Quaderni di Teoria Sociale 1.2023
ISSN (online) 2724-0991 | DOI: 10.57611/qts.v2i1.242
www.riviste.morlacchilibri.com/index.php/quaderniditeoriasociale

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Sociology of Structures, Structures of Sociology. An Interview with Charles Crothers

Introduction

Charles Crothers is Emeritus Professor of Sociology at Auckland University of Technology (AUT), New Zealand. He was born in Christchurch, NZ, but grew up in small seaside town Tauranga, attending a multi-disciplinary social science degree at the brand-new University of Waikato. He then switched Sociology as a discipline, completed his Ph.D. at Victoria University of Wellington and followed up with several years as a junior lecturer at VUW. A further 5 years with the research section of the "Town and Country Planning Division" at the Ministry of Works and Development followed, providing a very useful platform for policy work and data analysis. He then spent a decade teaching sociology at the University of Auckland and half a decade at the University of Natal, Durban (South Africa), with the final two decades back in Auckland, lecturing at AUT. He now continues as a Senior Research Associate of the Department of Sociology, University of Johannesburg, South Africa. Throughout his career, Crothers has experienced many sabbatical periods around the world: at the University of Oregon, Waterloo Canada, Kent at Canterbury and Sussex, with the most recent at Sapienza-University of Rome in 2022. From this vantage point, mixing NZ's peculiar perspective and a variety of stays abroad, Crothers' position in the field of sociology is fairly unique, at least socio-geographically.

His career-long interests have lain with the analysis of social structures (and methods for doing so) and, hence, with Robert K. Merton, one of the leading social structural theorists, and finally how Merton's work was set within that of the Columbia Tradition and more broadly the structuralist-functionalist school. Although these themes have been quite continuous, they have varied by context,

such as settler societies and concerning linking theory and methods, as well as the connection of pure and applied sociology. Without a particular mainstream specialty, his focus turned to what might be thought of an empirical version of structural functionalism ("quantitative ethnography", one could say in Bourdieusian parlance): the examination of social patterns of the social structure, and how these relate to patterns in the cultural structure.

Further, his participation in the discipline of Sociology has been important (partly as a good unionist): he has served as President or other executive role in both the NZ Sociology Association and within the International Sociological Association, also editing journals and book series.

This interview arose from the topics discussed during two seminars delivered to postgraduate students at the Department of Communication and Social Research at Sapienza-University of Rome (October/November 2022). Two main topics are constantly (although diversely) discussed in the following pages: the sociological analysis of social structures and the particular structures that inform the field of sociology – their preponderance in this interview justifies the somehow Debordian title we have chosen.

I would like to start with some self-analytical questions: what was the state of academia in New Zealand (as well as of sociology) when you entered it? How did you get into sociology in the first place? Can you sociologically explain the beginning of your intellectual trajectory? Did you go through any kind of watershed moment?

I enrolled for a Bachelor's in social science (BSS) at the brand-new University of Waikato in 1966 – although, as it happened, exiting earlier than planned, I graduated with a run-of-the-mill Bachelor of Arts (BA) in 1968.

The late 1960s was an era of some change in New Zealand higher education: the federal University of NZ had not long broken up, with each of its constituent Colleges becoming stand-alone Universities (7 of them). The University of Waikato (hosted by a provincial city – Hamilton – with some 100,000 population) was a 'greenfield site' university (emulating UK developments such as the University of Sussex.) The older style NZ curriculum which required 9 units to

be gained over at least 3 years, and with a foreign language requirement, was being dropped, and there were some stirrings of locally-orientated material being included in studies. I was attracted by the notions of a planned and structured generic social science degree which was being offered – but it was also the closest university to my hometown (Tauranga). The integrated program included philosophy, geography, sociology, economics, history, and psychology, although my main specialty (given that sociology was still a minor taught by a single staff member) was geography, which was my main subject of interest at High School. (Incidentally, geography is something of an Anglo-Saxon discipline with concern for the environment and its social and physical interactions and how these cluster within identifiable "regions", and certainly then, and for decades after, was resolutely empiricist in tone and often quite collectivist in its social arrangements, for example by classes taking "field trips". I think the division of the social sciences into an array of highly overlapping disciplines sharing the same theorists and the same social research methods is ridiculous and perhaps deleterious – while mindful of Burawoy's point that the "social disciplines" have a particular responsibility to focus on "civil society").

Although Hamilton boasted little by way of cultural institutions, there was a procreative Hamilton bookshop (which was also involved in book publishing) which invested in the opportunities opened up through the town's new university by stocking up some relevant books. One was Merton's *Social Theory and Social Structure* (1957 edition!) which I eagerly bought and read. A while later Merton's essay on the 'Self Fulfilling Prophecy' was discussed in class and a classmate read and praised this essay which led me in turn to seize on his praise as a talisman pointing me to take further interest.

