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1993

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How to cite

SCHERER, Klaus R. Two faces of social psychology: European and North American perspectives. In: Social Science Information, 1993, vol. 32, n° 4, p. 515–552. doi: 10.1177/053901893032004001

This publication URL: <https://archive-ouverte.unige.ch/unige:102026>

Publication DOI: [10.1177/053901893032004001](https://doi.org/10.1177/053901893032004001)

Klaus R. Scherer

Two faces of social psychology: European and North American perspectives

I recently participated in a task force of scholars that had been charged to prepare a state-of-the-art report on "European Social Sciences in Transition". I was to discuss key developments in social psychology and point to promising research topics for the 1990s. I had hesitated to accept this charge, being quite aware of the danger of being accused of "self-appointment as judge and jury" in a case where clear-cut, indisputable evidence is virtually impossible to obtain. I decided to search for clues. And even though I had been asked to supply my personal, necessarily biased, assessment of the state of the art, I decided to make an effort at reality testing. For months I examined written traces of social psychological research, talked to and corresponded with esteemed colleagues in the discipline, and pondered over the question of how to evaluate progress in the field. Having come to the conclusion, shared with most of the colleagues polled, that major advances seemed less numerous than one might have hoped 30 years ago, I attempted a diagnosis of

This paper reports on materials that were used in the preparation of a commissioned state-of-the-art report on social psychology (Scherer, 1992). While some of the major points from the report are summarized in this paper, the former also contains several sections which are not touched on at all here. An earlier version of this paper was submitted to a social psychology journal and critically reviewed by several anonymous reviewers. I am grateful to these reviewers for having pointed to a number of oversights and for reminding me of some striking advances in our field, helping me to revise the paper appreciably. But I am particularly grateful for their unwitting illustration of some of the points made in this paper which I could not help but also include in the revision.

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Social Science Information (SAGE, London, Newbury Park and New Delhi), 32, 4 (1993), pp. 515-552.

the problems that might possibly account for this situation.¹ Finally, I had to muster sufficient courage to dare suggest possible cures for the perceived ills and to outline promising research areas for the future. All this with a strong sense of responsibility for the appropriate public representation of the discipline, knowing full well that a critical report would be severely challenged. The results of my labours are available for critical scrutiny by my colleagues (Scherer, 1992).

The first part of this paper presents some of the raw material upon which I based my analysis: a mail survey of social psychologists in Europe and the US, and an informal comparison of textbook coverage. Obviously, this material can only claim illustrative value. If I had been intent upon conducting a representative survey of social psychologists' opinions on the field or a systematic content analysis of textbooks, both the assessment procedures used and the report of the data would have taken rather different forms. However, this is not my research priority — I am not a professional stock-taker or social historian of research. In consequence, the observations presented here have to be taken for what they are — impressionistic glimpses of the practice of social psychology on two continents. I can also be accused of neglecting outright some of that practice, such as hermeneutics, interpretation theory, social construction approaches, rhetorical analysis, and similar areas, mentioned by the reviewers of an earlier version of this paper. Undoubtedly, all of these represent recent developments but, to my mind at least, not "key developments". I admit to one glaring neglect, namely the sociological branch of social psychology as it is taught in North American sociology departments. This tradition of research is quite different from the psychological branch of North American social psychology, which I focus upon exclusively in this paper. While my analysis may be biased, some of the conclusions may contribute to the ongoing debate about the past and future role of social psychology as a discipline straddling the fence between mainstream psychology and the social sciences (see also Fisch and Daniel, 1982; Israel and Tajfel, 1972; Leary, 1987; Manstead, 1990).

The remaining part of the paper consists of a brief summary of the analysis of the problems and the suggestions for change that I presented earlier (see Scherer, 1992, for further details). Much of this material may have limited news value for professional watchers of the field of social psychology; indeed, some of the issues have

been belaboured so frequently that they are now distasteful to many. Since this paper does not attempt to present an exercise in the sociology or history of the science of social psychology, no detailed review of the copious literature (Gergen, 1973; Hendrick, 1977; Israel and Tajfel, 1972; Jaspars, 1986; McGuire, 1973; Ring, 1967; and see Leary, 1987, for a comprehensive overview) will be provided. Suffice it to say that I am well aware of the fact that many of the points made below have been made repeatedly by other social psychologists in the past and that I am exposing myself to being accused of lack of scholarship. In no way do I claim any originality for the observations made, except for the fact that I have tried to engage in some empirical activities to back up my personal impressions. One of the reviewers of an earlier version of this paper argued: "if one is not able to further the field by cutting through the actual shortcomings and its reasons then one should not spend time repeating potential diagnoses and causes. This in itself does not seem to lead to progress and the development that is expected of the discipline" (anonymous reviewer). I could not disagree more.

Stock-taking does not require originality or innovation. Observations that were true twenty years ago might still be true today. And they might need to be stated over and over again. It is quite possible, of course, that some long-standing observations are no longer true, or at least not to the same extent. This is where the need for debate comes in. I would be very happy indeed to be proved wrong and see all my errors of judgement corrected. Taking stock is a collective activity and any one individual can do little more than provide a stimulus for debate. This is what this paper intends to do. Yet, this is not a "crisis paper". I do not believe that social psychology is in more of a crisis than other areas of psychology or other social and behavioural disciplines. However, this evaluation does not necessarily imply that we should continue happily to do what we are doing without asking too many questions, because questions are being asked of us — for example, questions concerning funding priorities in a period of shrinking research support. We can leave it to the powers that be — or to the established networks in the field — to make those decisions. It seems preferable to debate openly about where advances have been made and where more can be expected. In any case, self-indulgent resignation or forced optimism are unlikely to produce change.

A debate about advances and key developments needs to take the important differences between the European and the North

American traditions of social psychology into account. As will become clear on the basis of the material covered in this paper, it is difficult to treat social psychology as a monolithic discipline. The discussion of the differences in the European and North American approaches to studying phenomena of interest to social psychology may reveal more clearly some of the choices to be made and some of the criteria that might be useful in evaluating the development of the discipline. In consequence, I comment throughout the paper on the differences that I perceive (based on my experience in both of these worlds), and that my efforts at empirical study have unearthed.

An informal survey of opinions on key developments

To identify the areas that at present draw the attention of the social psychological research community, I consulted a small sample of well-known researchers in the field. I sent a questionnaire to approximately 80 social psychologists in the United States and Europe (40 each, drawn from the mailing lists of the Society for Experimental Social Psychology and the European Association of Experimental Social Psychology respectively), asking them, among other things, to identify the *key developments* which, in their mind, had “marked progress in our discipline during the past 20 years — theoretical breakthroughs, methodological advances, new areas of study, etc.”. No claim is made that the responses obtained are in any way representative of either European or North American social psychology, given that most requirements for random sampling have been violated (on the contrary, I tried systematically to include colleagues from many different research traditions). An approximately equal number of responses from Europeans and Americans was received; the response rate reached about 80%. The replies received to this and a number of other questions are used in the following in a qualitative and illustrative fashion. While this material does not constitute “hard data”, one can argue that anecdotal evidence such as this is preferable to no evidence at all — as has often been the case in papers and statements examining the state of social psychology.

The 1960s brought a “cognitive revolution” in psychology, affecting many areas in the discipline, including social psychology, and making cognitive psychology one of the prestige labels in the field.

As one might expect, cognitive social psychology or social cognition is very frequently mentioned as a key development area, particularly by American social psychologists. Unfortunately, "cognition", including social cognition, is used to cover very different areas of research and it is difficult to know which particular approaches are considered as having produced major contributions (except for attribution theory, which is repeatedly mentioned explicitly). It is quite probable, though, that many American colleagues think not so much of a specific cognitive phenomenon or process as of a particular approach to human functioning: the study of information processing. This is, of course, the general principle that has fuelled the cognitive revolution as a whole, partly due to the major role of computer models of information processing. Cognitive social psychologists, similar to the proponents of a "new look" on perception several decades earlier, have pointed to the shortcomings of the human being as an information processing device, and in particular to the powerful biases due to attitudes, moods and motives, to name but a few extra-cognitive factors that seem strongly to affect cognitive processing.

