

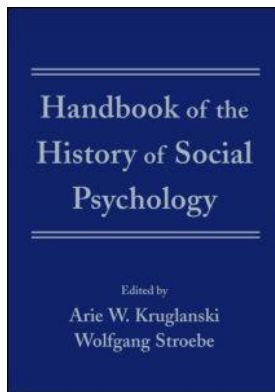
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Publisher: *Routledge*

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## **Handbook of the History of Social Psychology**

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### **A History of Small Group Research**

Publication details

<https://test.routledgehandbooks.com/doi/10.4324/9780203808498.ch18>

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**Published online on: 01 Dec 2011**

**How to cite :-** John M. Levine, Richard L. Moreland. 01 Dec 2011, *A History of Small Group Research from*: Handbook of the History of Social Psychology Routledge

Accessed on: 10 Jun 2023

<https://test.routledgehandbooks.com/doi/10.4324/9780203808498.ch18>

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# 18 A history of small group research

*John M. Levine and Richard L. Moreland*

Small groups have occupied a prominent place in social psychology since it began at the end of the 19th century. Indeed, many of the core principles critical to the development of the discipline (e.g., imitation, suggestion) have a distinctly group flavor. Moreover, the dominant methodology in social psychology—experimentation—is rooted in the study of groups (see Allport, 1954). More recent historical accounts of social psychology have also emphasized the importance of small groups. For example, in discussing central research areas in social psychology, Jones (1985) focused on interdependence and group dynamics. This emphasis was recently echoed by Ross, Lepper, and Ward (2010), who identified intragroup/intergroup processes as a basic content area in social psychology and group influence as a central theme in the discipline.

Finally, even a cursory examination of important research reviews covering psychology in general, and social psychology in particular, reveals an enduring interest in small groups. Consider, for example, the *Annual Review of Psychology*. For the first 10 years of its existence (1950–1959), its chapters on social psychology were titled “Social psychology and group processes,” and many chapters on groups were published in subsequent years. Group research has also been well represented in major handbooks in the field, including the *Handbook of Social Psychology*, *Social Psychology: Handbook of Basic Principles*, and the *Sage Handbook of Social Psychology*.

## Scope of the chapter

The literature on groups is vast, in part because relevant work is done by people from several disciplines, including social and organizational psychology, sociology, anthropology, communication, and economics. Even within psychology, the amount of work on groups is extensive, as revealed by the 13,029 hits produced by a recent PsycINFO search using the phrase “group dynamics.” We will therefore limit our chapter in several ways. First, we will focus on (a) small groups (seldom containing more than a handful of members) and (b) intragroup rather than intergroup processes. We will deal with dyads only in rare cases, and we will spend little time discussing particular kinds of groups (e.g., families, therapy groups, children’s groups), which have specialized literatures of their own. Although every group is unique in some sense, and particular kinds of groups differ from one another in interesting ways, many important

phenomena are common to all groups, and these are focus of our chapter. Moreover, most of our attention will be devoted to research done by social (and sometimes organizational) psychologists, though we will mention major contributions from other fields. Finally, our review will be selective rather than exhaustive. This is unavoidable because of the size of the literature we must cover and because we bring our own perspectives and biases to the task, having studied group processes (together and separately) for many years.

## Recurrent issues

Three recurrent issues have arisen in the study of groups over the past century. Although these issues will reappear (more or less explicitly) later in the chapter, it is useful to discuss them briefly now.

### *Defining groups*

One recurrent issue is how to define groups. Several criteria (see DeLamater, 1974) have been proposed for this purpose (e.g., interdependence, social influence, shared social identity), and there is little consensus about which should be emphasized. Some commentators view this as a weakness of the field, but that criticism seems unwarranted. A “complete” definition that captures the richness of group life would include numerous criteria and be unwieldy as a result. So, it makes sense for investigators interested in particular aspects of groups to focus on specific criteria in defining groups. This is what has indeed occurred with changes in the popularity of different research questions. Consider, for example, efforts by many organizational psychologists to differentiate “teams” from other small groups. In our opinion, the fact that definitions of groups can shift to accommodate specific research questions is a strength of the field, not a weakness.

Some commentators have argued against using any definition to distinguish “groups” from “nongroups,” suggesting it is better to view “groupness” as a dimension along which all sets of people can vary (McGrath, 1984; Moreland, 1987). According to this view, people feel, think, and act more like group members, and less like individuals, as their levels of “social integration” rise. This approach cannot avoid the criterion question, of course, because determinants of social

integration must be identified. For McGrath, these include the number of people involved, their interdependence, and their level of past and anticipated future interaction. For Moreland, the critical factors are the extent to which people are environmentally, behaviorally, affectively, and cognitively integrated. Each form of integration has its own indicators, which could (in principle) be measured and then combined to produce an overall index.

Even if groupness is viewed as a continuum rather than a dichotomy, it seems useful to insist that groups contain three or more members. Although dyads are important in their own right, we believe that they should be treated separately from groups for several reasons (see Moreland, 2010). First, dyads are more ephemeral than groups—they seem to form and dissolve more quickly. Second, people experience stronger, and often different, emotions in dyads than in groups (e.g., romantic love). Third, dyads are simpler than groups, and hence important group phenomena (e.g., socialization, coalition formation, majority/minority influence) cannot occur in dyads. Moreover, some phenomena that occur in both dyads and groups (e.g., negotiation, ostracism) are less complicated in dyads or operate differently there. Finally, assuming that dyads are simply (very) small groups can lead to methodological choices that reduce one's ability to understand either dyads or groups (e.g., studying dyads and then drawing conclusions about groups, studying dyadic relations in groups without considering how other group members influence the dyad). For all these reasons, it makes sense to treat groups separately from dyads.

### ***Reality of groups***

A second recurrent issue is whether groups are “real,” in the sense that they are more than just an assembly of people whose behavior can be explained in terms of individual psychological processes. Early observers of group behavior (including Durkheim, LeBon, and McDougall) were impressed by the negative impact of some groups on their members. This led them to claim that groups possess “emergent properties” that arise from interactions among members and cannot be predicted from members' individual characteristics. An important implication of this perspective is that different theories must be developed to explain group and individual behavior. This claim did not go unchallenged. Allport (1924) believed that questions about how people behave in groups can (and must) be answered by studying the characteristics of individual members. (It is worth noting that Allport eventually recanted this claim.) Debates about the reality of groups and the existence of emergent group properties have never been completely settled. Nonetheless, group researchers have generally adopted, either explicitly or implicitly, the “neorealist” view (see Warriner, 1956) that groups may be abstractions, but they have very real effects on people.

Several arguments can be offered to support this view of groups. First, both members and nonmembers of groups frequently perceive them as real (e.g., Campbell, 1958; Hamilton

& Sherman, 1996), although this “entitativity” varies across different kinds of groups. Second, members of groups often behave differently than either isolated individuals or dyad members. For example, people who observe other group members respond incorrectly to simple questions give more incorrect answers than people who did not observe such behavior (Asch, 1951, 1956), and people working on effortful tasks expend less energy when their output will be combined with that of others than when it will not (Latane, Williams, & Harkins, 1979). Third, groups sometimes perform better or worse than would be expected on the basis of their members' individual abilities (Moreland & Levine, 1992). Finally, as McGrath (1984) noted, in the final analysis the existential “reality” of groups is less important than the usefulness of the concept of groups for understanding human behavior.

Although most group researchers believe that behavior in groups should be explained at the group rather than the individual level of analysis, their theories and methods often betray subtle forms of reductionism. “Group” research often focuses on the thoughts, feelings, and actions of individuals embedded in group contexts, rather than the responses of the group as a whole. And in some cases, group (collective) phenomena are explained in terms of the psychological processes of individual members. Many theories about group phenomena are basically “scaled-up” explanations of individual phenomena (e.g., efforts to reconceptualize individual characteristics, such as self-efficacy or mood, as group characteristics). Clearly, the individual level of analysis plays an important role in group research, something that has been both celebrated (e.g., Mullen & Goethals, 1987) and lamented (e.g., Steiner, 1986).

### ***Groups: A force for good or evil?***

A third recurrent issue concerns the impact of groups on human welfare. Philosophers and social scientists have long argued about whether groups are a force for good or evil. The evidence is mixed. Clearly, membership in groups can have negative consequences for the individual, the group, and society at large. These include diffusion of responsibility in emergency situations, poor decision-making (e.g. groupthink), conformity to erroneous group norms, ethnic and racial conflict, and so on (Buys, 1978). But these negative consequences are not inevitable, and groups sometimes produce important positive outcomes for their members (see Correll & Park, 2005), as well as for nonmembers. In regard to members, groups play a critical role in fulfilling such basic needs as survival, psychological wellbeing, information acquisition, and social identity (cf. Mackie & Goethals, 1987). These benefits have led some analysts to conclude that humans have a basic need to belong to groups (e.g., Baumeister & Leary, 1995). Although a group can thwart as well as satisfy its members' needs, the rewards of membership are typically sufficient to ensure that people affiliate with groups from birth to death. In regard to nonmembers, work groups often produce benefits for various “stakeholders” who are not actually members. For example, an efficient

automobile assembly team benefits a variety of stakeholders, including the workers' families, top management, and investors. And a wise presidential advisory group can benefit the nation and even the world. Although either the positive or negative effects of groups may be emphasized in a particular study, most researchers believe that both can occur. The question, then, is not *whether* groups have positive or negative effects, but rather *when* the two kinds of effects occur and how to maximize the positive effects of groups.

### Approaches to analyzing the history of small groups research

There are several possible ways to analyze the history of small group research. Two general options are the quantitative approach and the narrative approach. The quantitative approach involves numerical analyses of data on group studies, with the goal of tracking significant changes in research topics, theoretical orientations, and methodological preferences. In contrast, the narrative approach involves telling a "story" about what has happened in the field over the years, explaining what changed and why.