However, as I was the only candidate interested in advanced academic work (for an Honors degree), the Geography academic staff persuaded me to continue elsewhere. So, I then transferred to an older NZ university (Victoria University of Wellington), which had a particularly sociologically-oriented Geography department and the most in-depth NZ Sociology Department. Exhibiting prejudice towards a graduate from the brand-spanking new Waikato University I had come from, the VUW geography department required me to revisit a 3rd Undergraduate year. But when I was interviewed by their HOD, he picked up that I really want-

ed to do sociology, so I transferred disciplines, in the following year completing a BA(Hons) in Sociology with 1st class Honors. Essays had to be written every fortnight, so I quickly learned how to become organized to produce them. A formative experience was being asked to prepare a seminar slot on the mysteries of survey data analysis, which led me to begin to grasp that particular nettle. The general approach in the department was fairly empiricist, with sociology deployed as a scientific study of "social facts". However, this was complemented by the anthropology professor who was a full-on Levi-Strauss' disciple and provided helpful insights into the then burgeoning Continental social theory.

I immediately followed up with a Ph.D., although writing a Master's might have been a very useful preparatory step. Selecting a topic struck me as difficult, so I launched a study on "The Social Context of Problem Selection" in which I interviewed NZ's academic Geographers and Educationalists. But my progress towards completion was stumbling. In the meantime, I was much engaged with Tutoring in which a key objective was to induce each (small) tutorial group to *discuss*: this experience subsequently affected my way of teaching, where I've endeavored to be interactive. I also gained much experience as an applied sociologist as I was living in an older inner-city suburb of Wellington which the Council were threatening to bulldoze and then redevelop. With the usual armory of sociology, such as surveys, I worked with a group of fellow residents to help shift the Council's goals to a much more partial redevelopment.

By this time (late 1970s), the glut of University teaching slots desperate for occupants had disappeared and permanent academic positions in Sociology in New Zealand became hard to obtain. Therefore, I turned to applied social research, joining the small research section in the Town and Country Planning Division of the Ministry of Works and Development. Being a civil servant induces a systematic "bureaucratic" mind-set and led to disciplined approach to team work and being a government social researcher reinforced an interest in social change. Moreover, I was able to firm up my growing interest in quantitative data analysis utilizing the SPSS software on the MWD computer which was the biggest in the Southern hemisphere.

After a few years at MWD, I was interviewed for a lecturer's job at the University of Auckland. At my interview, I was asked what I might do if award-

ed the position: I declared my ambition to write a book on Robert K. Merton, which I accomplished after a few more years [Crothers 1987]. I'm not quite sure, now, where that idea came from, but I recall the suggestion being favorably received. Fortunately, there was a book series on *Key Sociologists*, edited by Peter Hamilton, to which a volume on Merton could be readily added (the publisher was later taken over by Routledge which was useful as it gave the book a longer shelf-life). I plunged into decoding Merton's theoretical apparatus as it appeared to have received little attention thereto. For a sociologist at the end of the earth, the experience also served as a good test of my mettle "against" one of the first-class sociology minds of the time. Writing the book was exhilarating and I dashed off the small volume in 6 weeks while shuffling a pile of material on the dining table.

You were in touch with Merton back then, right?

I visited Merton in New York with the intention of expanding on the background of his work. However, he was interested in the draft of my book which he made comments on. On one occasion he was angry about the chapter in which my Marxian critique of his work is contained; he was upset, but I thought "Oh dear, Robert K. Merton is going ape-shit" but didn't feel defensive about it. My access was good, and I was told once that a US sociologist wouldn't go directly to Merton but would need to reach out via an intermediary. I had a strange confidence. One delightful consequence was that, about that time, Merton was awarded the first Amalfi prize and I was flown over by the *Associazione Italiana di Sociologia* to be included on the program (twice: I also offered an *intervento* which was useful as I learned how to help the translators by pausing at more appropriate points in sentences).

I sent off my draft which Peter accepted with our demur and when I asked for feedback replied that he "trusted his authors to produce good material" (!). Returning to NZ after this short visit, I rewrote some passages and sent the manuscript to be published just before Piotr Sztompka's volume was published, also in a UK published sociology series [Sztompka 1986]. In a way, that was a case of

Mertonian "independent simultaneous discovery". Luckily, the two accounts take similar stances in many respects, although (as I later wrote) I felt that trying to squeeze Merton «into a *systems* frame (Polish romanticism?) that was just» not appropriate since his central thrust tended to negate such broad systemic viewpoints.

