whole. Thoreau's analysis of the essential aspects of life is as rigorous as any twentieth-century functionalism, and his analysis of the agricultural year is very close to that of Srinivas the professional sociologist 120 years later. His analysis of action, Celarent argues, is more profound than Weber's because his concept of living deliberately rests on common experience, not lawyerly abstraction. Indeed, what opposes him most completely to Berelson and Steiner is precisely this refusal of the kind of abstraction that is necessary within the standard understanding of cumulation. The world of Thoreau is a world of concrete particulars: one man, one pond, one field, one cabin. "Abstraction" here takes the form of intensifying the particular, as if one could directly find the universal, the human, by a sufficiently close contemplation of one example. Nothing could be further from the world of experiments and variables that we find in Berelson and Steiner.

In formal terms, most of the Celarent writers are closer to Thoreau than to Berelson and Steiner. Some take up the biographical approach that we find in Thoreau. Bâ writes about the memories of a Muslim widow as she reappraises her past life during the obligatory mourning period for her deceased husband. Sarmiento writes of the spectacular rise and fall of his anti-hero Juan Facundo Quiroga. Even Chen pursues biography, albeit collective biography, seeking the various avenues and adaptations by which millions of Chinese journeyed across the *Nan Yang* to work and flourish abroad. Similarly, Ramabai's book on Hindu widows examines the position and inevitable biography of a type of person, as do the earlier sections of Sarmiento, with their typology of gauchos.

Other works focus not on a particular person or type of persons but a particular place or type of places – not Thoreau as man, but *Walden* as pond. Here we find the ethnographies: Srinivas's intense lyric about the village of Rampura remembered after 20 years, Kenyatta's political ethnography of his own tribe, Donovan's studies of waitressing, retail selling, and school teaching, and Chen's painstaking analysis of three communities from which emigrants depart. Like *Walden*, these works all cover the functional necessities of life, and strikingly, most, like Thoreau, insist on

the essential relation of humans to nature. One might even place in this category Qu's *Law and Society in Traditional China*. Quoting cases from thousands of years apart, Qu finds the continuities and constancies that make of classical China – in his eyes at least – one single, great place and moment, just as Rampura in 1948 is one place and moment for Srinivas. Interestingly, Qu's other book – on *Local Administration under the Ch'ing* – takes the other, more biographical approach, dissolving the Qing bureaucracy into the collective biographies of dozens of types of actors, each with its own complex forms of development.

All these works are thus formally quite close to Thoreau, and like him try to recreate a moment or a place or a person or a type in all its essential and quite particular complexity.

The other form among Celarent's writers is the work that confronts a great historical change or process. Hence Fanon tries to capture the epochal event that was the Algerian revolution. Sarmiento chronicles the warfare between what he calls "civilization and barbarism" in Argentina. Ghurye studies the endless permutations of caste and race in India's long history. Freyre celebrates the long and passionate story of race, power, and sensuality in Brazil. Each of these analyses an enormous but particular historical process, yet in terms that while not universal nonetheless invite comparison, critique and development with respect to other cases.

Cox's analysis of *Caste, Class, and Race* is however different. Its attempt at formal theory brings us back towards Berelson and Steiner, for theorizing in the sociological mode pulls Cox away from the particularity that dominates nearly all the other works. His universal categories and his eclectic Marxism draw us toward an abstract universalism, away from the universalism of a particular biographical type – like Chen's migrants or Ramabai's young widows – or of a concrete historical unit like Srinivas's village or Kenyatta's tribe. It is a universalism whose entities exist only in the theoretical world of abstractions: proletarians, exploited races, ruling classes, and so on. Despite his political distance from Berelson and Steiner, he is the closest to them in formal terms.

Interestingly, the two fantasies on Celarent's list – Young's *Rise of Meritocracy* and Marcuse's *Eros and Civilization* – return us towards concreteness and particularity. It is difficult to imagine the future purely in universal abstractions. Both works – Young more successfully to be sure – therefore sketch a particular kind of future with particular practices, and if Marcuse's book has a weakness that weakness lies in its failure fully to concretize its vision.

Turning to national styles, we see, however, that Celarent finds no particular association between forms of writing and national or regional traditions. In formal terms, Qu shares more with Ghurye than he does with Chen. Srinivas shares the ethnographic form with Kenyatta and his literary stylistics with Bâ, but Bâ and Kenyatta's common African-ness amounts to very little in formal terms. Ghurye, Srinivas, and Ramabai were all Brahmins, but while there are a number of attitudinal similarities, their forms of writing are quite different. It is true, to be sure, that Sarmiento and Freyre's books share a grandiosity and sweep that seems recognizably Latin American, but one has only to recall Gökalp to see that the

common factor here is not the particular nation or region, but rather the project of nation-building itself.