#### Quantitative analyses

A quantitative analysis of the history of small group research has both advantages and disadvantages. On the positive side, it makes the analytical process less subjective and can answer specific questions about the history of the field, such as when an important concept was proposed and how influential that concept later became. On the negative side, it is limited by the number and type of archival sources that are used, both of which may be restricted for pragmatic reasons. These restrictions can weaken the generalizability of any findings. Also, archival data sources may not always represent accurately what was happening within a field. Finally, the quantitative approach is often difficult to carry out. Scanning archival sources can involve many hours of work, and complex statistical issues can arise in analyzing the resulting data.

We will focus here on several quantitative analyses by Moreland and his colleagues. In an initial paper, Moreland, Hogg, and Hains (1994) tested Steiner's (1974) theory that interest in small groups among social psychologists varies (with an 8- to 10-year time lag) with the levels of conflict occurring in society. When such levels are high, interest in groups increases, because societal conflicts are often caused by tension among groups. But when conflict levels are low, interest in groups decreases. Steiner used this reasoning to explain why social psychologists studied groups in the 1950s, but not in the 1960s. He also predicted a renewed interest in studying groups during the 1970s, because so much societal conflict occurred during the 1960s.

To test Steiner's (1974) ideas and learn more about how studies of small groups are done and what they investigate, Moreland et al. (1994) examined all the articles published

between 1975 and 1993 in several major social psychological journals. Articles that focused on group behavior were first identified and then coded for several variables. Descriptive analyses showed that most of the articles reported just one study and that the most common methodology was the experiment. The most common research topic was intergroup relations, followed by group performance and conflict in groups. Other topics, such as group composition, group structure, and the ecology of groups, were seldom studied. Several analyses of temporal changes in group research were also performed. The principal dependent measure for these analyses was "interest in groups," operationalized as the proportion of journal pages each year devoted to group research. The results provided little support for Steiner's ideas. Interest in groups fell during the late 1970s, stayed low (with a few exceptions) through much of the 1980s, and then rose sharply during the 1990s.

Moreland and his colleagues identified two factors that might explain why social psychologists had recently regained interest in studying small groups. These factors, which began to operate in the early 1980s and became increasingly powerful thereafter, were the influence of (a) European theory and research on groups and (b) social cognition theories and research methods. Articles were coded for those factors, and further analyses of temporal changes in small group research were conducted. When the two factors were "controlled" by removing from the database any articles that showed their influence, interest in groups no longer rose during the 1990s, suggesting that the resurgence of interest was indeed caused by European and social cognition factors. (There was also a shift over the years toward intergroup relations and away from intragroup relations, which may have been important as well, because articles on intergroup relations tended to exhibit both European and social cognition influences.)

The paper by Moreland et al. (1994) led to others in which similar analyses were performed. For example, Sanna and Parks (1997) examined articles published over the same period in several major organizational psychology journals. They found that organizational psychologists used a broader range of methodologies and focused their research on group performance rather than intergroup relations. However, the same general temporal trends were observed—interest in groups fell from 1975 until the early 1980s and then rose steadily, peaking in 1993. Why? Both European and social cognition influences are much weaker in organizational than in social psychology, so they are not plausible causes. Maybe the desire of practitioners to boost organizational performance through the use of teams was important. That desire, which became clear in the late 1970s and has grown steadily ever since, could explain organizational psychologists' rising interest in small groups and their frequent focus on group performance.

Wittenbaum and Moreland (2008) extended Moreland et al.'s (1994) database by incorporating articles on groups (in the same journals) published between 1994 and 2006. Interest in groups showed the same pattern of temporal change as before, except that growth seemed to "stall" in 2004. Why? Maybe this



was an artifact of the journals that were considered. In recent years, many social psychological articles on groups have been published in specialized journals or journals associated with other fields. Insofar as this trend reduced the proportion of articles on groups in the journals that Wittenbaum and Moreland studied, social psychologists' true interest in studying groups may have been underestimated.

Baumeister, Vohs, and Funder (2007) recently argued that social psychologists of all sorts have moved away from measuring behavior in their research. Has this trend also occurred in research on groups? To answer that question, Moreland, Fetterman, Flagg, and Swanenburg (2010) coded the articles in Wittenbaum and Moreland's (2008) database for whether behavior was assessed, and if so, how that was done. A strong decline in behavioral assessment was observed. This was attributed to three factors. One factor was increasing pressure (from journal editors and reviewers) to include more studies in articles. Given all the resources that group research requires, multiple studies can be difficult to complete. The other two factors were (again) the growing influences of European and social cognition approaches to groups. Both approaches legitimize the study of groups whose members never interact, making assessments of their behavior unnecessary. (Again, the shift in research topics toward intergroup relations and away from intragroup relations may have been important as well. Articles on intergroup relations contained more studies, were more likely to exhibit both European and social cognition approaches, and seldom involved behavioral assessments.)

### *Narrative analyses*

An alternative approach to the history of small group research is to create a narrative account. Such accounts are not difficult to find (e.g., chapters in the *Annual Review of Psychology*, any of several *Handbooks*), but they tend to be narrow in scope, covering research on just one or a few topics over a limited period. And their purpose is primarily to describe relevant theories and research findings, rather than to evaluate changes over time. Broader narrative accounts of the history of small group research are rare. Some examples that influenced our thinking were Cartwright and Zander (1968), Zander (1979), several papers by Steiner (1974, 1983, 1986), McGrath (1997), and a book by Patnoe (1988).

A narrative approach to the history of small group research has its own strengths and weaknesses. Because storytelling is such a basic form of human cognition and behavior (see Bruner, 1991), stories about a field can be both interesting and compelling. However, there is a risk of exaggerating some factors and minimizing others when explaining what happened and why it happened. Some narrative analyses, for instance, focus on key individuals in a field, attributing much or all of what occurred to their influence. Kurt Lewin was arguably such an individual in small group research. Lewin took small groups seriously as a topic of study; pioneered the use of experimental methods for studying groups; saw the possibility of developing a general

theory of group processes; and provided settings and resources that facilitated the work of many people who became major contributors to the field. Despite all this, Lewin's direct contributions to the literature on small groups were modest. Although he wrote occasionally about groups, Lewin never offered any clear theories about group processes, and he conducted relatively few group studies. It can even be argued (see Moreland, 1996) that Lewin's field theory (with its emphasis on subjectivism) weakened subsequent group research and thereby contributed to its decline in later years. In any event, it seems to us that it is rare for any individual to truly shape a field, and even when such a person can be identified, the value of his or her contributions is often debatable. When the story of a field focuses too closely on heroic characters, it can thus be misleading.

We prefer a story that focuses on plot and setting rather than on characters. In our opinion, a narrative analysis of this sort can provide a more complete and nuanced account of a field. So, that is the kind of story we offer in the rest of this chapter.

### **The first 50 years**

Every story must have a beginning (if not an end). Where does the story of small group research begin? There are several possible answers to that question, all plausible to various degrees, so any choice may seem arbitrary. Yet a choice must be made, so let us consider the candidates . . .

#### *Durkheim and Le Bon*

One candidate is the year 1895, when two influential books about groups were published by French sociologists. Those books were *The Rules of Sociological Method*, by Emile Durkheim, and *The Crowd: A Study of the Popular Mind*, by Gustave Le Bon. Both authors were "inspired" by contemporary events in Paris, where street mobs of ordinary citizens misbehaved in surprising and sometimes shocking ways. An issue for both Durkheim and Le Bon was why people who were normally law-abiding would exhibit criminal behavior in groups.

The primary goal of Durkheim's book was to introduce readers to the new field of sociology. But an important focus of the book was what Durkheim called "social facts"—phenomena involving groups of people, rather than individuals. Durkheim made strong claims about how such facts should be studied. Groups are often more than just the sum of their members, he argued, and so trying to understand groups by studying individual members is misguided. Durkheim's arguments shaped much of the work on groups thereafter by legitimizing groups as a target for research and encouraging work on "emergent phenomena" in groups—phenomena that seem surprising, given the kinds of people who belong to a group. Many examples of such work can be found in research on group performance, where attempts are often made to relate a group's outcomes to the abilities of its members (see Larson, 2010).

Le Bon's book, which was less formal and reached a broader audience, described and tried to explain the mob behavior in Paris. He attributed that behavior to the emergence of a "collective mind" that influenced people (unconsciously) to act in unusual ways. That mind, according to Le Bon, has a variety of negative qualities, including impulsiveness, irritability, lack of judgment, and strong emotions. In fact, it resembles (he argued) the minds of savages, small children, and women (!).

One person who read Le Bon's book was Sigmund Freud, who later analyzed it in his 1922 book, *Group Psychology and the Analysis of the Ego*. Freud was struck by the resemblance between the collective mind and his notion of the Id. Freud wondered why membership in a group should strengthen the Id and/or weaken other parts of personality. He concluded that leadership was the reason, arguing that group members tend to identify with their leader (especially if he is an older, more powerful male), and this shared identification (rather than their feelings toward one another) is what binds them together. Identification with the leader causes him to replace the superego in members' personalities, so their normal values are suspended. In less psychodynamic terms, groups can "crystallize" around a central person whom all members admire (see Moreland, 1987). Other psychologists later claimed that the feelings group members share toward a central person can be negative as well as positive, and that groups can even crystallize around things other than a person (e.g., objects, places, ideas). Echoes of crystallization can be found today in self-categorization theory, which claims that a fundamental aspect of groups is a shared mental image (prototype) of someone who embodies the qualities that set the group apart (usually in positive ways) from relevant outgroups (see Hogg's 1992 discussion of social attraction). And much of the research on deindividuation (e.g., Postmes & Spears, 1998; Reicher, 1987) can be traced back to Le Bon's descriptions of group misbehavior.