In terms of my sociological work as a whole, a difficulty had immediately intruded which was that during my time "on the applied bench" there had been a surge of theoretical work in sociology – as opposed to my empiricist heritage – and it was not clear to me how sociologists were to carry out their work incorporating theory and research in these changed, more sophisticated, circumstances. It was the time of much Althusserian theorizing and «I remembered» with relish the oft-used refrain of the "presence of an absence". But my sociology colleagues were of little help when I asked. Nevertheless, I was now poised to continue an academic career with both sociological and applied interests and a toolkit of skills to carry these out. I mainly taught social research methods but was, otherwise, a generalist without a specific field of interest that other sociologists could recognize. Unfortunately for my interests, over the longer term I became much diverted into community orientated work partly to provide useful information and partly as training for (senior) students. NZ plunged into a recession after the 1987 crash in «the middle of as virulent neo-liberal» "reform" period and unemployment and other social ills escalated, and so research documenting the tide of rising social ills beckoned. During this period an interesting episode illustrated the need for great care in carrying out statistical research and I landed an article in the *American Journal of Sociology* [Crothers 1979]. Two NZ-based American sociologists (Webb & Collette 1977) published in *AJS* a study of prescription rates of stress-alleviative drugs as an indicator of personal stress, declaring that there were higher rates amongst rural dwellers. However, they had failed to consider the effect of the people living in the rural hinterlands surrounding the towns where the pharmacists practiced, and correcting for this evened out the effect – as subsequently confirmed by survey research. Writing a critique is one way of sometimes gaining publishing access to a very high-ranking journal.

That's a curious strategy indeed. I'd like to go back to your relationship with Robert K. Merton: what about the specific watershed represented by your encounter with his sociology? It seems that you experienced a sort of "Merton effect" that cut your intellectual life into a before and an after – he even somehow contributed to securing your initial lecturer's position. What is unique about his approach? You wrote extensively on this theme, and also recently on the necessity of reintroducing Merton's sociological framework. Is it now perceived as passé?

Alan Sica has indeed suggested Merton is in some respects passé and of course in his writings some events, terms and approaches are clearly past their "use-by" dates: but, in general to the contrary at least in my opinion, his work ages well. Maybe this is at least in part as he used essay formats in his writing and his writing style is magisterial (sometimes over the top with layered footnotes in some passages). But even if the language sometimes bears the signs of its time of writing, the underlying message still holds true.

The crucial insight is that there is more to Merton's doctrine of middle range theory and to the structural functionalism which fleshed this approach out. What I believe to be misguided «pronouncements about general social theory (as opposed to grand social theory)» – shared in large part by Merton himself – clouded his thinking. One of Merton's major doctrines involved paradigms (in a pre-Kuhnian sense): his emphasis on them indicated by several reprintings of the same material. Paradigms are problem-setting frames, «with each issue likely to be» addressed by several competing middle range theories. But middle range theories nest within paradigms. Usually, a middle range theory identifies a particular causal mechanism and provides one causal element in a multi-causal framework.

Stinchcombe (1975) had brilliantly identified the micromodel underpinning Merton's sociology, and to this it was necessary to add structural aspects, most recently opportunity structure. This provides a toolbox of analyses of various aspects of social structure and to some extent of cultural structures. Thus, Merton provides a general sociological theory [Crothers 2004] – «as he has somewhat grudgingly admitted himself».

It is a large challenge to set social analysis on a solid footing and to avoid the various traps as Giddens and Bourdieu in particular have also shown. Adroit anal-

ysis is needed to find ways of accommodating various antimonies: macro/micro; structure/agency; etc. Merton's rendition of appropriate theoretical paths through these antimonies is not particularly sophisticated, but it is a workmanlike approach and allows us to proceed. As sociology has developed as a discipline, it has generated much theoretical discussion and a vast array of empirical investigations. But it lacks a disciplinary core of understandings about the nature of social reality and how this might be fruitfully analyzed. Merton's contributions provide ways of doing both. I believe the future development of sociology would be enhanced were more sociologists to take careful notice of Merton's analytical frameworks.

You sure have been a Mertonian of the first hour, contributing a lot to dissecting, applying, and disseminating his sociological framework.

