

Lost in translation

Research is . . . an informal, messy-looking process. It is so different from the stereotype that it has even been suggested – somewhat facetiously – that the history of science be rated X (Brush 1974) and kept from impressionable young students because it does such violence to the image of scientists as careful weighers of evidence pro and con, concerned only with being objective. The most important parts of research are, in fact, subjective and have little to do with elaborate quantitative analyses or expensive laboratory equipment. The essential components of research – where it begins and where it leads – have to do with observations of phenomena and the development of hunches, ideas, and questions about the hows and whys of things. (Jones, R. 1985: 3)

Well over a decade ago, I started to return to the social psychological research that had captured my imagination as an undergraduate in the 1960s. First on my list was a study by Muzafer Sherif and his associates on conflict between two groups of boys at a summer camp in Oklahoma, as it had a very personal appeal for me. I had attended a summer camp in my early teens and a string of rainy days must have prompted the counsellors to think about how they were going to manage an increasingly unruly set of campers who were beginning to factionalize and turn on counsellors and one another. (Either that or we were unwittingly participating in someone's research study!) Whatever the case, they came up with a plan to unite us.

We were told that a madman had escaped from a nearby asylum and that we would all have to cooperate and band together to make sure no harm came to anyone. We were fed together, housed in the main dining area rather than in our separate cabins and sent out on walks together in common search of the madman. I may well have been the only camper who reported seeing him in the woods wearing a plaid shirt, a sighting that turned out to be one of the great humiliations of my life when we were told that it was all a hoax. When I came across the Robbers' Cave study in my introductory social psychology class, I remembered my anger at being manipulated by our counsellors despite their good

intentions to keep campers from making each others' and their lives miserable.

I returned to the boys' summer camp studies (Sherif *et al.* 1961) in a more systematic¹ fashion shortly after I heard a colloquium Ben Harris gave at Carleton University in the 1979-80 academic year. I was motivated to look again at the Robbers' Cave experiment just as Harris (1979) had with John Watson and Rosalie Rayner's 'classic' study of Albert and conditioned fear (Watson and Rayner 1920) to see if the reporting of the research had become systematically transformed over the years or whether this activity was something unique to the career of Watson. If there were 'origin myths' and transformations in social psychology's construction of its past, I was curious to know what themes operated in the telling of the field's history?

Harris had concluded in his research that 'most accounts of Watson and Rayner's research with Albert feature as much fabrication and distortion as they do fact' (Harris 1979: 151). He cited several reasons for this, namely the reliance placed on secondary sources by textbook authors and their desire to make experimental evidence consistent with contemporary theories, an attempt to make Watson more credible to students coming into the field of psychology, and finally Watson's own active participation in changing the description of the original study. Harris' perspective on the 'Little Albert' research was that the matter was one of psychological mythology and provided a good example of aspects of the sociological dimensions of science.

Harris's research warned that one should be wary of secondhand accounts of psychological research, particularly classic studies, but one should also examine further the process 'by which secondary sources themselves come to err in their description of classic studies' (Harris 1979: 157). Harris was chiefly preoccupied with how classic studies come to shape the origin myths of a discipline and to build into a discipline a 'false sense of continuity'. Harris noted that origin myths are not intentionally fraudulent but rather a part of the extra-scientific world that shapes a particular discipline. Rejoinders to Harris asked 'How typical is the Little Albert case in the history of rereporting psychological research?' (Cornwell *et al.* 1980). These authors wondered 'How many other notable experiments have multiple originals?' Harris saw more in the 'Little Albert' scenario than a cautionary tale and proposed building 'a socially informed, critical history of psychology' by examining the political and social context within which psychology developed. He put his emphasis less on the biographical and personal intentions of individual psychologists and more on the social forces that shaped the seemingly 'linear' recording of the discipline's development.

This approach is quite important in writing a critical history of social psychology. The practice of social psychology has developed within both

academic sociology and psychology. It has been shaped by behaviourism, psychoanalysis, evolutionary theory and field theory. Its practices have fluctuated with the extra-scientific political climate of the times in ways demonstrable through historical research (see Collier *et al.* 1991 in general; see Bramel and Friend 1981; Samelson 1980 and 1986 more specifically).