Le Bon's book also influenced later research on groups by serving as a kind of foil. Many social psychologists felt that the "group mind" (see McDougall, 1920) was too abstract to be studied scientifically. Some, such as Allport (1924), used this as a reason to argue that individuals, not groups, were the only appropriate targets for research. But other social psychologists, such as Wegner (1987), tried to find ways in which group minds could be studied at the individual level. This led Wegner to develop the concept of transactive memory, which we will discuss later.

### **Group membership and individual performance**

Another candidate for the origins of small group research is a period around 1900, when several intriguing studies were reported on how group membership changes individual performance. One set of studies was performed by Max Ringelmann in the 1880s, though the results were not reported until nearly 30 years later (see Kravitz & Martin, 1986). In Ringelmann's research, male participants (alone or in groups of different sizes) were asked to push or pull things (e.g., a cart or a rope),

and a dynamometer was used to measure how much force they exerted. What Ringelmann found, and other researchers later replicated (e.g., Ingham, Levinger, Graves, & Peckham, 1974), was that the force exerted by a person decreased as the size of the group increased. This problem was worse in larger groups. Why did it occur? Later analysts (e.g., Steiner, 1972) suggested that coordination problems (members of larger groups have more trouble coordinating their efforts) and motivation losses (members of larger groups devote less effort to their task) might be responsible. Some researchers, such as Ingham and his colleagues, attempted to separate these factors experimentally and found that both contributed to productivity losses in groups (see also Latane et al., 1979).

Ringelmann's research led (directly or indirectly) to a great deal of research, many years later, on social loafing (see Williams & Karau, 1993) and other kinds of motivation losses in groups, such as free riding and "sucker effects" (Kerr, 1983). Some researchers have even studied why people occasionally devote *more* effort to a task when they work in larger groups (see Hertel, Kerr, & Messe, 2000; Williams & Karau, 1991). Finally, there has been research on how groups try to solve their coordination problems (see Wittenbaum, Vaughan, & Stasser, 1998).

Research on how group membership changes individual performance was also conducted by Norman Triplett (1898), a psychologist by profession who was also an amateur bicycle racer. After years of observing and participating in bicycle races, it seemed to Triplett that riders went faster when they raced with others than when they raced alone (against the clock). To test this idea, he obtained access to records that revealed the average speeds (over several years) for races in which people raced alone, or with another person who merely set a pace for them, or with another person who was competing against them. Triplett discovered that average speeds for bicycle races involving others were indeed faster than those for races against the clock, but it did not matter much whether the other person merely paced the racer or competed against him—what mattered was whether the racer was alone on the track or accompanied by others.

Triplett considered several possible explanations for this phenomenon, which later came to be called social facilitation (see Allport, 1924). Because it was difficult for Triplett to test these explanations in real bicycle races, he created a laboratory analog where they could be tested more readily. Triplett built an elaborate apparatus that allowed children to move small objects (with flags attached) around a track by spinning the handles of fishing reels, which were connected to the objects by fishing lines. The children could be assigned randomly to conditions, and the apparatus eliminated many of the physical factors that can influence bicycle racing, leaving the psychological factors intact.

In his main experiment, Triplett randomly assigned children to one of two groups, each of which performed the fishing reel task several times. On some trials, the children performed that task alone, moving their objects around the track as quickly as

possible. On other trials, they engaged in a “race” with another person. The results showed that about half of the children performed the task more quickly when they were with someone else than when they were alone. Another fourth of the children, however, showed exactly the opposite pattern, which later came to be called social inhibition. The remaining children performed the same whether they were with others or alone.

Triplet’s experiment led to nearly 30 years of research on social facilitation. However, this research produced puzzling results. Some researchers found social facilitation effects, but others found social inhibition effects. A few researchers even found effects of both kinds in the same experiments. No one could explain these results, so interest eventually waned. Zajonc (1965) revived this research area years later, however, by suggesting that increased arousal (caused by the presence of another person) interacted with task type to determine whether social facilitation or social inhibition occurred. Basing his analysis on animal research, where arousal was manipulated through drugs, Zajonc reasoned that dominant responses should improve performance on simple or familiar tasks, but impair performance on complex or novel tasks. In a review of the earlier research on social facilitation and inhibition, Zajonc found that he could predict (after the fact) which effect would occur by classifying the tasks as either simple/familiar or complex/novel. New research by Zajonc and others, in which performance on simple/familiar versus complex/novel tasks was compared, indeed tended to show social facilitation on the former tasks and social inhibition on the latter ones. Researchers then turned their attention to testing possible explanations (e.g., mere presence, evaluation apprehension, distraction/conflict) for why the presence of another person might increase someone’s arousal. But no clear conclusions could be drawn from this work, in the sense that none of the possible sources of arousal could be ruled out when all the evidence was considered. At some point, people began to ignore this issue and develop instead new theories of social facilitation and inhibition in which arousal was not a major factor (e.g., Bond, 1982).

Triplet’s study clearly stimulated much research. But how important was that study for small group research? One issue to consider is that nearly all research on social facilitation and social inhibition involves dyads, and (as we argued earlier) dyads are not groups. Another issue involves the quality of Triplet’s study and the strength of his results. A few years ago, Strube (2005) re-examined Triplet’s study and analyzed his data in more sophisticated ways. Strube found several serious methodological problems, and his new analyses revealed almost no evidence of social facilitation (or social inhibition) effects. Thus, Triplet’s study may have been more influential all of these years than it deserved to be.

### *Classic pre-war studies*

A final candidate for the origins of small group research is a set of studies published in the years before the Second World War—studies that are now regarded as “classics.” This is

the leading candidate, in our opinion, because these studies (taken together) examined a variety of phenomena and used diverse methodologies. These are signs of a healthy science—signs that (ironically) seem to be missing from modern social psychological research on small groups (which focuses heavily on intergroup relations and almost always involves laboratory experiments).

The first study involved the observation (from 1931 to 1932) of work groups at an industrial plant operated by the Western Electric Company (see Mayo, 1949). Earlier studies at that plant examined how changes in working conditions affected the productivity of people working alone and in groups. These studies revealed the famous “Hawthorne effect”—people who participate in research projects often change their behavior simply because they are being observed. That effect suggested that social factors might play a larger role in workplace behavior than anyone had realized.

To investigate this possibility, some workers were brought to a special site at the plant that replicated their actual workplace and observed as they assembled wiring banks for telephone exchanges. Pay was based on group rather than individual productivity, and there was an official group goal regarding how much should be accomplished each day. Consistent failure to meet the goal could result in disciplinary action, whereas consistent success could cause the goal to be increased. Not surprisingly, workers viewed both outcomes as undesirable. Workers reported their own productivity levels to a manager, who visited the workplace occasionally to evaluate the accuracy of those reports.

Many unexpected worker behaviors were observed. For example, workers often traded jobs with one another or even stopped working altogether to gossip or play games. And there were surprisingly complex social relationships among the workers. Informal friendship groups emerged, for example, and these groups affected both work and nonwork activities. Several group norms were also observed. One norm stipulated that workers never act as if they were “better” than one another. Another norm prohibited giving any information to management that might evoke disciplinary action for the group. And some norms focused on keeping group productivity stable across members and over time. To enforce these norms, workers whose productivity was unusually low or high were punished by being punched on the arm by other members (“binging”). Another way to stabilize group productivity levels was to report higher or lower productivity levels than actually occurred. The occasional underreporting of productivity levels, punishment of high-performing workers, and nonproductive behavior on the job all suggested that pay was a less powerful motivator than commonly assumed, because these behaviors all reduced workers’ pay.

Another classic study carried out during the 1930s involved survey research by Newcomb (1943) at Bennington College, a small, prestigious women’s school. In 1935, as a young faculty member at Bennington, Newcomb was struck by the contrast between the values held by new students just entering the



college and by students who were already enrolled (and the faculty). New students, who often came from wealthy families, tended to be conservative, whereas students who had been at Bennington awhile (and the faculty) tended to be liberal. Newcomb wondered whether and how this apparent discrepancy in values would be resolved over the course of a student's time at Bennington.

To find out, Newcomb surveyed students' political, religious, and economic attitudes each year for four years. He found that most freshmen became more liberal as they went through school. These changes were related to personality factors—students whose attitudes showed greater change tended to have lower self-esteem and felt less socially secure. But the changes were also due to social pressure—students whose attitudes showed greater change tended to be more popular and participated in more school activities, whereas students whose attitudes showed less change had stronger family ties.

Many years later, Newcomb and several colleagues re-contacted members of the original sample to determine whether their attitudes had later drifted back to their original conservatism (Newcomb, Koenig, Flacks, & Warwick, 1967). The researchers found that most of the Bennington women remained liberal over the years, apparently because they kept in touch with their old school friends and chose new friends and married men who were also liberals.

A participant observation study by Whyte (1943), carried out over several years in a poor Italian neighborhood of Boston, is another classic study from the 1930s. Late in that decade, Whyte lived with a family from the neighborhood in order to become familiar with the neighborhood's social structure, especially “corner boys”—small gangs of young men that claimed a street corner as their own, using it as a base for their activities. Whyte spent many hours with a particular gang and took part in many of its activities. As a result, he gained several insights into small groups.