The book on Merton [Crothers 1987] was then translated into Japanese and Portuguese. Some years after, I wrote a book on the cognate topic of *Social Structure* in the parallel Routledge series of small books on Key Sociological Ideas [Crothers 1996]. Although social structure is clearly one of the crucial (if not indeed the most crucial) concepts in sociology there is no clear consensus on its characteristics, and books focused on it are only published a few times in any decade. I think my text was useful in mapping out the relevant conceptual territory. More recently, I was asked by a Routledge commissioning editor to launch a new series on somewhat-forgotten theorists by writing an updated small volume *Reintroducing Robert K. Merton* [Crothers 2021]. In this rewrite, I switched from a thematic to a decade-by-decade treatment and was able to add in writing published in the interim as well as reworking many of the passages. Over the last decades I have written more on Merton [e.g., Crothers 2004; 2011; 2020], and I honestly find it unfortunate that so many accounts of Merton fail to read studies such as mine. Even more recently, as you know very well (since it was at your suggestion), we edited an *Anthem Companion to Robert K. Merton* [Crothers and Sabetta 2022] which provided fresh insights on historical material and theoretical writings on Merton. My conviction remains firm that «sociology has overshoot the consolidated» systematic theoretical development in continues to need and that the needed foundations would be particularly enhanced by building further on Merton's work.

You just said that the concept of social structure is perhaps the most pivotal notion in sociology; 60 years ago, no one would have disagreed with this statement, either in Europe or in the US (as well as in NZ, I surmise). How have things changed in the meantime? In a seminar recently delivered at Sapienza (tellingly titled Researching Social Structures: Empirical Requirement of Structural Analyses[1]*), you argued that the number of synonyms (e.g., social organization, social system, social networks, etc.) make it difficult to identify the exact perimeter of this framework. Is it really declining, or it has merely taken on new guises?*

We need to recover more consensus around sociological understanding of social structure and a capacity to deliver good structural analyses. One key component of this must be role theory [Crothers 2022]. Yet, there is some disciplinary amnesia around the use of status-role theory, which sat along structural-functionalism as related key conceptual structures of the post-war period. Role theory had developed in the interwar period as a replacement in Sociology for "personality" and cascaded across the post-war decades until somewhat collapsing under the weight of its conceptual overelaboration by the 1990s or so. The use of the term is still widespread but as pressed into narrowly specific analyses rather than as a platform for theorizing. Indeed, role theory was swallowed by "identity" studies which very happily stressed the complementarity of subjective aspects while obviating much of the strength of «more material-based role analysis». Role analysis has got something of a bad press from a variety of quarters and I stress that the necessary theorization is "critical role theory" in which role/statuses are seen in wider structural contexts.

An important task for role theory is acting as a bridge between macro, meso and micro levels. Roles sit mainly at a meso level, and are evoked/reproduced at the micro-level from situation to situation without much variation. But in any situation the positionality "pulled down" from the meso level is often crucial: people act according to the positionality they bring with them to situations, although they add further layers of situation-specific role characteristics. At the macro-level, roles have a sometimes-murkier existence in that they are summoned up as social categories broadly threaded through society. These social categories might be mobilized (as perhaps in a general strike), but more often

1. See https://web.uniroma1.it/coris/archivionotizie/two-seminars-charles-crothers

they are but latent potentialities. Merton's concept of "opportunity structures" provides some tantalizing possibilities for understanding how roles are shaped by (and in turn shape) various resources [Merton 1995].

Earlier role theory tended to focus on analysis of the ways in which role incumbents were variously influenced by those in their status-set (e.g., how parents interrelated with the teachers of their children). Network studies which trace effects of links (e.g., friendships) amongst various roles need to be added since there is a quantitative variation in linkages. Social categories often become mobilized as more formal groups and organizations. While this level of analysis is important, role structures need to be set within wider analyses of the symbolic structuring of role systems and their material foundations (e.g., teachers' positions in relation to the education system as a whole – their symbolic construction, material assets, power, etc.) are all relevant in their dealings with parents. How particular roles collectively move up and down various hierarchies also needs to be analyzed.

A two-part analytical sequence is required: analysis of the structural positions and then how the various social characteristics of occupants are patterned, including the "career structures" of patterned movement through status-sequences (e.g., vacancy chains). Complementing the two-part sequence is a need for studies of how role structures often change when characteristics of their occupants changes.

By emphasizing the ascendancy of "role theory" in any structural understanding of social features, it seems to me that you are consonant with certain developments of the so-called "relational turn" in sociology [Dépelteau 2013] – you just cited the concept of vacancy chains [White 1970], so I'm thinking in particular of Harrison White and his mentees (Eric Leifer, Peter Bearman). Am I misguided or do you actually feel that this is one of the most promising paths for bringing structure back under the spotlight?

I think we have to be cautious in perhaps overusing the concept "structure". Structure, after all, just refers to a social form or pattern, perhaps one more sophisticated or stable, so "structural sociology" cannot refer to any *particular* approach within sociology. But I agree, relational sociology is a promising path for exploring many important aspects of social structure, and of course there are many variants which examine how people and other nodes relate to each other.