While quite a number of the European respondents mentioned "social cognition", probably in the sense described in the foregoing the large majority of them identified three eminently "European" research traditions as the most important recent developments: social identity and intergroup relations, and, somewhat less frequently, social representation. All of these concern, at least in part, cognitive processes but the emphasis is very different from that advocated by a social cognition approach: what is studied is not information processing per se but the sociocultural moulding of the content of major cognitive domains and their impact on attitudes and social behaviour. In each case, the roots of the theoretical concern go back much further than the relatively recent interest in cognitive processes as spurred by the development of "cognitive science": to Durkheim in the case of social representation (and to Moscovici's [1981] early insistence on the importance of this concept as an object of study for social psychologists) and to early social psychological work on perception and intergroup dynamics in the case of social identity/intergroup relations (due mainly to the strong impact of Tajfel's work on European psychologists; see Tajfel, 1981). It is true, of course, that there were early, and genuinely social psychological, cognitive approaches in American social psychology (e.g. Asch, 1956; Heider, 1958, and many others). But

the newer social cognition approach in American social psychology does not always build on this earlier tradition.

In spite of the small number of responses and the non-representative selection of respondents, these two modal tendencies for North American and European social psychologists strongly confirm an impression shared by many observers of the discipline: in North America the field of social psychology as a whole is moving much closer to mainstream psychology, and in particular towards cognitive psychology, embracing many of the same paradigms and models for research. Europeans, except for those exclusively trained in the US, remain strongly interested in some of the traditional concerns of social psychology as a discipline wedged between psychology and sociology. Whereas the individual and its functioning is becoming the paramount object of study in North American social psychology (with the "social" being part of the information to be processed) much of European social psychology, while studying individuals, is more interested in the social and cultural determinants of cognition and behaviour.

Next in line in the list of key developments is "emotion" and here North Americans and Europeans seem to agree. There are three major developments that could account for the fact that research on affect and emotion is being considered as an important new development: (1) the realization, as mentioned earlier, that many of the biases in human information processing could be due to underlying affective states (this is particularly noticeable in the "mood and cognition" literature), (2) new theoretical developments that claim a major role for cognitive processes in emotion elicitation and control and (3) the usual cyclical upswing of interest in non-rational explanations of human behaviour after a period of excessive reliance on rational models of man. These potential sources of the new interest in emotion make it understandable that much of the concern centres on "emotion-cognition-interaction", a key development mentioned specifically by quite a few of the respondents.

Only a few additional topics were suggested by more than one person: close relationships were mentioned by some of the North Americans; minority influence, language, and the self were referred to by some of the Europeans. Further, a number of different topics were listed by individual researchers (e.g. health, conflict, automaticity, prosocial behaviour, innovation, social psychophysiology, etc.) but one could consider it doubtful that these would be endorsed by many social psychologists as key developments in the

field as a whole. In summary, social cognition is the favourite for the North Americans, and the classic phenomena of social identity/intergroup dynamics and social representation are pre-eminent for many European social psychologists. A new area of promise, possibly for social psychologists everywhere, may be emotion research.

The question concerning "key developments" in social psychology generated mostly responses concerning topic areas and only very few "meta"-developments such as "emphasis on empirical approaches", "more general theories", "more emphasis on processes", "abandoning the rational model of man", "interest in cultural influences on social behaviour", or "influence from ethology and sociobiology" — to name but a few individual responses. None of these seems to have impressed the field strongly enough for them to be mentioned more readily in response to the question about what is *shaping* the discipline. This corresponds to a view that, apart from the growing importance of the information processing approach in North American social psychology, there has not been a major shift of paradigms in the field. This should not be taken to mean that there have not been shifts in attention and efforts to enlarge the gamut of alternative approaches to the study of social psychological questions — there have indeed been all of the tendencies mentioned above in addition to attempts toward greater interdisciplinarity, use of field studies, cross-cultural comparison, study of naive theories of behaviour, renewed interest in applied social psychology, and other developments. However, looking at the field as a whole, it seems that none of these has provoked a major reorientation so far.

A similar picture emerges for methodological developments. Refinements in multivariate statistics and general causal modelling are mentioned by several North Americans. For the rest there is a mixed bag of individual observations concerning longitudinal designs, meta analysis, video observation possibilities and non-obtrusive methods. Again, there can be no doubt that major advances have been made in a number of methodological approaches and that these have affected the way social psychologists work. Yet, no major *reorientation* of how social psychologists go about the business of doing research has been forthcoming as a result of methodological development.

To repeat: there can be no doubt that meta analyses, structural modelling and many other methodological developments do have a

strong impact on the field, are found with increasing frequency in the journals, and often result in a striking increase in sophistication. However, most of these methods have not originally been developed by social psychologists nor are they necessarily changing research procedures and priorities to the point where they could be considered *key developments*.

An informal comparison of textbook coverage

Most scientists do not like textbooks. They are considered to be too selective, too superficial, too trendy and too much geared to the uninitiated. Yet, they are also the display window of a discipline. It is here that a field tries to put its best foot forward to lure unsuspecting undergraduates into a career or at least to elicit some positive response. In consequence, textbook authors will try their best to mention the key developments and to highlight what they consider “advances” in knowledge, understanding and ability to predict behaviour.

I re-examined three social psychology textbooks with which I am reasonably familiar: the one I read during my undergraduate days (Krech et al., *Individual in Society*, 1962 — henceforth referred to as OldUS), a relatively recent and popular North American textbook (Wrightsmann and Deaux, *Social Psychology in the 80ies*, 1981 — from now on NewUS) and a recent European volume (Hewstone et al., *Introduction to Social Psychology*, 1988, below referred to as NewEU).² In comparing the coverage of the three texts, I tried to determine how much the topics and questions had changed and to what extent there were new answers to questions that still seem central to the field.

I used OldUS as a basis for outlining some of the classic issues for social psychological inquiry. The book is divided into four major parts: (1) basic psychological factors (cognition, motivation, personality), (2) social attitudes, (3) social and cultural habitat, and (4) groups, organizations and the individual. Of these four topics, we find attitudes and group phenomena represented in a similar fashion in NewUS and NewEU. The third theme — language, society and culture — is altogether absent from the two recent texts. Basic psychological factors are treated in somewhat different ways in the latter: there are occasional references to motivation and personality differences, often inserted in the discussion of different topics.

Social cognition

Given the obvious importance of social cognition in present-day social psychology, it is interesting to examine the issue of advances in this respect. The only clear case of continuity is the cognitive inference of cause and effect. The authors of OldUS note: "This is an extremely important question because much of our social action is shaped by the way we perceive cause and effect" (p. 27) and go on to describe the early work of Heider (1958) and Michotte (1954). In the recent texts this topic is dealt with extensively under the heading of attribution. Even the cursory treatment in the textbooks shows that major advances have been made in this area, based on the work of the pioneers. On the theoretical level, elegant and rather sophisticated models of the personal and situational factors that determine the attribution process have been proposed, and detailed predictions made. On the empirical level an almost countless number of studies in the laboratory, but also in the field, have demonstrated the validity of many of the theoretical predictions. Small wonder, then, that attribution is consistently put forward as the show-horse of social psychology. One reason for this success is the relatively large extent of cumulateness in this field which has resulted in a better-than-average description of the factors and the processes underlying causal attribution. Furthermore, many of the predictions are relevant for real-life phenomena and provide important insights for trying to analyse concrete behaviour.