In short, Celarent finds no association between forms and nations or regions; the forms of social analysis seem to be a common heritage, bent in particular ways in particular places, but available to all. If there is a common quality to form, it has to do with moments in history: the grand narratives tend to coincide with nation-building moments, while the ethnographies and collective biographies tell the emergence of other solidarities. It is then not nation itself that matters, but the prominence of nation-building over other kinds of group creation.

One final point about the form of these books: We find in many of these works a powerful and self-consciously literary tone. This is obvious in the writers Thoreau and Bâ, of course, as it is in Young, who also writes in the specifically literary form of science fiction. But many others show the same ambition. Sarmineto and Freyre are both self-consciously grandiose in their writing, and indeed Sarmiento's book is widely credited with having spawned the genre of dictator novels. Or again, a substantial amount of Gökalp's oeuvre consists not of social science but of poetry – he was manufacturing Turkishness – and Srinivas clearly learned much from his close friend R.K. Narayan, whose fictional town of Malgudi is a thinly disguised portrait of the Mysore in which they were both born.

Does Celarent mean by this that a global sociology ought to be literary? I do not think so. But she seems to think that social analyses of great power are likely to be passionately written and that passionate writing will be self-consciously excellent. The divorce that has arisen between sociology and excellent writing in metropolitan work seems to be rejected in much of the rest of the world. Moreover, this linkage seems to exist in the biographical approaches as well as the lyrical ones and the narratively structured ones. It is perhaps the focus on concrete particulars that drives this literary quality. It is difficult to wax poetic about abstract theory.

The Universal and the Particular

As this discussion suggests, surely the dominant theme of Celarent's books and reviews is that of the universal and the particular. We have already seen that her choices often fall on works about complex particulars: groups, types, individuals, and so on. We must now see how she views the relation between nationality and other forms of difference in global sociology.

It is useful to begin with an observation. National and regional difference occupied a privileged place in midcentury metropolitan sociology, as they did in popular consciousness throughout much of the world in that period. There were thought to be two different kinds of differences between humans. First were the varying properties of individuals in a society – ethnicity, gender, race, religion, class. Second were those differences related to the difference *between* societies: nationalism, language, and so on. Just as in the analysis of variance we speak of variance within and between groups, so mid-century social thought recognized

differences within and between societies, and by the word “societies” it meant – in fact – nations. The one kind of difference was nested within the other. But as we shall see, while Celarent’s authors take both kinds of difference seriously, they do not so nest them.

That said, a first insight from Celarent’s collection is that a surprising amount of social analysis has been organized around the task of inventing the nation. Gökalp is the obvious example here, using Durkheimian sociology as a blueprint for creating an ethnic Turkey out of the ruins of the Ottoman Empire. As Celarent notes, Gökalp is explicitly concerned with rejecting those solidarities that reach beyond the nation (such as religion) precisely he thinks that they cannot be strong enough to give meaning to life. Fanon’s position is much the same. Shari’ati by contrast seeks to found a new nation on a return to religious commitment, but evades Gökalp’s problem because the particular form of Islam he supports is coextensive with the boundaries of the nation he wishes to create. Sarmiento too is concerned with defining a nation, but his Argentina is poised precariously between the gaucho’s authentic and spontaneous amoralism and the middle class’s progressive but colorless self-control. By contrast, Freyre too is interested in nation-building but he almost ignores the middle class, focusing only on the extremes. Freyre’s focal concern about the nation is race. He aims to submerge the races in a stew of miscegenation in order to found a composite Brazilian identity. Indeed, one can find nationalism throughout all of these works in one way or another: Kenyatta over-identifying his nation with his tribe (an idea that would divide Kenyan politics for decades); Qu taking Chinese unity so thoroughly for granted that he has no problem thinking China’s civilization to be completely continuous over thousands of years; Srinivas studying villages as a way of helping create the new and independent India.

But of all these writers, only Gökalp and Fanon are primarily concerned with national as *against* other differences. The others invoke national differences, speak them, even create them but national differences are only one among their many interests and if they loom large it is merely because the problem of nationalism loomed large in the times of these writers, not because it was their central concern. They are often concerned with examining or even creating a difference, to be sure. But this difference is seldom a national one. For Bâ and Ramabai it is gender, for Shari’ati religion, for Chen immigration, for Cox and Freyre race, for Sarmiento class.