Stated fairly generally, *social reality* is a net of linkages between various nodes, involving different types of nodes (with higher-order nodes comprising clusters which can be decomposed into components). Individuals are crucial nodes as they are the ultimate actors with agency. Most sociology centers on characteristics of nodes, and apparent causal relations amongst these characteristics, such as how the social characteristics of individuals might affect their voting choices[2]. It seems to me that there is a sequence of possibilities in relational sociology:

(-) Network characteristics of individuals might be added in a study of voters and included in regression-type models of variables we are trying to explain (for example voting choice, both before and after «various other social characteristics» are included). In many instances the effect might be causally quite trivial, once major social characteristics are included: for example, a trade union-affiliated worker is more likely to vote for leftist parties (leaving aside working-class Tories!) will tend to have that preference reinforced by their social ties (who they interact with in general and those they specifically discuss politics with). But others, in more complex situations, may be more likely to be swayed by social contacts.

(-) The next step is to examine network linkages per se – removed from the social characteristics of respondents/people (or other type of node). Here, we can explore the effects of the shape of networks along the lines of Simmel's geometrical finding that some numbers and configurations have different potentials (e.g., stability) than others. And such networks can be built on top of each other in hierarchies eventually spanning the globe. As scale increases, the people (other nodes) bearing these relationships fade more and more from the focus, although macro-level nodes continue to be important (see Blau 1987a and 1987b on the – rather detached – relation of micro- and macro-sociology). We need to look more on how relations interrelate with node characteristics.

Relational sociology is also particularly valuable when sociologists access the "data clouds" of results which emerge from the analysis of "big data". Relational sociologists often can provide mathematics/statistics underpinning such large-scale investigations. This allows direct analysis of aggregates/groups – their patterns and changes.

Consideration of Harrison White's work brings in a very interesting point because relational sociology opens up a very large area of unsettled, cutting-edge

2. To give an example that goes back to early Columbia School voting studies: Eulau 1980.

sociology. The potential is important. However, I also hope that some more standardized analyses can be fairly quickly wrangled from all the cutting-edge developments. The exploratory frontier is not only exciting but needs some standardization and more consolidated development. Too often we spinoff into difference for the sake of difference instead of putting more effort into conceptual standardization and coherence, and to the careful building on shared agreed foundations[3].

More generally I would argue that for the most part the social sciences are not "in the market" for newly *discovering* things: we know much about the social configurations as we are part of them. Sociology, then, to some extent is a "monitoring" science with a heavy emphasis on description: social realities do change (usually in fairly minor ways) and the job of sociology is to track these. This requires a set of analytical tools but not necessarily a highly creative theoretically adventurous discipline. Or is this the task of applied sociology?

Another question it raises is what varieties of sociology are currently "in play". It is important for all sociologists to monitor what is happening with particular debates and concerns, but keeping track of all of the current developing areas of sociology can be difficult. We need to have (institutionalized) mechanisms which can help with this.

Speaking of monitoring different "varieties of sociology", I know that you have a project in the pipeline about mapping and charting all the different theoretical and methodological approaches that characterize sociology's landscape and its evolution through time. I have two questions in this regard: the perspective of the history of sociology (or, as it is called in Italy, "history of sociological thought") is now in such disrepute – why do you feel it is actually promising and worth researching? Also, do you have a sense of where the field is going?

The history of sociology (HOS) did fall somewhat into disrepute circa at the turn of the Millennium. The HOS section of the American Sociological Association faltered and certainly needed to be propped up by non-American practitioners and a relevant journal (*Journal of the History of Sociology*) was short

3. A similar perspective is formulated by Besbris and Khan 2017.

lived [Porter 2004]. On the other hand, the ISA's Research Committee on the history of sociology was one of the earlier formed (8th in the listings) and has steadily expanded its scope of activities. The disrepute arises from the often-poor quality historical work which was often paraded as checklists of theoretical positions without broader context or analysis of interrelations. Merton particularly drew attention to the need for history of sociology to be upgraded to bring in analysis of content through meeting the standards for academic history (albeit often carried out by sociologists since there is also a need for disciplinary knowledge). However, I think that a further step-up in terms of expertise is needed: analytical history or the application of sociology of knowledge to understand patterns in the development and utilization of sociological knowledge. Fortunately, we have a growing (but unconsolidated) literature doing just that – with several journals now carrying such studies.

However, my interest doesn't extend to older (classical) history but rather more on the contemporary and recent history; in fact, I would claim to be more concerned with the sociology of contemporary sociology although this is not well recognized as a disciplinary field. We need to survey the development of sociological knowledge, by identifying gaps, strong-points, opportunities, degree of empirical support. This, in turn, should lead sociologists to collectively gain some degree of control over the direction of their discipline. How this guidance should be provided is an important organization design issue (for example, disciplinary organizations might organize panels to discuss such issues). This provides opportunities to guide funding and other social research organizations, such peer-review has long been built into many finding mechanisms but can be provided on a more collectively shaped basis. To be clear, such discussions need to be exploratory and certainly not definitive.