In social psychology there are numerous examples of classic studies: Asch's studies of conformity and independence, Milgram's studies of obedience, to name but two lines of inquiry. These are studies that have given researchers a way of conversing with one another in a common vocabulary. They delimit the field for incoming students and provide a 'sense' of tradition from a point of origin. A classic study is used to mark the origin by signalling a break with a previous tradition: for example, Triplett's demonstration of social influence marks the movement of social psychology beyond philosophical speculation or archival evidence into a scientific realm, by virtue of its use of experimentation. Scientific rigour is claimed and a sense of cumulative progress from the classic study onwards can be charted.

By the time I came to learn about the Robbers' Cave study it was well on its way to being a 'classic'. Those who wrote about it thought about it in this way – as a memorable and ground-breaking piece of research. While it was most often cited in the 1950s and 1960s textbooks in chapters on intergroup conflict and prejudice, it was also cited as an example of international and organizational conflict.

By the mid-1970s the Robbers' Cave study was often seen as a classic in methodology with less emphasis on the content. Textbook authors in social psychology variously referred to Sherif's research as 'ground-breaking work', 'the now classic Robbers' Cave experiment', 'the elegant field experiments' and a 'justifiably famous experiment'. In this sense of 'classic' the study combined sociological description of groups in a field setting with the laboratory methods more often found in general psychology at a time when research methods were playing an important role in defining disciplinary boundaries. The study showed that experimentation could be woven into everyday life with as little suspicion as in the laboratory.

The Robbers' Cave Study can be considered 'classic' as an idea with wide applications to fields other than the original group used in the research. While conflict among eleven-year-old boys at summer camps served as a convenient starting place, once extrapolated to conflict among adults in large organizations, among ethnic and racial groups and on the international level, the ideas have more wide-ranging appeal and approximate the rhetoric of those sought-after universal laws of social behaviour. Robbers' Cave is also a classic in a more narrative sense – it is a memorable story documenting a deeply felt hope of many social

scientists (and liberal thinkers more generally) of the post-Second World War era that social arrangements could be created whereby human beings could reach peaceful rather than violent ends.

As I began my research I wanted to know how this particular classic study compared with Harris's analysis of the mythological proportions of Little Albert? To do this, I looked first at the possibility of multiple originals. What became apparent when I looked more closely into the 'classic Robbers' Cave experiment' is that there were three summer camp studies. While Robbers' Cave is better cited and remembered it is the last of a series of experiments with groups of boys at summer camps, each of which had its own unique features. Here are the details of each of the studies.

Date of study	Location	Names of groups of boys
1949	Connecticut	Bull Dogs and Red Devils
1953	Upstate New York	Panthers and Pythons
1954	Robbers' Cave, Oklahoma	Rattlers and Eagles

The texts that can be considered 'originals' – that is, the texts where the findings of each of the three studies can be found – indicate that there were also multiple originals. These are as follows:

- Rohrer, J.H. and Sherif, M. (1951) *Social Psychology at the Crossroads*, New York: Harper and Bros (Chapter 17 contains the 1949 study).
- Sherif, M. and Sherif, C. W. (1953) *Groups in Harmony and Tension. An Integration of Studies in Intergroup Relations*, New York: Harper and Bros (Chapters 9 and 10 contain the 1949 study).
- Sherif, M. (1954) 'Integrating field work and laboratory work in small group research', *American Sociological Review* 19, 759–71 (contains summaries of 1949 and 1953 studies).
- Sherif, M., Harvey, O.J., White, B.J., Hood, W.R. and Sherif, C.W. (1954) 'Study of positive and negative intergroup attitudes between experimentally produced groups', Robbers' Cave Study, University of Oklahoma, Norman, Oklahoma (multilith containing part of 1954 study).
- Sherif, M., White, B.J. and Harvey, O.J. (1955) 'Status relations in experimentally produced groups through judgemental indices', *American Journal of Sociology* 50, 370–9 (reports the hand-toss experiment from the 1953 study).
- Sherif, M. (1956) 'Experiments in group conflict', *Scientific American* 195(5) (Scientific Offprint #454 contains a composite of 1949, 1953, 1954 studies).
- Sherif, M. and Sherif, C.W. (1956) *An Outline of Social Psychology*, New York: Harper and Bros (2nd edn, presents 1949 and 1953 studies in Chapter 6; contains 1954 study in Chapter 9).