For example, Whyte witnessed many examples of what later came to be called “status congruence.” Every member of a group has several statuses, based (for example) on the person's appearance, strength, seniority, and contributions to the group. Whyte argued that groups want these statuses to be congruent—someone with high (low) status on one dimension should have comparably high (low) statuses on other dimensions. When status incongruence arises, it is confusing and disturbing to group members. As a result, they try to raise (or lower) the person's status on one dimension so that it is closer to his or her statuses on other dimensions. For example, while observing the gang at a bowling alley, Whyte noticed that one person whose status was generally low was a very good bowler, creating status incongruence. To solve this “problem,” the rest of the group undermined that person's bowling performance (e.g., through heckling), which caused his scores to drop to the level that someone of his status “ought” to have. He was then allowed to bowl in peace.

A fourth classic study from the 1930s is a laboratory experiment by Sherif (1935) on the development and maintenance

of social norms. This study involved the “autokinetic phenomenon”—a visual illusion whereby a stationary light, viewed in a darkened setting, appears to move. Sherif began the experiment by placing each participant alone in a dark room and then asking him repeatedly how much motion he saw in a dot of light projected on the wall. Participants differed in how much motion they “saw,” but each person typically gave a similar answer every time he was questioned.

Next, groups of three participants were brought together in a dark room where they again viewed a dot of light, but now gave public estimates of how far it seemed to move. Even though different participants saw different amounts of motion at first, their responses soon converged to a common estimate, or norm. To determine the power and stability of this norm, Sherif brought individual group members back to the laboratory several weeks later and asked them again to sit alone in a dark room and estimate how far the dot of light moved. At issue was whether group members, once separated, would (a) continue to be influenced by the group norm or (b) revert to their initial individual responses. Sherif found that most participants continued to follow the group norm, suggesting that they had internalized it.

Finally, a field experiment on leadership styles by Lewin, Lippitt, and White (1939) is yet another classic study carried out during the 1930s. Lewin and his colleagues arranged for small groups of boys to meet after school for several months to work at hobbies. These groups met under the supervision of adults trained to exhibit different leadership styles. Every six weeks, the leaders were shifted from one group to another, changing their styles at that time. In this way, every group experienced every style of leadership. Autocratic leaders set goals and planned activities for their followers, without consulting them, and ignoring their suggestions. They were also unfriendly toward their followers. Democratic leaders were friendly and encouraged their followers to set their own goals and plan their own activities. Finally, *laissez-faire* leaders also allowed their followers to organize themselves, but did not provide much guidance or express much interest in them. The choice of these leadership styles was probably influenced by the political context of the late 1930s, when authoritarian (e.g., Hitler) and democratic (e.g., Roosevelt) leaders were on everyone's mind.

Observations of the boys during their group meetings, and interviews with them later, indicated that the three leadership styles had distinct effects on the groups and their members. For example, autocratic leaders induced dependence in their followers, who enjoyed their groups less and were aggressive toward one another. Democratic leaders inspired their followers to work harder and be more creative. These boys also enjoyed their groups more and were less aggressive toward one another. Finally, little was accomplished when groups were “led” in a *laissez-faire* manner. Boys in these groups had trouble working together effectively and spent most of their time playing rather than working.

All five of these studies had a major impact on later research into small groups. For example, the Hawthorne studies



(although they have been criticized on both scientific and ethical grounds) are viewed as a landmark in the development of organizational psychology. They dramatized how important social relations in general, and work groups in particular, can be in shaping workers' motivation. As a result, work groups (teams) are now a major focus of research for organizational psychologists (see Kozlowski & Ilgen, 2006). Newcomb's study of changes over time in Bennington students' attitudes was also influential. For example, this study contributed to interest in reference groups and their effects (e.g., Singer, 1981) and also influenced our own work (and that of others) on socialization in groups (see Levine, Moreland, & Hausmann, 2005; Moreland & Levine, 2000). Whyte's study of "street corner boys" stimulated research on gangs of all kinds and served as a model for participant observation research on groups (see Becker, 1958; McCall & Simmons, 1988). Sherif's study contributed a paradigm that was later used by others (e.g., Jacobs & Campbell, 1961) to study norm formation and transmission, and his results contributed to a general appreciation of the differences between informational and normative influence in groups (e.g., Deutsch & Gerard, 1955). Finally, the study by Lewin and his colleagues stimulated interest in leadership styles and their effects. Evidence of that interest can be found in (for example) the ongoing search for "new" styles of leadership (see Avolio, Walumbwa, & Weber, 2009); efforts to determine which leadership styles are most effective (Judge & Piccolo, 2004); and various contingency theories of leadership effectiveness (e.g., Fiedler, 1964), which argue that the effects of leadership styles depend on the circumstances in which they are used.

But the impact of these five studies on our field is clearest when they are considered as a set, rather than separately. The fact that many researchers, all working at about the same time, were interested in small groups and producing intriguing findings with potentially important implications probably communicated to the field that small groups was an exciting research area that deserved more attention. In that way, these studies helped to pave the way for the remarkable burst of research on small groups that occurred not long after, in the 1950s.

### The 1950s—The Golden Age of group research

As one might expect, little research on small groups was carried out during the Second World War. Yet in some ways, the war encouraged such research. Why? One reason is that military experiences during the war (consider the film *Band of Brothers*) provided many people with graphic evidence of how important the success or failure of small groups can be. Another reason is that the war was won in part through the contributions of scientists (e.g., the Manhattan Project), so people had a new respect for what science could accomplish and were thus more willing to support scientific research. In fact, several government agencies (especially those associated with the military) began to provide funding for research on small groups after the war. Third, the horrifying events associated with the Holocaust led

many people to become curious about issues of leadership and conformity—how could so many people in Nazi Germany have acted in ways that were so clearly immoral? Finally, a related reason was immigration before the war, from Germany (to the United States and elsewhere), by many talented social scientists (e.g., Solomon Asch, Kurt Lewin) who later made important contributions to small group research.

Evidence for the health of small group research in the 1950s can be found in the contributions of many researchers. Much of this work was done at the University of Michigan by people who were influenced (to some degree) by Kurt Lewin. Lewin had begun to study groups at the University of Iowa and then moved to the Massachusetts Institute of Technology, where he created the Research Center for Group Dynamics. Following Lewin's death, the center moved to the University of Michigan, where it became (and remains) part of the Institute for Social Research there. Lewin's colleagues and students at Iowa included Dorwin Cartwright, Leon Festinger, Ronald Lippitt, and Alvin Zander. Many of these people moved with Lewin to MIT, where they were joined by Kurt Back, John French, Harold Kelley, Stanley Schachter, and John Thibaut. Nearly all of the MIT group (and Zander) later moved to Michigan.

At Michigan, these people (and their associates) began several lines of research that had substantial impact on the field. Some of that research focused on the causes and consequences of cohesiveness. In their famous study of cohesiveness and deviance in student housing units at MIT, Festinger, Schachter, and Back (1950) operationalized group cohesiveness as the proportion of residents' social contacts with ingroup vs. outgroup members. They found evidence that more cohesive groups had fewer members who deviated from group opinions about a tenants' organization. Deviates were also less popular than conformers.

Subsequent work on cohesiveness and reaction to deviance was closely tied to Festinger's (1950) theory of informal social communication, which posited that group members desire opinion uniformity because it allows (a) validation of opinions not anchored in physical reality (social reality) and (b) movement toward group goals (group locomotion). According to Festinger, opinion discrepancies in a group produce uniformity pressures, which stimulate communication toward opinion deviates. If such communication fails, then a redefinition of the group's boundaries occurs through rejection of the deviates. Festinger predicted, among other things, that both the magnitude of communication pressure and the tendency to reject opinion deviates vary positively with the degree of opinion discrepancy, the relevance of the issue to group function, and the level of group cohesiveness. These and related ideas were tested in several experiments (e.g., Festinger & Thibaut, 1951; Schachter, 1951). Although Festinger's predictions were only partially confirmed, these studies stimulated a great deal of work on both cohesiveness (see Hogg, 1992) and reaction to opinion deviance (see Levine, 1989). In addition, Festinger's identification of social reality and group locomotion as important motives had a major impact on how later researchers

thought about and studied groups. In 1954, Festinger extended and elaborated his earlier ideas concerning the causes and consequences of uniformity pressures in groups. In a theory of social comparison processes, Festinger broadened his focus to include abilities as well as opinions, offered new hypotheses regarding reaction to deviance, and discussed the implications of social comparison for group formation and structure. This theory stimulated great interest in when people seek comparison information and how they respond to it.

Another important stream of research at Michigan concerned behavior in mixed-motive settings. In 1959, John Thibaut and Harold Kelley presented a social exchange analysis of dyadic interaction. Using outcome matrices to represent the joint outcomes that dyad members can obtain through social interaction, they developed a powerful formulation for analyzing a variety of dyadic and group phenomena, including power and dependence, norms and roles, status relations, and collective goals. Thibaut and Kelley's analysis (see also Deutsch, 1949) helped stimulate the extensive use of experimental games for studying mixed-motive interaction (Pruitt & Kimmel, 1977). In 1978, Kelley and Thibaut elaborated and extended their earlier ideas.

Power, which played an important role in Thibaut and Kelley's framework, was the focus of two other Michigan researchers—John French and Bertram Raven. French and Raven defined power as *potential influence* (cf. Cartwright, 1959) and identified five bases of power—reward, coercive, legitimate, referent, and expert. A sixth power base, informational, was later added. This typology had an enduring impact on how power was conceptualized and studied in a variety of contexts. For example, Collins and Raven (1969) used the typology to organize the large literature on power, influence, and change in small groups. And Fiske and Berdahl (2007) labeled the typology “the single most influential view of power to date” (p. 680).

As important as it was, the work done at Michigan was not the only significant group research conducted during the 1950s. Another important stream of work occurred at Harvard. In 1946, Talcott Parsons created there a special Department of Social Relations for Interdisciplinary Social Science Studies. This department, which lasted until 1972, brought together faculty and students from psychology, sociology, and anthropology, many of whom shared an interest in small groups. (For many years, graduate students at Michigan were also trained in a multidisciplinary program that combined psychology and sociology.)

