A Thought Collective without Collective Style?
The Western European Activities of the SSRC's Committee on Transnational Social Psychology 1963–1971

by Verena Lehmbrock

Universität Erfurt

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Abstract

This paper investigates parts of the trajectory of the Committee on Transnational Social Psychology, which was established in 1963 by the Social Science Research Council (SSRC) with the aim of internationalizing US experimental social psychology. It challenges previous depictions of the Committee as a Fleckian thought collective, by incorporating sociologist Ludvik Fleck's additional concept of style of thought. Specifically, this report traces the ways in which the Committee’s activities in Western Europe disrupted the intellectual integrity it started out with, suggesting that processes of Europeanization, as well as a previously unacknowledged structural influence of the SSRC, prevented the Committee from taking the form of a consistent thought collective.
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In 2006, Serge Moscovici and Ivana Markova published The Making of Modern Social Psychology: The Hidden Story of How an International Social Science Was Created, which is the first historical account on the history of the SSRC's Committee on Transnational Social Psychology [henceforth: Committee]. Established in 1963 under the leadership of social psychologist Leon Festinger (1919–1989), the Committee's objective was to create indigenous social psychology centers in Western Europe, Eastern Europe, and South America. Moscovici (1925–2014), who joined the Committee as a member in late 1964, together with his co-author, depicted the Committee as a Fleckian thought collective, “a group of individuals having intellectual contact with one another, exchanging ideas, mutually influencing each other in pursuit of the same goal” (Markova & Moscovici 2006: xvi).

Scope and Approach of This Report

This report will be dedicated to the question of whether the Committee members did actually pursue the same goal. I will focus on the group’s intellectual trajectory, thereby remaining within the theoretical framework of Ludvik Fleck (1935: 41), who paired the concept of thought collective with the additional concept of style of thought. Individuals within a thought collective are hardly ever conscious of the prevailing style of thought, according to Fleck, which exerts an “absolutely compulsive force” upon the individuals’ thinking (ibid.). What makes the case of the Committee especially interesting is the fact that its members came to reflect upon their own style of thinking in the most explicit ways, and the archival materials of the SSRC kept at the Rockefeller
Archive Center provide an excellent basis for analyzing this self-reflection. It is hardly surprising that the documents paint a rather complex picture, not least because the Committee’s intellectual development was characterized by a remarkable malleability. I will argue that the Committee, even if not explicitly called as such, or having been planned as such, increasingly took the form of an open system, which, in turn, can account for much of its dynamism over time.

My analysis is limited to the Committee’s activities in Western Europe, leaving aside, for now, Eastern Europe and South America. After introducing the initial objective of the Committee (*Festinger’s vision*), I will describe the influences brought about by the Western European extensions of the network, and the ways in which this challenged the intellectual integrity of the Committee. I use Moscovici’s memoirs as a contrasting foil throughout. Potentially, the perspective of each other member brings new and different insights, some of which may be added elsewhere in the future, others of which may be irretrievable.

**Festinger’s Vision**

The original vision of the proposed Committee on Transnational Social Psychology was simple and straightforward: having realized that the results of US social psychological research were likely bound to the US brand of Western culture, in the context of which, as Festinger estimated, about 99% of the pertinent research was done, “it is hard to separate what may be true of its society from what may be found universally.” The young discipline had not been able to solve this problem by replication studies in foreign countries. The proposed solution was therefore to render experimental social psychology a full-fledged international discipline comparable to more traditional disciplines. Relying on competent “indigenous” scholars worldwide would eventually help to tackle the “morass of uncertainty” regarding cross-cultural research.²
The establishment of new committees fell under the purview of an SSRC governing body called the Committee on Problems and Policy (P&P). As the P&P files suggest, the Committee was viewed as Festinger’s personal project: “He would like to bring together a number of such psychologists who share his research interests.” Some P&P officers pointed out that representatives of dissimilar cultures could already be found in Europe, while Festinger was still looking for partners in Asia and the Middle East. The SSRC also agreed to act as an intermediary, channeling funds from the US Office of Naval Research (ONR) for two social psychology conferences to be held in Western Europe. This parallel project was initiated by US social psychologist John Lanzetta (1926–1989), who was affiliated with the Group Psychology Branch of the ONR and had traveled Western Europe during his sabbatical in London in 1962. Lanzetta was especially keen on identifying experimentalists in social psychology and concluded that these rare people needed to be brought together (Lanzetta 1963: 3-4, quoted from Schruijer 2012). Lanzetta later became one of the Committee’s first members, and Festinger credited him with having provided the major influence in precipitating the formation of the Committee.