- Sherif, M. (1958) 'Superordinate goals in the reduction of conflict', *American Journal of Sociology* 63, 349-56 (contains part of the 1954 study).
- Sherif, M., Harvey, O.J., White, B.J., Hood, W.R. and Sherif, C.W. (1961) *Intergroup Conflict and Cooperation. The Robbers' Cave Experiment*, publication of the Institute of Group Relations, Norman, Oklahoma, 1961. (presents ASR paper, Chapter 1; contains the multilith of 1954 as Chapter 2).
- Sherif, M. (1967) *Social Interaction: Process and Products*, Chicago: Aldine Publishing (contains the 1954 multilith as Chapter 22).
- Sherif, M. and Sherif, C.W. (1969) *An Outline of Social Psychology*, New York: Harper and Row (3rd edn contains 1949, 1953, 1954 study results in Chapter 11).
- Sherif, C.W. (1976) *Orientation in Social Psychology*, New York: Harper and Row (contains 1954 study in Chapter 5).

Unlike accounts of the Little Albert study, the details provided in each of the accounts of the boys' summer camp studies are quite similar. In subsequent accounts by social psychology textbook authors, there are minor errors that confuse details of the studies, for example, the names of the groups, the exact location of the camps, the types of tasks required by campers or the procedures for assigning the boys to groups (i.e. random assignment vs. carefully matching). For the most part, the sequence of successive stages in each of the intergroup experiments is described consistently in each of the originals and Table 8.1 highlights the similarities and differences among the studies.

Table 8.1 Stages and activities for each of the intergroup studies (I=Connecticut; II=Upstate New York; III=Robbers' Cave Oklahoma)

Stage of experiment	Experiment		
	I	II	III
In-group formation			
Spontaneous interpersonal choices		X	X
Arbitrary division into two matched sets according to specific criteria	X	X	X
Intergroup conflict			
Win-lose competition	X	X	X
Planned frustration of in-groups	X	X	
Reduction of conflict			
Common enemy, individual activities, adult intervention	X		
Contact without interdependence	X		X
Series of superordinate goals			X

Source: Table 11.1 from Sherif, M. and Sherif, C.W. (1969) *Social Psychology*, 3rd edn, New York: Harper & Row. Reprinted with permission of HarperCollins, New York. Copyright © 1969.

In his writings Muzafer Sherif often described the three studies as a progression from the first to the third where, by the last study, intergroup conflict was reduced to a greater extent than in the first two studies. He located the research in terms of his previous work on norm formation linked to an interest in conflict and cooperation between groups. More generally, Sherif saw the 'common threads' of his work as research that could have bearing on 'actualities', that could address a 'significant persistent problem in human affairs', and that selected methods according to the dictates of the problem in question (Sherif, M. 1967: 8). He wrote of the personal background for the common themes in his work in this way:

As an adolescent with a great deal of curiosity about things, I saw the effects of war: families who lost their men and dislocations of human beings. I saw hunger. I saw people killed on my side of national affiliation; I saw people killed on the other side. In fact, it was a miracle that I was not killed along with hundreds of other civilians who happened to be near one of the invasion points the day Izmir (Smyrna) was occupied by an army, with the blessing of the victorious Western colonial powers at the end of World War I . . .

I was profoundly affected as a young boy when I witnessed the serious business of transaction between human groups. It influenced me deeply to see each group with a selfless degree of comradeship within its bounds and a correspondingly intense degree of animosity, destructiveness, and vindictiveness toward the detested outgroup - their behavior characterized by compassion and prejudice, heights of self-sacrifice, and bestial destructiveness. At that early age I decided to devote my life to studying and understanding the causes of these things. Of course for some years I did not know how to go about it, but I started reading whatever I could lay my hands on about history and social problems. By the time I came to the United States for graduate study, I had firmly decided that my life's work would be social psychology . . .

(Sherif, M. 1967: 9)

Muzafer Sherif's wife and colleague, C.W. Sherif, elaborated in her textbook, *Orientation in Social Psychology*, on her personal context for the research reflecting the superordinate goal of raising a family, combining two careers, and living a marriage of two cultural backgrounds. In keeping with the intent of her book to tell students about 'how research happens', she described how the summer camp experiments came about. She wrote:

Any research worth doing starts with questions or a puzzle that plagues the researcher . . . Surely it is no accident that these particular

experiments on intergroup relations should take shape in the mind of a Turkish social psychologist in the United States as the conflagrations and tragedies of World War II smouldered, who had married an American. We were keenly aware that our marriage was an intergroup affair. Had we not been, we would have soon learned.

