

Commentaries

Seventy Years of Social Psychology: A Cultural and Personal Critique

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Abstract

This paper traces some salient aspects of my research career, focusing largely on work in West Africa. From this lessons are drawn about the shortcomings of social psychology, especially in its laboratory version. It tends to tacitly ignore the effects of cultural influences, assuming that its findings are universally valid. Studies are mainly conducted with adults, generally college students, who are unrepresentative even of the general population of the United States where the bulk of social psychological studies are concentrated. This is justified in terms an alleged 'psychic unity'. Social psychology pays little attention to the processes whereby children become socialized into particular cultures, which then governs their social behaviour. Methods are usually formal, and observational ones are eschewed, so that research takes place in artificial setting. This brings me to the almost complete absence of links with cognate disciplines, notably anthropology, which could greatly enrich social psychology. Suggestions are made for more wide-ranging approaches which would overcome the aridity of a great deal of current experimental social psychological research.

Keywords: autobiography, history, weaknesses of social psychology, experimentation, West Africa, anthropology, child development

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My first encounter with psychology was at school in Vienna in the 1930s. We had a class called 'philosophy', but the teacher dealt with (mainly Freudian) psychology, which I found intriguing. My initial awareness of cultural differences was awakened by my experience of living first in Austria, then spending some years in France, before coming to England and then to Scotland. Later, after some adventures, I was serving in the British Army's Pioneer Corps. It was only in retrospect that I came to appreciate that the Corps at that time had many distinguished names, such as that of Arthur Koestler.

In 1942, having been invalided out of the army, I embarked on a university course in the social sciences. The text recommended for social psychology was the 25th edition of *An Introduction to Social Psychology* by William McDougall, first published in 1908. It was in nearly all respects totally different from current texts. Inspired by Darwinism,

its theoretical basis was that human action is determined by instincts, singly or in combination – a view then fairly common, though at the time on the verge of being questioned.

No empirical studies are reported in McDougall's text, only anecdotal illustrations. For instance, the attraction of cities as opposed to rural life is explained in terms of a postulated 'gregarious instinct'. In addition to 'instincts' McDougall (1943) also made use of the concept of 'sentiment' which refers to 'an organized system of emotional tendencies centred about the idea of an object' (p. 437). So if I have a positive sentiment about my friend F, then I shall be pleased if he does well and sad if he fails. The converse would apply to my enemy, E, and I would not be too sorry if he broke his leg. The notion of enduring sentiments seems to me a useful one, independent of 'instincts'. It overlaps to some extent with what in current social psychology is known as 'attitudes', a concept that used to be central to it.

It would be wrong to suggest that nothing happened in social psychology during the inter-war periods. Especially in the USA there was a great deal of research, as may be seen from a handbook edited by Murchison (1935). After that, two sets of important studies were carried out by Muzafer Sherif and Kurt Lewin respectively, but at the time these were not well known in Europe.

In Britain Frederic Bartlett took a very different approach to social psychology in his famous book entitled *Remembering: A Study in Experimental and Social Psychology* (Bartlett, 1932). In it he criticizes McDougall's reliance on 'instinct' as an explanatory concept. Bartlett defined social psychology as 'the systematic study of the modifications of individual experience and response due directly to membership of a group'. Provided we can agree what a 'group' is, we should be able to make progress. But then Bartlett immediately introduces complications, based on anthropological data, which show that social determination of behaviour is group-specific. This is an important consideration, often ignored by present-day social psychologists who too readily make implicit claims of universality for their generalizations, a topic to which I shall return.

In 1939 an edited book was published, intended to survey the state of the social sciences (Bartlett, Ginsberg, Lindgren, & Thouless, 1939), in which T.H. Pear had a chapter on 'Some problems and topics in contemporary Social psychology'. The methods described were: observation, self-observation, interviews, questionnaires, and tests. Topics of research listed were:

- *Effects of unemployment*. The findings of a then famous study in Austria are reported, plus similar ones in Britain.
- *Attitudes*. Here the contributions of Gordon Allport are noted, and several measures are listed, e.g. the Bogardus 'Social distance' scale.
- *Mass media (radio, television and cinema)*. Only a few studies are mentioned, most of the discussion being programmatic.
- *Other topics* were propaganda, war and conversation. The last refers to conversations overheard in the street, and had nothing to do with 'conversation analysis' as currently understood.

Generally, the meaning of 'social psychology' in Britain was then severely restricted, and experiments were almost completely absent – though they were already quite prevalent in the USA at the time.