**The Western European Experience**

In January 1964, the Committee finally came into existence. The Committee’s first members, in addition to Festinger, were Leonardo Ancona (Italy), Jaap Koekebakker (Netherlands), Lanzetta (US), Serge Moscovici (France), Ithiel de Sola Pool (US), Ragnar Rommetveit (Norway), and Stanley Schachter (US). In 1965, Henri Tajfel (UK) succeeded Ancona, and in 1966, Harold H. Kelley (US) succeeded de Sola Pool. Moscovici (& Markova 2006: 73) later recalled how membership came as a “revelation” to both him and Rommetveit, as they were nominated without prior consultation. Moscovici had been an appointed member for months without even knowing about the existence of the Committee, whose clandestine way of operating, without official letterhead etc., he emphasized as most characteristic. As Lanzetta had researched, there were some highly capable, yet scattered, experimental social psychologists in Western Europe, especially at the handful of universities, where US social
psychologists had been working as Fulbright professors. A conference with colleagues from the Middle East and Asia continued to be explored, but, as the sources seem to suggest, Western Europe was now moving at the center of efforts. The fact that Europeans were hardly noting each other underlined the usefulness of the sponsored European conferences.

One year later, Festinger reported to Pendleton Herring (1903–2004), the acting president of the SSRC, that the methodological training and research experience of the European psychologists had actually proven to be “not very adequate.” While Western Europe had good people, they still needed massive support from Americans. However, if in the next five years significant money and help were made available, social psychology in Western Europe could start making a decisive contribution to science. Judged by these early documents, the Committee appeared as a development agent, bringing West Europeans into contact with each other and building new structures of research and training, motivated by the prospect of transgressing cultural bounds of US research to approach universal social “laws.” The Committee’s practical agenda combined with an unquestioned epistemology, or style of thought, arguably gave it the shape of a consistent thought collective.

By 1967, Festinger’s vision had borne fruit. The two sponsored conferences took place in Italy, a “stylistic and prestigious” enterprise (Moscovici & Markova 2006: 59), bringing selected psychologists into contact and stimulating the rise of a Western European community. During a third conference, held in France in 1966 and financed by their own funds, the Europeans quickly established their own professional society, the European Association of Experimental Social Psychology (EAESP) (Rimé 1999; Moscovici & Markova: 88–100). In addition, annual research training seminars for students were established as an important building block of the Committee’s mission. The first of these summer schools, however, held in the Hague in 1965, was directed by a European group unknown to the Committee, which also organized most of the needed funds. Dedicated to social psychological problems in organizations, this summer school did not reflect the interests of the Committee, as the P&P discussed. The seminar even threatened to turn out as a management training institute for
industrial personnel rather than for experimental psychologists, much to the disapproval of Festinger. The SSRC and the Committee were unanimous that a greater influence of the Committee had to be ensured, connected to funding, for the future summer schools, and that the independent European groups needed to be integrated with the EAESP (see ibid.: 74-76, 104-105). Festinger inculcated in Moscovici, the organizer of the second summer school in Louvain, Belgium, that the goal was to find a training formula for young social psychologists that would turn them into experimental researchers (ibid.: 106). The program of the 1967 summer school in Louvain indicates that this goal was achieved. An international group of thirty young researchers rehearsed the entire course of a research project with eminent researchers, such as Kelley, Rommetveit, and Philip G. Zimbardo (born 1933), for several weeks at a time (ibid.: 113).

**Unexpected Developments**

The Committee’s further trajectory suggests, however, that its transnational ambitions gradually began to challenge some of the original precepts and policies. Unusual for an SSRC committee at the time, its initial membership already consisted of an equal number of US and European members. Despite this balance, the Committee was nevertheless still seen as an American body, as Festinger reported in 1964, suggesting the need to increase European membership. Dutch Committee member Jaap Koekebakker reported that European social psychologists were bent on organizing their own training institutes and tried to avoid American funding. Various archival documents, as well as published sources, give vivid testimony of how Europe’s indigenous social psychologists, once brought together, became preoccupied with their own intellectual identity. In his trend report on Western Europe, presented at a seminal 1967 conference jointly organized by the Committee and the EAESP in Vienna, Koekebakker described a specific European style based on pre-World War II traditions, a topos which contemporary European textbooks still evoke (Hewstone et al. 2008:16). Social scientists, such as Charlotte Bühler, Paul F.
Lazarsfeld, and Marie Jahoda, laid the basis for empirical approaches to social psychology, according to Koekebakker, while, after World War II, the outstanding source of inspiration had been Kurt Lewin (1890–1947). Field theory and group dynamics marked the beginning of social psychology’s “renaissance” everywhere in Europe.\textsuperscript{10}