A major contributor to small group research at Harvard was Robert Freed Bales, one of the first people Parsons asked to join the faculty of his new department. Bales made many valuable contributions to theory and research on small groups. One was the development of interaction process analysis (IPA), a methodology for observing and analyzing the verbal behavior of task group members (Bales, 1950). IPA allows every behavior exhibited by members to be classified into one of 12 categories—half involving task issues and the other half

involving emotional issues. This division reflected Bales's belief that every group struggles to achieve and maintain a balance between task and emotional issues. IPA was used extensively at Harvard and later in many other laboratories, in research on a variety of issues. Years later, Bales developed SYMLOG, a more streamlined system for observing and analyzing groups and their members (Bales, Cohen, & Williamson, 1979). In SYMLOG (System for the Multiple-Level Observation of Groups), any behavior can be related to three basic dimensions of group life: dominance vs. submission, friendliness vs. unfriendliness, and acceptance vs. rejection of authority. SYMLOG also became popular among group researchers, although as the years passed, people who studied groups became less likely to adopt a standard system for behavioral categorization and more likely to develop unique systems tailored to fit the phenomena they were studying.

Bales also trained many outstanding graduate students at Harvard, including Edgar Borgatta, Christoph Heinicke, Richard Mann, Theodore Mills, Philip Slater, and Fred Strodbeck. These students did considerable research on groups while in graduate school, and many of them later went on to productive research careers.

Borgatta, for example, did studies on the emergence of “great men” within groups and the benefits that they generate for those groups (Borgatta, Bales, & Couch, 1954); how to compose groups (given the behavioral characteristics of potential members) in optimal ways (Borgatta & Bales, 1953), and how interaction patterns among members vary across groups of different sizes (Bales & Borgatta, 1955). Borgatta also had an interest in exploring the “basic dimensions” of groups (see Borgatta & Cottrell, 1955; Borgatta, Cottrell, & Meyer, 1955), and he coedited the first edition of an influential book on groups (Hare, Borgatta, & Bales, 1955) that contained a variety of theoretical and empirical papers. This book was revised and reissued every decade for many years thereafter.

Mills, another student of Bales, was fascinated by the work of the sociologist Georg Simmel (see Mills, 1958) and performed several studies exploring interpersonal relationships in triads (Mills, 1953, 1954). Mills also contributed to a classic study of communication patterns in small groups (see Bales, Strodbeck, Mills, & Roseborough, 1951). That study showed, for example, that group discussions are often dominated by just one or two persons (who earn high status as a result), whereas other people speak far less often and do not differ much in how often they speak. Also, most group members speak more often to individuals than they do to the group as a whole, except for the most talkative member(s), who display the opposite pattern.

Finally, Strodbeck not only contributed (like Mills) to the 1951 study on communication patterns in groups, but also collaborated with Bales on a famous study of developmental phases during group problem solving (Bales & Strodbeck, 1951). That study showed that groups often move from orientation to evaluation to control issues as they work through problems. Strodbeck is even better known for his extensive research

on juries. That research continued throughout his career and caused him trouble during the McCarthy era, when politicians raised questions about his research practices and motives (see Amrine & Sanford, 1956). As a graduate student, Strodtbeck explored the effects of social status and gender on how often (mock) jury members spoke during deliberations, what they spoke about, and the impact of their speech on their status in the jury (Strodtbeck, James, & Hawkins, 1957; Strodtbeck & Mann, 1956). Strodtbeck found, for example, that blue-collar workers and women were automatically (quickly, and without discussion of the reasons why) assigned lower status than white-collar workers and men in many juries.

The contributions that Harvard made during the 1950s to the study of small groups were not limited to Bales and his students. Other Harvard faculty and students, more closely linked to sociology than to psychology, deserve credit as well. One example is George Homans, whose book *The Human Group* (1950) attracted widespread attention and generated considerable discussion about groups. In that book, Homans offered a general system for analyzing all groups and applied that system to several actual groups, relying on descriptive studies of the groups for his data. Homans also made some important contributions to the development of social exchange theory (see Homans, 1958), which blended ideas from behavioral psychology and economics to explain why people develop, maintain, and sometimes end social relationships. And one should not forget Joseph Berger, Bernard Cohen, and Morris Zelditch, all sociology students at Harvard during the 1950s. These frequent collaborators went on to develop status characteristics theory (Berger, Cohen, & Zelditch, 1966), a cognitive account of how status systems develop within groups. That theory has generated much work by a variety of researchers over the years.

Researchers at many other institutions were also busy studying groups during the 1950s, focusing on such phenomena as problem solving and leadership (see Hoffman, 1965), communication networks (see Collins & Raven, 1969; Shaw, 1964), and conformity and independence (see Prislin & Crano, this volume). Although problem solving and leadership continued to elicit substantial interest after the 1950s, work on communication networks and conformity declined precipitously, though for different reasons.

Interest in communication networks was stimulated by the work of Bavelas (1950) and Leavitt (1951), who (like Bales) wanted to understand how communication patterns influence group processes and outcomes. By the end of the 1950s, a large literature had developed on how different communication networks (e.g., wheel, chain, circle) affect group performance and member satisfaction. In a review of this literature, Shaw (1964) identified several interesting problems that seemed worthy of future investigation, but few of these were studied in the years that followed. Why? Two factors kept researchers away from this topic area, in our opinion. First, the highly artificial and impoverished experimental paradigms typically used to study communication networks may have caused people to

lose interest in the phenomena those paradigms were developed to study. Second, Shaw's elegant explanation of communication network effects (group members' freedom of action affects their satisfaction, whereas communication and task demands affect their performance) may have led researchers to conclude that nothing of interest was left to discover. However, there has been renewed interest recently in group communication focusing on how technology affects group process (e.g., McGrath & Hollingshead, 1994).

Interest in conformity and independence was stimulated by the work of Asch (1951, 1956), who wanted to understand the conditions under which people resist or yield to group pressure. This research is clearly relevant to the social reality motive identified by Festinger (1950). Asch was interested in how individuals respond when they find themselves disagreeing with group consensus on unambiguous issues, whereas Sherif (1935) was interested in how individuals initially develop group consensus (i.e., norms) on ambiguous issues. Asch placed participants in the uncomfortable situation of discovering that their judgments about simple perceptual stimuli (lengths of lines) disagreed with the unanimous judgment of several other people. Contrary to many subsequent characterizations of his work, Asch did not believe that conformity is the dominant response to group pressure (see Levine, 1999). In fact, Asch observed a substantial amount of independence in his studies, and he was able to increase this independence dramatically by having a single group member dissent from the erroneous majority by giving correct answers.

In the years following Asch's classic studies, researchers studied many determinants of conformity and independence (see Allen, 1965; Levine & Russo, 1987). Several of these were originally identified by Asch, including the extremity of the majority's response, the minority's response mode (public vs. private), the characteristics of the stimulus, and the relative sizes of the majority and minority (which received particular attention). Although Asch's theoretical and empirical work profoundly affected subsequent research on social influence in groups, interest in conformity and independence waned substantially by the middle 1960s. In our opinion, this occurred for two reasons. First, as in the case of communication networks, the constrained experimental paradigms employed in most social influence studies (e.g., absence of free discussion among participants) may have reduced investigators' interest. Second, no comprehensive theory was proposed that could both account for existing findings and suggest new hypotheses, so researchers may have concluded that further work was unlikely to be worthwhile.

Before moving on to the 1960s and 1970s, it is worth pausing a moment to reflect on the factors that led to the decline of interest in communication networks and conformity/independence. In regard to the reliance on artificial and constrained experimental paradigms, these cases are similar. However, in regard to the availability of satisfying theoretical explanations, the cases are quite different. For communication networks, there was "too much" theory; for conformity and independence,



there was “too little.” As we shall see later, these factors have also played an important role in the decline of other research areas.

### The 1960s and 1970s

All good things must end, and so it was with the popularity of research on small groups during the 1950s. During the 1960s, and especially the 1970s, social psychologists lost interest in studying groups and began to study individuals instead. A case in point is Leon Festinger, who began the 1950s by studying groups, but just a few years later proposed his famous theory of cognitive dissonance (Festinger, 1957). Although dissonance can play a role in group life (e.g., Aronson & Mills, 1959), it is usually studied at the individual level. Dissonance theory became a popular research topic during the 1960s and 1970s, along with attribution theory, which involves another individual-level phenomenon that can play a role in groups (see Calder, 1977) but is seldom studied that way.

Why did social psychologists turn away from research on groups as the 1950s ended? Several explanations have been offered. McGrath (1984), for example, argued that research findings accumulated more quickly than theoretical insights during the 1950s, leaving researchers confused and uncertain about whether (and how) to proceed. Zander (1979) argued that research on small groups became unattractive as people learned that the costs associated with such work (e.g., recruiting many participants, performing complex data analyses) often exceeded its rewards (e.g., publishing papers, obtaining research support, making important discoveries). And, as noted earlier, Steiner (1974) argued that interest in small groups declined during the 1960s because little societal conflict occurred during the 1950s.

Although research on small groups slowed considerably during the 1960s and 1970s, it did not stop. Important work was done during that period, much of it by people at the University of Illinois who were interested in various aspects of group performance (or locomotion). A good example is Ivan Steiner, who offered a highly influential classification of group tasks (Steiner, 1972). Steiner distinguished, for example, between unitary and divisible tasks. In unitary tasks, all group members do the same thing (there is no division of labor), and their contributions must be combined to yield a single outcome. Individual contributions can be combined in several ways. When a group performs a disjunctive task, for example, its outcome depends solely on the contributions of the best member, whereas the outcome of a group performing a conjunctive task depends solely on the contributions of the worst member. In divisible tasks, members do different things, so their responses must be coordinated. Steiner also proposed the important concept of process loss (which causes a group’s actual productivity to be lower than its potential productivity) and described its major causes (motivation and coordination losses). Finally, Steiner used his framework to explain how group performance is affected by such factors as a group’s size, composition, and payoff system.