Having completed my psychology degree, I went on to do first a Masters and then a PhD at the London School of Economics, where I also attended lectures in sociology and anthropology. I was particularly attracted to anthro-

pology, which historically had close links with psychology. For example, Pike (1870) expressed the view that 'Without psychology there is no anthropology' (p. xi) and more recently Lévi-Strauss (1962) wrote '*L'ethnologie est d'abord une psychologie*' (p. 174); for a fuller account see Jahoda (2014). I might mention in this context that I edited, together with the anthropologist Ioan Lewis, a book on comparative child development in various cultures (Jahoda & Lewis, 1988). This orientation, i.e. taking account of anthropological contributions, was not, and still is not shared by mainstream social psychologists; and I think this explains at least in part why I have remained at the margins of social psychology throughout my career.

After a period as an extra-mural lecturer at Oxford, I obtained a post in social psychology at Manchester University, which led to a turning point. Theodor Newcomb, a prominent American experimental social psychologist, came to visit. He gave an account of exciting new developments in experimental social psychology, which inspired me as well as some others. At last social psychology was really becoming scientific! Another (for me) influential visitor was Ernest Beaglehole from New Zealand, who had done both psychological and anthropological studies in the Pacific region. He advised me that in order to become a really good social psychologist, I ought to experience life in a greatly different culture. This made sense to me, and so I obtained a post in what was then still the Gold Coast (now Ghana) in West Africa. My ambition was to replicate there the new experiments described by Newcomb. It would not have been possible to do that before settling into a new and very different environment.

On the boat on the way out I met the professor of physics. It turned out that he was very interested in the work of J.B. Rhine who had a parapsychology laboratory at Duke University. What interested him most were the then recent studies of *psychokinesis*, this being the alleged capacity to influence material motion by mental effort. Rhine had tested this by throwing dice, but my future colleague regarded that as very inefficient and told me that he had a better idea; this was to set up a source of radiation and two Geiger counters labelled A and B. Subjects would be asked to concentrate on either A or B (randomly selected) and try to divert the radiation towards it. In that way a huge amount of data could be obtained rather rapidly. Since I had dabbled in ESP (extrasensory perception) before, he persuaded me to help him by running experiments.

The professor organised the set-up from the Physics Department which could not possibly be allowed nowadays, since there was no shielding whatsoever. Anyway, for two months I spent a lot of time testing. I made a point of asking each volunteer at the outset and again at the end whether they thought they would be – had been – able to change the paths of the rays. Some were highly confident, e.g. a woman had spent time in India in an ashram; another was the son of a fetish priest. At the end of the designated period I looked at the data, and neither had scored above average. However, some others scored significantly above chance. So I re-tested them, and they regressed to the mean. The computations were slow and painful, as I had to turn the handle of a calculator.

Given the novelty of the method, I wrote up the results in the hope that a joint paper might be accepted for publication in spite of the lack of significant results, but was disappointed.

After that, feeling that I had properly settled in, I decided to embark on the attempted replications of American experiments. Alas, in spite of considerable efforts I had no better luck than with psychokinesis. Since I discussed the problems in more detail elsewhere (Jahoda, 1979a), I shall confine myself to some brief remarks.

Experiments of that kind often require some quite elaborate staging, and the use of stooges who of course should not be recognized as such by the participants. In a setting where students knew each other well that was hard to

achieve. Or again, where the manipulation ended in voting to exclude someone, any effect of the manipulation was usually overridden by the preferences of students to vote for their 'brothers' from the same tribe.

It should be said that my failure was not unique, nor did the same factors operate elsewhere. When [Amir and Sharon \(1987\)](#) subsequently attempted such replications in Israel – a culture much more similar to the US than Ghana – they had very limited success, and the differences in the results were striking. All that made me somewhat disillusioned, not so much with social psychology as such, but certainly with the (usually implicit) claim that its findings are universally valid. Anyway, I gave up this work with a sense of liberation, since there were so many other research opportunities, some of which I shall outline.

Having been asked to contribute to an adult education course in Kumasi, capital of Ashanti, I gave a talk on the factors influencing the development of children to a group consisting mainly of teachers and welfare officers. In the discussion that followed I was told that I had omitted one important factor influencing the character of the child, namely the day of the week on which a child is born. According to traditional beliefs every child in Ashanti is given a so-called *kradin* or 'soul name'; it refers to the day on which the soul is 'washed', or purified ([Rattray, 1927](#)). There are some regional variations, but as far as boys are concerned, there was a wide consensus that the Monday child (*Kwadwo*) tends to be quiet, retiring and peaceable; by contrast, a Wednesday boy (*Kwaku*) is held to become quick-tempered, aggressive, and a trouble-maker.

