



History of Psychology and Social Practice:

Introduction to the Special Issue



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The origins of this special issue lie in another project that was completed last year, an edited book titled, *Internationalizing the history of psychology* (Brock, 2006a). As a historian of psychology with a special interest in global issues, particularly relating to what are sometimes called 'developing' countries, I have always thought that history of psychology has not paid sufficient attention to countries outside of North-Western Europe and North America. The aim of the volume was to try to restore some balance in this regard and the editor of this journal, Aydan Gülerce contributed a chapter on the history of psychology in Turkey to that book (Gülerce, 2006).

It was while we were working on this project that she set up the web journal, *Social Practice/Psychological Theorizing* and subsequently asked if I would be the guest editor of a special issue. Anyone who has had experience of editing a book, particularly as the sole editor, may appreciate that taking on another editorial project so soon after the last one was not the most attractive option that I could think of. However, I agreed to do it after my old friend and collaborator, Johann Louw of the University of Cape Town, agreed to share the editorial work with me and I thank him for being so generous with his time. Two things made the project attractive: 1. the format of the journal; 2. the subject of the journal and the special issue. I will deal with each one in turn.

The format of the journal

Most academics will be aware of how difficult it is to gain access to journals in their field. When I took up my present position at University College Dublin in 1995, I was the first specialist in history of psychology to be appointed to a university in Ireland. There were consequently no specialist journals in my university library. They had obviously not been considered a priority at any university in Dublin or indeed in Ireland as a whole. Changing this situation was not going to be easy. The university library had introduced a rule whereby a new journal subscription could only be taken out if an existing subscription were cancelled and a survey of my new colleagues revealed that at least one person was prepared to veto the cancellation of every journal subscription that we had.

My only option was to take out personal subscriptions to the journals concerned. In the meantime, I began to offer several undergraduate courses on the history of psychology, some at a fairly advanced level, and was supervising around three postgraduate or postdoctoral researchers at any given time. After many years of trying, I managed to persuade the university library to subscribe to one journal in the field and that was the APA journal, *History of Psychology*. Even the choice of the journal was made for me. It had to be the cheapest one. This continues to be the situation after twelve years.

If this can happen to someone in the capital city of one of the richest countries in the European Union, the problems will inevitably be much greater in poorer countries. Here the prospect of getting one's library to take out subscriptions to international journals can be remote and academic salaries when translated into dollars, pounds or euros are hardly enough to pay for personal subscriptions either. This situation does not only apply to the so-called 'developing countries'. According to a colleague in Moscow, it is exactly the same there. Researchers are heavily reliant on material that is freely available on the internet. [1]

Most readers will be aware that the standard journals have electronic versions which can be accessed on the internet but these too come at a price - usually a price that institutions and individuals in poorer countries are unable to pay. It is not unusual for a publisher to ask for the equivalent of two or three weeks' salary in some countries in order to download a single article. What is even more interesting is that the author of that article was probably not paid for it and may have had to pay the publisher for offprints of his or her own work. The charges that are made are allegedly due to 'production costs' and yet it is reasonable to wonder how much it costs to upload a file that was submitted in electronic form onto the internet.

It was therefore pleasing to see that Aydan had decided to 'cut out the middleman' and provide access to her journal at no cost. It is probably no accident that someone who lives outside Western Europe and North America should have come up with this idea. This situation is not only advantageous to the reader who pays nothing to access the work. It is

advantageous to the author. For anyone who wants to give their work maximum exposure, an electronic journal for which no subscription is required is the obvious way to do it.

Unfortunately, there are other factors at work. I knew about these factors prior to agreeing to organise the special issue but I had no idea how important they were. Here I am referring to the practice among universities to reward their employees for publications. This may be a direct reward in the form of pay or it may be more indirect in the form of research grants, sabbaticals and promotion. The term, 'publications' is usually defined in a narrow sense; that is, impressions made by ink on pieces of paper. In my own university, we are asked to provide details of our publications from time to time and it is assumed that we will be able to provide volume and page numbers for any journal articles that we list. Perhaps more significantly, the powers-that-be refer to 'reputable publishers' and I strongly suspect that a publication without a publisher, like this one, would be seen as a contradiction in terms. Even if such a 'publication' were even recognised, it would almost certainly be regarded as inferior to a publication in a traditional journal.