(C.W. Sherif 1976: 115)

While there is general consistency in Muzafer Sherif's accountings of his work, once the studies make their way into the lore of social psychology the context for describing them often changes to suit the textbook author's point of view. The finding that superordinate goals can reduce conflict between groups, which is only found in the 1954 Robbers' Cave experiment, can be found in chapters on prejudice and discrimination, social change, war and peace and in the introductory chapter of a textbook as a classic in experimentation. As I traced these studies through more than seventy textbooks of social psychology I found that authors generally discussed the more elaborate and successful Robbers' Cave study of 1954 leaving out the earlier studies of 1949 and 1953 altogether. It was the finding of cooperation that was to live on rather than the more dismal message of the first two studies, where the two groups of boys joined in a larger unit to fight a common enemy, be that another group of boys in the first study, or, as I found out in my research, the experimenters themselves in the second study. The 'happy ending' study prevailed despite Sherif's appreciation of social relations as 'messy, contradictory, and fraught with conflict, suffering, and agony' (Sherif, M. 1967: 9).

What strikes me as I reread all three studies is that there was no more reason to valorize the third experiment than the first two. The world is made up of all the outcomes seen in these studies and it has been for quite extra-scientific reasons that the third study – reduction of conflict through superordinate goals – has prevailed in the historical record. The collective writings of the Sherifs speak to our common humanity. Their writings, while acknowledging horrendous intergroup events, are framed with optimism about the ability to transcend human difference to achieve our common humanity. In the era of cold war thinking, with its language of tough-talking deterrence, the Sherifs were bringing an optimistic and liberatory message that people could get along and work for common goals if the conditions were right.

I remember being drawn to their optimistic message. It was quite consistent with much of the liberal social science I met up with as an undergraduate in the late 1960s. North American social psychology, in particular, stressed the malleability of a person's social behaviour. Humans were not locked into fixed destinies but were able to discover and potentially transform the environmental pressures weighing on

them. While the extreme downside of this was to be found in Milgram's obedience to authority experiments, one could see the other side in Muzafer Sherif's third study of cooperation among boys at a summer camp. If some of the literature of social psychology confronted the human being with his or her foibles – conformity for no good reason; biased, faulty and irrational reasoning most of the time – one could look to the human potential for cooperation in the Robbers' Cave study. It was a narrative of archetypal transformation from individual selfishness to collective social growth.

What kept my perspective about the summer camp studies in balance was the theoretical work of Michael Billig. Sometime in the course of my research on these studies in the mid-1980s I came across his analysis of the three studies that led credence to another way of thinking about them. It is worth repeating at length (the fuller treatment is in his book, *Social Psychology and Intergroup Relations* (1976)). Billig was concerned with the way in which false consciousness is maintained in subordinate groups to the advantage of dominant groups. When he came to analyse the summer camp studies, he saw not two groups of boys, but three groups of people, that is, two groups of boys and one group of experimenters (camp staff and researchers). He acknowledged that Sherif himself did not want to reduce intergroup problems to individual personality or attitudinal problems but emphasized the 'importance of studying the relations between groups over and above their own internal properties' (Billig 1976: 302). Like so many social scientists, Billig expressed his admiration for the boys' summer camp studies in his own way:

The research itself is a veritable *tour de force* and an important landmark in social psychological research into intergroup relations. The richness of the results, as well as Sherif's own methodological skill and organisation, ensure that the research will bear re-examination and re-interpretation.

(Billig 1976: 302)

What Billig added that was unique was a different theoretical framework, one that saw the interaction as being among three groups rather than two. He speculated that if the boys, who, he argued, were competitive well before they were given 'experience with competition', were to have found out that the camp was an experiment, they would have behaved quite differently. He reread the studies as three-way group interactions in which one group (experimenters) created a scenario for two other less powerful groups (the boys). Billig succinctly summarized the crux of the three experiments. He noted that in the first two experiments, the boys were brought to camp, became acquainted and were split into groups wherein features of in-group behaviour were noted. Opportunities for competition were arranged wherein out-group antagonisms appeared. In

the third experiment, the members of the artificially created hostile groups were presented with superordinate goals that facilitated intergroup cooperation and diminished intergroup hostility somewhat. Not as much, I would add, as textbook authors have claimed in their romanticized versions of these studies and not as much as many of us would like to believe is possible in conflict settings.