Another important contribution by people at Illinois involves James Davis’s work on group decision making and problem solving. Davis used mathematical analyses to develop a social decision scheme (SDS) model of group decision making (Davis, 1973) that stipulated rules (e.g., unanimity, majority) by which the preferences of individual members could be combined into a group decision (see Stasser, Kerr, & Davis, 1989). Davis and his colleagues used the SDS model to study group decisions on judgmental tasks that do not have demonstrably correct solutions, especially mock jury decisions. The SDS model has also been applied to intellectual tasks that do have such solutions (Laughlin & Ellis, 1986) and to collective induction tasks that have both judgmental and intellectual characteristics (Laughlin, 1999). On collective induction tasks, group members observe regularities in a domain, propose hypotheses to explain those regularities, and then evaluate the adequacy of their hypotheses. A major goal of SDS research is to assess how the social decision scheme that a group uses varies as a function of task demands. Research has shown, for example, that initial faction size is a stronger determinant of the final group decision for some tasks than for others. Davis’s SDS model has also been extended and elaborated in various ways. For example, Kerr (1981) used it to study transitions from one configuration of member preferences to another, and Stasser and Davis (1981) used it to study changes over time in group members’ preferences and subjective certainty levels.

Yet another important person at Illinois was Fred Fiedler, who proposed and tested an influential contingency model of leadership effectiveness (Fiedler, 1964). Prior work on leadership had focused on leaders’ traits and behavioral styles, but neither of these approaches proved fruitful. Fiedler proposed that leadership effectiveness depends on the fit (compatibility) between (a) the leader’s personal style (task-oriented vs. relationship-oriented) and (b) the level of situational control offered by the group (e.g., the positivity of the leader’s relations with other members, the clarity of the group’s task, the power associated with the leadership position). Relationship-oriented leaders were said to be most effective in groups that offered moderate situational control, whereas task-oriented leaders were said to be most effective when situational control was either low or high. The model has received strong but not unequivocal support (e.g., Peters, Hartke, & Pohlmann, 1983). Although other contingency theories of leadership were developed later (e.g., normative decision theory, path-goal theory), Fiedler’s theory has remained influential, both in and out of social psychology.

Another topic studied at Illinois was coalition formation, in which subsets of group members agree to allocate payoffs to themselves and one another at the expense of people who are excluded from the coalition. Samuel Komorita and his colleagues tested predictions from several competing theories about which coalitions would form in a given situation and how members of those coalitions would divide the rewards that their coalitions generate (Komorita, 1984). In spite of their work,



social psychological research on coalitions became less popular as time passed, almost disappearing by the early 1980s. Komorita and Parks (1995) attributed that decline to two factors (both of which we cited earlier as reasons for the demise of early research on conformity), namely the artificiality of the experiments and the lack of adequate theories. But the coalition literature was positively littered with theories. The problem was not their number, but rather their narrowness and failure to specify the social processes that occur in coalition situations.

Finally, the contributions of Joseph McGrath must be mentioned. For some 40 years, beginning with his 1966 synthesis of small group research with Irwin Altman, McGrath was the foremost integrator and systematizer of small group research. Although he studied many topics, McGrath is perhaps best known for his analyses of (a) the role of temporal factors in group life (e.g., McGrath & Kelly, 1986); (b) groups and technology (e.g., McGrath & Hollingshead, 1994); and (c) groups as complex systems (e.g., Arrow, McGrath, and Berdahl, 2000). And one must not forget McGrath's 1984 book, which clearly reflected the Illinois interest in group tasks.

Several other aspects of small groups were studied during the 1960s and 1970s at places other than Illinois. Two notable examples are bargaining and attitude polarization. Hundreds of bargaining studies were conducted with dyads who played experimental games—frequently the (in)famous Prisoner's Dilemma game. As noted earlier in our discussion of Thibaut and Kelley (1959), such games place people in an interdependent relationship where they make choices from a set of predefined alternatives that influence their own and their partner's outcomes. By the middle 1970s, reviewers were expressing considerable pessimism about the state of bargaining research using such games (e.g., Nemeth, 1972; Pruitt & Kimmel, 1977). The reasons for this pessimism should not be surprising at this point. Once again, the artificiality of the experimental paradigms used to study bargaining (with the attendant difficulty of generalizing findings to the real world) and the absence of theory were often blamed. It is important to note that the decline in enthusiasm for bargaining experiments using matrix games did not mean that social psychologists lost interest in how people behave in competitive (typically mixed-motive) situations. As we shall see later, there has been substantial work over the past 40 years on such topics as social dilemmas and negotiations within and between groups.

Interest in attitude polarization began with Stoner's (1961) finding that people were more prone to advocate risky courses of action after participating in a group discussion of some situation than they were before the discussion, when considering the situation on their own. This finding, dubbed the "risky shift," evoked a tidal wave of studies attempting to determine the generality of the phenomenon and the psychological processes that produce it (see Cartwright, 1971; Dion, Baron, & Miller, 1970). That work produced some surprising results, namely that (a) group discussion sometimes leads people to advocate more cautious courses of action, rather than riskier ones, and (b) group discussion can produce opinion shifts on

issues that have nothing to do with risk or caution. As a result, the risky shift came to be viewed as just one example of group polarization (Moscovici & Zavalloni, 1969), a broader phenomenon in which people's opinions become more extreme (in whatever direction they initially favored) after group discussion than before (Lamm & Myers, 1978). After years of research, in which different theories about group polarization were proposed and tested (see Isenberg, 1986), both informational and normative influence (Deutsch & Gerard, 1955) appeared to be important factors. The dominant explanation involving informational influence is persuasive arguments theory, which claims that influence occurs when a person encounters new and compelling arguments during group discussion (see Burnstein & Vinokur, 1977). Theories that involve normative influence focus on the tendency of group members to compare themselves to others and their desire to differentiate themselves in a socially valued direction. Although other processes, such as social identity (e.g., Mackie, 1986), have since been implicated in group polarization, the fact that both informational *and* normative influence seem to play a role in that phenomenon effectively killed interest in it. Without the excitement associated with competing theories, researchers abandoned group polarization in favor of other topics.

### The 1980s and beyond

After a long, relatively quiet period during the 1960s and 1970s, research on small groups became more popular again in the early 1980s. From that point on, its popularity grew dramatically, apparently peaking just a year or two ago (see Wittenbaum & Moreland, 2008). As we noted earlier, Moreland, Hogg, and Hains (1994) attributed this rise in popularity to the growing influence of (a) European approaches to small groups and (b) theories and methods drawn from cognitive psychology. The most important European approaches to small groups were associated with Henri Tajfel's work on social identity and Serge Moscovici's work on minority versus majority influence.

Several historical accounts of work on social identity are available (e.g., Hornsey, 2008), so a brief description of that work should suffice. Tajfel was intrigued by the role that social categorization might play in intergroup relations. Is simply categorizing people as members of different groups enough to produce prejudice, stereotyping, and discrimination? To answer that question, Tajfel, Billig, Bundy, and Flament (1971) carried out two "minimal group" experiments, so called because most of the factors that normally produce conflict between groups were eliminated. These experiments showed that social categorization was indeed sufficient for the creation of intergroup biases (as reflected in participants' allocations of resources to ingroup and outgroup members). This surprising finding caught the attention of many researchers around the world. Some simply sought to replicate the results (usually with success), whereas others sought to make minimal groups even less meaningful to research participants, to see whether intergroup biases would still occur. Several people criticized the minimal group

paradigm, raising doubts about the results it produces (e.g., Bornstein et al., 2006; Hartstone & Augoustinos, 1995; Schiffmann & Wicklund, 1992). Nonetheless, the findings reported by Tajfel and his colleagues were generally accepted as evidence that social categorization is a sufficient condition for intergroup bias.

Several years later, Tajfel and Turner (1979) developed social identity theory to explain the apparent effects of social categorization on intergroup relations. That theory was built around three constructs: social categorization, social identity, and social comparison. Social categorization is a way of viewing people that emphasizes their group memberships rather than their personal qualities. Social identity is that part of the self-concept that derives from the groups to which a person belongs. It can be contrasted with personal identity, which derives from the qualities (e.g., appearance, personality, intelligence) that define someone as a unique individual. According to social identity theory, people are motivated to enhance their self-esteem, which is accomplished by creating and maintaining a positive social (and personal) identity. Finally, social comparison involves comparing groups with one another to determine which ones are best.

Social identity theory also described several options available to someone suffering from a poor social identity. One option is social mobility—leaving the ingroup to join a better outgroup. Another option is social competition—making the ingroup objectively superior to the outgroup on some criterion. (This was the option allegedly chosen by participants in minimal groups experiments.) A third option is social creativity—mental tricks that yield a more favorable intergroup comparison. These include identifying other (worse) outgroups with which to compare the ingroup; re-evaluating the criterion on which the ingroup seems inferior to make it seem superior; and focusing on a new criterion that makes the ingroup seem superior to the outgroup.