At the heart of the problem lies a system of assessment that might seem bizarre to those who encounter it for the first time: the publications of academics are assessed by people who have never read them. Perhaps familiarity has dulled our senses to the absurdity of the situation. It is like the food critic of a newspaper writing an evaluation of a restaurant without tasting the food or the theatre critic writing a review of a play that he or she has never seen. Perhaps the best known strategy with regard to publications is to count them. The *reductio ad absurdum* of this approach occurred not very long ago at a university in North America which appointed a professor whose publications were in a language that not one of the hiring committee could understand. They were simply impressed by the number of them. Other absurdities include attempts to rank journals in terms of 'prestige'. This ignores the fact that all journals have clearly defined areas of interest and they will not publish work that is outside those areas, irrespective of how good or bad it is. Then there is the great emphasis that is placed on 'peer review', as if the editors of journals and books were not qualified to recognise good work. Such measures can only provide a crude approximation of a person's performance and many of us will be familiar with academics who are mediocre at best but who have become highly successful by 'playing the system'.

Many academics are under pressure to publish and, when they do publish their work, they want it to 'count'. This is especially true of those at an early stage in their careers who are applying for academic positions, tenure, and/or promotion. Even senior academics can be reliant on publications for things like sabbaticals and grants. The problem is that these forms of assessment hinder the dissemination of information, which is what publications are allegedly supposed to do. This suggests, at the very least, that the dissemination of information is not their only social role. It may not be their most important social role.

All this can explain why we had such difficulty in finding contributors. For every person who agreed to contribute an article, at least six declined. By way of comparison, only one in five of the people who were invited to contribute a chapter to *Internationalizing the history of psychology* (Brock, 2006a) declined. These are rejection rates of 86% and 20% respectively. In making our invitations, we pointed out that a web journal might not be the most 'prestigious' place to publish one's work but we also referred to the advantages of having one's work freely available to all. Of the people who bothered to reply to our invitation, some were honest about preferring to publish their work in a forum where it would 'count'. This was particularly true of those who did not hold an academic position or held an academic position but had yet to receive tenure. Several respondents in more senior positions declined to participate for a variety of reasons but were also keen to point out that the nature of the journal had not played a role in their decision. Although they might live in a Skinner box, they were not concerned with pressing the bar in return for pellets of food. That may have been true in some cases but the numbers still did not add up. Both editors had been involved in a number of editorial projects before and neither had seen a rejection rate that was even remotely like this. We could smell a rat - or at least a pigeon.

[2]

I am not in any way criticising particular individuals here. For someone who is looking for his or her first academic position, or perhaps trying to get tenure, it would make no sense to publish one's work in a forum that does not count. Senior academics may be less concerned, though even here it might have a detrimental effect on their research budgets, travel to conferences and the like. The only people who may be unaffected by this situation are the independent scholars and retired people who work outside the Skinner box.

We can only hope that one day the dissemination of information and academic rewards will not be at odds. In the meantime, it is important to stress that the difficulty of finding contributors did not lead us to drop our standards. We rejected several offers of contributions. We even rejected three of the first drafts. This was mainly because they were not relevant to the theme of the special issue but one of the first drafts was explicitly rejected because it did not meet our standards of scholarship. All of the first drafts that were accepted were sent back with detailed reviews by the editors, some of which were critical in tone. My own contribution was reviewed by Kurt Danziger and Irmingard Staeuble and I would like to thank both of them for their generous and insightful feedback.