The opposite is true for another important topic within the OldUS's cognition section: person perception ("the cognitive process of greatest importance to the social psychologist: how people perceive and judge one another and how they perceive and judge public figures — the power elite in the societies of the world", OldUS, p. 51). The topic is virtually absent from NewEU; it is dealt with in 6–8 pages in NewUS which reviews essentially the same studies as those reviewed in OldUS twenty years earlier. One would be hard pressed to find any advance in this area, which is so obviously relevant for social cognition and social interaction, since it deals with the input to whatever dynamics will unfold afterwards. This impression is confirmed by the conspicuous absence of research monographs and the rather small number of papers in this area. Partly, this may be an effect of what one could call "paradigm oriented research" — the relatively fruitless effort to deal with this topic on the level of judgements of adjective lists, an example of the absence

of an ecologically valid phenomenon (how often do we judge other people on the basis of a list of adjectives on a piece of paper?). Interestingly enough, it is an important, real-world phenomenon, eyewitness testimony, that constitutes the only recent example in NewUS's discussion of social perception, and one where one might indeed be tempted to speak of an advance in our understanding of the phenomenon.³

What about some of the magic words in modern social cognition research: schemata, scripts, prototypes, heuristics, etc? NewUS is remarkably silent about all this — maybe the peak of this key development occurred after the authors finished their third edition. NewEU has a chapter in which some of the essential approaches are presented, together with a rather nice discussion of alternative models of the person as a social cognizer. Are these developments to be considered as advances over OldUS's early treatment of cognitive organization and change? The concepts have certainly become more precise and more professional. And, as a perusal of the three-volume *Handbook of Social Cognition* (Wyer and Srull, 1984) shows, so have the methods and paradigms in the empirical studies. It is not certain, though, whether the same questions are still being asked. Together with the concepts, the theories, the research paradigms (masking, priming) and the dependent variables (reaction time above all), social psychologists have imported the research agenda of cognitive psychology and applied it to things social. Thus, here is a clear case of a key development. It may be too early to decide on the degree of real advancement but the chances seem good, particularly with respect to the models of cognitive processing mechanisms.

Emotion and motivation

To note briefly the other basic psychological factors discussed in OldUS: social motivation has been neglected generally in the field and is treated haphazardly in the two recent texts; emotion is absent altogether (as it had been, to be fair, in OldUS), except for occasional references to Schachter's two-factor emotion theory. The study of the self, the importance of which is highlighted in OldUS, is not represented in NewEU, and is treated almost exclusively under the angle of impression management in NewUS. "Interpersonal response traits" in OldUS, i.e. social effects of personality, are

partly dealt with under "individual differences" in NewUS; they are absent in NewEU. If the final outcome of the bitterly fought person-situation debate in personality psychology (i.e. the rather obvious conclusion that behaviour is determined by an interaction of both factors) is anything to go by, we should not count too much on major advances in this domain.

On the whole, then, as far as advances in the social psychological study of the basic psychological processes involved in social behaviour are concerned (perception and cognition, learning, motivation and affect, personality dispositions), advances are few and far between, with the major exception of the causal attribution area and some other social cognition domains.

Social attitudes

Attitudes, their nature and their change, are about the only topic treated in a very similar manner and with comparable emphasis in all three texts. Thus, we should have a good basis for comparison and for judging advances. If quantity of research were a criterion, attitude research would be a rather advanced field and the sheer size and complexity of the empirical work makes it very hard to decide how much the plethora of significant results has increased our understanding of the concept. Two questions can be used as a litmus test for the presence of advancement: (1) to what extent can attitudes predict behaviour, and (2) how can we change someone's attitude, by changing his/her behaviour or via persuasion? Regarding the first question — the received opinion seems to be that Ajzen and Fishbein's (1980) theory of reasoned action is the final answer to the problem, and one that seems supported by empirical data. The theory holds that a person's attitude toward a very specific behaviour (i.e. thinking it good or bad to carry empty bottles to a recycling container) and his/her subjective evaluation of normative pressure existing for or against that specific action, jointly determine the occurrence of the behaviour (assuming that the behaviour in question is under the individual's volitional control). Indeed, this seems an eminently reasonable position and one that will undoubtedly continue to be supported by data in properly controlled experiments. But does it constitute an advance over what the pioneers thought about the determinants of behaviour or can it be attacked as something that our grandmothers could have told us a

long time ago? It certainly does mark a major step forward with respect to the earlier idea that *general* attitudes could predict *specific* behaviours and the respondent response to the discovery that this was not the case (e.g. Wicker, 1969). In fact, we may have to count the rectification of errors and the abandonment of blind alleys among the advances. Also, recent work on the processes whereby attitudes affect behaviour holds much promise for advancing our understanding of the attitude-behaviour relationship (e.g. Fazio and Williams, 1986).

Regarding the second question — attitude change — the volumes of social psychological journals abound with papers on attitude change as produced by experimenter-induced counter-attitudinal behaviour, attempting to demonstrate the superiority of a specific theoretical account to explain the phenomenon, i.e. via dissonance theory, reinforcement theory, self-perception theory, or impression management theory, to name only the most important ones. The authors of the respective chapter in NewEU conclude their concise summary of the debate as follows: “After decades of attempts to pit these theories against each other, a consensus seems to have emerged that they should be regarded as complementary rather than mutually exclusive formulations. Thus, instead of attempting to conduct ‘crucial’ experiments, researchers have begun to develop and explore possible conceptual frameworks that would integrate the psychological processes described by these theories” (NewEU, p. 193). Again, an eminently reasonable and probably wise decision by the scholars concerned — there is bound to be a kernel of truth in any theory — but how does this hermeneutically meaningful approach help to predict, and, yes, produce attitude change in specific situations? Would we not need to know whether an apparent change in attitude was indeed real, and that it had been produced by the motivational dissonance dynamic due to our manipulation, or whether it was feigned, and part of the target person’s impression management designed to prove the consistency of his/her behaviour? The latter type of compliance is unlikely to have lasting effects on attitudes or behaviour (Kelman, 1961). This does not exclude the postulate that impression management can, in the long run, also produce real and enduring attitude change. Yet, it would seem that the crucial advance in our knowledge would consist of our ability actually to induce very specific processes. It remains to be seen to what extent “integrative conceptual frameworks” alone can further our understanding of the underlying processes.

Persuasion, particularly in realistic contexts and via mass media, has been greatly neglected in social psychology, with textbook and handbook chapter authors content to reiterate the issues studied by the pioneers (e.g. source, message, channel, context, recipient, etc.; see McGuire, 1969; NewUS, pp. 358–64). Recent texts highlight bi-modal or dual route concepts proposed by Petty and Cacioppo (1986) and Eagly and Chaiken (1984). To simplify, these authors argue that the recipient will only under certain conditions (e.g. high ego involvement in the issue) consciously perceive and cognitively elaborate the content of a persuasive message. Under this condition of in-depth processing, the person may in fact be affected by the information or the argument in the message, that is, via the central route. If there is not enough attention or interest, there may be influence via heuristic processing on a peripheral route, e.g. an effect of the prestige of the source or the affective impact of the smiling face of a pretty woman in a TV commercial, these effects being of less amplitude and duration than central effects. While the reinvigoration of persuasion research by these approaches is most welcome, their status in terms of advances is less clear. The partitioning of the persuasive process into two distinct modes may deflect attention from the integral nature of the process. Furthermore, thus far these formulations are not very specific in terms of independently defined factors that can actually bring about persuasive effects.

On the whole, then, despite several decades of attitude change research in social psychology, our discipline does not seem to threaten the livelihood of professional attitude changers such as advertising copy writers, political campaign advisers, or public relations specialists (although there are obviously examples of social psychological findings being employed in applied settings — especially in the health area). Yet, it would seem that the proof of the advances is right there — can we, depending on the ethics involved, predict and produce, or counteract attitudinal change? Can we reduce prejudice faster than that which seems to occur via enforced contact over lengthy periods of time? So far, the evidence is not overwhelming.