The special blend of Lewinian social psychology, combining experimentation with action research, social theory, and application, could certainly have served as an intellectual bridge, when the Committee ventured into Europe. While some of Lewin’s works on field theory had already been published in German before his emigration to the US in the 1930s, Festinger and most other US members of the Committee graduated with Lewin at the MIT Research Center for Group Dynamics in the 1940s. However, Koekebakker critically noted that, after Lewin’s untimely death in 1947, social psychology had lost its belief in comprehensive theories. Instead, empiricism and middle range theories dominated the field on both sides of the Atlantic, despite Europeans’ traditional “taste” for philosophy and fundamental issues.\textsuperscript{11} Festinger’s focus had shifted from group dynamics to intra-individual processes during the 1950s, resulting in his theory of cognitive dissonance, published in 1957. His research trajectory aligned with what has been described as the “new rigorism” (Schorske 1997) or “scientism” (Solovey 2020) in the human sciences in the US postwar era, when protagonists tried to push closer to the hard sciences through increased quantification and data-driven research, testing hypotheses on causal relationships between known and controlled inputs and carefully measured outputs. By contrast, the European post-war reconstruction era was different, according to Koekebakker, because psychologists focused on social issues and considered diverse methods as useful, from sociometry and Hawthorne-type observational techniques to non-hypothetical survey approaches, or psycho-analytical concepts.\textsuperscript{12} Social psychology was a young discipline and only institutionalized at a handful of European universities. Koekebakker acknowledged that American Fulbright scholars helped a great deal in making social psychology respectable at these places.
In this context, it seems appropriate that the inclusion of the second “E” for “Experimental” in the EAESP's name was controversial. Moscovici (& Markova 2006: 63, 67, 257) recalled that as early as during the first two sponsored European conferences, participants debated in informal groups whether social psychology should be limited to an experimental methodology. Against this background, it is rather surprising that he did not report any controversies at the time of the formation of the EAESP during the Royaumont conference in France in 1966 (ibid.: 94–100). This is all the more astonishing as, in the same book, he formulated a sharp retrospect critique, calling it a “historical mistake” that some European “mavericks” missed to reclaim their ancestors and defined their field via the experimental method, attracted by the American example (ibid.: 257). Attempting to develop a genuinely European theoretical perspective, which notably was fostered by a Ford Foundation grant from 1968 (ibid.: 139), EAESP members later started to vote on whether to remove the term “experimental” from their organization’s name (Tajfel 1972; Jahoda 1974). However, the recurrent motion only found a majority in 2008, when the organization’s name was changed to EASP.

**Implicit Theory**

Internal documents in 1967 still identify the Committee’s aim as encouraging the development of social psychology as a “basic experimental science.” However, in the same year, the Committee commissioned Claude Faucheux (1929–2015), a French social psychologist, who had translated Lewinian group dynamics into French and stood close to Moscovici, to produce a critical review of recent cross-cultural research in social psychology. The Committee spent a considerable amount of time on handling this text, which was only published nine years later in 1976. The files do not reveal the exact reason for the delay, but it is worth noting that a significant initial draft was already available in 1967. Rather than a mere literature review, Faucheux provided a fundamental critique of experimental studies in social psychology. Unlike Lewinian social psychology, recent research had become too reductionist as to still encompass
cultural reality, according to Faucheux, who distilled three recommendations for social psychologists from this finding: 1) achieve a better understanding of the epistemology of the social sciences, 2) become more broadly interdisciplinary, and 3) become better acquainted with foreign researchers in order to avoid their ethnocentrism. As we know, the Committee promoted indigenous research centers precisely to overcome the shortcomings of ethnocentrism. However, epistemological and methodological questions had not been controversial for the founders of the Committee, who adhered to the comparative approach of testing similar hypotheses in dissimilar cultures. By the late 1960s, however, at least some of the Committee members displayed a keen interest in reviewing fundamental assumptions underlying their discipline’s epistemology. In the midst of this, Festinger, for the first time, expressed his desire to step down from the chairmanship. Henry W. Riecken (1917–2012), the new acting president of the SSRC, himself a social psychologist, welcomed this decision, and he also endorsed the Committee’s representation of both American approaches and “European developments” in social psychology. Thus, in 1967, the records suggest that the Committee was adjusting its course, supported by the SSRC leadership.