A similarly pervasive (but seldom central) theme is the great regional difference between the metropolis and the non-metropolis. Thus when Ramabai holds up to her countryman the example of a new nation, her interest in the United States is chiefly that it has thrown off the imperial rule of Britain. With only one exception, all of Celarent’s non-metropolitan writers spend substantial time on relations with the metropolis. One strand of this work is resolutely negative: Kenyatta’s sly folktales about the British, Fanon’s dramatic rage, or Shari’ati’s witty and occasionally illogical dismissal of Western philosophy. But another strand of it is quite positive. Freyre’s Brazil inherits Portugal’s interstitial status between Christian and Moorish culture, Sarmiento takes the metropolis to be the epicenter

of middle-class enlightenment, and even Ramabai believed the British position about *sati* to be correct, although perhaps for the wrong reasons and without understanding the problems raised by suppression. And the view of the metropolis is sometimes differentiated and subtle. Ghurye notes that many British census officers knew perfectly well that counting caste membership was absurd. Chen treats metropolitan restrictions on overseas Chinese as having ironically been the origin of overseas Chinese power (because restrictions against landowning drove the overseas Chinese into their control of commerce).

Bâ alone does not mention the metropole, which appears in her novel only as a place to which an excessively oppressed Senegalese woman might want to escape. That Bâ explores a level of experience at which metropolitan relations do not matter seems an important caveat. It can be all too easy to think that a global sociology must focus completely on the relation between the metropolis and the rest. Bâ reminds us that such a focus itself overrates the metropolis.

We see then that Celarent's writers take nation and nationalism seriously, as they do relations between their home societies and the expanding European empires. But they have many other foci as well, and they are willing to devote their attention elsewhere. Nationalism is an important particularity for them, but not the only one and not the enviroing one.

More often, the particularities that concern Celarent's authors are other differences. They write of race, gender, class, religion. All of these, at least potentially, reach across national borders.

Gender is well and truly investigated. Bâ, Ramabai, and Donovan write at length and explicitly about women's social position. Donovan, Srinivas, Marcuse, Chen, and Freyre write at length about sexuality and sexual activity. Both Thoreau and Sarmiento write about gender ideologies, and indeed just as Gökalp wrote articles to create a new nation, they wrote books to create a new masculinity, as Bâ and Donovan did to create a new femininity.

As for class, it is perhaps less effectively analysed here. Many of these writers come from the new middle classes of the non-metropolitan societies, and were caught up in the politics of nationalism and imperialism more than in the complexities of internal class politics. (Perhaps the turn to concerns of nationalism and imperialism enabled them to conduct class politics by other means.) As might be expected, the Latin Americans have the most differentiated class analyses. Sarmiento is very clear about the middle classes and the rural world that opposes them, although he ignores the rise of the new capitalist agriculture of grazing. By contrast, Freyre is an aristocrat to his fingertips, and his analysis has the clear eye of one whose class is doomed. Chen's analysis concerns the creation of an immigrant middle class and its impact on the sending villages back home, while Bâ's is an analysis of middle-class Muslim life in itself. So there are a variety of views of class, but one would not conclude from these works that class dynamics was the governing logic of modern social life. It is important for most of Celarent's writers, but it is central only for Cox.

There are also a variety of views of a variety of religions. Gökalp, Bâ, and Shari'ati give us secular, Sufi, and Shi'i Islam respectively. And Ghurye gives us the long history of Brahmanism, while Srinivas by contrast gives us the lived religion of an Indian village at a given moment. But other than Freyre's picture of the sensual and intensive Catholicism of old Brazil, we get very little sense of Christianity. It is true that Ramabai's life story is an epic of conversion, but the works of Ramabai that Celarent reads do not show us the forces that made that epic.

Race by contrast is a crucial topic, yet sometimes decentered. Race is central for Kenyatta and Chen. Yet for Cox, race difference is swept into class difference. For Freyre it vanishes in a haze of miscegenation. For Ghurye it is submerged under the endless churning of blurring castes and Brahman re-clarification.

Celarent's choices thus show that she does not expect there to be one, systematic theory of race or religion or class or gender, as indeed she does not expect there to be one view of nations and nationalism. None of these dimensions of difference seems primary for her, and none can withstand the swirling variety imposed by the other differences. Once again, she seems to take the world as always constituted of complex particulars, even if one or another dimension of differences seems to emerge as dominant in one time or place, as did nationalism worldwide in the twentieth century. Thus, her reviews always raise questions about the more single-minded of her writers: about Shari'ati's desire for religion to drive all social life, about Gökalp's and Fanon's failure to see the dangers in nationalism, about Cox's often extravagant Marxism. It is essential – even good – to build and develop one's group, but not to the point of destroying difference. The same, she seems to be saying, is true of sociological theories as well.