Another cautionary point is that when I specify sociology above, I mean this in the widest sense. The social sciences as currently organized in the American model is poorly organized. In particular, many of the several social science disciplines (sociology, social psychology, social history, human geography, anthropology and many of the "studies") have huge areas of overlap drawing on the same (often French!) theorists, social research methods, and subject-matters. Wallerstein has argued strongly for the unity of the social sciences which Burawoy pushed back

on through arguing that sociology was focused around "civil society" (whereas the economy and polity «each had their own disciplinary» studies). This is too simplistic, but perhaps it could make sense that the three key social science disciplines are each "based" in those various sectors while also casting a perspective over the whole societal apparatuses.

The main reason for beginning with recent history of sociology is that in order to build up a more comprehensive framework for sociology requires us to return to the last point when sociology was more coherently intellectually ordered, and then trace forward the very many subsequent developments. That period was perhaps the early 1970s when so-called structural-functional analysis was flanked by emergent macro and micro sociologies. It is only by tracing forward from this earlier consensus period that we can a better idea of how the various theoretical and methodological perspectives might better fit together in the future.

You mentioned certain research committees and sections of national and international sociological associations, and indeed you have been in these circles for a while (actually, you still are, if I'm not wrong). To me, it is like a series of microcosms with solid infrastructure and their whole paraphernalia (boards, newsletters, conferences, journals, book series, etc.). What is your opinion in this regard? Are these RCs, sections, and RNs also centers of power? Are they authoritative and, if so, in which regard? Is their relevance declining or gaining momentum? Are there anecdotes you feel like sharing? Especially in Italy, the "political" dimension of the discipline has always been rather influential and compulsive [Cossu and Bortolini 2017]. Also, coming from a somehow peripheric country, your very position in these networks and systems of relationships has been fairly special, right?

The question raises two issues: (1) the relation between the structures of academic production in sociology and the associations which provide its support structures and (2) how this relationship is affected by the way the "world system" of academic production is organized.

As with other academic disciplines, the production system of sociology reflects the organization of the "world system": core, periphery, and in-between – with the added element of the domination of the English language. The core

of sociology lies in the USA and to a lesser extent in the UK and the "old British Dominions" and increasingly Europe (including collective European productive arrangements). This elevates the meetings of their national associations into key events where, inter alia, powerful sociologists perform to secure attention for their analyses, partly through participation in the regular conference sessions but also amplified through participation on panels or through key addresses. These oral presentations are, of course, trials for the subsequent publications needed to reinforce academic power. For the most part, the associational activities are mainly support structures for intellectual activities. Of course, Presidents (and other office holders) need some academic heft in order to achieve the respectability needed to acquire official office, «but this not always involves» high academic leadership. Presidents are provided with an opportunity to constitute a platform for a particular approach, and several ASA Presidents have developed programs – like "Public Sociology" [Burawoy 2005]. Amongst ISA Presidents, Wallerstein pushed for wider involvement of sociologists from around the world and for a wider range of languages to be used, whereas Sztompka was less enthusiastic (Alberto Martinelli, by the way, handled his Presidential work well).

I think ISA has a mixed record, although broadly it has been successful in providing structures which facilitate some cross-country linkages amongst sociologists. But it has failed to "fire" in terms of broad intellectual leadership. One example is that ISA Presidents have included a trail of Touraine devotes and subsequently he was often given key speaking spots: but this failed to fire. ISA also provides international publishing infrastructure through 2 key journals and a book series: the journals are useful steady providers, but the book series has largely failed to gain momentum as it has been dominated by rather dreary ISA association leaders and groups. The congresses are organized largely in terms of programs run by the 57 "research committees" (plus some related units) and these continue between congresses with newsletters and other activities. Undoubtedly, performance varies. Sometimes research committees are captured by «national groups, and this may be good or bad». RCs can be a useful platform for cooperative work.

While some sociologists do not join any RC, and some spread their interests across several, I decided to put all my eggs in the basket of RCHS: RC08 History of Sociology, although I have interests with several other RCs. My interest is a

reflexive one, the sociology of sociology: I've written several items in this area [Crothers 2010]. But sociology of sociology doesn't have a particular home and it can be best fitted under history of sociology albeit "contemporary history". So, although I have a passing interest in classical sociology, «I'm far more interested in how» we might reflect on contemporary and recent sociology to monitor and improve it. RCHS has been a good RC which has worked solidly in providing conference platforms and newsletter links. By accident I was Vice-president twice, also secretary (during which I repurposed the newsletter for e-delivery). It enabled me to keep in contact with other sociologists with similar interests, but I wouldn't claim that I'd had any particular intellectual influence on any members or that ISA members have had much influence on me.