Aspects of Faucheux’s critique reappeared in a 1970 idea for a conference on “implicit theory.” According to the Committee minutes, the explicit purpose of the conference was to re-examine US social psychology in the light of other approaches from countries where “different values and philosophies” were prevalent, not least to facilitate international understanding and communication. All members had encountered marked differences in approach, especially from Marxism and the New Left. P&P supported the Committee to explore these views by inviting advocates of diverse social theories, who were also experimentalists, to a planning meeting. The choice fell on epistemologist Hans Albert, sociologist Joachim Israel, and critical psychologist and Marxist Klaus Holzkamp. (Holzkamp could not be convinced to participate.) SSRC officer David Jenness took notes at this meeting, held near Heidelberg, Germany in 1971. Discussing the relationship of hermeneutics and experimental science, one participant claimed that the idea of cycles of examination of premises and assumptions counters any “logical-positivistic
position” in science, to which Jenness added “e.g. American experimental social [psychology].” US Committee member Donald T. Campbell (1916–1996) replied by arguing that science does include self-checking aspects, so “don’t let hermeneutics challenge ‘science’.” Moscovici, in turn, referred to hermeneutics as a “metadimension” that was not a threat to experimental science, and Joachim Israel (1920–2001) brought up the additional viewpoint of a Cold War “situational imbalance”: while the USSR needed to be more empirical, the West was “data-poisoned” and needed more theoretical and metatheoretical explanation. Israel’s remark was timely, as the network’s further expansion into Eastern Europe was already underway.

It is not clear from the files whether the planned conference actually took place. The Committee minutes maintain that Tajfel suggested to continue planning a conference, while Campbell preferred a more flexible format, allowing for the inclusion of university students, i.e., generational and ideological confrontation. Moscovici and Tajfel agreed on working out a programmatic proposal. However, an EAESP-sponsored working group on the same topic, including Tajfel and Israel, had already convened in Denmark in 1970, resulting in a collected volume in 1972. Tellingly, one US reviewer estimated that this book displayed the best of European thought on the largely American subdiscipline of social psychology. At the same time, he held that this thought was culturally so distant that it was unlikely to have any impact on American social science (Smith 1973). Given the Committee's original vision, the consideration of alternative social theories itself appears as rather surprising. Moscovici claimed that, while organizing the first European conferences, Lanzetta and his co-organizers had still taken care to reduce any metaphysical and epistemological aspects of social psychology, merely tolerating these aspects as European “foibles” (Moscovici & Markova 2006: 67). He also remembered his first conversation with Festinger and Stanley Schachter (1922–1997), during which he made frequent use of arguments derived from the philosophy of science, until Schachter exclaimed: “too French!” (ibid.: 72). The archival evidence suggests that the meta-theoretical discussions indeed only gained momentum after the European expansion of the Committee, and that
these Europeanization processes began to disrupt the Committee’s previously unquestioned epistemology.

At this point, it is important to note that the SSRC endorsed the Committee’s meta-theoretical explorations. In his history of the SSRC, Kenton W. Worcester (2001:77) has claimed that during the extended period of the 1970s the SSRC encouraged scientists to engage in foundational controversies about interpretative versus positivist logics of research, which he refers to as a quest for a new post-behavioral intellectual superstructure. From a perspective of systems theory, according to which a social system, such as the Committee, tends to filter and reduce the complexity of the environment in order to maintain its boundaries and inner equilibrium, the SSRC’s influence on the Committee appears as a significant factor (Parsons 1951; Luhmann 1984). Open systems, in particular, are characterized by an increased capacity to incorporate and process external stimuli, instead of counteracting them, and to reorganize internally to the effect of becoming more complex themselves, which ultimately helps the system to better adapt to changes occurring in the environment (Clegg & Bailey 2008). Through this lens, the endorsement of European traditions by the SSRC leadership and staff may be viewed as facilitating the incorporation of an external stimulus, which, in turn, prompted the Committee to reorganize internally. It is interesting to note that Moscovici did not recognize this influence of the SSRC in his memoirs. However, he emphasized another aspect, in order to make sense of the Committee’s receptiveness to European influences, namely that most Americans involved in the Committee were Lewinians. The Lewinian tradition formed an “invisible college,” according to Moscovici and Markova (2006: 202), which allowed the Committee to distance itself from the “pure” American mainstream of the 1950s and 60s.