The only article that was not reviewed was that of Louw and Danziger and this was because a shorter version of the article had already been published elsewhere (Louw & Danziger, 2000). It was published in the now-defunct Dutch journal, *Psychologie en Maatschappij* (Psychology and Society) and it is republished here with the permission of the editors of that journal. The idea of republishing it was my own. I have long been a fan of the article and have cited it extensively elsewhere (Brock, 2006b). At the same time, I always thought that the limited circulation of the journal meant that it had never had the wide readership that it deserved. Even worse, the more theoretical parts of the original manuscript had been deleted on the instructions of the editor and these seemed to me to be the most interesting and significant parts. The article appears here in its entirety for the first time. It has also been revised and expanded.

The other five articles are published here for the first time.

Subject of the journal and the special issue

I mentioned earlier that my interest in organising the special issue was only partly due to its format. I am also supportive of its aims:

It deductively/inductively derives from an assumption/observation that social practice and psychological theorizing are in an ongoing co-constructive relation. In principle, it focuses on innovative works which support the idea that (psychological) theorizing is not a mere conceptual endeavor and also that anything psychological is simultaneously social, and vice versa.

Given the title of the journal - *Social Practice/ Psychological Theorizing* - it did not take a great leap of imagination to come up with a topic for a special issue, "History of Psychology and Social Practice". It is expected that the emphasis on history will make the issue 'special'; that is, different from the articles that usually appear in *SP/PT*.

Like theoretical psychology, history of psychology has suffered from an excessive 'Idealism' in relation to its focus. The history of psychology has traditionally been identified with 'the history of ideas', as if the content of modern psychology is nothing more than a series of disembodied thoughts. Thus the emphasis has been on psychologists and their ideas. Typical of this phenomenon is the traditional way of organising history around 'schools' or 'systems' of psychology: structuralism, functionalism, psychoanalysis, Gestalt psychology, behaviourism, humanistic psychology and so on. Here the emphasis is very firmly on '-isms'; that is, particular modes of thought. Terms such as 'history and philosophy', 'history and theory' and 'history and conceptual issues' are only slight variations on this theme. [3]

What all this neglects is that psychologists do not merely think, they also do. In particular, they do things to other people. They provide psychotherapy, design programmes of behaviour modification, administer intelligence and personality tests and make recommendations based on their results, conduct experiments using human and non-human subjects etc. etc. We can learn at least as much about psychology from what psychologists do as we can from what they say. Indeed, there is a piece of folk wisdom which suggests that we should ignore what people say and pay attention to what they do. While this may sometimes be useful advice, it is not the basis of the present work.

The word, 'practice' can have many meanings. It might be used by psychologists to refer to 'a private practice', as opposed to working for a hospital or some government agency. The work of Louw and Danziger (this issue) and Abma (this issue) comes closest to this definition of 'social practice' since it is concerned with the interface of psychology and society at large. Louw and Danziger tend to refer to 'psychological practice' rather than 'social practice'. This is simply a matter of emphasis. 'Psychological practices' are nothing more than the practices of psychologists. Given that these practices are not unique to one psychologist but are shared by the community of psychologists, and given that they have direct effects on other people, they are 'social practices'. One might designate them 'social practices' in both respects. Three of the articles - those of Walsh-Bowers, Greer and I - are concerned with the investigative practices of psychologists, involving experimentation, measurement and statistics. Although these practices are mainly kept within the psychological community, they are 'social practices' nonetheless. Finally, Winston's article serves as a reminder that 'discursive practice' can be a form of social practice as well. Our societies are held together by language as much as anything else and each community has a certain way of using language which serves specific social ends. This is not to be

confused with the realm of 'ideas', which has traditionally been viewed in less social terms.

There has been a noticeable shift towards the study of social practice in recent years. This has, at least to some extent, its origins in the work of Michel Foucault and it has been developed with particular relevance to the history of psychology by the British sociologist, Nikolas Rose (see Louw & Danziger, this issue). Having said that, Foucauldians do not have a monopoly on the approach. One of the innovations of Danziger's well-known book, *Constructing the Subject* (1990) is that it provided an analysis of what psychologists have traditionally called, 'methodology' from the point of view of social practice. The textbook, *A Social History of Psychology* by Jansz and van Drunen (2004) also has this particular focus.