Many researchers have tested social identity theory, using a variety of paradigms. In general, the results have been supportive (see, for example, Bettencourt, Charlton, Dorr, & Hume, 2001; Brewer, 1979; Mullen, Brown, & Smith, 1992). Along the way, several variables that can moderate the strength of intergroup biases were also discovered. These include a person's status within the ingroup (Noel, Wann, & Branscombe, 1995), the perceived entitativity of the ingroup (Castano, Yzerbyt, & Bourguignon, 2003), and existential terror arising from thoughts of death (Castano, 2004). Consequences of social categorization other than intergroup biases in reward allocation were discovered as well. These include linguistic biases (Maass, Salvi, Arcuri, & Semin, 1989), perceptions of variability among group members (Boldry, Gaertner, & Quinn, 2007; Mullen & Hu, 1989), reactions to persuasive messages (Wilder, 1990), and especially the processing of information about group members (see Grier & McGill, 2000; Wilder, 1986).

Social identity theory has also led to new approaches for resolving conflicts between groups. One approach focuses on individuation—encouraging people to avoid social

categorization altogether. Evidence for the effectiveness of this tactic can be found in research on cross-cutting categorization (see Crisp & Hewstone, 2007). Another approach focuses on recategorizing people from different groups into a larger social category that includes everyone. There is good evidence that this tactic can weaken intergroup biases as well (Gaertner & Dovidio, 2000).

Of course, social identity theory has drawn some criticism (see Brown, 2000). For example, self-esteem enhancement does not seem to be as strong a motive as the theory implies (Aberson, Healy, & Romero, 2000; Abrams & Hogg, 1988; Rubin & Hewstone, 1998); intergroup biases are much weaker when people are asked to punish rather than reward ingroup and outgroup members (Mummendey & Otten, 1998); and intergroup biases can occur even when there is no clear outgroup (Gaertner, Iuzzini, Guerrero Witt, & Orifia, 2006). Despite these problems, social identity theory seems as strong today as when it was first proposed.

John Turner and several colleagues went on to develop self-categorization theory (see Turner, Hogg, Oakes, Reicher, & Wetherell, 1987), which is related to, but different from, social identity theory. Three differences between the theories are noteworthy. First, self-categorization theory offers a more sophisticated analysis of the process by which people sort themselves and others into groups. That process is guided by the meta-contrast principle, which leads people to search for social categories that minimize differences among people within the same group and maximize differences between groups. An important byproduct of such categorization is the development of a group prototype—a mental image (shared by those who view themselves as group members) of the kind of person who best embodies whatever qualities make the group distinctive. Second, self-categorization theory seems to downplay the role of self-esteem as a motivational factor. Later formulations of the theory emphasize uncertainty reduction (see Hogg, 2007; Hogg & Abrams, 1993), arguing that people join and remain in groups because membership helps them make sense of the social world and their place in it (cf. Festinger, 1950). Finally, self-categorization theory has stimulated research on intragroup (as opposed to intergroup) relations. In fact, the theory has been applied to nearly every aspect of groups, including cohesiveness (Hogg, 1992; Van Vugt & Hart, 2004), conformity and deviance (e.g., Marques & Yzerbyt, 1988; Reicher, Spears, & Postmes, 1995), leadership (e.g., Ellemers, de Gilder, & Haslam, 2004; Hogg & van Knippenberg, 2003), and productivity (e.g., van Knippenberg, 2000; Worchel, Rothberger, Day, Hart, & Butemeyer, 1998).

The “social identity approach” to groups, which combines social identity and self-categorization theory (see Postmes & Jetten, 2006), has played a critical role in revitalizing social psychological research on small groups. Moreover, this approach has also begun to influence such work in other fields, including organizational behavior, sociology, communication, and political science. Theoretically speaking, the social identity approach is now the major game in town.

Tajfel was not the only European whose work encouraged people to study groups. Moscovici's (1976, 1980) work on minority influence was also important. In that work, Moscovici attacked the prevailing view that social influence in groups is unidirectional, with (numerical) majorities influencing minorities but not vice versa. He argued that social psychologists had paid too much attention to social control (majority influence) at the expense of social change (minority influence). In explaining minority influence, Moscovici focused on the production and resolution of conflict in groups and the importance of the minority's behavioral style (particularly its consistency). Moreover, he suggested that majority and minority influence represent qualitatively different means of resolving conflict. Minorities focus on the social implications of conflict, which leads them to accept the majority's position publicly but not privately. In contrast, majorities focus on the issues underlying conflict, which leads them to accept the minority's position privately but not publicly.

This novel viewpoint stimulated considerable work on both minority and majority influence by researchers on both sides of the Atlantic (see Wood, Lundgren, Ouellete, Busceme, & Blackstone, 1994). Moscovici's ideas also stimulated the development of competing theories arguing that majority and minority influence are based on either one process or two processes and that the antecedents and consequences of the two kinds of influence are either invariant or contingent on such factors as the source's ingroup versus outgroup status (e.g., Crano, 2010; Kruglanski & Mackie, 1990; Latane & Wolf, 1981; Nemeth, 1986; see Martin & Hewstone, 2003, for a review). Aside from the theories that it tested, research on minority and majority influence changed in other ways over the years. Over time, researchers turned away from the social dynamics of groups and toward the cognitive dynamics of individuals (see Levine & Kaarbo, 2001; Stroebe, 2010). This led to a heavy reliance on persuasion paradigms and a focus on how people process messages attributed to ostensible minorities or majorities. But with a few notable exceptions (e.g., Crano, 2010; Martin & Hewstone, 2008), the latter kind of work has recently become less popular, and more researchers are now studying minority influence within interacting groups (e.g., Levine & Choi, 2010; Prislin, 2010).

Moreland and his colleagues (1994) also attributed the rise in popularity of small group research during the 1990s to a growing enthusiasm for cognitive theories and methods. Early examples of that approach include research on cognitive processes in stereotyping (e.g., Hamilton, 1981), perceptions of variability among group members (e.g., Jones, Wood, & Quattrone, 1981), and encoding and recall of information about group members (e.g., Rothbart, Fulero, Jensen, Howard, & Burrell, 1978). By the 1990s, research on small groups often featured cognitive theories and methods.

A good example of this trend is group brainstorming research (see Paulus & Brown, 2003, for a review). That research began soon after Osborn (1957) described brainstorming as an effective way to capture the creative power of groups. For

effective brainstorming, Osborn argued that certain rules must be followed (do not criticize others' ideas; be as wild and crazy as you like; produce as many ideas as possible without worrying about their quality; try to build on the ideas of others). Early research showed that people brainstorming together were indeed more productive than people brainstorming alone. But it soon became clear that this was an inappropriate comparison, so researchers began to compare the productivity of interacting brainstorming groups with that of "nominal" groups containing the same number of individuals brainstorming alone. That comparison yielded disappointing results; in study after study, nominal groups were more productive than interacting groups in terms of both the number and quality of their ideas (see Lamm & Trommsdorff, 1973; Mullen, Johnson, & Salas, 1991). This is ironic because members of brainstorming groups often believe that they are very productive, perhaps because they cannot distinguish their own ideas from those of others.

Why are brainstorming groups less productive than they "should" be? In an influential paper, Diehl and Stroebe (1987) proposed and tested three possible explanations, namely that when people brainstorm together, they (a) indulge in more free riding; (b) suffer more evaluation apprehension; and (c) undergo production blocking (everyone cannot speak at once, so people must take turns, causing them to forget some ideas while they are waiting). The results of their research led Diehl and Stroebe to conclude that production blocking was the most likely explanation. Additional explanations were later offered, including the tendency for group members to match their performance to that of their least productive colleagues (Paulus & Dzindolet, 1993).

In spite of the evidence that group brainstorming is ineffective, many scientists and practitioners seem reluctant to abandon it. For example, some practitioners have argued that group brainstorming has special benefits that can compensate for its apparent inability to produce many ideas (Sutton & Hargadon, 1996). And several scientists have searched for interventions that can make group brainstorming more productive. These include goal setting (Litchfield, 2008; Wegge & Haslam, 2005), training group members (Baruah & Paulus, 2008), providing a facilitator (Offner, Kramer, & Winter, 1996; Oxley, Dzindolet, & Paulus, 1998), and arranging for group members to exchange ideas in written rather than oral form (Paulus & Yang, 2000). The latter intervention is often implemented through special computer software, and several studies show that such software does improve the productivity of brainstorming groups, especially when the groups are larger and their members are allowed to remain anonymous (see Dennis & Williams, 2005; De Rosa, Smith, & Hantula, 2007).

Some scientists, convinced that the exchange of ideas among group members must somehow help each person develop new ideas, have pursued another option (see Brown, Tumeo, Larey, & Paulus, 1998; Nijstad & Stroebe, 2006). In accordance with the general trend we described earlier, these researchers have adapted theories from cognitive psychology about idea generation by individuals to explain idea generation within groups.



And in some cases, they have tested their theories using methodologies (e.g., computer simulation) associated with cognitive psychology. A good example is the SIAM (Search for Ideas in Associative Memory) model of group brainstorming proposed by Nijstad and Stroebe. Their model assumes that idea generation is a repeated search process in associative memory that occurs in two stages (knowledge activation and idea production) and is controlled through negative feedback loops and cognitive failures (when no idea is generated). This model has been used to clarify many aspects of group brainstorming, including why production blocking limits group productivity, how ideas suggested by others stimulate a person's thinking, and how cognitive failures affect a group's satisfaction with brainstorming and decisions about whether or not to stop brainstorming.

Enthusiasm among small group researchers for theories and methods drawn from cognitive psychology has also led to another kind of work that focuses more directly on groups rather than individuals. This work involves what has come to be called "socially shared cognition"—phenomena that involve collaboration among people who are processing information together (see Levine, Resnick, & Higgins, 1993; Nye & Brower, 1996; Tindale, Meisenhelder, Dykema-Engblade, & Hogg, 2001). Consider, for example, research by Stasser and his colleagues showing that group members often fail to discuss all the information they possess, concentrating instead on whatever information they initially share (see Stasser, 1999). This bias causes problems when the unshared information is needed for a correct group decision. Fortunately, the tendency for groups to discuss shared information is weaker under some conditions, such as when discussions are longer (e.g., Larson, Foster-Fishman, & Keys, 1994), group members know about everyone's expertise (e.g., Stasser, Stewart, & Wittenbaum, 1995), and the task has a demonstrably correct answer (e.g., Stasser & Stewart, 1992).