**Diverse Opinions on Success or Failure**

By September 1972, change was explicitly addressed in both the P&P and Committee protocols: Jenness reported to the P&P that the Committee was
concerned about its future functions and membership. One or two US members were likely to resign soon after, because the Committee’s interests “no longer encompass theirs.” The Committee proposed appointing a European chairman, as Europeans had been most active. However, the P&P did not agree, because social psychology in Europe was already well developed, “in significant part” due to the Committee’s efforts. Future activities should concentrate on Latin America or other regions; in any case, the SSRC preferred a broader geographic range rather than a narrower one.21 The P&P also discussed the two upcoming meetings on applied social psychology. One officer interjected that the intellectual substance of both the Mallorca and Hungarian conferences “seemed diffuse.” The Mallorca meeting only brought together people who already knew each other, and the Hungarian conference was likely to only generate new personal contacts. Social psychology was not highly regarded in Eastern Europe and the Soviet Union, where interest centered on problem-defined areas and “concrete” work. Another officer’s response is remarkable; he objected that the Committee itself represented “a tradition” in social psychology that was “in the process of changing.” While the original mission of the Committee had been to “proselytize” American methods and theory, the field was now moving toward socially relevant concerns. The Europeans, with their “deeply anti-positivist” traditions, had initiated this change. In some ways, he concluded, the roles of Americans and Europeans have been reversed, and Americans were now learning from Europeans. Reinforcing this view, another officer added that the Committee was initially appointed when a “narrow” experimental social psychology was being energetically developed in this US. However, since then, the field had broadened, and currently, there was no longer a single model to export.22

Following this discussion, the P&P rated the Committee’s success as “more than modest,” because the whole structure of social psychology had been reorganized in Western Europe. The establishment of the European Journal of Social Psychology could also be seen as a by-product of the Committee’s activities. Therefore, the P&P unanimously decided to consider disbanding the Committee in the near future since its original mission was accomplished. However, if the Committee members wanted to continue, membership should change again to
include representation from other regions of the world and younger researchers. In other words, to continue existing, the Committee would need to remain an open system.

Following Worcester (2001: 34, footnote), the SSRC was itself designed as an “open-ended” structure—an open system—without any fixed direction or objective, except the conspicuously indeterminate true north of innovation and progress. In order to push the boundaries of both fundamental and practical knowledge, the SSRC relied on the cross-fertilizing impact of unusual constellations of researchers from two or more of its seven constituent societies ranging from history and political science to psychology. Since the 1920s, interdisciplinarity had been the “bureaucratic shorthand” for what the SSRC saw as its chief function. The Committee, with its focus on one single discipline, fell out of this general pattern, yet it also entailed cross-fertilizing moments due to its international composition. Understood as a fast-paced stimulus machine, the SSRC typically provided support for five to seven short years for each committee, the outcomes of which were to be cast into new institutional structures independent from the SSRC. Thus, the termination of a committee’s work was a sign of success, rather than failure, with the SSRC already racing forward to the next frontier.