Group dynamics

The other large domain which remains centrally important across generations of textbooks concerns group dynamics. The major issues reviewed are norms, roles, group performance and effectiveness, leadership and social influence (particularly conformity). While the two recent texts (1984, 1986) treat the area in several chapters, although less extensively than OldUS, there are differences. To begin with, the more recent texts discuss classic topics such as group structure and formation, and large organizations in less detail and with fewer applied examples — the emphasis here is on small-group research and on social influence. Two specific issues are chosen to allow direct comparison: leadership and conformity. For brevity's sake, I will summarize my conclusions fairly bluntly: neither for leadership (where the discussion usually ends with Fiedler's [1967] contingency model and the imperfections thereof) nor for conformity — dealing mostly with Sherif's (1951) and Asch's (1956) work — are major advances visible, even though, as for all the other topics discussed, there have been many new studies, introducing new variables, showing complicated interactions. Yet, for the essential business of explaining and predicting, the textbook authors seem to be quite content to stay with the pioneers. It should be added that there has been a key development in Europe with respect to conformity, namely Moscovici's (1980) demonstration of minority influence effects which is gradually beginning to be discussed in North American social psychology (Nemeth, 1986). Interestingly enough, compared with paradigm oriented studies in much of the classical research, Moscovici was cued to the effect by real phenomena — such as the powerful effects of minority opinions in revolutionary settings, as in the 1968 student revolt.

Apart from the interesting minority influence effect, then, we find few real advances in the area of group studies, confirming the impression voiced by quite a few of the respondents in the informal survey, that small-group and organization research had been greatly neglected in the past decade. This is all the more regrettable because of the important role of these studies for applied social psychological work in private and public organizations and institutions. It is also surprising given the origin of the pioneering studies and the former status of this area in the field. In line with the earlier discussion of a special European focus, in NewEU we find two separate chapters on issues that are often only mentioned in passing in North

American texts: conflict vs. cooperation and intergroup relations. The large number of studies in these areas, often using applied contexts, are reassuring proof of the vitality of research on these important topics. Again, it may be too early to decide whether major advances over the contributions of the pioneers have been made.

From the discussion so far it may seem as though social psychology did not show very much progress, with the two more recent texts in our sample seeming actually more limited in coverage than the 1962 edition of OldUS. Obviously this is not the case. There is in fact a lot of new information in the two recent texts. To begin with, there are chapters on methodology which testify to important advances in the variety, inventiveness and methodological rigour of the experimental and analytical techniques now employed in social psychology. The NewEU even has chapters on the history of social psychology, on ethology and its importance for social psychology, as well as on developmental aspects, nicely demonstrating European concern with both history and a phenomenon-oriented interdisciplinary approach.

In addition, in both recent texts we find chapters on issues which are not even listed in the index of OldUS. These are helping/prosocial behaviour and aggression (covered in both texts), attraction/love, sexual behaviour, morality/justice, physical environment (NewUS only), and relationships (NewEU only). Evidently, social psychologists have not been idle — work in the past two decades has greatly expanded the number of phenomena studied by social psychologists, it has brought about an enlargement and refinement of the conceptual apparatus, it has produced an enormous body of empirical data, mostly obtained in methodologically sophisticated studies, and it has opened the field for future cooperation with neighbouring disciplines.

Yet, one may wonder whether there are many new answers, or more definitive ones, to the questions asked by the pioneers in the field. There remains a lingering doubt about the capitalization on the countless important research contributions — which often seem exceedingly difficult to order and to integrate — for the business at hand: the explanation and prediction of social behaviour and the understanding of the processes involved. As shown earlier social psychologists have multiplied the number of phenomena considered relevant for psychological research, and the number of concepts, theories, models and mechanisms used for the explanation of these phenomena. They have also multiplied the number of methods and

research paradigms, and, last but not least, the number of complex findings involving ever more variables and their interactions. Clearly, the dominant research strategy, based on clean ANOVA designs, encourages the production of complexity and fragmentation in terms of the number and interrelation of variables that may have an effect on a type of behaviour in the laboratory. This is no reproach. Social behaviour is indeed exceedingly complex and may defy simple prediction as suggested by the natural science model. It is also true, of course, that as a discipline progresses "big" advances and insights like those characteristically made by the pioneers in a new area become increasingly rare. They are necessarily followed by more small-scale research, asking more refined questions and yielding less spectacular results. As a reviewer of an earlier version of this paper pointed out: now that the double helix is established, molecular genetics is also turning toward more modest issues such as particular combinations of proteins or the characteristics of specific receptors. Now that the pioneering days are over, social psychologists may also have to be content with identifying ever more determinants, factors and variables. Unfortunately, it is unlikely that we can build quite such deterministic models as are possible in the natural sciences. We may have to deal with this complexity in a hermeneutic rather than strictly predictive fashion.

It is exceedingly difficult to take stock of the advances in the field given the enormous differences in the criteria one could use and the conclusions that would follow. The personality disposition of the stock-taker may also be an important factor: optimists might see a much brighter picture than pessimists. Some of the reviewers of an earlier version of this paper tended to be wildly enthusiastic about the progress of social psychology and were in total disagreement with my assessment, based on my own impressions and on the informal survey. However, given that a somewhat sceptical outlook is shared by a number of colleagues, one might inquire into the possible causes.

Perceived causes for the assumed lack of major advancement

In the questionnaire which I sent out to social psychologists in Europe and North America, a number of issues relevant to a diagnosis of this situation were raised.

Shortcomings

Do social psychologists agree on what could be holding up progress in the field? There is one issue of overriding concern for a large number of respondents in both North America and Europe: the strong feeling that research is too oriented toward the laboratory, that field research or the use of non-experimental, non-laboratory methods is lacking, resulting in the inability to generalize findings to settings in the real world. This is a perennial issue in social psychology and a very sensitive one, particularly when coupled with the accusation that social psychological research is "trivial". Another shortcoming felt by quite a few North American social psychologists is the non-social nature of much of (North American?) social psychology and the almost exclusive concern with individualistic phenomena. As we shall see below, North American and European social psychologists seem to agree that this is a specific characteristic of North American social psychology. Several European social psychologists, on the other hand, deplore the lack of integration in the field and the proliferation of very diverse topics.

No other shortcoming incites the furore of more than a sprinkling of respondents, but the collection of woes is quite impressive and may reflect a deep-seated feeling of unease: narrow disciplinary focus, research driven by previous work rather than by important questions, atheoreticalness, ahistoricity, absence of creativity, ethnocentric theorizing, lack of daring in theorizing, not enough process orientation, lack of agreement on conceptualizations/definitions/operationalizations, lack of shared variables, dearth of valid instruments, too little attention to sampling issues, lack of methodological sophistication, ignoring negative results, lack of replication, lack of cumulativeness, and so on. Two of the respondents turn against these critics: they believe that what is wrong with social psychology is the "preference for sterile criticism" and "lack of confidence combined with bad marketing".

Neglected areas

Another question in the informal survey concerned potential neglect of important phenomena in theory and research. All of the respondents, except one or two, had a favourite area of neglect

although there was very little overlap of the topics mentioned. While most of these are being studied, albeit possibly not sufficiently intensively, a few of the issues proposed might indeed be fairly said to be untouched by social psychological researchers: greed, money, “world mindedness” and social psychological interpretations of past history. Yet, lack of progress in social psychology does not seem to be due to the flagrant neglect of one or more phenomena essential to the field.