By that time I had learnt to not just dismiss indigenous ideas, especially as a highly intelligent headmaster told me that from his records it was clear that *Kwakus* were troublemakers.¹ Looking for a way to test these notions more systematically, since it seems plausible that expectations of others may exert an influence, I found that a Juvenile Court existed in Kumasi. So I collected information on the distribution of more than 1000 randomly selected boys' names from ordinary schools, and compared these to the incidence of names on the register of juvenile delinquents. It turned out that *Kwadwo* appeared significantly *less* often than any other name, while *Kwakus* were significantly *more* likely to have committed offences against the person (see [Jahoda, 1954](#)). It may be noted that some of my Ashanti friends saw this result as a confirmation of their beliefs, and it was hard to argue that my interpretation was the correct one!

In order to offer a rationale for another study, one has to explain that at that time the 19th-century notion that 'primitives' were incapable of abstract thought was still widely believed. For instance, [Carothers \(1953\)](#), in a volume sponsored by the World Health Organization, felt able to write that the African is inapt 'for sound abstraction and logic' (p. 87). Working with African students it was obvious to me that this was nonsense, but a more systematic demonstration was needed. For this purpose I administered a number of tests to adolescents in Accra. One was the Goldstein-Scheerer Cube Test, often used to assess people's position on a postulated abstract-concrete continuum. Another was the Kohs Block Test, serving a similar purpose, and finally Raven's Progressive Matrices were given on three successive weeks without any feedback. The aim of these exercises was not so much that of arriving at numerical scores (which were well below the Euramerican averages), but to analyse the nature of the errors, largely due to total unfamiliarity with the materials.

Here I shall just note that children who had a least one literate parent did significantly better, indicating the importance of environmental factors. Moreover, with a modicum of training in what was expected, the performances improved considerably. On the Matrices, though there had been no feedback, the scores were on average 28% higher between the first and third tests ([Jahoda, 1956a](#)).

My reason for mentioning this paper is that it brought about my first meeting with Henri Tajfel. He had come across it, and at a conference he sought me out to talk about it. We became friends and did some joint research at one time (Tajfel, Jahoda, Nemeth, & Campbell, 1970; Tajfel, Jahoda, Nemeth, Rim, & Johnson, 1972). He had an unusually broad perspective on social psychology, including an interest in anthropology and social sciences in general. Today he is best known for being the originator of Social Identity Theory, on which there is an extensive literature.

Another piece that influenced my life was a study based on a psychoanalytic hypothesis. Fortunately for me, it yielded positive results (Jahoda, 1956b). The interests of the late professor Pickford at Glasgow University, where I later applied for a post in social psychology, were psychoanalysis and colour vision. During the interview he focused on and discussed just that one study of mine. It meant that I got the job, in spite of the fact that least one other candidate was better qualified than me.

I continued my research while at Glasgow and later Strathclyde, during sabbaticals generously granted, and/or the summer vacation. One of these is perhaps of general interest. It used to be generally believed that education serves to dispel superstitions. In order to test this, I gave a questionnaire to Ghanaian students in their first and final years, dealing with such topics as witchcraft and magic. It turned out that there were no significant differences in the extent to which traditional beliefs were maintained – even for science students (Jahoda, 1968). After publication there were several replications with students in the USA, which surprisingly obtained similar findings.

There is one more thing I should mention about my work in West Africa. I kept a diary, in which I specially noted African responses to other Europeans and myself. This resulted in my first book, in which I related (then) current behaviour to historical antecedents. It was my first serious effort to delve into history, which I much enjoyed (Jahoda, 1961). In that work I described how attitudes to Europeans varied according to educational level and situational context. Illiterate villagers displayed a subservience conditioned by years of colonial rule, while educated people tended to interact with Europeans in an egalitarian fashion. In order to illustrate the situational element, I shall describe an episode that occurred when I attended a mass meeting of the then dominant political party as the only European, sitting deliberately right at the back. Two stewards came and invited me courteously to sit at the front with dignitaries. When I refused, they unceremoniously threw me out.

By the time the book appeared, I had been at Glasgow University for some three years, and the stay there was a pleasant one: the teaching load was modest, I had hardly any administrative duties, and was given a free hand to do whatever research I fancied. I encouraged my students to attend lectures by Ioan Lewis, an anthropologist, and we planned to set up a joint degree. The proposal went through all kinds of committees for over a year, and we got the message that it was not going to happen. So we decided to leave when an opportunity arose. He went in due course to UCL (University College London), while I was invited to set up a psychology department at Strathclyde – from scratch. If I had known what that entailed, I might not have done it. The first few years were rather a nightmare, but I was determined not to give up research – I had seen too many cases where it was never re-started.