Billig aimed 'at progressing beyond Sherif's analysis' (Billig 1976: 305) and he did so by asking 'under what objective social conditions do groups subjectively develop superordinate and competitive goals. In short, what relations are there between group beliefs and goals and the objective situations in which groups find themselves?' (p. 306). His answer to this question of whose interest the institution of competitive and/or superordinate goals served resulted in the following analysis:

One has to face one of the most glaring, and yet neglected features of the whole situation: the one group in the boys' camp with a definite vested interest in the institution of competition and the 'semi-institution' of group cooperation was, in fact neither of the two groups of boys. It was the third group – the experimenters/camp authorities. ... This third group, the experimenters, is the social group which creates the other two groups – giving them their social meaning and their social reality. This group constitutes the legitimate authority in the camp.

(Billig 1976: 307)

In the descriptive re-analysis of the studies that followed, Billig showed just how often the group of 'authorities' intervened to keep the boys from coming to blows. His detailed analysis showed just how orchestrated competition and cooperation can often be in groups where there is a powerful majority group in charge of less powerful 'minority' groups fighting with one another. Billig went further to state that:

organised competitions do not arise in some sort of social vacuum, but are created by specific people or groups in specific situations. In the case of the boys' camp experiments, the simplistic explanation would state that the authority group created the competition and this in turn gave rise to the resulting intergroup hostility.

(Billig 1976: 310)

For Billig the more complex analysis involved going beyond the manipulation of superordinate goals to achieve harmony between groups. Rather, he wanted to look at the real authority third parties have – managers, world economic powers, privileged groups of all sorts – in setting the goals and creating false consciousness for less privileged groups. In this regard, he concluded that one must return in the boys' summer camp experiments to a wider context of power relations among

three parties in the form of 'the original distinction between the camp authorities and the boy subjects'.

Billig went on to suggest that the two groups of boys did not see the dominant group (the authorities) as the source of their intergroup hostility, despite the fact that the conflicts were arranged by these authorities. The ups and downs of the conflicts between the two subordinate groups in the three experiments never altered their relationship to the dominant group. True? False.

Without benefit of archival material and relying on only one 'original' for the second study in 1953, Billig posed the following question:

whether the groups would have behaved in the same way, would have developed the same ingroup and outgroup perceptions, and would have unquestionably accepted the authority of the camp leaders, had they been aware of the experimenters' intentions and manipulations. Although this is an empirical question for which there is no immediate data, one can ask it with reference to particular features in the experiments. For instance, one can wonder whether the behaviour of the boys would have been different, if they had known that after stage one the authorities were deliberately splitting up close friends – and they were doing this to see whether these friends would turn on each other.

(Billig 1976: 318)

Billig's reinterpretation was borne out by information provided by the Sherifs in the second edition of their social psychology textbook, *An Outline of Social Psychology* (1956), which was replaced in 1969 with a third edition.² The reference is obscure and Billig appears not to have known that the boys in Study Two did figure out that they were being manipulated, and staged a mutiny. Tucked away in a footnote in one of the 'originals', I found the following reason given for ending the 1953 experiment in Stage Two: 'In the 1953 study, this stage was not completed. In a frustration episode, the subjects attributed the plan to the camp administration. Since testing hypotheses required that the source of frustration be attributed to the experimental outgroup, the 1953 study was terminated at this point' (Sherif and Sherif 1956: 311).

In other accounts the matter is stated differently. For example, earlier on, in another 'original' Sherif wrote about the 1953 experiment as a failed attempt at integrating field and laboratory research:

The scope of the experiment embodying laboratory-type procedures at crucial points in each stage proved to be too great for a single attempt. During the period of intergroup relations (Stage III), the study was terminated as an experiment due to various difficulties and unfavorable conditions, including errors of judgment in the directing of the experiment.

(Sherif, M. 1954: 769)

In a more popular 'original' in *Scientific American* published in 1956, Sherif wrote a composite analysis of the three summer camp studies. Here he wrote that 'None of the boys was aware that he was part of an experiment on group relations' and he emphasized that the researchers 'set up projects which were so interesting and attractive that the boys plunged into them enthusiastically without suspecting that they might be test situations'. It is this version that prevails in subsequent writings about the studies, contradicted only by the lonely footnote cited above in a 1956 textbook that was, as mentioned previously, replaced in 1969 with a third edition.