Given the widespread emphasis on the need for interdisciplinary approaches (which are often considered to be neglected by social psychologists), I asked whether the respondents had personally encountered interdisciplinary projects that really worked. A surprisingly large number answered in the affirmative, in both regions, pointing in particular to areas such as organizations and political behaviour, health and emotion. A similar question concerned cross-cultural comparison — does it help to advance knowledge or establish generalizability in the respondent’s area of research? Except for very few respondents who indicated that this would not be of great importance to their research, almost all agreed that this would be most desirable, often giving specific examples of what could be learned. However, virtually no specific projects were reported.

Summarizing the well-known diagnoses of the problem

I have summarized the criticisms mentioned in the questionnaire responses, and indeed by most earlier commentators, as the “seven deadly sins” of social psychological research: lack of integrative theories, lack of conceptual and definitional overlap, lack of methodological standardization, lack of replication, lack of cumulativeness, lack of generalizability, and lack of ecological validity. I will summarize briefly the arguments on each point (see Scherer, 1992, for details and examples).

Lack of integrative theories

George Miller has suggested that psychology, prone to succumb habitually to a state of “analytic pathology”, has been repeatedly snatched from the brink of futility by a new application, or a new

integrative theory providing synthetic insights, or both (Miller, 1986). Two conditions must be met for the salutary effect of theory to work: one, the theory must provide real integration — not just of the work of the person proposing the theory but also of the work of other researchers; two, negative results, and their publication, are essential to evaluate the merits of a theory. In social psychology, researchers have questions, expectations, and often even clearly operationalized hypotheses but these rarely emanate from a more integrative theory. Often, whatever the results of the study, there is little consequence for a more encompassing explanatory framework (see also Hendrick, 1977; McCallum and McCallum and McCallum, 1987; McGuire, 1973; Ring, 1967).

Clearly, integrative theories that allow stringent predictions and require modification upon disconfirmation are rare. Even when such theories exist, little effort is expended in testing them in an appropriate fashion. In some cases, given the complexity of the phenomena under study, this might require the joint efforts of several research groups. Partial models or theories are unlikely to provide the degree of integration and synthesis which could save social psychology, following Miller's analysis, from futility and analytic pathology.

Lack of conceptual and definitional overlap

There is very little standardization of concepts and definitions in our field. Few researchers work on the same concept or use the same variables (which does not facilitate cumulativeness; see below), they often use the same terms to mean quite different things. As one might expect, this generates much fruitless controversy clogging up the literature (as an example, see Leventhal and Scherer, 1987, for a review of the emotion-cognition controversy which is largely due to the widely discrepant notions of both emotion and cognition between the parties concerned).

Lack of methodological standardization

Social psychologists often use rather different operationalizations, variables and measurement procedures for the same phenomenon. This is probably unavoidable in the case of psychological constructs for which there are competing approaches to measurement. It becomes less understandable in cases where even fairly simple measurement operations are widely divergent in a particular field of research (e.g. non-verbal behaviour or group behaviour).

Lack of replication

This problem is well known and frequently discussed. We cannot blame researchers for shying away from doing replication studies given the predictable responses of many journal editors to this kind of data. To exaggerate just slightly: if there is confirmation, the study is judged to be unoriginal and of little interest. If there is lack of confirmation, the study is judged to be troublesome, potentially flawed and also of little interest. On the other hand, one has to see the point of the journal editor: in the absence of an integrative theory or at least an agreed explanatory framework, both confirmation and non-confirmation are less interesting than some striking new data or imaginative paradigm. The lack of interest in replication and negative results can be taken as a sign of the absence of the guiding force of powerful real-life phenomena to be explained, and of serious theorizing.

It could be argued that the advent of meta-analysis has rendered exact replication superfluous. This is only partly true. The results of meta-analyses can only be as good as the set of comparable studies upon which they are based. Replication, at least in a broad sense, or, if one prefers, copious research activity on a defined topic with comparable methodology, is thus a precondition for meta-analysis.

Lack of cumulativeness

It is frequently suggested that there is little real accumulation of findings in social psychology and that much of the empirical evidence published seems piecemeal. This may again be due to the absence of an overarching framework which would permit the integration of individual findings into a larger knowledge set, like fitting a piece of a puzzle into a predefined Gestalt and making sure to accommodate other interlocking pieces. It is often difficult to define the larger Gestalt to which a particular study is supposed to contribute an element. Furthermore, it is not always evident that researchers worry about fitting in neighbouring pieces. Tools for meta-analysis have been available for some time now and the number of review articles using meta-analysis of prior studies increases regularly. Unfortunately, in many areas there is a dearth of studies that are sufficiently comparable with respect to conceptual assumptions and methodological procedures to allow the straightforward use of meta-analytic procedures. And even the most sophisticated meta-analytic procedures are unlikely to solve the problem of the

underrepresentation of negative results in the published literature (see Rosenthal, 1990).

Lack of generalizability

Contrary to areas like perception, or physiology, where the object of study is a relatively universal organismic mechanism, the cognitions and behaviours studied by social psychologists almost always depend on a sociocultural context. In consequence, many critics have argued that the validity of many social psychological findings is limited to contexts similar to those of the original study, a problem which is particularly worrying for relatively contrived laboratory situations. Furthermore, subjects are almost always college undergraduates with a special interest in psychology. Do we manufacture our own reality, limited to college students in bogus laboratory situations? It is neither students as subjects nor a laboratory setting that necessarily produces generalization problems. For example, if one uses realistic verbal aggression rather than electroshock in a realistic laboratory setting, the results are likely to be much more readily generalizable to other settings. The major problem, then, seems to be the artificiality of the paradigms rather than the method of study.

Lack of ecological validity

When Brunswik (1956) introduced the term he did not exclude the fact that one could run experiments or that one had to conduct field studies, or simulating the real world as truthfully as possible, to attain this goal. Rather, he pointed out that ecological validity is directly related to problems of generalizability. The basic prerequisite for ecological validity *and* generalizability is the existence of a real *phenomenon* as an object of study. Furthermore, this phenomenon should first be observed closely in the field and only then carried to the laboratory (maintaining its major structural features in order to allow later generalization). In social psychology one often encounters a focus on "standard research paradigms" (handed down from generation to generation in particular research "traditions") rather than concern with specific phenomena. It is understandable, of course, that researchers try to introduce ever more subtle variations into classic designs to understand better the complex interactions of the causal factors. However, one should not lose touch with the real phenomenon to be explained, simply in

order to avoid problems with both generalizability and ecological validity.

It has been pointed out repeatedly that the statistical significance of our results does not protect us against such problems. Social psychologists often create an impoverished situation, a social vacuum, as a context for their studies. In this case any kind of experimental manipulation will produce a strong difference, given the absence of resistance of any prior anchoring structure. Just as very gentle winds can blow dry leaves on the ground through the air, trivial persuasion messages can produce significant effects in domains where subjects have no strong prior convictions or commitments. There is no need here to point to the large literature on suggestibility, demand characteristics and the experimenter's expectations (see Rosenthal and Rosnow, 1969). I believe that the social psychological literature is replete with strong (and often highly replicable) but relatively meaningless effects, mostly due to the absence of ecological validity in experimental situations and manipulations.

The well-known Schachter-Singer experiment (Schachter and Singer, 1962) can be used to illustrate the damage done by ready acceptance of unreplicated results of laboratory experiments lacking ecological validity. The attempts at replication that have been made seem to have been quite unsuccessful (see Reisenzein, 1983). In addition, the experimental manipulation (injection of epinephrine to produce physiological arousal and the consequent manipulation of situational context) cannot claim strong ecological validity. Few would disagree that in real life the emotion-eliciting situations or events tend to precede the emotion-accompanying arousal (which may in fact be quite specific to particular emotions; see Frijda, 1986). Yet, the Schachter-Singer theory of the emotions, based on that particular experiment and claiming that emotions consist of cognitive attributions following unspecific peripheral arousal, has dominated the social psychology textbook sections devoted to emotion (see the foregoing). One of the reasons for the ready acceptance of this rather implausible theory may be the very lack of ecological validity itself: the experiment was cute and attention-provoking, the results non-trivial. In addition, the theory was well matched to the cognitivist mood dominant in the field at the time. The price may have been a serious retardation of ecologically valid emotion research during the last three decades.