My work in West Africa had been really stimulating and by comparison I found social psychology rather pedestrian. However, there were then no posts for cross-cultural psychology – and there are still hardly any now – and I needed a job. So I went to Glasgow University as a social psychologist and did my best to make the course interesting and indicated to the students some of my reservations about the subject. Here I can expound my critiques of the subject openly. They are based on my experiences in West Africa, which led me to doubt the value of ex-

periments and also some aspects of today's Critical Psychology (Jahoda, 2013). I shall now discuss the problems as I see them, beginning with an historical sketch.

On the Post-War Period in Social Psychology

Originally, US psychology had been powerfully influenced by Wundt, with the exception of the *Völkerpsychologie* which comes closest, though not really very close, to social psychology (see Danziger, 2006). Then around the 1960s a reverse movement occurred, when US social psychologists came as kinds of missionaries to teach us Europeans the advances we had missed owing to the war. Under their guidance, the *European Association of Experimental Social Psychology* was founded. In addition to encouragement, the Americans also provided generous resources, whose distribution resulted in some friction (cf. Schruijer, 2012). A broader story, viewed from the perspective of a key figure, is offered by Moscovici and Markova (2006).

By the 1970s social psychology (especially in the US) was 'in crisis', mainly due to two elements. One was ethical concern with the prevalence of deception in social psychological experiments. The other was more fundamental, largely prompted by Kenneth Gergen's (1973) paper 'Social Psychology as History'. This paper questioned whether social psychology could ever be a science, and led to an approach known as 'social constructivism'. Early in the 21st century a new crisis emerged when some research findings were found to have been simply invented; in other words it was simply fraud, though it was given the politically less damaging label of 'replication crisis'. The issue is much broader than just that of fraud, which only highlighted the infrequency of replications in social psychology. This produced some heart-searching, and promises of changed editorial policies that would make it easier to publish replications.

What Is Wrong With Mainstream Social Psychology?

1. Individualism

Textbook definitions of social psychology nearly always refer to the influence of the real or imagined presence of others on *individual* thought or behaviour. This has often been criticized (notably by Greenwood, 2004), but with little noticeable effect.ⁱⁱ One consequence is that social behaviour in ordinary everyday life is seldom discussed, perhaps because it appears unproblematic. The vast bulk of that behaviour is governed by social normsⁱⁱⁱ and roles, which are the business of sociology and anthropology – perhaps another reason why this is seldom touched upon. There are, however, a few exceptions such as Tankard and Paluck (2016).

2. Assumption of Universality

My views on this have been partly shaped by my failure to replicate in Africa some studies carried out in Western industrial societies. My thinking has also been influenced by considering work in anthropology, which is hardly ever mentioned in social psychology texts. Social psychologists, dealing with a wide variety of topics (e.g. social influence, conformity, social cognition, group dynamics) usually write as though research findings in the area they are discussing applied to all humans. This is hardly ever stated directly, rather it is a *suggestio falsi*. When chal-

lenged on this, the authors tend to respond along the following lines, after pointing out the difficulties and costs of cross-cultural studies:

Fortunately there is a nice shortcut to all of this. Given the abiding faith in the basic universals of humankind, the social psychologist might just as well work with a subject population he knows something about and that is close to hand – the students in his classes. (Gerard & Connolly, 1972, p. 242)

This of course begs the question of what the 'basic universals' are, which is by no means clear.^{iv} Anyway, the vast bulk of social psychology experiments has college students as its participants, and students are hardly representative of the US population at large, and even less of humanity. As documented by Henrich et al. (2010), the USA itself is rather unusual among the peoples of the world. Hence the extent to which US findings can be extrapolated to other countries/cultures is likely to be limited. Such comparative studies as have been carried out demonstrate the existence of wide variations according both time and place (cf. Bond & Smith, 1996; Moghaddam, Taylor, & Wright, 1993; Smith, Bond, & Kagitcibasi, 2006).

It has to be admitted, though, that Gerard and Connolly (1972) are right when they say that cross-cultural work is expensive, and one may add, it is hard. On the other hand, hundreds, and in some cases thousands of studies on a single topic are done in the USA. If these numbers were curtailed, and the resources thereby freed were devoted to some cross-cultural replications, it would become feasible.

3. Experimentalism

As indicated above, the relevant contributions of neighbouring disciplines tend to be largely omitted. More will be said about this later.