Moscovici and Markova (2006: xvi, xv, 3) shared the SSRC's positive assessment of the Committee’s accomplishments, calling its mission “magnificent,” and its results “significant” or even “spectacular.” Other members of the Committee were more critical, such as Harold H. Kelley (1921–2003), who rated the Committee’s overall achievements as “moderately successful.” It is interesting that Kelley, in 1973, defined the Committee’s central purpose as stimulating the development and contacts of social psychology outside the US—without defining the kind of social psychology. Kelley’s opinion on the Committee’s achievements was largely in sync with Festinger’s view, who, shortly before ending his service on the Committee in 1973, produced a brief history of it. Festinger noted, “When a committee that has been established for a specific purpose remains in existence for such a long time, it is legitimate to wonder whether it still has a viable purpose or continues out of
inertia.” As one of two remaining members from the original membership, along with Moscovici, Festinger wondered whether “this represents instability or purposeful action.” He concurred with the SSRC’s view that the Western European operation had been a success, resulting in the establishment of the EAESP, summer schools for methodological training, and a European journal: “In this respect, the committee is happy to have become obsolete.” Ignoring the Committee’s interlude of implicit theory entirely, Festinger’s report then turns to the further extensions of the network. Because social psychologists in Eastern Europe and Latin America tended to emphasize applied work, he explained, the accompanying changes in membership did also entail a gradual movement towards problem-defined topics, as the Committee’s activities moved into “those directions.” In his view, the first East-West conference in Vienna in 1967 was only “seemingly highly successful,” as it did not translate into continuing effective cooperation, which was, after all, the actual purpose of the Committee. The second East-West meeting, held despite political turmoil in Prague in 1968, was “warm, active and, above all, emotionally arousing”—yet unproductive. Judged from 1973, it might even have been a hindrance, according to Festinger, because the Committee was unable to organize any other East-West meeting, despite all efforts, during the five years that followed. Reluctant to give up, the Committee only recently achieved a breakthrough by sending Tajfel as a personal emissary.

Festinger’s brief history testifies to the particular importance he attached to the substantive accomplishments of the Committee, of which he saw only three attempts being made. The first attempt aimed to produce a “coherent, integrated monograph” on the published literature relevant to transnational social psychology, pointing out the advantages or problems of various kinds of studies—Faucheux’s work. However, no coherent monograph was ever produced, according to Festinger, the Committee knew “little to nothing” about this matter. Thus, this project was the Committee’s “single outstanding unequivocal failure.” It is noteworthy that Festinger’s brief history does not mention the existence and content of Faucheux’s first substantial draft. Presented as early as in 1967, it criticized the bulk of experimental research as reductionist and offered concrete recommendations, including a thorough
rethinking of social psychology’s meta-scientific premises. As sketched above, the Committee subsequently engaged in foundational questions, during a period of critical self-reflection, which Festinger’s brief history does not mention at all. The second significant attempt Festinger was willing to acknowledge as an achievement without reservation: fostering the new European research program on minority influence, led by Moscovici. As the third notable attempt, Festinger identified the turn to application, which he attributed to the Committee’s encounters with Eastern Europe and Latin America, and which was still ongoing when he wrote his brief account.

In his final assessment of the Committee, Festinger’s verdict was rather bitter, and it is worth noting that SSRC officer Jenness was going to delete this paragraph in a revised version in 1973. According to Festinger, the Committee’s outcomes were “actually very narrow,” because its work “must ultimately be assessed in terms of whether or not it has made significant progress in moving toward a social psychology that has validity beyond national and cultural groupings.” While acknowledging that the US, Europe, and Latin America may have been too homogenous for this task, Festinger maintained his conviction that outcomes must be on a substantial level. The Committee had not really stimulated the development of new ideas. “Perhaps this is difficult for a committee to do but it might be useful to try.”

**Conclusion: From Thought Collective to Open System**

After having received sponsorship from the SSRC for ten years, Committee members managed to secure sponsorship from the International Council for Social Sciences (ICSS) in 1974, with Moscovici being appointed as the chairman. Tajfel and Moscovici worked out arrangements which would allow the Committee to have full autonomy regarding its membership, program, and method of operation. Moscovici recalled that being welcomed by the ICSS felt very good, compared to the continuous critical evaluation by the SSRC (ibid.: 249). It is striking that his memoirs hardly reflect the structural influences that the SSRC exerted with regard to the evolutionary development of the
Committee. Without further SSRC-induced pressure to continue evolving and changing membership, the Committee possibly acquired the form of a consistent thought collective in the following years as an ICSS committee. However, this part of the story is no longer documented by the SSRC archival files.