Concern with generalization and ecological validity does not at all

imply that we have to give up experimentation, or stop using college students in our studies. Experimentation remains the royal road toward increasing our understanding and the social psychological experiment is a method we cannot do without. We also have to rely on our students to serve as subjects in these studies. But we need to be careful in choosing the phenomena and the mechanisms that lend themselves to be studied profitably in such manner.

European and North American social psychology are equally likely to be affected by the problems reviewed in the foregoing. Yet, there might be some differences worth exploring. One has the impression that European social psychologists find it somewhat easier to engage in theorizing, possibly because of a somewhat closer affinity to the philosophical bases of psychology. They also seem to abhor dust-bowl empiricism and strive to anchor research in a clearly identified theoretical context. US social psychologists, on the other hand, seem to find it relatively easier to stick to paradigm-oriented research. This may be partly due to the fact that there are many more established PhD programmes where paradigms pioneered by eminent researchers are often perpetuated through several generations of graduate students. This is much less frequent in European social psychology where doctoral students are less numerous and often less closely affiliated to ongoing research activities in large teams. Because of the different national research traditions in Europe, both conceptual and methodological standardization are much more difficult to achieve compared with the US, where frequent meetings of the scientific societies and the demands of reviewers (both for grants and publications) exert a much stronger pressure for standardization. Replication is equally problematic in both areas, although it is true that there have been a number of attempts to replicate US findings in Europe, given the accrued interest in trying to establish whether important results hold up in different cultural contexts. However, such efforts seem to have become less frequent compared with the 1960s and 1970s.

With respect to cumulativeness, one would expect much more favourable conditions in North America given the common language, the efficient organization of scientific societies, funding and publication, as well as the large number of researchers. However, the relatively strong competition for grants and journal space (see below) bestows a premium on creativity and individual achievement (often in the sense of contrast enhancement), which do not always further cumulativeness. As one might expect, generalizability and

ecological validity may suffer from strong paradigmatic orientations (to the detriment of the underlying phenomenon). North American social psychology may be more affected by this risk because of the powerful imprinting in graduate schools as described earlier. Furthermore, the ready availability of undergraduate college students as subjects in most US universities (due to the system of experimental credits as course requirements — something which is much less generalized in Europe) obviously encourages a heavy reliance by North American social psychologists on this population for study (with the resulting effects on generalizability and ecological validity described previously).

Finding explanations

From the analysis of the shortcomings of our discipline one might gain the impression that social psychologists are worse scientists than their colleagues in other disciplines. Nothing could be further from the truth, of course. Of the many possible causes for the problems faced in advancing our knowledge about human social behaviour, the following four seem to be particularly salient (see Scherer, 1992, for further details).

Complexity of the phenomena studied

The phenomena studied by social psychology are excessively complex, spanning the range from internal psychological processes that control behaviour, over the interaction between individuals in dyads and small groups, to highly aggregated concepts such as ethnic groups and cultures. The vast repertoire of possible behaviours, the extreme variability of behavioural responses to events, the strong context-dependence of human behaviour and the sizeable individual differences between people, do not lend themselves easily to systematic, experimental study. Individual differences are particularly troublesome for social psychologists since it is each individual's subjective interpretation of a situation which defines social reality and in consequence the individual's reaction. This makes it difficult to introduce stringently controlled experimental manipulations. A major problem for experimental manipulation, particularly with respect to deception about the purpose of an

experiment, is the ethical restrictions imposed by individual concern and human subjects committees. Finally, there is the cost factor involved in studying large numbers of human subjects.

Contradiction between different demands

Social psychologists often encounter conflicting demands with respect to research design. The need for balanced experimental designs with complete permutation of order of presentation and other requirements automatically reduces the chances of maximizing ecological validity. It is not surprising that powerful statistical methods were originally developed in agriculture rather than in the behavioural sciences: plants and plots are more readily permutable than human behaviour. The requirement for precise measurement often conflicts with the demand for unobtrusiveness. Methodological precision is often seen as more prestigious and more readily evaluated, and journal editors and reviewers consistently favour papers excelling in this respect, to the detriment of the conflicting values of ecological validity, generalizability, cumulativeness, or theoretical integration. It is not difficult to find many other examples of such conflicting demands — originality vs. replication, innovation vs. standardization, phenomenon orientation vs. theory orientation, etc.

Development of the discipline

The development of social psychology has been explosive. The tremendous increase in research activities which occurred in a fairly short period of time can be likened to a gold rush with diggers streaming into the area, busily burrowing into the ground and jealously defending their small claims. In more established disciplines researchers can afford to engage in procedures of ploughing, sowing and reaping on traditionally well-delimited plots of land. The relative lack of clear definitions of important phenomena, of conceptual consensus, of methodological standardization and of research guiding integrative theories might be due to the lack of sufficient time and peace of mind to sift out the useful from the useless, the essential from the ephemeral, and to develop theories and research procedures with a view to long-term accumulation and generalization rather than short-term gain.

Academic reward structure

There can be little doubt that the academic reward structures in the field favour short-term gain over long-term development. Since number of publications and quality of the respective journals are the most prominent factors in an academic career, and since journal editors, as shown earlier, generally focus on methodological elegance, it is evident that cumulateness, generalizability, replication, ecological validity and theoretical integration will produce less pay-off than methodologically clean, rigorous studies reporting novel and interesting findings. Of course, such studies often contribute to the advancement of a particular area, but the emphasis is often on novelty rather than integration and accumulation.

It seems reasonable to argue that the strong pressure to publish has led to a number of undesirable side-effects — a multiplication of journals and collective volumes with somewhat uneven standards, a fragmentation of the field with the emergence of subgroups of insiders (often referred to, with more or less justification, as “citation clubs”), a relative lack of concern with prior work on a phenomenon (particularly when published in a language other than English), and insufficient attention to the theoretical underpinnings of an empirical study.

While European and North American social psychologists are equally affected by the complexity of the phenomena under study (although the Europeans may be confronted with even greater complexity given the manifold cultural differences), conflicting demands with respect to research approaches, and the lack of established, time-honoured research traditions, there is a major difference with respect to academic reward structures. US social psychologists have to pursue their career in one single system of promotion, grant support and publication activity, and thus are in direct competition, particularly with those colleagues who work in the same domain. European social psychologists working in a particular area often do not compete directly with each other — they apply for grants to different national granting agencies, promotion practices of the different universities do not foster direct comparison, and even publication outlets often involve different languages. The strong Europeanization of science (European societies and journals) has changed this situation somewhat in the last decade, but direct competition is still much more infrequent than in the US. In addition, most US scientists are unable to engage

in research projects without grant support while many European universities provide a minimum of hard money support that allows some research activity without outside funding. On the other hand, the grant award system in the US allows teaching time to be 'bought' for research, something that is quite unheard of in most European countries. Obviously, these differences in research organization and academic reward structure are likely to affect research practices strongly.

Suggestions for remedial action

Much of my earlier state-of-the-art report was devoted to an attempt to point toward possibilities for remedial action. This included listing what seem to be particularly promising areas for further research (based partly on the conclusions of an interdisciplinary committee on basic research in the behavioural and social sciences, sponsored by the National Science Foundation and private foundations in the US; see Gerstein et al., 1988) and suggesting some more general approaches that might help to overcome some of the weaknesses of social psychological research. The major elements of the argument are outlined briefly below (for further details, see Scherer, 1992).