Because ISA seems largely captured by theorists and theory-commentators, at least one major strategy for building ties amongst sociologists across the world has not been pursued. Studies carried out outside sociology have produced very considerable bodies of data, including demographic but more «relevantly attitudinal survey information (e.g., the Gallup World Poll)». Sociologists could join up with such enterprises though carrying out analyses of this data, perhaps using similar methodology, and supported by appropriate workshops and training programs. This massive free gift should be used. A huge amount of this data is available at country-level and even can be downloaded at unit record (i.e., person) level. One indication of sociology's rejection of this was the launch of results (across over 60 countries) at IPSA (not ISA) conferences. Of course, there is also a data collection, for instance books on comparative quantitative sociology (including the work of Alberto Martinelli, which I've reviewed [Crothers 2009]). I have tried to make some minor pushes to support this direction. I prepared and ISA put up on its website a listing of cross-national data sources[4]. I have also reviewed the work of several cross-national sociologists for international journals to bring their work more widely to the attention of sociologists around the world.

A subsequent issue is the need better for a more appropriate publication system so that publications from all over can be fitted in to the jigsaw: maybe some very large low-risk publications outlets.

4. See https://www.isa-sociology.org/en/opportunities/world-social-indices.

Finally, issues arise with *Sociology from the South*. As a New Zealander I feel some connection with this debate as Maori scholars are central to this[5]. My view is that we should spend less time on imagining new and more appropriate analytical tools and more on getting on with the job of using our analytical toolkit (albeit while necessarily adapting it to nonwestern circumstances) but within wider frameworks which pinpoint the impacts of colonialism.

Doesn't putting so much effort into editing and related collective activities detract from your own scholarly work?

The voluntary work supporting the discipline as a collectivity is essential to its health. I have edited a succession of journals: *NZ Population Review* (the NZ Demographic Society journal), *«Society in Transition»* (the official South African Sociological Association Journal), *«NZ sociology»* (the NZ Sociological Association journal), and *Kotuitui* (published by the Royal Society of NZ) plus stints as a newsletter editor. And now I have set up a small online journal to cover what I've seen as a gap in the line-up of NZ social science journals: the *Aotearoa NZ Journal of Social Issues.*

This is an area of service to one's discipline, but also as with much philanthropy there lurks intimations of power. This is not power wielded over hapless article submitters, although there is an aspect to this (e.g., degree of support and kindliness offered to paper submitters: some of my experiences have not been great). The areas of leverage possible when editing a journal lie particularly with commissioning special issues and also through the handling of less mainstream material such as book reviewing, "news", or even interviews. Themed special issues must be somewhat sparingly offered as otherwise the more general purposes of a journal can be subverted. However, they can be used to (quickly?) generate material in an area of sociology which might be judged to be underdeveloped (and perhaps to boost the journal more generally). The areas of NZ sociology which I considered most lacking (and therefore deserving of special issues) were

5. See for example Linda Smith's book on *decolonizing methodologies* [Smith 1999].

social class and the history of NZ sociology, but also social wellbeing which had become a major policy concern[6].

Honestly, I particularly liked being a book review editor. In many ways, for sociology books are still where arguments can be unfolded at requisite depth and appropriate evidence assembled. So, they are often important and yet they also tend to be fugitive: there is no easy way of finding out about many relevant books (although good publishers will seek to draw their books to review editors' attention.) Books can generate debate as well as linking to the book itself (some journals now publish review type material, debates, comments or methodological details of data-sets in the public domain). On the other hand, being a book review editor is annoying when someone agrees to write a review but then doesn't and merely keeps the book: this is criminal but happens too often!

In reflecting on disciplinary life, NZ allows easier reflection because there are only 50 academic sociologists out of 5 million population – compared to Italy for example, where you have some 1000 sociologists from a 60M population (although population rates are not that different). Each can learn from the other.

I'm particularly fascinated by the reflexive theme implied by "the sociology of sociology". It seems to me that this (meta-) approach is generally in disrepute among social scientists: it's perceived as navel-gazing, too indulgent and abstract, (paradoxically) unconnected to the rest of the discipline, and basically useless. What does reflexivity mean to you? And, if you would like to get a bit normative, what should it mean to other sociologists?

It depends on what sort of sociology of sociology is done and how much of it is done. I see this subfield as involving understanding sociology, seen in the broadest terms encompassing also its several competitors (e.g., political party ideologies purporting to explain aspects of the social). And the explanations must draw deeply on sociological analyses.