Social psychology texts often stress the importance of experiments in rendering the subject truly scientific, and devote a large part of their contents to accounts of experiments. A typical justification of the extensive use of this method is that it is supposedly capable of establishing causality. But in practice that is rare, and what social psychologists usually arrive at are what J.S. Mill called 'empirical generalizations'.

Let me note first of all that the reliance on experiments means that social psychologists rarely resort to direct observation,^v which is very labour-intensive. Yet the control achieved by the manipulation of 'variables' is often illusory. This is so because possibly the most significant 'variable' might be the social context of the experiment, which is usually not open to manipulation.

This kind of limitation has been pointed out long ago:

It must be recognized that nearly all the experimental work in social psychology ... has value and is definitely meaningful only in relation to the particular culture in which the investigation was carried out. Such psychological laws as we can discover are for the most part statements of relations between stimuli and responses in civilized man, and perhaps many of them hold good only in specific groups or under specific social conditions. (Murphy, Murphy, & Newcomb, 1937, p. 7)

In spite of what we now see as racist and sexist overtones, this statement remains an unpalatable truth. As noted already, US students are hardly representative of humanity at large, though that is sometimes denied on the grounds of 'psychic unity', as illustrated above.

There was a time when, in experiments with groups, group members were actually present. Recently it has become the practice to put a participant into a cubicle with a computer, instructing her or him that s/he is part of a group linked up by computers; participants are deceived by the instructions, and all communications are in fact to experimenters (e.g. [Petrocelli, Clarkson, Tormala, & Hendrix, 2010](#)). This seems to me objectionable, and not just on ethical grounds. In a real group all kinds of interactional processes take place which should not be ignored.

Why Culture Matters

One consequence of the intra-individual emphasis of most definitions of social psychology is a tendency to neglect external factors. So most social psychology texts fail to point out that the bulk of everyday social behaviour is largely determined by the social norms and roles of the society and culture in which we live. When one moves to a very different culture, as I did, that becomes very obvious.

Take, for instance, the following observation: a man walks ahead of a woman, free of any burden; she follows behind, carrying a heavy load on her head. This practice is said to be explained by the need for the woman to be protected from wild animals; but that has no longer been the case for quite a long time.

In traditional households, the man is the first to be fed during a meal; children come next, and only then the wife. The one sphere where the wife is independent is that of trading, where she is entitled to keep anything she earns.

Such sharp contrasts are nowadays probably exceptional, though by no means absent. But even quite subtle differences can be highly significant. Thus many times I have heard Europeans saying that 'you cannot trust Africans'. When challenged to justify this belief, the great majority respond by saying that their servants will not look them in the eye. Now, among Europeans looking another in the eye is believed to be a sign of honesty, and not doing it the opposite. Such Europeans are ignorant of the fact that it is regarded as impertinent to look a 'superior' in the eye, it being seen an act of defiance; so they are explicitly taught not to do it. Other rules concern the use of right and left hands; e.g. in an exchange of money, it must be proffered and received with the left. Again personal space (how near one stands to the partner in a social interaction) varies. These examples of differences in social behaviours apply to West Africa, and practices vary both in space and time. Cultural factors of course also influence behaviour in the West, such as the practice of having three meals a day. But these are not always static, as for example the changing role of the father in the family.

Finally I shall just briefly mention a cross-cultural study of conformity ([Bond & Smith, 1996](#)). Considerable variation was found, and the authors propose that 'Conformity research must attend more to cultural variables ...' (p. 111).

Although it would be hard to arrange for students to be exposed to cultural differences (though in a multi-cultural society something might be done along these lines), students could be encouraged at least to read ethnographic reports. During the inter-war years US psychology students did encounter the works of Margaret Mead or Ruth Benedict. These days, social psychology texts very seldom if ever refer to the anthropological literature. Occasionally mention is made of cultural differences in individual responses in such areas as perception; but very seldom one finds accounts of cultural differences in social behaviours, possibly because that would undermine the tacit assumption of universality of the results of social psychological research.

Critiques of Experimentation

Social psychology texts regularly stress the importance of experiments in rendering the subject truly scientific, and they devote a large part of their contents to accounts of experiments. A typical justification of the extensive use of this method, supposedly capable of establishing causality, is to argue that many phenomena cannot be studied in real life. Therefore the researcher takes this phenomenon to the laboratory, where one supposedly has maximal control over the variables of interest.