I mainly propose a shift from *paradigm-guided* to *phenomenon-focused* research, emphasizing the need for integrative theories that can direct research activity and help to evaluate the significance of the results. Of course, this is more easily said than done. However, applied research, interdisciplinary approaches, and cross-cultural comparison may all help to attain the aim of theory-guided research on important real-life phenomena.

Applied research

Many of the insights that our textbooks heralded as major advances in our knowledge have been obtained in the context of applied work or action research. It is obvious that the need for application of findings requires the researcher to keep focusing on the phenomenon under study and does not allow the pursuit of elegant paradigms which only excite the cognoscenti in the reference group of a particular research tradition. Until recently, applied research has had

little prestige in social psychology, to the point of being considered slightly disreputable.

It is useful to distinguish between *utilitarian* vs. *scientifically minded* applied research. Utilitarian applied research tries to find a quick technological fix for urgent real-world problems without much concern for underlying mechanisms. Scientifically minded applied research takes an important real-world phenomenon and attempts to build an explanatory structure on a theoretical level, which can be empirically tested, independent of whether immediate practical intervention or prevention is desirable or possible. While one could be concerned with scientific integrity in the first case, it would seem that the second approach is every bit as scientific, and prestigious, as basic research that nourishes itself by perpetuating established laboratory research paradigms whose only claim to reality is their publishability. We should take the natural sciences (which so often serve as our example with respect to methodological rigour) as an example with respect to applied research: it is obvious that in these disciplines scientifically minded applied research not only has high prestige but has often been the promoter of major scientific discoveries.

Scientifically motivated applied research not only helps focus on real and powerful phenomena, and thus is likely to foster generalizability and cumulativeness, but also greatly facilitates the attainment of ecological validity. Applied research settings prohibit neither the study of basic social psychological phenomena nor the use of powerful experimental methodology and multivariate data analysis, although they do require much more effort than normal laboratory experiments. This should not be interpreted as an attack on research in the laboratory. Much systematic experimentation is only possible in the laboratory and for many phenomena the setting may be less important than the method to provoke the desired effect. However, one should not automatically assume that the phenomenon under study can be easily produced in the laboratory. In trying to produce a phenomenon in the laboratory we have to pay more attention to the problem of the structural equivalence of the laboratory version of the phenomenon under study with respect to its real-life instantiation.

Only a few of the respondents in the informal survey mentioned "renewed interest in applied research" as one of the key developments in social psychology. It is to be hoped that this clearly discernible tendency will grow in strength. One encouraging factor

is that the prestige value of this type of research seems to be on the increase. Frequently cited research areas of this type in psychology generally as well as in social psychology are, among many others, the following: eyewitness testimony, justice, speech recognition, relationships, organizations and political behaviour.

Much remains to be done, however. It is striking, particularly in Europe, that often *psychoanalysts* rather than social psychologists are invited as experts for public symposia, panels, or media interviews to pronounce on genuinely social psychological issues. This is probably partly due to the fact that many psychoanalysts do not hesitate to express opinions and give advice with much conviction, a stance that is much preferred by media people, politicians and the public at large over the guarded position of scientifically oriented social psychologists who generally call for more research. Part of the reason may also be that applied social psychological research has not been very visible, much less so than sociological and political science research into applied social psychological issues. In many countries, academically trained sociologists, and often also educational scientists, have responded to the lack of appropriate positions in their own disciplines by moving into applied social psychological research; for example, with regard to organizational behaviour, consumer behaviour, political behaviour and many other domains. Although statistics, to my knowledge, are not available, I have a strong feeling that social psychologists participate less in bids for government-initiated research programmes than one might expect on the basis of the research expertise and manpower available. It is surprising, for example, that the European Community, which finances many different ambitious research programmes in several disciplines, does not support, as far as I know, social psychological research programmes on ethnic or regional identity, the social psychology of language use and bilingualism, the emotional climate in a rapidly changing political environment, prejudice and attitudes toward strangers, perceived social justice, or other topics that would seem to be of the greatest significance for the development of a true European "community". Again, social psychologists are probably to be blamed for being less aggressive than researchers from other disciplines in obtaining funding for this type of applied work, much of which could be scientifically minded rather than just utilitarian. One of the reasons for the relative lack of this type of activity might be that the scientific associations for psychology in most European countries have not made it one of their priorities to encourage

applied research. This is also true of the European Association of Social Psychology which has been most effective in furthering contact and collaboration among European social psychologists, both East and West, and which could play a major role in coordinating efforts at obtaining funds for scientifically minded applied research in individual countries and at the level of the European Community.

US psychology has on the whole been much more successful in this respect, probably in part because of the explicit lobbying by the American Psychological Association (APA), although social psychology has probably had less of a share in applied research than other psychological subdisciplines. The foundation of the American Psychological Society (APS) by scientific psychologists who felt that the APA was dominated by clinical and applied psychologists, no longer properly representing the scientific part of the discipline, might result in an even greater difficulty in encouraging scientifically minded, in addition to purely utilitarian, applied research.

Interdisciplinary approaches

Interdisciplinary work will only be successful if the contributors from different disciplines focus on a real phenomenon of interest to all of them. Since the concepts and methods, often even basic notions of theory of science, are different, only the phenomenon can provide a powerful link. Trying to understand a phenomenon from many different angles and on many different levels would seem to be the basic rationale for interdisciplinary endeavours. In consequence, the need to agree with partners from other disciplines, who have different favourite paradigms, will force us to keep focusing on the phenomenon and on ecological validity.

Social psychology, as has been pointed out frequently, is an interdisciplinary domain par excellence. Unfortunately, lip-service to the need for interdisciplinarity is far more frequent than actual practice. The reviewers of an earlier version of this paper strongly attacked me on this point, arguing that there are countless examples of steadily increasing interdisciplinary efforts, particularly in the health and organization areas. I had, in fact, singled out these areas as very promising examples and obviously I do not deny that in the last twenty years social psychologists have broadened their disciplinary allegiance considerably. Yet, we have to distinguish between *applying* social psychological tools and concepts to issues

that are also dealt with by other scientific disciplines and truly interdisciplinary research. For example, the fact that social psychologists are studying patient–doctor interaction does not necessarily mean that they are doing this in an interdisciplinary fashion, i.e. closely interacting with medical researchers, sociologists, pragmatic linguists and communication scientists.

For all practical purposes, social psychology has become a discipline of its own — with all the accompanying centripetal tendencies. This is not surprising as long as relatively narrow mainstream work continues to be rewarded more quickly and more amply than time-consuming, laborious and potentially conflict-laden interdisciplinary collaboration. If the discipline really wants to encourage interdisciplinary work the reward structure has to be changed dramatically. The same goes for training — interdisciplinary approaches require that the participants from all disciplines involved have at least rudimentary knowledge of the concepts and methods of the other fields.

Interdisciplinary collaboration also requires there to be a serious input from all the disciplines concerned and at all levels of the research activity. This is not always the case. In some contexts, particularly in the relationship to medicine, one has the impression that the role of psychologists has been reduced to that of research officers. Obviously, the fact that many psychologists have received strong methodological training predisposes them to be in charge of research procedures and analyses. It would seem problematical, however, if this means that there is little or no contribution to the definition of the research aims, using genuinely social psychological concepts and mechanisms.

Cross-cultural approaches

Just as in the case of interdisciplinary research, much lip-service is paid to the need for cross-cultural comparison. In most social psychology textbooks one finds a short paragraph mentioning the problem of cultural diversity, with the author then happily going on to present findings that have been obtained mainly with North American college students, as principles of social psychology. The respective authors are not to be blamed — if they were to restrict coverage to cross-culturally generalizable findings there would be no textbooks of social psychology.