Interest in reflexivity goes through phases, often presaging paradigm change. Gouldner (in the particularly reflexive period as the structural-functional paradigm was being challenged) famously responded to the epigram of Merton's

6. For a more extended discussion, see Crothers 2016b.

Social Theory and Social Structure suggesting that origins needed to be forgotten by pointing out we don't know where were going until we can work out where were coming from, which I would add a more presentism twist… where we are socially placed.

We all have to engage in sociology of sociology to some extent: any responsible sociologist should be appalled at the idea that some sociologists might pursue their craft with "eyes shut", which would be utterly professionally irresponsible and perhaps unethical as it seems to be a part of a denial of responsibility for the social consequences of one's actions. Any "capstone" course for graduating sociology students should include some of this reflexive material, besides other "professional" topics such as social research ethics. But there shouldn't be too much sociology of sociology lest sociologists only gaze at their own navels which is hardly productive.

But reflexivity must be based on a high-quality foundation, so I'm arguing for higher standards in such reflexivity, and have tried to add my weight to studies which would improve and expand on the necessary scholarship.

Opening the "bad sociology" lens more widely, I think there are many areas of relatively useless sociological work which we might need to restrain. A common criticism is the methodologically esoteric data-analyses which fill the pages of a range of top-flight journals. This is a very common and perhaps lazy critique flowing from ignorance, many such papers nevertheless address extremely important and complex topics. Another is the theoretically esoteric articles which fill wide swathes of other journals «where difficulty terms» seem more a display of cleverness than concerned to deepen social understanding. In all this, I am much guided by Charles Wright Mills who kept on trying to direct the attention of sociologists to tackling the understanding central concerns of society and not get overly diverted into secondary tasks. So ongoing debates are necessary about how each national sociology is tracking against this target are necessary – Mills wrote, of course, much sociology of sociology (for an application to NZ, see Crothers 2016a).

More generally, sociologists are humans too and their cultural and social apparatus deserve an appropriate level of attention. Recently, some aspects of Italian sociology have been under superb examination in a series of papers from Akbaritabar and colleagues [Akbaritabar, Traag, Caimo and Squazzoni 2020]. And including social theory as an element itself in understandings of contemporary societies is essential. A key point made by many theorists [e.g., Coleman

1990] is that over recent decades society has changed to include more constructed entities and that sociology (in the widest sense) is not outside the social system but needs itself to be explained: there is wide consensus amongst theorists on this need for reflexivity. Contemporary societies are "knowledge societies" [Comunello, Martire, and Sabetta 2022] and that the social components of knowledge need consideration as much as other areas.

Last question. Do you have any advice (as practical as possible) for young sociologists, non-tenured ones in particular?

I think we live in a period when the odds are deeply stacked against young graduates wanting to pursue academic careers, since universities have probably bottomed out in terms of student growth, while continuing to enroll at least as many doctoral candidates and tenured positions have been eaten away by the scourge of the replacement of tenured by non-tenured staff. Moreover, the competition for vacant posts is often international which may be searing.

Strategies for securing a post are complex: an academic post is more likely to open up if you develop a visible specialism (preferably in some "hot" topic area) and start publishing early. I'm a bad example. I've never found a comfortable niche to occupy as my specialist area, which is not helpful in trying to build an academic career. However, this has allowed me to explore many areas of interest, and moreover in a small-scale sociology establishment one needs to be more generalist. One should try to write on interesting topics [Davis 1971]. A strategy I've long followed (with occasional success!) is to keep my eye out for succulent data which will enable the advancement of some ideas and shed light on controversial points: much as Newton spoke of the curiosity barely sated by picking up interesting pebbles from beaches. For a while one is the only person in the worked to access particular items and so the data-acquisition game can produce nice challenges.

Developing a clear image of sociology and how it might be best pursued in important. In my own image of sociology, I think of C.W. Mills as providing a role model for the scope of attention national sociologies should provide. Their job is to provide the commentary and analysis «of national level phenomenon» including power, social class, political economy, and civil society.

A path that some might successfully pursue is data analysis. There is an appalling lack of quantitative skills in many areas of the world (especially outside US). Yet, how amongst very large-scale societies is it possible to understand them and their dynamics without good analyses using census and survey data? Moreover, sometimes researchers gather and make available superb datasets ripe for proper analysis. But I also have a critique of quantitative sociology. Visualizations are often unreadable, and too often underlying data is not made available for other analysis. Log linear models (with their annoying reference categories) often produce rather meaningless rows of very small coefficients. This is in part as their concern was with regression slopes rather than the % of variance explained yielded by correlations (and their partials).

However, this narrowly focused approach may be at odds with a broader career strategy which might be to see "applied sociology" (e.g., working for a government research unit) as a safety-net strategy which rather requires a broad framework and the development of employable research skills, especially quantitative data analysis.

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