There are several outcomes in these three experiments – each one pointing to the enormous difficulties groups have in working towards cooperative goals. Study One ends with the two groups of boys banding together against a common enemy. Study Two ends with the two groups turning on the authorities who are manipulating them. Study Three ends with the two groups achieving a reduction of hostility through cooperative goals being 'arranged' by the authorities. Each outcome is possible and plausible. Study Two is far more interesting in some ways, particularly given Billig's theoretical reinterpretation. Yet, it is rarely found in textbooks referencing intergroup conflict.

Our textbook practices of citing 'classics' that fit with prevailing political notions of the time relegate other less 'successful' studies to the empirical graveyard as 'failures to replicate' or not quite what the researcher is wanting to demonstrate. Billig's detailed analysis of Study Two addressed the issue of what happens in intergroup situations where groups do not have equal power. This is much more often the real world case in the workplace and on the international scene – both areas to which Sherif wanted to generalize. After reading his work and completing a search through all the 'originals' I found myself re-oriented to these studies. Like Billig, I wasn't taking exception to Sherif's methods or to the axiom that experimenters have powerful effects. We need our textbooks to report all the summer camp studies so that the difficult struggles of survival between groups are not lost in translation but posed directly to those who might dream up new ways to find common ground and continue the Sherifs' project of enlarging the 'we'.

These studies signal to me the deeply subjective and important affinity between the researcher and all aspects of the research process: theoretical framework, investigative method, analysis of the findings and the communication of the results across generations of students. It continues to trouble me that when we package and shape social psychology as a scientific discipline that purports to study individuals and groups, we smooth over the mess that has historically existed and still exists in the field. We diminish meaning and understanding. Something is lost in translation. We homogenize, make diversity and conflict vanish and

come up with something like a bunch of statements that are meant to mimic 'universal laws' of social behaviour rather than case studies of struggles between differently advantaged groups. There are historical, political, and moral aspects underlying all that we do in social psychology and it seems to me that making those features of our work explicit makes those of us who teach and write social psychology an active part of transforming the social world rather than detached observers of it.

Current research and pedagogical materials in experimental social psychology place a greater emphasis on scientific credibility than political and historical understanding. Through its development in North America from the 1930s to the present, social psychology became the experimental study of individuals in interpersonal rather than collective situations. Standard textbooks continue to give us the contemporary view – social psychology is best conceptualized within the natural science approach through experimentation, quantification and statistical models. Imagine a social psychology grounded in the 'stubborn particulars' of time, place and the lives of people who practise research and social analysis. It would start to tell us more about the origins and changing nature of social inequalities; more about the basis for conflicting perspectives relevant to understanding social problems; and more about the ameliorative effects of different types of social action. This would require teaching and studying social psychology in ways that balance the seemingly individual way lives are often lived with the collective vested interests that each of us brings to the study of social issues but which sometimes remain invisible to awareness.

For me, practising a social psychology grounded in historical particulars continually raises self-contradictions and conflicts. My critical perspective comes from asking not 'What is *the* history of social psychology?', but 'How is social psychological knowledge constructed at different times in the history of the discipline and in whose interest is the knowledge constructed?' It comes from asking questions like these:

Why do textbooks have compassionately written chapters on discrimination against minorities rather than chapters on unearned privileges of dominant groups?

Why have experimentation and quantitative measurement become the hallmarks of social psychology in one culture and not in another?

What are the cultural assumptions in the theories we are reading about in social psychology textbooks? Assumptions about race, gender, class, sexual orientation among other social and political identities.

What happens if I trace a classic study through several generations (editions) of the same textbook or in different textbooks over a given period? Does the recounting of the study change over time or with different authors and if so how can I account for these changes? (Try

using the citation index to see how secondary sources reference these studies.)

What ever happened to the study of language or social movements in psychological social psychology? When do they appear in textbooks, when do they disappear? Where do they go?

What's the difference between the study of peace and the study of conflict resolution? What's the story about 'peace' in social psychology's involvement through two world wars and other regional conflicts?

As more of these questions are answered, it will become possible to construct a course in social psychology that begins with the historical context for its development within the field of psychology (as well as sociology). Eventually, a much broader cultural analysis of how 'the social' has been constructed in psychological social psychology will emerge to provide us with a socially informed and transformative project. Less will be lost in translation as our image of the social psychologist and social psychological research becomes more sensitive to conflicts of interest and contradictory perspectives.