Let me note again that the reliance on experiments means that social psychologists rarely resort to direct observation. Yet the control achieved by the manipulation of 'variables' is often illusory. As already mentioned, this is because possibly the most significant 'variable' might be the social context of the experiment, which is usually not open to manipulation.

This kind of limitation was pointed out by Murphy long ago, as noted above. His statement about the limitations of social psychology remains an unpalatable truth. As noted above, that is sometimes denied on the grounds of 'psychic unity', which is a fallacy since it implies the absence of any differences. It is not what the 19th century originator of the phrase meant; it referred to a common *potential*, which is a view shared by most cross-cultural psychologists.

The praise in social psychology texts conveys the impression that the outcome of social psychological experiments is always clear and decisive, but in fact that is not necessarily so. One of the most famous social psychology experiments is Milgram's study of obedience. Yet since it was first published in 1963, its meaning and implications have been repeatedly debated and re-interpreted (e.g. [Hollander, 2015](#)).

First, a brief indication of the several strands of thought which completely repudiate experiments. What these strands have in common is a stress on *discourse* as a key tool for grasping the nature of social life. I have already mentioned Gergen in the context of the 'crisis' of social psychology. He is a major exponent of what is known as 'social constructionism' ([Gergen, 1994](#)). It entails a theory of knowledge that is not readily capable of being summarised. Basically it rejects the notion that there is a social (or psychological) 'reality' that can be discovered by empirical methods. In other words, there is said to be no independently existing objective 'truth'; there can be multiple social realities, *constructed* by individuals in their mutual discourses.

Another important approach is that of 'discourse analysis', which far from rejecting empirical observations, obtains its material by recording linguistic interactions in natural situations, and from written texts. It refuses to attribute the conventional categories (e.g. emotions, cognitions) to people's inner life, since it is claimed that we cannot have independent evidence for their existence.

Then 'dialogism' should be mentioned. It is derived from the work of the Russian philosopher and literary theorist Mikhail Bakhtin, who stressed the relational aspect of all verbal communication. Some European social psychologists have proposed his theory as a more dynamic alternative to the Cartesian position, namely the rigid following of the natural science model, in studying human experience (cf. [Markova, 2003](#)).

Lastly, a somewhat different approach, less directly linguistic, is Moscovici's concept of 'social representations'. Modified from Durkheim's 'collective representations', it is concerned with sets of ideas, values, beliefs and practices in modern groups and communities, their origins and circulations. The manner in which new elements are introduced

and assimilated to existing sets by what Moscovici calls 'anchoring' is central to the theorizing and study. A parallel process of 'objectification' renders new elements more simple and concrete, so that they can be more readily assimilated (see [Farr & Moscovici, 1984](#)). It should be said that Moscovici himself was not opposed to experimentation, and was himself an experimenter.

It is a virtue of social representation theory that it deals with broad societal themes, whilst much of experimentation is devoted to testing 'mini-theories' which relate to individuals or small groups.

This broader perspective concerned with important social issues is also characteristic of Billig's 'rhetorical approach', which is historically sensitive and analyses among other things widespread beliefs and ideologies on the basis of textual and/or interview evidence. Examples are his writings on right-wing extremism (e.g. [Billig, 1982](#)) and nationalism ([Billig, 1995](#)). Its disadvantage is that such analyses require a subtle and sophisticated mind, while experiments are relatively easy to do when one follows a set of fairly simple rules.

However, that is not always true and let me now give an account of a large scale experiment which, in my view, is not open to the usual objections. It was conducted not by psychologists but by 'behavioural economists' who wanted to challenge the classical economists' assumptions that people everywhere make decisions guided by 'rational self-interest'. On the basis of evolutionary considerations, they believed that such decisions also consider fairness, trust, and concern for the common good. For this purpose they used the so-called Ultimatum Game ([Nowak, Page, & Sigmund, 2000](#)) played with a generous amount of local currency.

Here is what happens:

In this game subjects are paired, and the first player [or 'proposer'] is provisionally allocated a sum of money, the 'pie'. The proposer can then offer any portion of the pie to a second person [the 'responder']. The responder, knowing both the offer and the total amount of the pie, then has the opportunity either to accept or reject the proposer's offer. If the responder accepts, he or she receives the amount offered and the proposer receives the remainder (the pie minus the offer). If the responder rejects the offer, then neither player receives anything. ([Henrich et al., 2004](#), pp. 11-12)

The games were administered in eighteen small-scale societies across the globe, plus two US samples. It should be mentioned that this was only one, albeit an exceptionally extensive one, of a whole series of such researches.