Other work on socially shared cognition that has clear links to Festinger's (1950) idea of social reality involves group information processing (Hinsz, Tindale, & Vollrath, 1997; Larson & Christensen, 1993), group beliefs (Bar-Tal, 1990), social representations (e.g., Moscovici, 1981), shared reality (e.g., Kruglanski, Pierro, Mannetti, & De Grada, 2006; Levine & Higgins, 2001), shared mental models (e.g., Cannon-Bowers, Salas, & Converse, 1993; Klimoski & Mohammed, 1994), and transactive memory systems (e.g., Moreland, 1999). Work on the last two topics has been spurred by the burgeoning interest in team performance in general (see Hackman & Katz, 2010; Kozlowski & Ilgen, 2006) and team cognition in particular (see DeChurch & Mesmer-Magnus, 2010). We focus here on transactive memory systems.

Wegner (1987) analyzed transactive memory in dyads, but his ideas have often been applied to groups, especially work groups. Wegner noted that people can serve as memory aids for one another, if they know who knows what and can rely on one another to communicate what is known when that knowledge is required. As group members spend more time together and

develop stronger relationships, they develop a transactive memory system—a shared awareness of who knows what (or who is good at what). This system gives each person access to far more information than he or she would otherwise have.

Groups with stronger transactive memory systems are likely to perform better for a variety of reasons (e.g., greater access to information, better coordination, improved problem solving), and the performance benefits of transactive memory have indeed been demonstrated in several studies, including experiments on laboratory groups (see Lewis, Lange, & Gillis, 2005; Moreland, 1999) and field studies of natural groups (e.g., Austin, 2003; Palazzolo, 2005). Most of the field studies have involved work groups, but recent studies have focused on other kinds of group, such as teams of students working on course projects (Jackson & Moreland, 2009).

Of course, the European and social cognition approaches have not completely dominated research on small groups since 1980. The broad area of social conflict also received considerable attention (see De Dreu, 2010). We will highlight two important lines of relevant work—social dilemmas and intragroup/intergroup negotiation.

Social dilemmas encompass a broad range of situations in which group members face a conflict between their individual interests and the collective interest of their group. We will restrict our discussion to social dilemmas involving groups of three or more people (*N*-person dilemmas). In social dilemmas, each group member profits more from a competitive than a cooperative choice, regardless of what others choose, but all group members do worse if everyone fails to cooperate. Social dilemmas have drawn attention because they occur frequently in everyday life and have important consequences. Much of the research on social dilemmas has focused on commons dilemmas (in which group members harvest resources from a common pool) and public goods dilemmas (in which members contribute resources to a common pool).

Social psychological work on dilemmas began in earnest with the publication of influential papers by Dawes (1980; Orbell & Dawes, 1981) and Messick and Brewer (1983). Numerous studies were subsequently conducted to identify factors that influence behavior in social dilemmas and assess the effectiveness of structural solutions for those dilemmas (see Kerr & Park, 2001; Komorita & Parks, 1994). Characteristics of group members (e.g., social motives, level of group identification, self-efficacy) and aspects of the dilemma situation (e.g., group size, whether the game is framed as "give some" vs. "take some," opportunity for group discussion) were both found to influence cooperation in dilemmas. Discussion is especially important, because people who make commitments to cooperate during discussion often honor those commitments later (Kerr & Kaufman-Gilliland, 1994). Moreover, various structural solutions to dilemmas, such as electing a powerful leader, privatizing resources, and using a sanctioning system (e.g., Samuelson, Messick, Rutte, & Wilke, 1984; Yamagishi, 1986), have been found to increase cooperation, at least under some conditions. Two relatively



recent trends in social dilemma research are a focus on applied issues, such as the use of public transportation (e.g., van Vugt, van Lange, & Meertens, 1996), and the use of computational modeling to test theories (see Kerr & Park, 2001, for a review).

Both intragroup negotiation and intergroup negotiation also received increased attention in the 1980s and 1990s (e.g., Bazerman, Mannix, & Thompson, 1988). In regard to intragroup negotiation, several studies identified factors (e.g., self-interest and fairness concerns, preference distributions, decision rules) that affect negotiation strategies, coalition formation, and resource distribution (e.g., Thompson, Mannix, & Bazerman, 1988; Van Beest & Van Dijk, 2007; Weingart, Brett, Olekalns, & Smith, 2007). In regard to intergroup negotiation, some research examined negotiation between group representatives (e.g., Pruitt & Carnevale, 1993; Steinel, De Dreu, Ouwehand, & Ramirez-Marin, 2009) and negotiation between teams (e.g., Morgan & Tindale, 2002; Thompson, Peterson, & Brodt, 1996). However, much of the work focused on what has been called the “interindividual–intergroup discontinuity,” a robust finding that intergroup relations are more competitive than interindividual relations (Schopler & Insko, 1992; Wildschut, Pinter, Vevea, Insko, & Schopler, 2003). Of the several explanations proposed for this effect, fear and greed have received the most support—group members fear being exploited by an outgroup, so they compete fiercely with it, and group members are greedy for better personal outcomes, which can be obtained by competitive behavior that improves the ingroup’s outcomes (Wildschut & Insko, 2007).

The topic of social justice has a long history in social psychology, and several theories have been offered regarding its antecedents and consequences. A major distinction has been made between *distributive* justice (e.g., Adams, 1965; Walster, Walster, & Berscheid, 1978), which focuses on the fairness of one’s outcomes, and *procedural* justice (e.g., Lind & Tyler, 1988; Thibaut & Walker, 1975), which focuses on the fairness of the procedures generating those outcomes. Over the years, research attention has shifted from the former to the latter, and several models of procedural justice have been developed and applied to group processes (e.g., Tyler & Blader, 2000; Tyler & Lind, 1992).

## Conclusions

We will conclude our history of small group research by offering some brief speculations about the future of the field. One issue of enduring interest involves theory—do we have too few or too many theories, and, in either case, do we have the right ones? Theories, of course, vary in breadth. At the narrow end of the continuum are models designed to explain specific group phenomena. At the broad end are general approaches to the study of groups. In contrast to theories about specific phenomena, which can come and go rather quickly, general approaches tend to persist for longer periods. In our opinion, two such approaches—the social identity and evolutionary

approaches—will have a major impact on the field in the coming years. As already noted, the social identity approach is currently an important perspective on small groups, and we do not envision its imminent demise. And the evolutionary approach is becoming increasingly influential, because of its ability to generate novel and interesting hypotheses (e.g., Kameda, Takezawa, & Hastie, 2003; Van Vugt, 2006).

Moving from theories to processes and phenomena, what other predictions can be made? Our crystal ball (cloudy as it may be) suggests that cognitive processes will continue to elicit substantial research attention, though whether this work will focus on groups *qua* groups or on individuals embedded within groups is an open question. Moreover, it is likely that the emphasis on cognitive processes (of whichever sort) will be tempered by increasing attention to the role of affect in group life (e.g., Kelly & Barsade, 2001; Thompson, Medvec, Seiden, & Kopelman, 2001).

Interest in understanding and improving the performance of work teams (see Kozlowski & Ilgen, 2006) is likely to remain strong for the foreseeable future, and relevant research will probably be shaped by certain features of modern teams. These include a fluid set of members who are distributed across space and time, operate under emergent and distributed leadership, and interact using technology. To discover what makes these teams effective, researchers are likely to devote special attention to such topics as team learning, team stability, compositional diversity, team development and socialization, and technologically based communication (Hackman & Katz, 2010). In studying teams, researchers are also likely to employ a broader set of methodologies, including social network analysis and computational modeling.

The explosive growth of groups on the internet also opens up exciting research opportunities for group researchers (McKenna & Green, 2002). Online groups provide information, emotional support, and recreation to millions of people. Although increasing research attention is being devoted to such groups, relatively little effort is being made to integrate the resulting findings with theory and data from face-to-face groups. Not only are online groups a new social phenomenon worthy of study in their own right, but they also provide valuable, and in some cases unique, opportunities to study general group processes. The large number of these groups, their variety, and the rich data they provide about members’ interactions allow researchers to answer questions about temporal changes in group composition, structure, and process that are difficult, if not impossible, to answer in laboratory groups and in most other natural groups.

Finally, we offer a few comments about the general health of small group research. There are reasons to be both optimistic and pessimistic. On the positive side, much excellent research on groups is being published in social psychology and other disciplines, particularly organizational behavior. And research on socially shared cognition provides a common intellectual focus for (at least some) researchers interested in groups and others interested in individual cognition. On the negative side,

there is a chronic over-reliance on laboratory experiments and a disproportionate emphasis on some topics (e.g., intergroup relations) at the expense of others (e.g., group composition, group structure, the ecology of groups). We also worry that group research may be losing visibility within the broader field of social psychology. One reason is the availability of specialized journals devoted to groups that may siphon papers away from mainstream outlets read by colleagues in other areas. Such specialized journals (which certainly have benefits as well as costs) not only reduce our visibility to colleagues with other interests, but also reduce their visibility to us, increasing the likelihood that we will miss developments that could improve our work. And insofar as our journals become even more specialized by focusing on particular kinds of groups (e.g., work teams, therapy groups), researchers who study groups of different kinds will have less and less in common. Clearly we must be vigilant to these risks and work to build and maintain bridges across boundaries both within our field and between it and other fields (Moreland & Levine, 2009).

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