Systems theory suggests that systems can respond to challenging stimuli in two ways: counteracting them to maintain internal organization and equilibrium, or amplifying them by taking in the stimulus and reorganizing according to its information. I have argued in this report that the Committee's aspiration of transnationality set it on the amplifying path of an open system. In order to overcome the culture-boundedness of US research, the Committee constantly changed its membership along its geographic extensions, which, in turn, made the Committee's objectives change. The SSRC archival documents provide evidence that the SSRC played a crucial role in this development, because it started to actively sustain the Committee's acquired open-ended structure. Both the turn to meta-scientific theory and to applied research were backed by the SSRC, to the point that representatives of a more scientistic model of basic research, including Festinger, expressed their disappointment or even opted out of the group. While most SSRC leaders welcomed the new rigorism in the human sciences as a means to bolster the field, it is important to see that another part of the SSRC's identity was to act as a counterforce to overspecialization. As a result, the organization promoted multiple perspectives, which also entailed a pluralist stance towards questions of methods and evidence (Worcester 2001: 32). The case of the Committee has demonstrated that endorsed plurality—especially on the level of the discipline’s epistemology—prevented the group of being a consistent thought collective having a common style of thought that would have exerted an absolutely compulsive force (Fleck) on the individuals thinking. When the Committee prepared to settle in a new equilibrium to refocus exclusively on Europe, with a European chairmanship, the SSRC intervened again and demanded that the Committee continued evolving, expressed by a more diverse non-European membership.
The Committee members chose to find a new sponsor, the ICSS, that granted full autonomy on all levels, which might have resulted in a state of equilibrium allowing for a consistent thought collective to emerge. Without having followed this story any further here, it can be stated that the period of SSRC sponsorship does point to the significance of institutional frameworks regarding knowledge-making processes. As has been described, the SSRC was itself designed as an open-ended structure, not bound to any specific research program, and each generation was to make use of the SSRC's framework, which remained indeterminate in terms of content (ibid.: 34). This lack of commitment to certain contents or to a single philosophy of science is, in a way, reminiscent of a free-market economy’s mode of operation: the SSRC fostered permanent innovation and production of new variants, some of which were to prevail, others not, comparable to the quasi-evolutionary process of variation and selection in the context of market mechanisms. What the findings of this report also suggest is that to a certain extent, the trajectory of the Committee can be viewed as a reflection of the SSRC’s flexible structure. Moscovici found this structure stressful, at least towards the end of SSRC sponsorship, and Festinger expressed disappointment on the results of flexibility. In his view, the imposed structure failed to generate the substantial new ideas he had hoped for.

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2 SSRC records, accession 1, series 1, subseries 34, Committee on Transnational Social Psychology, Box 305, Folder 1759, Council minutes September 12-15, 1965, RAC. Festinger’s notion of “indigenous” research centers was already discussed in the P&P minutes on June 7, 1963 (ibid.). Cf. Moscovici’s recollection that he convinced Festinger of the idea of multi-centrism in 1964 (Moscovici & Markova 2006: 79-80).

3 Since the 1920s, the P&P’s purpose was to oversee the intellectual program of the SSRC between board meetings to support the Board of Directors and the Executive Committee. In 1996, the P&P was dissolved with its functions transferred to the Board. See Sills (1996).


5 On Lanzetta’s activities in Europe and the problem of ONR funding see Moscovici & Markova (2006: 12-15).

6 SSRC records, accession 1, series 1, subseries 34, Committee on Transnational Social Psychology, Box 304, Folder 1754, Minutes 1967-68, “Statement on aims and perspectives of the Committee on Transnational Social Psychology of the Social Science Research Council,” RAC.

7 Letter from Festinger to Herring on January 11, 1965, in: SSRC records, accession 2, series 1, Committee Projects, Subseries 74, Miscellaneous Files, Box 435, Folder 5243, RAC.


10 Koekebakker, Jaap, “Trends in Social Psychology in Western Europe,” p. 2, in: SSRC records, record group 1, series 1, Committee Projects – Miscellaneous Projects, subseries 19, box 223, folder 1355, RAC. Koekebakker identified as another European forerunner psychiatrist Jacob L. Moreno (1889–1974), who ran international conferences under the label of social psychology in the 1960s, a “popular confusion” from the viewpoint of some Committee members, see SSRC records, accession 1, series 1, subseries 34, Committee on Transnational Social Psychology, box 304, folder 1754, “Planning Committee Meeting, Aix-en-Provence, January 22-23, 1968,” p. 3, RAC.

11 Koekebakker, Jaap, “Trends in Social Psychology in Western Europe,” p. 8-9, in: SSRC records, record group 1, series 1, Committee Projects – Miscellaneous Projects, subseries 19, box 223, folder 1355, RAC.

12 Ibid.

13 See the two statements on aims and perspectives in SSRC records, accession 1, series 1, subseries 34, Committee on Transnational Social Psychology, box 304, folder 1754, RAC. See also Moscovici & Markova (2006: 11). This finding contradicts Moscovici’s interpretation that Americans were aware from the beginning that they could not promote only one field successful in the US. (ibid.)
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