What is more dangerous is the tendency to downplay the problem and to assume that the universality of the phenomena reported in the North American textbooks has been demonstrated. I cannot resist quoting verbatim a sentence from one of the reviews of an earlier version of this paper: "Furthermore, if the validity of our body of social psychological findings were limited to American 'undergraduates with special interest in psychology', how come that most of this stuff has been replicated by social psychologists all over Europe?" (anonymous reviewer). If that were to be a widely held view among North American psychologists (most of the body of North American social psychological findings replicated all over Europe!) we would really need to establish an inventory of exactly what has held up in intercultural replication to instil somewhat more realism in our more optimistic North American colleagues.

Unfortunately, the results of the few cross-cultural studies that have been published, including my own, are not very useful since they are often based on cross-national convenience samples collected without prior specification of theoretically predicted differences. If there are no differences, one presumes universality of the phenomenon in question which may be more or less plausible, depending on the degree of biological determination of the respective behaviour. Problems arise when there *are* differences, in particular as to the question of how to interpret them. For future cross-cultural research to be successful we need to develop a fairly comprehensive understanding of the phenomenon under study in our own culture, so as to be able to develop at least some hunches concerning the type of sociocultural variable that might have an effect on the behaviour patterns that are involved. Furthermore, we must hope that anthropologists will make progress in developing structural matrices for the comparison of cultures on a number of major behaviour-related dimensions (e.g. Hofstede, 1984).

So far, neither past performance nor actual possibilities distinguish a European tradition of cross-culturally comparative research. Possibly, European social psychologists, because of their historical heritage of culturally diverse countries and their increasingly closer contacts with colleagues from other countries, are more sensitized to the need to question the generalizability of social and behavioural science findings across cultural contexts. The motivation for comparative research and some gut-level intuition regarding important cultural dimensions certainly exists and could work wonders if encouraged and properly channelled. In addition to the

systematic theoretical underpinnings mentioned earlier this would require a major change in the priorities and decision criteria of journal editors and, in particular, granting agencies (see the following).

It is obvious that more large-scale, cross-culturally comparative studies are sorely needed in social psychology. They would greatly enhance the generalizability of findings and educate us about the role of cultural factors in the determination of behaviour. However, these studies will have to be run in a much more professional manner than has been the case thus far, in terms of both underlying theoretical concepts and methodological sophistication. We should not deceive ourselves about this being possible without the establishment of specialized research structures and appropriate funding arrangements. Unfortunately, it is well known that it is notoriously difficult to obtain funds for intercultural research. It would seem that the European Science Foundation, which is an umbrella organization supported by many national science organizations in Europe, has a particular responsibility for actively encouraging systematically planned and well-designed cross-cultural research. It would be most desirable, of course, to establish stronger links between the European Science Foundation and major North American foundations, both public and private, that could lead to joint funding of truly cross-cultural international projects. Such projects might also help to overcome some of the quasi-ideological differences between European and North American social psychology. If at all possible, cultures from other continents should obviously be included in such large-scale cross-cultural comparisons.

European vs. North American social psychology

A final question in the informal survey I conducted with colleagues on both continents concerned differences between North American and European social psychology: "Can one see a special role for European social psychology — past, present, or future?". This question produced strong regional differences: while almost half of the North American respondents did not see the existence or the need of a special role for European social psychology, virtually all of the European respondents strongly endorsed both existence and need. In general, there seemed to be agreement that European social psychology was less individualistic and more philosophical and

history-conscious in nature and had been particularly strong in intergroup relations. Its major role was seen as adding cultural and linguistic diversity, countering some of the biases of North American work.

The reasons for this striking discrepancy are quite obvious. Most North American social psychologists are convinced that science is international and non-ideological and that the regional or historical origin of a scientific contribution does not and should not matter. This is an idealization, of course, and one that is not even universally shared. But just as an ideal market economy requires total transparency of supply and demand, as well as the absence of monopolies, oligopolies, cartels and other limitations on the free market mechanism, universal science requires free and unlimited access to the publication market and total transparency of scientific contributions.

As we all know, present-day reality is a far cry from these ideal conditions. The central problem, of course, is the language differences between scientific communities. While most North American colleagues will readily and jokingly admit over dinner that Americans are terrible with foreign languages, the implications of this for their ability to follow the literature published in languages other than English is not always readily apparent to all. Many European scholars feel aggravated at having to struggle to put their ideas and findings into basic English, often losing the elegance and persuasiveness they would have been able to achieve in their native tongue, in order to have their work noticed by people outside their own linguistic community. While the need to publish in languages other than their own has always been felt by scientists from smaller countries, it is now commonly experienced by all non-English-speaking psychologists because of the special status of English-language journals and book publications.

The problem of language in scientific exchange is a difficult one. Some resentment about this issue, often coupled with the subjective perception of being prejudicially treated by Anglo-American editors, may explain in part why Europeans believe in a special role for European social psychology. More importantly, Europeans tend to be more cynical about the non-ideological, universalist nature of scientific endeavours. Many believe that there are powerful ideologies at work determining topics, theories and methods chosen, ideologies which may be linked to socio-historic developments or socio-economic conditions in a particular country. As we have seen

from the responses on North American-European differences, North American social psychology is generally seen, by Americans and Europeans alike, as too individualistic, too ahistorical, too ethnocentric, too neopositivist and too laboratory-oriented. Obviously, then, Europeans feel the need to counterbalance this situation — and the cultural, historic and linguistic diversity of the scientific traditions in the various European countries seems to reserve a special role for European social psychology under these conditions. It is instructive to cite Jos Jaspars who, in a personal assessment of the history of European social psychology, expressed the opinion that the European Association of Experimental Social Psychology may have “promoted a different perspective in social psychological research by emphasising more the collective nature of social behaviour rather than interpersonal processes and by favouring substance over method” (Jaspars, 1986: 3).

It is not by accident that this paper has focused on the issue of European vs. American social psychology (and I apologise for not including the other geographical areas of the world where social psychologists may have still different perspectives). I believe that the cleavage that emerged clearly from the questionnaire responses may be yet another reason why social psychology may be slow to advance. I know few other fields where researchers attribute such great importance to the geographical and cultural origin of a scientific contribution. If we cannot agree — across the Atlantic — on the goals, tasks and values in social psychological research, it will be difficult to achieve the objective for our science as set by the founding fathers: to better understand human social behaviour. I strongly doubt that we can get closer to this aim by giving a special role to European social psychology and its particular approach, but I doubt equally strongly that we can expect major advances if mainstream North American social psychology continues to focus almost exclusively on a relatively small number of established paradigms and largely to ignore the powerful role of language and culture. I am aware of the utopian nature of the suggestions which I have summarized briefly in this paper, but I remain convinced that focusing on phenomena rather than research paradigms and aiming towards scientifically minded application, interdisciplinarity and cross-cultural comparison may get us out of the rut. It might also help us to unite our discipline.

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Notes

1. In this, I was affected by painful memories of my first few years of teaching after graduate school in the US: the period of morose soul-searching of American social psychologists in the early 1970s, haunted by the ghosts of artifactuality and triviality.

2. While systematic comparisons of textbooks are a regular feature in *Contemporary Psychology*, they are generally directed toward the current offering on the North American market, rather than comparing recent texts with a classic text, or European texts with American texts. One can question the choice of two textbooks out of hundreds in social psychology — but given the multitude of choice it is unlikely that one could find consensus as to the "right" texts to choose short of doing a comparison of a randomly selected sample (at least of the US textbooks). A more serious problem is the time difference of 8 years in the publication dates of NewUS and NewEU. However, given the longer lead time for the preparation of the first-edition, multi-authored NewEU the coverage is most likely closer to 3-4 years apart. The comparison presented here does not claim to be original or systematic since the purpose was not to produce hard data but to illustrate developmental trends.

3. Obviously, the picture would change if we were to include all the work on "person or social cognition" — where there is undoubtedly a large amount of very good research — under the heading of *person perception*. However, there is a danger of these categories losing their meaning if stretched too far.

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