The outcome showed that there is some degree of fairness in all cultures, though its extent varies. The availability of historical and ethnographic information helped to throw light on the factors likely to be responsible for the differences found. The classical model of self-interest as the only or main determinant of choice was thus shown to be false.

This experiment relied in part on evolutionary considerations, and possible value for social psychology of evolutionary thinking will be discussed later.

Developmental and Social Psychology

Social development should be of considerable interest to social psychologists, since much, if not most, social behaviour is acquired in the course of development. Yet few social psychology texts say much if anything about

developmental issues. This applies to the acquisition of social and moral rules, as well as values, attitudes, and beliefs. There is a vast amount of research on child development, dealing with topics like ideas of causality, religion, or science. Among these the most relevant for social psychology are those concerned with the acquisition of moral rules (cf. Killen & Smetana, 2006), long ago initiated by Piaget. Yet most of the social psychology studies being reported deal only with adults.

There are some exceptions to this, notably a book by followers of Moscovici (Duveen & Lloyd, 1970). In a summary chapter Moscovici (1970) strongly advocates the need to study the development of social representations. He ends by writing:

We shall need more time before we know if the theory of social representations contributes its share to the *rapprochement* between social and developmental psychology... (p. 184)

He adds that the work described in the book constitutes a new departure in social psychology, and it should be said that there is now growing interest in this area.^{vi}

Two main lines of research are required. One is primarily cognitive, concerning the progressive understanding on the part of children of the institutions of their society. On this topic a number of studies are available (e.g. Furth, 1980; Jahoda, 1981a). On the other hand, much less is known about the development of beliefs and attitudes, especially social or political ones.

There are problems of method when one attempts research in such spheres with young children, because the kinds of questions or scales useful with older participants are inappropriate and can lead to misleading findings. The reason is that young children know a great deal more than they are able to articulate. Hence devices have to be designed which allow them to display their knowledge in a more indirect, preferably game-like manner (see Mitchell & Elwood, 2012).

In order to illustrate this more concretely, I shall outline a study of my own (Jahoda, 1983). Except for a famous work on moral development, Piaget's work dealt with children's growing understanding of logico-mathematical concepts. Cross-cultural research indicates that children in non-western traditional cultures lag behind by two or three years in the acquisition of such concepts (cf. Dasen & Heron, 1981). The question is whether these children lack the necessary ability, or whether their restricted experience in this sphere is responsible. In order to contribute to the answer, I conducted comparative studies in Zimbabwe and Scotland dealing with the socio-economic concept of 'profit'. Preliminary exploration showed that Scottish children below the age of ten, when asked verbally what profit is, have no idea how to respond. Hence a situation was set up which allowed them to display their understanding.

A mock shop setting was devised in which the child has the role of the shopper who buys some common object, e.g. an apple, with the price listed in simple figures, which were shown on labels and rehearsed several times. When the supply was exhausted, the shopkeeper [an experimenter] asked the child where a further supply could be obtained, and, critically, how much the shopkeeper would have to pay for each additional apple supplied. Most of the younger children mentioned the same sum as that they themselves had paid; only those who understood the nature of profit named a lower price.

Now it should be explained that in Zimbabwe, as in most parts of Africa south of the Sahara, mothers commonly were traders, often helped by their children in the market. Hence it was expected that the children who were such helpers would acquire the concept earlier, and that was how it turned out as shown in [Table 1](#) below:

Table 1

Understanding the Concept of 'Profit', According to Whether or not the Child Had Been Helping the Mother With Market Trading

	Children	
	Trading with mother	Not trading
Full understanding	33	11
Transitional	22	24
No understanding	5	17

Note. 'Transitional' refers to those who answered only one of two critical questions correctly.

A chi-square test showed that the relationship between children's trading experience and conceptual understanding of profit was significant ($p < .001$).

It was also found that the Scottish children were on average two years *behind* the African ones. For another related study focused on Scottish children's grasp of economic exchange, see [Jahoda \(1979b, 1981b\)](#).

Social psychologists might try to see if and when some phenomena begin to appear in childhood. If they occur in early childhood, they might be innately determined. Anyway, it seems worth exploring since our 'social brain' has many built-in dispositions.

In this connection it might be proposed that one task of developmental social psychology might be that of ascertaining whether the phenomena found in adults are already manifest in children. One instance of that has been shown in a pioneering study in relation to cognitive dissonance (cf. [Egan, Santos, & Bloom, 2007](#)). Furthermore, several studies have also been published indicating that there are neural correlates of cognitive dissonance (e.g. [Izuma, Matsumoto, Samejima, Sadato, & Matsumoto, 2010](#)); this may imply that cognitive dissonance involves fundamental processes, though that is merely speculative. Generally, it would be useful to discover the extent to which some significant social psychological phenomena apparent in adults exist already in early childhood, or are constituted in the course of development.