Implicit in the idea of complex particulars, moreover, is the notion that these differences can never be truly dissociated. Thus, if Celarent questions single-dimensional positions, she has also directly questioned the universalist position implicit in the mid-century social science we see in Berelson and Steiner. The conception of universalism employed there arises in classical liberal political theory. The liberal, universal world is not constituted of complex people, but rather of *tabula rasa* human beings, generic "individuals" or "citizens", to whom are added certain identifying properties. They are male or female, they are white or black or colored, they are Muslim or Christian or Hindu. For some purposes, they may indeed be some combination of these things, but in any given argument about them, we allow ourselves to be concerned only with one particular property or set of properties.

Celarent does not find this social ontology compelling. In several reviews she has questioned the pure liberal ideal precisely because although it is universal, it has no content. And no human being or group lacks specific, particular content. We cannot imagine that content as something extra, added onto mere existence. Humans are never merely existent and they are never merely citizens, but always a hundred other things beside. Between complex particulars, Celarent has argued, there can only be translation, not simple equivalence.

Celarent's position seems here to derive from one of her writers – Ramabai. For Ramabai gave an implicit theory of difference. In speaking of the many forms

of difference she observed in the United States, she used the same Marathi word to refer to them all – *jati*. In common usage, *jati* is the word for sub-caste, a local endogamous group. But Ramabai used it to mean what in English would be called “kind” or “character”. For her there are gender *jati*, in the sense of women as a group or men as a group. There are ethnic *jati* and racial *jati*. By so doing, Ramabai conceptualizes difference as a pervasive quality of humans and yokes the many kinds of difference under one concept. And her implicit ideal relation between them – which underlies her somewhat romantic view of the United States – is of translation, appropriately enough for a woman who probably knew ten or more languages.

Conclusion

Celarent’s reviews thus provide a useful foundation for considering my central questions: Do these works betray any clear national biases and should we expect those national biases to concretize into “national sociologies” of some sort? It seems that just as Celarent’s authors find in their different nations different ways to write and different things to write about, so also the growing sociologies in their different nations will find many ways to write and many things to write about. Celarent’s authors are a representative if very small sample of the possibilities of national sociologies, and what they tell us is that aside from a few occasional family resemblances (particularly in work that is on the topic of nation-building itself), great social analysts of diverse countries wrote in a variety of formats about a variety of topics. The circumstances of a nation tend to push it in a particular direction at a particular time. If other nations have similar experiences, we may find similar works there. Or we may not. While it is true that a nation’s geopolitical position, educational system, and culture (cultures?) cannot but influence how its sociology evolves, it is also true that national bodies of sociology will contain enough internal division to provide very strong cross-national ties along other lines; there are women everywhere and middle class people everywhere and religious people everywhere and so on. One could envision national styles of sociology, perhaps, or national emphases. But truly great social analysis will always engage topics that, like nationalism itself, are parts of human experience more generally and hence can be translatable into other styles and other emphases. And, finally, to the extent that there do emerge strongly national qualities to sociologies, they seem likely to pass with time: one has only to think of French sociology pre- and post-Bourdieu, Parsonianism in America, and so on.

Professor Celarent seems dubious, then, of the whole idea of national sociology. Rather, her vision of a global sociology is one that is characterized in the main by the tolerant juxtaposition of particular scholars and works across a wide range of different styles, nations, and interests. She does not pursue a targeted selection of works, aiming to find a “Latin American position” on nationalism, or an “Islamic position” on religion and society, and so on. She does not aim at abstraction of

the scientific, cumulating sort: “how do the various nations view race” and so on. Nor does she aim at replacing the variety of dimensions of difference – class, nation, gender, religion, and so on – with one dominant dimension. Rather she seeks to juxtapose important and passionate works from highly particular people in highly particular circumstances and read them for their themes and resonances. It is a process that places translation ahead of systematization, precisely because it finds systematization to be ultimately vacuous. Her aim is not to overcome the many diversities, but to embrace them in a systematic way that in turn makes our own work fruitful – not because it becomes broader or more abstract and universal, but because it evolves towards a more tolerant particularity. The view from Atlantis is then emphatically not a view from the nowhere of scientific or political abstraction. It is a view from a tolerant but very particular place. But of those particular qualities, I know nothing at all.

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