A history can be centred on many things. It can be centred on one individual, as in biography, or on a particular institution, such as the Vienna Institute of Psychology or the Tavistock Clinic. It can also be centred on the history of psychological concepts (e.g. Danziger, 1997). No claim is being made here is that the history of social practice is the only valid approach to history, much less the only possible approach to history, but it is an approach that can shed a great deal of light on psychology and it tends to be neglected relative to the three 'I's': individuals, institutions and ideas. It is also important to realise that, although it may be useful to have a particular focus to one's history, none of these things exist in splendid isolation. The article by Greer (this issue) provides a good example of how the concepts and theories of psychology are dependent on the social practices that are used to investigate them. Thus it can be said that any history of psychology which ignores or neglects social practices is seriously incomplete and one of the aims of the special issue is to encourage this approach to historical research.

The authors

The various contributors to the special issue are all specialist historians of psychology with established reputations in the field. Kurt Danziger is one of the most eminent historians of psychology in the world. An important part of his legacy, which is often overlooked, is the PhD programme in history and theory of psychology at York University, Toronto for which he received the Canadian Psychological Association's Award for Distinguished Contributions to Education and Training in 1995. This continues to be the biggest programme of its kind in the world. Three of the contributors to this special issue - Scott Greer, Richard Walsh-Bowers and I - are graduates of that programme. Johann Louw had a similar early start in the field, having written his PhD thesis on the history of psychology at the University of Amsterdam. Ruud Abma is the current President of ESHHS (European Society for the History of the Human Sciences), while Andrew Winston holds the equivalent position with Cheiron (International Society for History of the Social and Behavioral Sciences). [4]

A common criticism of edited volumes concerns the selection of contributors and I have engaged in such criticism myself (Brock, 2005). It might be thought, for example, that the selection of contributors here is overly narrow. One striking aspect of the current selection is that it is exclusively male. Another striking aspect is that no less than four of the seven contributors have Canadian addresses. What many readers will not realise is that only one of the four, Walsh-Bowers, is Canadian. Greer and Winston are American and Danziger is German/South African. Perhaps more importantly, their articles deal with a broad range of episodes in the history of psychology in South Africa, Europe and North America.

If we take into account all of the limiting factors mentioned above - 86% of the people we approached declined to take part, the unorthodox focus on 'social practice' which limited the number of people we could invite, our insistence on having contributors with an established reputation in history of psychology and the rigorous standards that we applied to their work - it is perhaps a minor miracle that we still have enough articles left for a special issue. There was no certainly no room for any more limiting criteria to be applied.

Acknowledgements

I would like to thank Aydan Gulerce for originally inviting me to edit a special issue of *Social Practice/Psychological Theorizing* and Johann Louw for accepting the burden of sharing the editorial work with me. Thanks are also due to Kurt Danziger and Irmingard Staeuble for reading and providing feedback on my own article. I would like to say that it was 'peer reviewed' but I am reluctant to use that term in relation to such distinguished colleagues. Most of the work for the special issue was done while I spent a sabbatical at the University of Cape Town in 2006 and I would also like to thank Johann Louw and his colleagues at UCT for organising my visit and for making me so welcome during my stay. I am especially grateful to the other contributors - Ruud Abma, Scott Greer, Richard Walsh-Bowers and Andrew Winston - for their excellent contributions and for their apparent lack of concern with publishing their work in a forum that would 'count'. Finally, I am deeply grateful to everyone with an investment in this special issue for their patience in waiting for it to materialise. It should have been ready at the end of 2006 but I developed a serious illness around that time and it threw my work schedule into disarray. Everyone has been very gracious in allowing me the time to finish off the project at my own pace.

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