Evolutionary Psychology

Evolutionary psychology will be dealt with more briefly, not because it is unimportant, but because it is more specialized and used to be somewhat marginal to social psychology.

Evolutionary psychology is a wide field, whose concerns overlap with those of biology and social psychology. Essentially, it seeks to discover those aspects of human behaviour, and centrally social behaviour, which are the outcome of evolutionary adaptations. Any aspects of behaviour which are shown to be universal, such as 'fairness' noted above, are prime candidates. Evolutionary psychology elaborates models, which are then tested empirically. The models themselves are based on, often rather speculative, hypotheses; and since we cannot directly observe human evolutionary process, indirect inferences are made which are sometimes contentious. For instance, Tooby and Cosmides (as cited in [Barkow, Cosmides, & Tooby, 1992](#)) postulated an innate 'cheat-detection' module

which protects a community from 'free-riders' who take advantage without themselves contributing. Their main evidence is the fact that on a Wason selection task, people are much more successful if the task is presented in social as opposed to an abstract formulation. However, the evidential value of that has been questioned, and alternative interpretations been put forward.

Yet many evolutionary hypotheses are more convincing, and it is striking how many problems tackled are social in character (i.e. cooperation among kin, reciprocity and sharing, mate choice and sexual selection). There are even similarities of methods, though the purposes differ. Take, for example, a study based on the postulate that information is likely to be shared much more in a small than in a large community. This was tested by monitoring radio conversations between fishing boats (Palmer, 1991). It was indeed found that fishermen from large communities were significantly *less* likely to provide information on the location of lobster concentrations than those from small ones. The relevant factor here is the greater intensity of social relations within small communities. Or, the functions of 'gossip' were investigated by observations (616 scan samples of topics of conversations); this was supplemented by an experiment in which participants were presented short vignettes of social gossip and asked to rate the extent of their approval or disapproval of the gossipers described (Dunbar, Duncan, & Marriott, 1997).

Some evolutionary psychology studies like one on the relation between group size and cognitive functioning (Dávid-Barrett & Dunbar, 2013) are highly technical. A useful introduction to the field is Barrett, Dunbar, and Lycett (2002).

This brief sketch will, I hope, at least give some idea of the ways in which social and evolutionary psychology have a good deal in common, though they are not alternatives. But the examples offered show the importance of an evolutionary approach for socio-cultural psychology.

Finally, brief mention should be made of social neuroscience, which has recently demonstrated how many brain structures are dedicated to forms of social behaviour, so that it makes sense to refer to a 'social brain'. For details see Cacioppo and Berntson (2009) as well as Kolstad (2015).

Concluding Comments

In this paper I have attempted to show that social psychology, especially as presented in standard texts, is unduly narrow. I cannot claim that this view is an original one. Some years ago a book with the title *Bridging Social Psychology* (Van Lange, 2006) appeared with no fewer than 62 chapters, most of which proposed other subjects that might or should be linked to social psychology. The range was very wide, but the chapters were short and accordingly lacking in depth.

Here I have tried to sketch the historical background in a personal context, indicating how social psychology began as an entirely non-empirical and speculative endeavour. Then I presented critiques of its present state, but my stance is certainly not wholly negative, and I want here to acknowledge that social psychology has very considerable achievements to its credit. But because of its isolation from neighbouring fields, it so far offers only a partial perspective on the roots of social life. There is of course a limit to what can be crammed into a textbook, but I think students ought at least to be made aware of the potential contributions of other disciplines.

Notes

- i) During the Second World War people talked about “*Kwaku* Hitler“.
- ii) For instance, Saroglou (2016) argued that fundamentalist violence has little if anything to do with religion, and proposed that *individual* personality features are the main cause. He mentions history, but gives no examples.
- iii) For the relevance of norms in culture see the Special Issue of the *Journal of Cross-Cultural Psychology* on ‘Intersubjective Norms’ (Vol. 4, 2015).
- iv) For a discussion of the difficulties in seeking to establish true ‘universals’ see Norenzayan and Heine (2005).
- v) Michael Billig drew my attention to the fact that Lewin, who is now regarded as the *fons et origo of experimentation*, recommended experiments that in effect were natural observations and did not involve any statistics (cf. Billig, 2014, 2015).
- vi) For instance, Cambridge and Brandeis Universities now offer postgraduate courses in social developmental psychology.

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